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THE HISTORY OF THE INTERVIEW

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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. 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.” It is not always easy to decide what should be treated as a part of interviewing as such; for instance, some discussion of interview questions is about the construction of schedules, without reference to how the questions are presented to the respondent. Here, the focus is on what happens while the interviewer is interacting with the respondent.

At each stage, the more fully institutionalized practices have been less likely to be written about in detail, except for the purpose of guiding trainees; therefore caution needs to be exercised in generalizing from the prescriptive literature to current practice. In principle, we aim here 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. Descriptions of it are more common when some aspect becomes salient because it is seen as novel, unconventional, or problematic. Even then, what is described is commonly a policy or strategy rather than the actual practice, which may not always conform to the policy. Thus, for our historical account, we have to draw largely on prescriptions for practice as it should be.

We concentrate on the book literature; the main points in the journals will have been taken up in books if they were practically influential, so this is adequate for a broad overview. It is with regret that the decision had also to be made, given the limitations of space, to focus almost entirely on the U.S. experience. For the prewar period, especially its earlier part, this can be quite misleading, as other national disciplines 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 they can perhaps be treated as representing the whole; after the high period of U.S. hegemony, however, this approach

becomes less reasonable. This chapter is 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. Those from other backgrounds are urged to supplement my examples with their own.

The U.S. book literature on interviewing falls into a number of categories, of which some illustrative examples are listed in Table 1.1. (Where possible these are chosen from works not extensively discussed below, to indicate more of the range of material.) There are relatively distinct intellectual and practical traditions here, despite overlaps and some strong influences across traditions, and this needs to be taken into account in placing the stances and concerns of single texts.

We concentrate on social-scientific interviewing, but that has not always been distinguished from the interviewing techniques of psychiatrists, social case-workers, or personnel managers. When it has been so distinguished, work in such fields has still often been drawn on by social scientists. But the character of the literature has changed historically. The earliest relevant work was not specifically social scientific. As new practices such as polling and bodies such as survey organizations emerged, they generated writing that expressed their concerns and led to methodological research on issues they were interested in. 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 the more philosophical aspects. Textbooks regularly strove to keep up with the main developments, while authors of empirical studies wrote about the experiences and needs specific to their particular topics. In later times, as the quantitative and qualitative worlds became increasingly separate, their discussions of interviewing diverged correspondingly. The quantitative 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 link has recently been established in the use by surveyors of conversation-analytic techniques to analyze what is happening in their questions and answers.

Below, a broad outline of the trajectory of the field is sketched in via selected examples of such writings, starting with the prescriptive methodological literature and going on to empirical work that has been treated as methodologically important. We then review some key analytical themes. The literature of research on interviewing is looked at as much for

Table 1.1 Genres of Books Related to Interviewing

<i>Genre</i>	<i>Examples</i>
Practitioner textbooks	Garrett, <i>Interviewing: Its Principles and Methods</i> , 1942
Polling practice	Gallup, <i>A Guide to Public Opinion Polls</i> , 1944
Social science methods textbooks	Goode and Hatt, <i>Methods in Social Research</i> , 1952
Instructions to survey interviewers	University of Michigan, Survey Research Center, <i>Manual for Interviewers</i> , 1954
Critiques of method, general or particular	Christie and Jahoda, <i>Studies in the Scope and Method of "The Authoritarian Personality,"</i> 1954; Cicourel, <i>Method and Measurement in Sociology</i> , 1964
Empirical work discussing its methods	Kinsey, Pomeroy, and Martin, <i>Sexual Behavior in the Human Male</i> , 1948
Handbooks	Denzin and Lincoln, <i>Handbook of Qualitative Research</i> , 1994
Monographs on special groups, novel approaches	Dexter, <i>Elite and Specialized Interviewing</i> , 1970; Douglas, <i>Creative Interviewing</i> , 1985
Philosophical/theoretical discussion	Sjoberg and Nett, <i>A Methodology for Social Research</i> , 1968
Reports of methodological research	Hyman, <i>Interviewing in Social Research</i> , 1954

what the concerns reflected there show us about the researchers' focuses of interest as for what the findings have been, though research has surely influenced practice. The interlinked issues of changing interest in and thought about validity, conceptions of the appropriate social relations between interviewer and respondent, and the types of data sought by those working in different styles are briefly explored; some effort is made 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, the strands are drawn 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, a sequence of arguably representative accounts of interviewing, in particular its forms and purposes, is presented below in order of historical appearance. Key points of content and assumptions are outlined, and each is briefly placed in its context.

HOWARD W. ODUM AND KATHARINE JOCHER, AN INTRODUCTION TO SOCIAL RESEARCH, 1929

This was one of the first general social science methods textbooks. In it, in addition to "interview," "schedule" (to be used by an enumerator) and "questionnaire" (to be answered unaided) are mentioned; for these, there is a discussion of questions and presentation but nothing on interviewing as such. (At this time, the conduct of structured interviews was not treated as being at all problematic and so was hardly discussed.) It is stated that

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. . . . If the student is not acquainted with the informant, some method of

introduction through a mutual acquaintance should be secured. (pp. 366–367)

Permission to take notes should be requested.

As here, in the 1920s and 1930s, an "interview" was often assumed to be of a key informant or gatekeeper rather than a respondent who is merely one member of a sample (cf. Bingham & Moore, 1931; Fry, 1934). The implicit model of the old, fact-finding survey in the Booth tradition is still in the background; Booth's data on the working-class family were provided by middle-class visitors (Bales, 1991). The interviewee may thus be an informant about the situation studied, as much as or more than being a part of it, and potentially of a status superior to the interviewer, another reason for allowing the respondent to structure the interaction. This does not mean that no questionnaires to mass samples were being used, though they were not common yet in academic social science, but that this was seen as a distinct method. It was often recommended that notes should not be taken during the interview, or only to a minimal extent, but that 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, e.g., Converse, 1987, p. 51). Clearly the role of respondent was not yet so institutionalized that no need to conceal the mechanics was felt.

PAULINE V. YOUNG, SCIENTIFIC SOCIAL SURVEYS AND RESEARCH, 1939

This was a very successful general methods textbook. "Interview" is again distinguished from "schedule" and "questionnaire," which are dealt with separately. Young distinguishes respondents who are adequate sources on factual matters from those who are of interest as subjects, individually or in relation to the larger situation. A personal introduction to the respondent 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 she 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. . . . [T]he field worker . . . needs to know in a general way why he is interviewing this particular person or group and what he intends asking . . . [but] needs to be open to unforeseen developments. (pp. 175, 179)

As few questions as possible should be asked:

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)

It is rarely advisable to complete an interview at one sitting (p. 195). It is better not to take notes, except maybe a few key words, and it is seen as controversial whether to record the interview in the first or the third person and whether a verbatim account is to be preferred to a summary by the interviewer (pp. 196, 200).

Young's department at the University of Southern California 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 to draw on. Its perceived relevance owed something to the widespread use by sociologists, especially at the University of Chicago where she was trained, of case histories collected by social workers; 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, p. 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, despite his strongly scientific tastes, though with a slight twist in the direction of the more modern concern with personality and psychoanalytical interests.

By the 1949 edition of her text, Young had mentioned the modern survey, though she was 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.

CHARLES F. CANNELL AND ROBERT L. KAHN, "THE COLLECTION OF DATA BY INTERVIEWING," 1953

This is a chapter in what became a standard general methods text, written by a group from the University of Michigan's Institute for Social Research (ISR). Cannell and Kahn, the former a clinical and the latter a social psychologist, were members of the team that became the wartime Division of Program Surveys (DPS) and after the war transformed itself into the Institute for Social Research. 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*, Kahn & Cannell, 1957, carries this forward, coming to the formulation of objectives and questions only after three chapters on the interviewing relationship.)

The following extract shows their relatively qualitative orientation, which nonetheless goes with a strong commitment to scientific procedure; one may detect some tensions 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. . . . 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 deeper-lying characteristics can be inferred. . . . [T]he interviewer cannot apply unvaryingly a specified set of techniques, because he is dealing with a varying situation. . . . [T]he 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. . . . [T]he interviewer's role with respect to the questionnaire is to treat it as a scientific instrument designed to administer a constant stimulus. (pp. 332, 358)

Cannell was a doctoral student of Carl Rogers, recruited to the DPS to draw on what he had learned with Rogers about nondirective styles of questioning. It is assumed in the book that a 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 groups and explains some of the assumptions made here about interviewing. At an early stage, there was controversy between the proponents of closed and open questions, contrasted by one participant in the DPS as the "neat reliables" and the "sloppy valids." This was reflected in a classic article by Paul F. Lazarsfeld (1944), in which he aimed to resolve the conflict between wartime research outfits with divergent styles. Converse (1987, pp. 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 but also was impossible to sustain with less educated interviewers scattered across the country, making training and supervision difficult.

***SELLTIZ, JAHODA, DEUTSCH, AND COOK,
RESEARCH METHODS IN SOCIAL
RELATIONS, 1965***

This classic textbook written by psychologists has passed through many editions. It still distinguishes between "interview" and "questionnaire," seeing the interview, which may be structured or unstructured, as practically advantageous because it does not require literacy and has a better response rate than postal questionnaires, is more flexible, and is "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 is on question wording, without distinguishing interview from questionnaire, and clearly a standard survey interview, by now well

established, is what they have in mind. 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 endeavoring to keep the respondent to the point" (p. 576) and 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). Many of those involved in the early development of polling and market research using 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. This shows development well beyond the approach of the early Gallup (1944) 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, and the need for accuracy and precision was emphasized, but uniformity of stimulus was not given the importance that it later acquired; validity was seen primarily in terms of getting the public predictions right.

***GIDEON SJOBERG AND ROGER NETT,
A METHODOLOGY FOR SOCIAL
RESEARCH, 1968***

This is quite a new genre of work, reflecting wider movements in sociology. The authors were not closely involved with survey units and were not writing 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"—as well as saving 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)" (pp. 193–195).

Four types of unstructured interview are described: (1) the free-association method, (2) the focused

interview, (3) the objectifying interview, and (4) the group interview. Of these, the objectifying interview is preferred:

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

Throughout the discussion, there is a stress on the social assumptions built into different choices of questions. Status effects in the interview situation, and the consequences of varying cultural backgrounds, especially for work in the Third World, are discussed.

The authors approached the matter from a theoretical and—in a turn characteristic of the period—a sociopolitical perspective; it was proposed to involve the respondent as an equal, not so much for instrumental reasons of technical efficacy as because a nonhierarchical, nonexploitive relationship is seen as intrinsically right. It is also noticeable that this is a sociologists' version; there is no orientation to psychologists' usual concerns. Although Galtung (1967) and Denzin (1970) wrote books more like conventional methods texts, those have key features in common with Sjoberg and Nett's book: the more theoretical and philosophical interests, the more distanced approach to surveys and their mundane practicalities, and a clearly sociological frame of reference. Interviewing of various kinds has now become a standard practice to which even those with theoretical interests relate their ideas.

**STEVEN J. TAYLOR AND ROBERT BOGDAN,
INTRODUCTION TO QUALITATIVE
RESEARCH METHODS, 1984, SECOND
EDITION**

This is a specialized methods textbook, again with a strong standpoint:

In stark contrast to structured interviewing, qualitative interviewing is flexible and dynamic . . . [with] 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)

Without direct observation to give context to what people say in an interview, the responses may not be adequately understood, and there may be problems of deception and distortion; it is important, therefore, to interview in depth,

getting to know people well enough to understand what they mean and creating an atmosphere in which they are likely to talk freely. . . . [I]t is only by designing the interview along the lines of natural interaction that the interviewer can tap into what is important to people. . . . [T]he interviewer has many parallels in everyday life: "the good listener," "the shoulder to cry on," "the confidante." . . . [T]here 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. 82–83, 93–94, 101)

This reaction against "robotlike" standard survey interviewing is part of the growth of a separate, "qualitative" stream, recommending many practices anathema to surveyors. 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, pp. 265–269). The ideal is clearly participant observation or ethnography, and this type of interviewing again blurs the boundary with them. It could not be adapted to large representative samples without

enormous costs, and makes implicit assumptions about likely research topics that, one somehow infers, exclude (for instance) the demographic or economic. Other representatives of this broad tendency are Douglas (1985), Holstein and Gubrium (1995), and Potter and Hepburn (2005). Potter and Hepburn set such high conversation-analytic standards and emphasize the significance of the interaction between interviewer and respondent so heavily that, after recognizing that the necessary quality would be bought at the expense of sample size, they suggest that it might be better anyway to use naturalistic records rather than interviews. (Perhaps their focus on interviewing for *psychology* may have led to a concern with fine detail less necessary for sociology or anthropology.)

Many feminists have practiced and argued in favor of similar styles on feminist grounds. 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)

She points out, however, that having close relations with every subject is not practicable and that too much emphasis on rapport may unduly limit the range of topics covered. (It is noticeable that the work she cites in this chapter is almost all on topics such as rape and hysterectomies.) The emphasis here is on letting the respondent's perspective dominate rather than analyzing the interaction with the interviewer. Recent advocacy of "narrative interviewing" goes further in the attempt to elicit narration with minimal intervention by the interviewer: "It is assumed that [uninterrupted] narrations preserve particular perspectives in a more genuine form" (Jovchelovitch & Bauer, 2007, p. 1), though the final interpretive product fuses the informants' relevance structures with those of the researcher.

One might speculate how much of this qualitative tendency rests on the increased availability of

good-quality portable tape recorders, which facilitate the detailed recording of free answers and their close textual analysis.¹ We may expect fresh creative developments facilitated by the digital revolution; there are already methodological and ethical discussions of the special features of online data collection.

♦ *Empirical Work and Its Influence*

Important contributions to discussion of interviewing have also been made by authors whose primary concern was their substantive topic; these do not necessarily relate directly to the professional methodological discussion and cannot be explained by their location within that. Below, we review some of them. It is probably not by chance that the empirical exemplars that come to mind, as well as much methodological research, are largely from work done in the period from 1935 to 1955. This was the time when the modern survey was emerging, and so the problems that its practice raised were live ones, confronted and argued over 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 is a conventional 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 to be sufficiently described by the schedule.

Roethlisberger and Dickson (1939/1964) make an early contribution to unstructured interviewing technique, though the intellectual responsibility for this arguably lies more with Elton Mayo, who led the work—his ideas on method were influenced both by his interest in Jungian psychoanalysis and by his friendship with the anthropologist and fieldwork pioneer Malinowski. The interviewing program reported started to collect employees' views about their work (for use in improving supervisor training), but it was 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' lead without changing the subject and asking only noncommittal questions. Interviews were recorded as far as possible verbatim,

¹In an earlier version of this chapter, I said that research on the consequences for practice of changing recording techniques and technologies was strikingly absent; Lee (2004) has responded with a valuable step toward filling that gap.

rather than under target headings, and the data were seen as information not so much on real problems as on the meanings that the worker 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” (pp. 286–287). This program, not initially intended for social-scientific purposes, became used for social science.

Warner and Lunt (1941) said that in their work they used techniques suggested by Roethlisberger and Dickson (1939/1964), although their research, an intensive community study, was of a very different character; Warner was an anthropologist by training, and the anthropological fieldwork tradition seems more relevant to their research. Many of their “interviews” were done without the subject’s awareness of being interviewed: “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” (Warner & Lunt, 1941, p. 46). Questionnaires were seen as liable to take items out of their social context and as useful only when one is already familiar with the general situation from interviews (Warner & Lunt, 1941, pp. 55–56). Although the authors called their main method “interviewing,” it should probably be regarded primarily as part of the history of what we now call participant observation.

Our next example, Kinsey, Pomeroy, and Martin’s *Sexual Behavior in the Human Male* (1948), is more idiosyncratic. Kinsey was a professor of zoology and devised techniques planned to suit his special topic. There was a list of items to be covered in the interview, but no fixed order or words for them, and additional items 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 the 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 aims of the interview were not at all concealed from respondents, and if they appeared not to be answering truthfully, the interview was broken off. Very long training was again seen as necessary for interviewers, who were also required, in the interests of confidentiality, to memorize a large number of codes for recording the answers. Any use of this method by others has not been identified in the mainstream sociological literature; Kinsey’s reasoning suggests that it would only have been appropriate in areas posing the same problems as sexual behavior (Kinsey et al., 1948).

Radically different, almost equally famous, and more influential in social science method was Adorno, Frenkel-Brunswik, Levinson, and 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” from “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 also 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 [Kinsey et al., 1948])

Perhaps surprisingly, given the lack of social-scientific precedent for Kinsey’s approach (Kinsey et al., 1948), Adorno et al. (1950) were treated more harshly in published critiques. Where the former were criticized, it was concluded that empirical evidence

for saying that their results were less valid than those of alternative approaches was not available (Cochran, Mosteller, & Tukey, 1954, pp. 78–79); Adorno et al. (1950) were, however, accused of inconsistency and speculative overinterpretation of data not appropriate for their use (Christie & Jahoda, 1954, pp. 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 colleagues. 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. . . . [S]uch 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 & Glazer, 1952, pp. 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, & Denney, *The Lonely Crowd*, 1950), which contains almost no direct interview data, and *Faces in the Crowd* (Riesman & Glazer, 1952), consisting mainly of raw interview data without analysis; the issue of how securely the data support the interpretation is thus avoided.²

The genre of publication of raw interview data has 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 thus:

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. (Parker, 1997, p. 169)

Thus, to treat the published version as showing just what took place in the interview would be misleading. Whole “life stories” have been published in sociology, though sometimes written by their subjects rather than elicited by interviewing;³ the genre was treated as of central importance in the interwar period, and much more recently, it has been revived. Some recent work on life stories (e.g., Atkinson, 1998) takes a similar approach—on the one hand, putting a very high value on the subject’s own version of events while, on the other hand, permitting the interviewer a considerable editorial role. 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, though, 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, pp. 129–130), and so some methodological ideas arise from the substance of the work being done: Kinsey’s conceptions of interviewing technique followed directly from what they saw as the requirements of work on sexual behavior (Kinsey et al., 1948). One might expect the influence of such work to follow the same paths, though whether it has done so cannot be explored here. It is clear that the choices of method did not simply follow from the current state of methodological discussion, though 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 Analytical Themes*

Discussions of empirical work have taken us a little nearer to what has happened in practice. Research on interviewing offers another window 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

²Later, however, in his chapter in *The Academic Mind* (Lazarsfeld & Thielens, 1958), Riesman (1958) contributed what is in effect—though he does not present it as such—an extended, research-based discussion of validity, based on respondent reports on the experience of being interviewed.

³James Bennett (1981) has suggested the circumstances under which some types of these appear appropriate.

are repeatedly shown to use their own ways of dealing with problems in eliciting the data wanted. 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 they were employed as "hired hands," without personal commitment to the research goal or control over the content and methods. Later 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 queries whether departure from specifications should be regarded as cheating—though 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. (An under-researched and under-theorized area of interviewing is that of the social relations between employed interviewers and their supervisors, and their consequences.) We do not know how far patterns such as those found by Peneff have held more widely, but we

ought not to be surprised if sometimes they do. In a very different style, Brenner (1982) elicited a large number of recordings of routine survey interviews and found that departures from instructions were common; individual interviewers showed considerable differences in asking questions as required and in probing. He treats this as a problem of interviewer skills and training rather than either "cheating" or creative fieldwork; the emphasis is on uniformity of stimulus, and he shows how departure from instructions could often lead to the collection of inadequate information.

Roth's (1966) and Peneff's (1988) 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 1.2.) It is not surprising that it should be those with continuing professional concern with the matter who do such work, but it does mean that the research has been skewed toward their distinctive preoccupations. What was 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 been variation in the answers elicited by different interviewers. This is commonly taken as a measure of "error," implying that validity is defined as arriving at the correct overall figures rather than as fully

Table 1.2 Key Works Presenting Research and Analysis on Interviewing

1947	Hadley Cantril, <i>Gauging Public Opinion</i>
1954	Herbert H. Hyman, <i>Interviewing in Social Research</i>
1965	Stephen A. Richardson, B. S. Dohrenwend, and D. Klein, <i>Interviewing: Its Forms and Functions</i>
1969	Raymond L. Gorden, <i>Interviewing: Strategy, Techniques and Tactics</i>
1974	Jean M. Converse and Howard Schuman, <i>Conversations at Random</i>
1979	Norman M. Bradburn and Seymour Sudman, <i>Improving Interview Method and Questionnaire Design</i>
1981	Charles F. Cannell, P. V. Miller, and L. Oksenberg, "Research on Interviewing Techniques"
1982	W. Dijkstra and J. van der Zouwen, <i>Response Behaviour in the Survey-Interview</i>
1984	Charles Turner and Elizabeth Martin, <i>Surveying Subjective Phenomena</i>
1990	Lucy Suchman and Brigitte Jordan, "Interactional Troubles in Face-to-Face Survey Interviews"
1991	Paul P. Biemer, R. M. Groves, L. E. Lyberg, N. A. Mathiowetz, and S. Sudman, <i>Measurement Errors in Surveys</i>
2002	Douglas W. Maynard, H. Houtkoop-Steenstra, N. C. Schaeffer, and J. van der Zouwen, <i>Standardization and Tacit Knowledge: Interaction and Practice in the Survey Interview</i>

grasping individuals' meanings or correctly identifying their real opinions. Cantril (1947) suggested that the problem of interviewer biases could be dealt with by selecting interviewers with canceling biases. Other writers saw careful selection of interviewers for their personal characteristics, whether of race or of personality, as valuable—though the real labor market often made this difficult. Fowler (1991, p. 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 interviewers for problems and to see the answer as more control over them. An extreme of this definition of the situation is suggested by Bradburn and Sudman's (1979) chapter on interviewer variations in asking questions, where the nonprogrammed interviewer behavior studied by tape recordings included minutiae such as stuttering, coughing, false starts, and corrected substitutions.⁴ Converse and 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 their interviewers were graduate students, members of “us” rather than “them.” Consequently, they emphasize the tensions interviewers experience between conflicting roles and expectations.

Later work, however, more often recognizes respondents' contributions and takes the interview as interaction more seriously. For Cannell, Miller, and Oksenberg (1981), the aim was to decrease reporting error due to the respondent rather than the interviewer. Because the study used in the research 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. It was 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 their performance.

More recent writing about “cognitive” interviewing has revived the issue of accuracy in ways that do deal with the issue of validity, if only in relation to “factual” questions. Suchman and 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. This article raised considerable discussion; perhaps its ideas would not have seemed so novel to the readership of a more social-scientific journal. 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. Schober and Conrad (1997) have shown that less standardized and more conversational interviewing can markedly increase the accuracy of the responses given—by, for instance, allowing the interviewer to help the respondents fit their relatively complicated circumstances into the categories of answer provided by the researcher. They illustrate the self-defeating extremes to which the pursuit of the uniform stimulus had gone, being used to forbid even the provision of guidance that would ensure that the meanings sought by the researcher were indeed conveyed in the answers chosen. It is noticeable that most of the examples used in these recent discussions are 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 of academics doing quantitative work to use high-quality data not created for their own purposes; that has led discussion in the directions suitable to the character of such work, but not equally applicable to the whole range of surveys.

Schober and Conrad's (1997) study exemplifies a recurrent pattern in which research shows that

⁴Some kinds of error, such as mistakes in following the schedule's instructions on which question to put next, have been eliminated by the computer-assisted methods now commonly used in survey organizations. Lyberg and Kasprzyk (1991, p. 257) point out, though, that computer-assisted telephone interviewing (CATI)-specific errors may still arise.

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 subject is by Bingham and 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; this 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 rapport needed to be established to get access and cooperation but that the interviewer should also when questioning appear unshockable, have no detectable personal opinions, and behind the front of friendliness be objective and scientific.⁵ Not every writer offered as business-like a conception of rapport as Goode and Hatt (1952), for whom rapport existed when the respondent “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 as ways of getting an informant “started” some devices—“to refer to important friends of the informant as if one were quite well acquainted with them; to tell of one’s own experiences or problems and ask the informant’s advice or reactions to them” (pp. 365–366)—of just the kind that survey organizations train their interviewers to avoid. Kinsey’s advocacy of a less impersonal and unbiased style was quoted above (Kinsey et al., 1948).

⁵This is another area where CATI must have changed the issues, though 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.

Elements of such an approach have now come round again in recent qualitative work, where there has often been a sociopolitical commitment to treat the respondent as an equal, which is taken to imply not playing a detached role while expecting the other party to reveal the self:

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 . . . get will be commensurable with the questions we ask and the way we ask them. (Fontana & Frey, 1994, p. 374)

This line can, however, be presented in a more manipulative way, as here in Douglas’s (1985) unique style:

Most Goddesses [beautiful women] 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 their perceptions of each other has shown that respondents do not necessarily detect the interviewer’s 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?

Holstein and Gubrium (1995) do not stress the interviewer’s revelation of self but treat the interviewer and the respondent as equal in another way, since both are creating meanings; both are also “active,” rather than the respondent being seen as just the passive object of the interviewer’s attempted control. For them, there is no such thing as the one correct answer to be found, but a range of possibilities depending on which of the respondent’s resources and potential standpoints are brought to

bear. The role of the interviewer is “to provide an environment conducive to the production of the range and complexity of meanings that address relevant issues and not be confined by predetermined agendas” (p. 17). The resultant conversation is not necessarily less authentic than “real,” normal ones, though the use of interviewers may be justified by their capacity to raise for comment matters on which everyday conversation is rare. Coding, by both interviewer and respondent, is seen as “endogenous to the interview” (p. 66), implicit in the emergent categories that they develop together to describe experience. When the materials collected in this way are put together to make a broader picture,⁶ it is certainly not done in quantitative terms, and this is clearly not an approach intended to be of use toward fact-finding or hypothesis-testing goals.

A method of data collection that cannot make plausible claims to validity is of no use, so it is surprising that a wide range of 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, though this has to be traded off against the greater reliability of the more structured methods. Concern with the problem has come more from those who employ other people to do their interviews; those who carry out their own interviews have usually seemed to regard their validity as self-evident and not requiring checks. This sometimes reflects a hostility to “science” or “positivism” prevalent among qualitative researchers. However, in the literature of the standard survey too there has been surprisingly little concern shown about validity as such. The question of the substantive meaningfulness of the data, except on purely factual questions, somehow gets elided in the concern about interviewer error and questionnaire improvement.

It is, of course, in the survey, as in other contexts, difficult to demonstrate validity, though some authors have suggested ways of doing so. Maccoby and 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, it has been used, but for many topics there is none. 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 only be applied in relatively deep and unstructured types of interview, where the interviewer has time to establish a relationship. For the “depth” or psychoanalytical style, of course, the issue of validity has not arisen in the same sense, since the focus has been not on correct factuality but on the interpretations made by the analyst. 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)

They assume the researcher to have ways of *knowing* that the respondent is lying. In intensive, long-term studies of a community, such as Warner and Lunt’s, that is a relatively plausible assumption; Vidich and Bensman (1954), conducting another such study, also report detecting much intentional misrepresentation. Plainly, however, in many other cases this assumption would not be met.

Galtung (1967) is one of the earliest representatives of what might be seen as a truly sociological position, even if it is 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 correspondence

⁶A remarkable discussion of the choice of good respondents (as distinct from a quantitatively representative sample) that, despite its sophisticated style, is reminiscent of some of the much earlier literature on informants.

between words and thoughts to that of how representative the interview situation is as social intercourse. (p. 124)

Holstein and Gubrium (1995) 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 takes it that there is no stable underlying reality to identify, thus in a sense abolishing the problem. 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 of bathroom equipment or medical treatment received. Each of the extremes of the discussion may write about "the interview," but they have had in mind different paradigms and different research topics and have shown little interest in the problems relevant to the needs and concerns of the other.

◆ *The Historical Pattern*

Not all the work reviewed fits into a clear historical pattern, and empirical studies may be idiosyncratic in relation to the methodological literature, but nonetheless we sketch a broad trajectory that thinking has followed. The dates suggested are not meant as precise; different workers move at different speeds.

Up to the later 1930s, the "interview" was distinguished from the "questionnaire," which was generally thought of as for self-completion; if it was administered by an interviewer, her 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 of professors themselves going into the field was a new one.) Little concern with reliability or validity was shown. A few rules of thumb were suggested for success. It was assumed that subjects might not accept overt interviewing, so some concealment was necessary. In parallel to this, however, much work was done under rubrics such as "life history," "fieldwork," and "case study," which we might call "interviewing" even if the writers did not. 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, e.g., Palmer, 1928, pp. 171–175).

Meanwhile, political polling and market research were developing. Here, interviews were conducted by forces 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 with a relatively elaborate fixed schedule. The work done was often to be published in the newspapers or was of direct commercial interest to the client, 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 were 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.

Then the hothouse atmosphere of wartime research brought different strands of work together, and the modern survey emerged fully. There were controversies between structured and unstructured approaches, or open and closed questions, and different teams developed different styles, but there was much cooperation and a 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 in the more qualitative styles. For those in the lead on surveys, 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 planned to deal with their particular subject matter; some were heavily criticized by the methodologists from the perspectives that they had now developed.

After the war, new practices were incorporated into textbooks and training procedures (see, e.g., Sheatsley, 1951). 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 sympathy defended alternatives, often under the banner of "participant observation" (Becker & Geer, 1957), which was differentiated from the survey by laying stress more on direct observation than on questioning, though certainly much "conversation with a purpose" (a frequently cited definition of "interview") was part of the observation. Discussions of participant observation technique have, though, 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.

Soon surveys were widespread enough for non-methodologists to take an interest in them—though often a skeptical one. From the later 1960s, the upheaval in the political and theoretical interests of the time was related to interviewing, and work was done on its implicit assumptions in areas such as

epistemology. Much more interest was shown in its social relations; this was the heyday of reflexivity and autobiographical accounts of research. Specialist work on interviewing particular groups (children, elites) also started to be written as the general application of survey method brought to light the special problems involved.

By the 1970s, interviewing was taken for granted as an established practice in the survey world; specialists continued with increasingly sophisticated methodological research and refined details of method still further, often in relation to new technologies using telephones and/or computers. (Meanwhile, for members of the general public, the idea of polling with quantitative results, and of the role to be played by respondents, became established; Back & Cross, 1982,⁷ and Igo, 2007, discuss what this meant.) The "qualitative" world became ideologically more separate and developed its own discussions, which showed little concern with the technical issues it might have in common with the survey world. Feminists often saw qualitative methods as particularly appropriate to women as subjects and developed ideas about their special requirements. The barrier between interviewer and respondent was attacked, and efforts were made to define ways of co-opting respondents rather than using them; whether these have 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 toward mass respondents was that of the social worker rather than of the social equal, for some sociologists the interviewer again has a high degree of freedom and initiative and may make direct use of personal experience. 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 unexamined, such as probing, are themselves intended to be programmed. What really happens in the field might not live up to those hopes—but less was done "in

⁷"One can say that the interview proceeds best if the social situation of the interview has been solidified in the culture, if survey research is an accepted institution, and if people have definite expectations of the performance in the interview. If these social conditions are met, the interview can proceed smoothly, while the respondent can disregard the characteristics of the interviewer or the nature of the questions" (pp. 201–202).

the field.” 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 area of initiative. Meanwhile, however, another strand of development, the cognitive approach, has reopened some of the earlier possibilities of unprogrammed conversational initiative by the survey interviewer, showing an interesting convergence between otherwise very separate areas of work.

Quantification can only be justified if it is in some sense instances of the same thing that are added up—but there is room for variation in how precisely uniform the stimuli need to be—and not all research has had goals to which quantification is appropriate. For exploratory or descriptive research, not aiming to test specific hypotheses, varying stimuli could be desirable if they help produce responses of more detail, precision, validity, and felt adequacy for the respondent—as long as those responses are not then fed into precodes. If the text of the answer is to be processed later, there are problems of recording and analysis, but many problems shift from the interviewing to the analysis stage. In the end, therefore, discussion cannot be confined to the interaction between interviewer and respondent.

Some of the changes over time in interviewing theory and practice have arisen internally, from methodological concerns, though which ones have been salient has depended on the topics studied and on the organizational and technological framework within which the studies have taken place. Other changes have responded to broader intellectual movements and to agendas defined in sociopolitical rather than methodological terms. Strong normative statements about method have often rested on assumptions appropriate to their original context but less relevant to other kinds of work. The interview remains an area of richly diverse practice about which few convincing generalizations can be made. We cannot tell which of the many current variants will appear to the later historian to have played a significant role or whether history will recognize all the distinctions made between them as meaningful.

◆ References

- Adorno, T. W., Frenkel-Brunswik, E., Levinson, D. J., & Sanford, R. N. (1950). *The authoritarian personality*. New York, NY: Harper & Row.
- Atkinson, R. (1998). *The life story interview*. Thousand Oaks, CA: Sage.
- Back, K. W., & Cross, T. S. (1982). Response effects of role-restricted respondent characteristics. In W. Dijkstra & J. van der Zouwen (Eds.), *Response behaviour in the survey-interview* (pp. 189–207). London, England: Academic Press.
- Bales, K. (1991). Charles Booth’s survey of *Life and Labour of the People in London 1889–1903*. In M. Bulmer, K. Bales, & K. K. Sklar (Eds.), *The social survey in historical perspective 1880–1940* (pp. 66–110). Cambridge, England: Cambridge University Press.
- Becker, H. S., & Geer, B. (1957). Participant observation and interviewing: A comparison. *Human Organization*, 16, 28–32.
- Bennett, J. (1981). *Oral history and delinquency: The rhetoric of criminology*. Chicago, IL: University of Chicago Press.
- Biemer, P. P., Groves, R. M., Lyberg, L. E., Mathiowetz, N. A., & Sudman, S. (Eds.). (1991). *Measurement errors in surveys*. New York, NY: Wiley.
- Bingham, W. V. D., & Moore, B. V. (1931). *How to interview*. New York, NY: Harper.
- Bradburn, N. M., & Sudman, S. (1979). *Improving interview method and questionnaire design*. San Francisco, CA: Jossey-Bass.
- Brenner, M. (1982). Response effects of “role-restricted” characteristics of the interviewer. In W. Dijkstra & J. van der Zouwen (Eds.), *Response behaviour in the survey-interview* (pp. 131–165). London, England: Academic Press.
- Cannell, C. F., & Kahn, R. L. (1953). The collection of data by interviewing. In L. Festinger & D. Katz (Eds.), *Research methods in the behavioral sciences* (pp. 327–380). New York, NY: Dryden Press.
- Cannell, C. F., Miller, P. V., & Oksenberg, L. (1981). Research on interviewing techniques. In S. Lienhardt (Ed.), *Sociological methodology 1981* (pp. 389–437). San Francisco, CA: Jossey-Bass.
- Cantril, H. (1947). *Gauging public opinion*. Princeton, NJ: Princeton University Press.
- Christie, R., & Jahoda, M. (Eds.). (1954). *Studies in the scope and method of “The Authoritarian Personality.”* Glencoe, IL: Free Press.
- Cicourel, A. V. (1964). *Method and measurement in sociology*. New York, NY: Free Press.
- Cochran, W. G., Mosteller, F., & Tukey, J. W. (1954). *Statistical problems of the Kinsey report*. Washington, DC: American Statistical Association.
- Converse, J. M. (1987). *Survey research in the US: Roots and emergence, 1890–1960*. Berkeley: University of California Press.
- Converse, J. M., & Schuman, H. (1974). *Conversations at random: Survey research as interviewers see it*. New York, NY: Wiley.

- Denzin, N. K. (1970). *The research act in sociology*. Chicago, IL: Aldine.
- Denzin, N. K., & Lincoln, Y. S. (1994). *Handbook of qualitative research*. Thousand Oaks, CA: Sage.
- Dexter, L. A. (1970). *Elite and specialized interviewing*. Evanston, IL: Northwestern University Press.
- Dijkstra, W., & van der Zouwen, J. (Eds.). (1982). *Response behaviour in the survey-interview*. London, England: Academic Press.
- Douglas, J. D. (1985). *Creative interviewing*. Beverly Hills, CA: Sage.
- Fontana, A., & Frey, J. H. (1994). Interviewing: The art of science. In N. K. Denzin & Y. S. Lincoln (Eds.), *Handbook of qualitative research* (pp. 361–376). Thousand Oaks, CA: Sage.
- Fowler, F. J. (1991). Reducing interviewer-related error through interviewer training, supervision and other means. In P. B. Biemer, R. M. Groves, L. E. Lyberg, N. A. Mathiowetz, & S. Sudman (Eds.), *Measurement errors in surveys* (pp. 260–279). New York, NY: Wiley.
- Fry, C. L. (1934). *The technique of social investigation*. New York, NY: Harper.
- Gallup, G. (1944). *A guide to public opinion polls*. Princeton, NJ: Princeton University Press.
- Galtung, J. (1967). *Theory and methods of social research*. London, England: Allen & Unwin.
- Garrett, A. (1942). *Interviewing: Its principles and methods*. New York, NY: Family Service Association of America.
- Goode, W. J., & Hatt, P. K. (1952). *Methods in social research*. New York, NY: McGraw-Hill.
- Gorden, R. L. (1969). *Interviewing: Strategy, techniques and tactics*. Homewood, IL: Dorsey Press.
- Holstein, J. A., & Gubrium, J. F. (1995). *The active interview*. Thousand Oaks, CA: Sage.
- Hyman, H. H. (1954). *Interviewing in social research*. Chicago, IL: University of Chicago Press.
- Igo, S. E. (2007). *The averaged American: Surveys, citizens and the making of a mass public*. Cambridge, MA: Harvard University Press.
- Jovchelovitch, S., & Bauer, M. W. (2007). *Narrative interviewing*. London, England. Retrieved from <http://eprints.lse.ac.uk/2633/1/Narrativeinterviewing.pdf>
- Kahn, R. L., & Cannell, C. F. (1957). *The dynamics of interviewing*. New York, NY: Wiley.
- Kinsey, A. C., Pomeroy, W. B., & Martin, C. E. (1948). *Sexual behavior in the human male*. Philadelphia, PA: W. B. Saunders.
- Lazarsfeld, P. F. (1944). The controversy over detailed interviews: An offer for negotiation. *Public Opinion Quarterly*, 8, 38–60.
- Lazarsfeld, P. F., & Thielens, W., Jr. (Eds.). (1958). *The academic mind*. Glencoe, IL: Free Press.
- Lee, R. M. (2004). Recording technologies and the interview in sociology, 1920–2000. *Sociology*, 38, 869–889.
- Lundberg, G. A. (1942). *Social research*. New York, NY: Longmans, Green.
- Lyberg, L., & Kasprzyk, D. (1991). Data collection methods and measurement error: An overview. In P. B. Biemer, R. M. Groves, L. E. Lyberg, N. A. Mathiowetz, & S. Sudman (Eds.), *Measurement errors in surveys* (pp. 237–257). New York, NY: Wiley.
- Maccoby, E. E., & Maccoby, N. (1954). The interview: A tool of social science. In G. Lindzey (Ed.), *Handbook of social psychology* (Vol. 1, pp. 449–487). Reading, MA: Addison-Wesley.
- Maynard, D. W., Houtkoop-Steenstra, H., Schaeffer, N. C., & van der Zouwen, J. (Eds.). (2002). *Standardization and tacit knowledge*. New York, NY: Wiley.
- Mishler, E. G. (1986). *Research interviewing: Context and narrative*. Cambridge, MA: Harvard University Press.
- Odum, H. W., & Jocher, K. (1929). *An introduction to social research*. New York, NY: Holt.
- Palmer, V. (1928). *Field studies in sociology*. Chicago, IL: University of Chicago Press.
- Parker, T. (1997). *Studs Terkel: A life in words*. London, England: HarperCollins.
- Peneff, J. (1988). The observers observed: French survey researchers at work. *Social Problems*, 35, 520–535.
- Platt, J. (1996). *A history of sociological research methods in America, 1920–1960*. Cambridge, England: Cambridge University Press.
- Potter, J., & Hepburn, A. (2005). Qualitative interviews in psychology: Problems and possibilities. *Qualitative Research in Psychology*, 2, 281–307.
- Reinharz, S. (1992). *Feminist methods in social research*. New York, NY: Oxford University Press.
- Richardson, S. A., Dohrenwend, B. S., & Klein, D. (1965). *Interviewing: Its forms and functions*. New York, NY: Basic Books.
- Riesman, D. (1958). Some observations on the interviewing in the Teacher Apprehension Study. In P. S. Lazarsfeld & W. Thielens Jr. (Eds.), *The academic mind* (pp. 266–370). Glencoe, IL: Free Press.
- Riesman, D., & Glazer, N. (1952). *Faces in the crowd*. New Haven, CT: Yale University Press.
- Riesman, D., Glazer, N., & Denney, R. (1950). *The lonely crowd*. New Haven, CT: Yale University Press.
- Roethlisberger, F. J., & Dickson, W. J. (1964). *Management and the worker*. New York, NY: Wiley. (Original work published 1939)
- Roth, J. A. (1966). Hired hand research. *The American Sociologist*, 1, 190–196.
- Schaeffer, N. C. (1991). Conversation with a purpose—or conversation? Interaction in the standardized interview. In P. B. Biemer, R. M. Groves, L. E. Lyberg, N. A. Mathiowetz, & S. Sudman (Eds.), *Measurement errors in surveys* (pp. 367–391). New York, NY: Wiley.
- Schober, M. F., & Conrad, F. G. (1997). Does conversational interviewing reduce survey measurement error? *Public Opinion Quarterly*, 61, 576–602.

- Selltiz, C., Jahoda, M., Deutsch, M., & Cook, S. W. (1965). *Research methods in social relations*. London, England: Methuen.
- Sheatsley, P. B. (1951). The art of interviewing and a guide to interviewer selection and training. In M. Jahoda, M. Deutsch, & S. W. Cook (Eds.), *Research methods in social relations* (Pt. 2, pp. 463–492). New York, NY: Dryden Press.
- Sjoberg, G., & Nett, R. (1968). *A methodology for social research*. New York, NY: Harper & Row.
- Suchman, L., & Jordan, B. (1990). Interactional troubles in face-to-face survey interviews. *Journal of the American Statistical Association*, 85, 232–241.
- Taylor, S. J., & Bogdan, R. (1984). *Introduction to qualitative research methods*. New York, NY: Wiley.
- Turner, C., & Martin, E. (Eds.). (1984). *Surveying subjective phenomena*. New York, NY: Russell Sage Foundation.
- University of Michigan. (1954). *Manual for interviewers*. Ann Arbor: University of Michigan, Survey Research Center.
- Vidich, A., & Bensman, J. (1954). The validity of field data. *Human Organization*, 13, 20–27.
- Warner, W. L., & Lunt, P. S. (1941). *The social life of a modern community*. New Haven, CT: Yale University Press.
- Young, P. V. (1935). *Interviewing in social work*. New York, NY: McGraw-Hill.
- Young, P. V. (1939). *Scientific social surveys and research*. New York, NY: Prentice Hall.
- Young, P. V. (1949). *Scientific social surveys and research* (2nd ed.). New York, NY: Prentice Hall.