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Studies in the Social and Cultural Foundations of Language

The aim of this series is to develop theoretical perspectives on the essential social and cultural character of language by methodological and empirical emphasis on the occurrence of language in its communicative and interactional settings, on the socioculturally grounded "meanings" and "functions" of linguistic forms, and on the social scientific study of language use across cultures. It will thus explicate the essentially ethnographic nature of linguistic data, whether spontaneously occurring or experimentally induced, whether normative or variational, whether synchronic or diachronic. Works appearing in the series will make substantive and theoretical contributions to the debate over the sociocultural–functional and structural–formal nature of language, and will represent the concerns of scholars in the sociology and anthropology of language, anthropological linguistics, sociolinguistics, and socioculturally informed psycholinguistics.

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Learning how to ask

A sociolinguistic appraisal of the role of the interview in social science research

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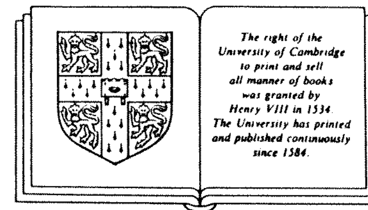

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1. Introduction

Interviewing has become a powerful force in modern society. Starting almost from birth, we are confronted by questions posed by educators, psychologists, pollsters, medical practitioners, and employers, and we listen to flamboyant interviewers on radio and television. Our skill at playing the role of interviewee influences our success in education and employment; our answers will help determine whether we receive such basic services as bank loans or disability pay. On a societal level, polling "pundits" are no longer employed exclusively by such specialized agencies as the National Opinion Research Center in Chicago or the Gallup Poll. Major corporations spend millions of dollars on market surveys that estimate customer wants and resources. Pollsters form integral members of major political campaigns, and their findings have a profound effect on the way candidates approach the voters. "Exit polls" now enable the media to advise West Coast residents as to how the East Coast has voted in national elections – even before the polls have closed.

Research in the social sciences is the great bastion of the interview. Estimates suggest that 90 percent of all social science investigations use interview data (cf. Brenner 1981b:115). Interviews are used in a wide variety of social contexts. A central component of the anthropological tool kit, interviews have produced a good bit of the information we possess about contemporary non-Western societies. Interviewing is, however, also a mainstay of research within modern industrial societies. We use interviews in exploring people's beliefs about the future (e.g., "Who do you think will win the election?") as well as their recollections of the past. The validity of a great deal of what we believe to be true about human beings and the way they relate to one another hinges on the viability of the interview as a methodological strategy.

Our faith in the interview is not entirely unexamined. An overwhelming mass of literature in psychology, sociology, anthropology, linguistics, political science, folklore, oral history, and other fields has

focused on interview techniques. Many of these works are of the "cookbook" type, providing recipes for better baking using interviews yet without seriously considering the nature of the interview or its inherent weaknesses. Others are devoted to analysis of the factors that "bias" interviews, skewing the results in a particular direction. The latter body of material has substantially increased the level of awareness with respect to the possibility that the interviewer's gender, race, political beliefs, linguistic characteristics, and the like may distort the results.

Given the ubiquity of interviews and the proliferation of works on the subject, I would hardly blame the reader for asking why we need one more book on interviewing. The reason is simple: We still know very little about the nature of the interview as a communicative event. Worse yet, because the interview is an accepted speech event in our own native speech communities, we take for granted that we know what it is and what it produces. One major problem is that the interview is most unusual, as communicative routines go. Accordingly, researchers base their interview strategies and the way they interpret the data on a number of false assumptions. This is, unfortunately, not a simple, naive mistake; I argue in later chapters that our methodological shortsightedness reflects our reluctance to face some thorny theoretical issues.

This mystification of the interview emerges primarily in three ways. First, interviews provide examples of *metacommunication*, statements that report, describe, interpret, and evaluate communicative acts and processes. All speech communities possess repertoires of metacommunicative events that they use in generating shared understandings with respect to themselves and their experiences. As I argue in Chapter 4, these native metacommunicative events are rich in the pragmatic features that root speech events in a particular social situation and imbue them with force and meaning. Unfortunately, researchers seldom gain competence (in Hymes's [1974a:92–97] sense of the term) in these repertoires, relying instead on the metacommunicative routine that figures so prominently in their own speech community – the interview. This practice deprives the researcher of an adequate sense as to how the information she or he obtains fits into broader patterns of thinking, feeling, and speaking.

An even more serious problem is inherent in the structure of the interview. By participating in an interview, both parties are implicitly agreeing to abide by certain communicative norms. The interview moves the roles that each normally occupies in life into the background and structures the encounter with respect to the roles of interviewer and interviewee. Attention is concentrated on the topics introduced by

the researcher's questions. Preliminary "small talk" may highlight the participants' present states of mind and body ("How are you?") and their relationship ("It's good to see you. I appreciate your letting me interview you again"). But the initial question then shifts the focus away from the interaction to another time, place, or process ("Now tell me about . . .").

The problem here is that this movement away from the interview as a speech event mystifies researchers to such an extent that they generally retain this focus in the course of their analysis. What is said is seen as a reflection of what is "out there" rather than as an interpretation which is jointly produced by interviewer and respondent. Since the context-sensitive features of such discourse are more clearly tied to the context of the interview than to that of the situation it describes, the researcher is likely to misinterpret the meaning of the responses.

A third difficulty arises because suppression of the norms that guide other types of communicative events is not always complete. Some potential respondents are drawn from communities whose sociolinguistic norms stand in opposition to those embedded in the interview. This is likely to be the case in groups that do not feature the interview as an established speech-event type. Lacking experience in this means of relating, such individuals are less likely to be able and willing to adhere to its rules. The farther we move away from home, culturally and linguistically, the greater the problem. This hiatus between the communicative norms of interviewer and researcher can greatly hinder research, and the problems it engenders have sometimes abruptly terminated the investigation. If the fieldworker does not take this gap into account, he or she will fail to see how native communicative patterns have shaped responses; this will lead the researcher to misconstrue their meaning.

It has not been possible to limit the discussion to a critique of interview methods alone, however, because broaching these methodological issues raises much broader questions. Why are interviews ubiquitous in the human sciences? Why is the nature of the interview process so poorly understood, and why has it not been more adequately researched? Why are we so reluctant to modify our research methodology, particularly in the light of theoretical advances? The answer is easy: *Interview techniques smuggle outmoded preconceptions out of the realm of conscious theory and into that of methodology.* Both our unquestioned faith in the interview and our reluctance to adopt a more sophisticated means of analyzing its findings emerge from the fact that the interview encapsulates our own native theories of communication and of reality.

The refusal to rely more heavily on native metacommunicative re-

pertoires as sources of information and our unquestioned belief that we have the right to impose interview techniques on our consultants have serious political implications. They indicate that social research is characterized by less sensitivity and willingness to expose oneself to other modes of learning than we may have imagined. By leaving the interview situation itself out of the analysis, we have cleverly circumvented the need to examine our own role in the research process. A clearer understanding of the interview will accordingly not only enhance its usefulness as a research tool but will greatly expand our consciousness of what studying our fellow humans is all about.

Lest the reader gain the wrong impression, let me make my position on the interview clear. *I am not trying to persuade researchers to abandon interviewing altogether.* In addition to being utterly unrealistic, such an attempt would undermine my project entirely. The presentation of a simple and unfeasible solution would ultimately lead most interviewers to lose interest in the task of critically examining the nature and limitations of interview techniques. The point is that the communicative underpinnings of the interview are tied to basic theoretical as well as methodological issues. My goal is to elucidate the nature of the interview as a communicative event and to contribute to our understanding of these basic methodological and theoretical problems.

I will approach this task in four primary ways. As I will argue in later chapters, one of the most important tasks confronting students of the interview is to examine transcripts of interviews in great detail. The point here is not simply to explain the problems that become explicit in the course of the interview. This is the orientation of many researchers who have focused on the problems of rapport-building and bias. My approach is rather to study transcripts (and tape recordings) as a whole in order to ascertain exactly what was said (the linguistic forms), what each question and reply meant to the interviewer and interviewee, and what the researcher can glean from these data. This technique reveals the points at which interviewer and interviewee have misunderstood each other and where one or both are likely to be misinterpreted by the researcher, even when such misunderstandings do not become explicit in the interview.

Unfortunately, it is difficult indeed to obtain verbatim transcripts of complete interviews in the published literature. I have accordingly concentrated my analysis on interviews I conducted over a thirteen-year period in a Spanish-speaking community in northern New Mexico. (A brief account of *Mexicano* society and the research site is provided in Chapter 2.) The reason for choosing these data is that I have tape recordings of interviews covering the span between my first few days in

the community and my most recent research. The interviews are of a number of types, from the most nondirected and informal to quantitatively oriented formal interviews that utilized questionnaires. The research foci consisted of material culture (the production of carved images of Catholic saints), oral history, political economy, sociolinguistics, and folklore (oral literature). My ability to interpret the interview data is thus aided by systematic study of sociolinguistic patterns and social relations. I also conducted a social survey of a city of 14,000 inhabitants (Smith and Briggs 1972) and am currently studying job interviews between college seniors and prospective employers. Although these investigations have informed my understanding of interview techniques, they do not form primary sources of data.

The second basic thrust of my analysis is an exploration of the communicative roots of the interview. This approach emerges mainly from my training in sociolinguistics and discourse analysis. These fields utilize concepts derived from other types of linguistic analysis, anthropology, sociology, psychology, and literary criticism in studying the way language is used in a variety of settings. Discourse analysis has focused a great deal of attention on the heretofore neglected study of conversation. My purpose here is to see what types of linguistic and social norms are presupposed by the interview and to compare them with the norms characteristic of other types of speech events. This task should reveal the basic communicative features that are most likely to prove problematic in interviews.

The third dimension is the presentation of steps that might be taken to overcome the problems posed by these communicative obstacles. I argue that one of the most important facets of this process is the development of a heightened awareness of the theoretical problems that lie behind methodological naiveté. This discussion is taken up primarily in Chapter 6. A practical approach to this task is developed in Chapter 5. The basic steps in designing, conducting, and interpreting research using interviews are outlined to show how investigators can make the best possible use of interviews.

These suggestions are addressed to interviewing in the social sciences as a whole as well as in linguistics, folklore, and oral history. Most of the examples will be drawn from fieldwork conducted in another society. This reflects the fact that the data used in this study were collected in the course of a fieldwork project and that the bulk of my training was in anthropology and linguistics. The book is addressed, however, to all practitioners who use interview and/or survey data in their research. Some of my remarks are directed specifically at one type of interviewing or to the way in which interviews are used in a particular discipline. I have nevertheless tried to avoid spelling out the implica-

tions of each point for the different fields in order to avoid burying the argument in excessive complication and tedium. The reader will thus find it necessary to assess the bearing of my remarks on her or his own concerns.

The process of critically examining the nature and limitations of interview techniques involves another step as well. As I noted above, an impressive number of sources have examined the way interviews are (or should be) used in research. This literature has increased the sophistication with which interviewers deal with problems of sampling, bias, the wording of questions, and so forth. Unfortunately, very few writers have contributed significantly to our understanding of the nature of the interview as a communicative event and of the metacommunicative norms it presupposes. This oversight leaves us without a clear understanding of the problems that result from gaps between the metacommunicative norms of the interview and those connected with other types of speech events. The result is that most students of the interview seem unaware of many of the basic obstacles confronting this type of research. In other words, the literature on interviewing has also contributed to the *mystification* of the interview. Given the influence these sources exert on the way interviews are conducted, an examination of these works is a necessary starting point for any effort to rethink the interview.

Previous research on the interview

The task of summarizing the literature that deals with the methodology of the social sciences is daunting. My treatment of these sources is confronted by two constraints. On the one hand, I seek to point out problems that confront a wide range of different types of interviews. I must perforce deal with sources on interviewing that emerge from a number of disciplines. My goal in this book is, however, to analyze unexamined aspects of interviewing, not to produce a monographic summary of the literature. I will accordingly treat selected sources that deal with interviews as used by ethnographers, oral historians, folklorists, sociologists, and political scientists. The point in each section will be to grasp the basic problems that underlie the body of literature in question, not to adumbrate each relevant work.

A couple of definitions might help prevent interdisciplinary confusion. I will use the term "interview" to cover a wide range of research activities from the most "informal," "open-ended" interviews to the use of "formal" instruments in survey research. In order to be considered an interview according to my definition, the collection of data

must occur in a face-to-face situation. The interaction must also occur in a research context and involve the posing of questions by the investigator. I thus exclude such events as telephone polling, the use of written questionnaires, and employment interviews. Although many points of my analysis apply to them as well, they also present special problems that I cannot elucidate in the course of this study. I will also use the term "fieldwork" in its anthropological sense to refer to research that involves intense interaction between a researcher and a given population over a substantial period of time. Fieldwork generally includes a number of different research modalities, including interviews of one or more types. My usage is thus to be distinguished from a common use of the term in sociology; here "fieldwork" often involves observation and other procedures *rather than* interviewing.

Anthropology

Classically, anthropologists have used a combination of observation and open-ended interviews in conducting fieldwork. Observing is not constituted in formal terms as it is, for instance, in the study of nonverbal communication. The classical paradigm is provided by Kluckhohn's article on "The Participant-Observation Technique in Small Communities" (1940). She urges fieldworkers to assume roles, such as housewife, teacher, and the like, that will afford extensive contact with members of the community in areas of interest to the research. In the eyes of Kluckhohn and of most anthropologists, participant-observation is not opposed to informal interviewing; the former rather provides opportunities for the latter.

Ethnographers generally rely on open-ended interviews rather than on surveys or questionnaires. Even those practitioners who urge fieldworkers to use surveys suggest that formal instruments should be introduced after basic cultural patterns have been established through observation and informal interviewing. Ethnographers use open-ended interviews in two basic ways. The basic thrust of the first type is captured by Powdermaker (1966:156–7):

> I used no interviewing schedule, but I had well in mind the problems to be discussed, and the interviews tended to follow a general pattern. They were always by appointment and usually in the informant's home. The tone was that of a social visit. After an exchange of polite greetings, my hostess often made an admiring comment on my dress or suit. I might note a photograph on the mantel over the coal grate fireplace, and the informant would point with pride to the members of the family in it and this often led to talking about them. My questions were open-ended, and directed towards certain areas for both factual information and attitudes.

This account pertains to her work with blacks in Mississippi. Many ethnographers arrange interviews more informally, without appointments. Sessions are often conducted in the ethnographer's residence in order to isolate the interviewee and obtain privacy. The basic pattern of inaugurating and ending the session with "normal conversation," the absence of a formal instrument, and the direction of the discussion toward the research goals of the ethnographer is, however, quite common.

The second major type is "key-informant interviewing." In the course of conducting informal interviews with a number of members of the community, ethnographers generally form close working and often personal relationships with a few consultants. These individuals are then singled out for much more intensive interviews on a more frequent basis, and, as Edgerton and Langness (1974:33) note, "most anthropologists . . . come to rely upon certain persons for much of their detailed or specialized information." The possible dangers of too great a reliance on a few individuals, particularly with regard to sampling and observer effect, have often been described (cf. Young and Young 1961). Why, then, is key-informant interviewing used to a high degree?

The rationale emerges in a statement by Pelto and Pelto (1978:72) that "humans differ in their willingness as well as their capabilities for verbally expressing cultural information. Consequently, the anthropologist usually finds that only a small number of individuals in any community are good key informants." This motive is reiterated by such authors as Chagnon (1974:60), Edgerton and Langness (1974:33), and Kobben (1967:42). As I will argue in Chapters 4 and 6, this facet of ethnographic interviewing is quite revealing with respect to the communicative basis of ethnographic interviewing in general.

Finally, formal interviewing has been used to a limited extent in fieldwork. Obtaining a census of the population that contained basic demographic and economic information used to be de rigueur. This was generally accomplished in small communities by a door-to-door survey using an instrument with both open-ended and precoded questions. Taking a census seems to have lost its general appeal in recent years as ethnographers have become increasingly problem-oriented in focus. Nevertheless, anthropological fieldwork has come under attack from sociologists and quantitatively oriented anthropologists as being too reliant on "subjective" and nonquantitative observation and informal interviewing. As Pelto and Pelto (1973:267–70) report, many fieldworkers have accordingly turned to survey research as a means of providing more "controlled," "objective," and quantifiable data on

their research foci. A number of sources have reported attempts to create a rapprochement between formal and informal techniques (see, for example, Bennett and Thaiss 1970; Brim and Spain 1974; Burawoy 1979; Cancian 1965; Denzin 1970; Freilich 1970; Mitchell 1965; Myers 1977; Speckman 1967).

The literature on ethnographic methodology. The literature on methodological aspects of fieldwork is substantial. One of the most common types of work in the area is the presentation of an anthropologist's experiences in one or more societies, drawing out his or her research design, methods of data collection, and mode of interpretive data. A few of the better-known examples of this type of study are Beattie (1965), Berreman (1962), Chagnon (1974), Freilich (1970), Georges and Jones (1980), Golde (1970), Henry and Saberwal (1969), Lawless, Sutlive, and Zamora (1983), Middletown (1970), Powdermaker (1966), Spindler (1970), and Wax (1971). A related body of literature describes the personal experiences of anthropologists in the field. Belmonte (1979), Dwyer (1982), and Rabinow (1977) provide leading examples of this type of discussion.

Several volumes feature articles that deal with specific aspects of fieldwork (see, for example, Jongmans and Gutkind 1967; Naroll and Cohen 1970). A large body of articles undertakes this task as well, much of which has been published in the "Field Methods and Techniques" section of the journal *Human Organization.* A number of manual-type publications have also been written, many with the beginning fieldworker in mind. (See, for examaple, Agar 1980a; Brim and Spain 1974; Edgerton and Langness 1974; Langness 1965; Langness and Frank 1981; Paul 1953; Pelto 1970; Pelto and Pelto 1973, 1978; Spradley 1979; Whyte 1984; Williams 1967). A number of works have appeared that treat fieldwork in historical perspective (cf. Firth 1957; Stocking 1968, 1974, 1983).

A critical assessment. This body of literature has produced insights that hold the potential for increasing the sophistication with which we view the fieldwork process. The work of Agar (1980a, 1980b; Agar and Hobbs 1982), for example, has helped us understand the way in which interview data reflect both the events described and the context of the interview itself. Berreman (1962) has increased our sophistication with respect to the complex ways in which both "natives" and ethnographers present themselves at different times to different people and regarding the effect of their shifts on data collection. Karp and

Kendall (1982) have questioned the misplaced analogies that have shaped our conception of the role of the fieldworker and the limitations of a positivistic conception of "social facts." Owusu (1978) questions the way in which fieldwork reifies basic Western cultural conceptions by "finding" theoretical constructs in the field.

Unfortunately, these pioneering efforts have not succeeded in turning the ethnographic enterprise onto itself in such a way that the nature of the interview and other research strategies would be revealed. Although a number of authors suggest that we must look at the interview itself as a cultural encounter (e.g., Agar 1980a:91–2, Conklin 1968, and Mintz 1979), no author has yet presented an in-depth statement of how this can be undertaken. Ethnographers accordingly fall back on their own native understanding of the interview. As I will try to show in later chapters, this view is based on a systematic distortion of the nature of the interview as a speech event. In the absence of an adequate grasp on the nature of the interview, the bulk of the literature thus gives the appearance of a host of reiterations of the status quo in terms of basic interviewing procedures and descriptions of how these have been applied in given fieldwork cases.

The lack of an adequate grasp of the interactional and communicative norms that underlie the interview is matched by a failure to grasp the importance of studying the correlative norms of the society in question. Ethnographers sometimes note that other groups have differing kinds of restrictions on who may ask what questions of whom in what circumstances. It has also been argued that questions may not mean the same thing to a member of another speech community, even if translated "accurately" (Edgerton and Langness 1974:44; Hollander 1967:12–13; Leach 1967; Paul 1953:447). These sorts of problems are cited as reasons for remaining critical of the potential of *formal* interviews as fieldwork tools.

The problem here is that rejection of surveys may serve as a cover for the failure to systematically explore the possibility that informal interviewing may suffer from the same sorts of problems. This can only be accomplished by a careful consideration of the compatibility of native communicative patterns and the norms presupposed by the interview and by a careful examination of interviews for hidden misunderstandings. Not only has this task not been accomplished, but the importance of undertaking it has been articulated only rarely. A great deal of attention has been devoted to the idea that "natives" frequently lie and/or give inconsistent answers. Such distortions do occur, but they are dwarfed in comparison with the effects of communicative disparities between ethnographers and their consultants.

Folkloristics

The field of folkloristics exhibits a nearly schizophrenic character with respect to methodology. On the one hand, generations of amateur and professional folklorists have compiled masses of oral material through the most naive means. Collectors have traveled to communities with folkloric traditions for very short intervals, frequently only days or weeks. Once there, collectors query passersby with respect to the identity of the person "who knows the most" ballads, tales, or what have you. When permission to tape-record or transcribe the material is given, the informant is asked to tell (or sing) all the items that he or she knows in the desired genre. The collector may take notes on the performer and the social setting. The result is the collection of a vast number of items in a relatively short period of time. Although this approach is by no means as prevalent now, it is still used by a substantial number of practitioners.

During the past two decades, a new generation of scholars in folklore and related disciplines has discredited this orientation. Many of these individuals have been influenced by linguistic training, thus developing greater interest in the formal properties of performances. Scholars such as Bauman (1975) and Hymes (1981) have shown that the "tell me all the X that you know" technique generally produces reports or summaries of the content of folkloric traditions rather than performances. In other words, the presentation of materials in such an artificial situation transforms the overall structure and the stylistic details of the traditions. Worse still is the collector's lack of awareness that such a transformation has occurred, thus distorting the process of interpretation.

The influence of the old methodology has been countered by a growing emphasis on fieldwork methodology. Graduate students in folklore frequently take classes in sociolinguistics and ethnography, and courses in folklore methodology are generally de rigueur. Goldstein's *A Guide for Field Workers in Folklore* (1964) has become the standard reference work. Here, Goldstein stresses the importance of *systematic* use of a variety of techniques as well as a heightened awareness of the need to take the collecting situation explicitly into account in analyzing the materials (see also Ives 1974; MacDonald 1972). Like ethnographers, folklorists rely mainly on observation and informal interviewing in collecting folkloric items and related materials.

A technique developed by Goldstein, the *induced natural context*, has also gained in popularity (1964:87–90). This involves an initial

assessment of the situations in which a given genre is usually performed. An "accomplice" is then induced to invite other performers to a gathering; the real purpose of the meeting is not announced. The collector arrives "unexpectedly," thus theoretically minimizing the effects of his or her presence. Goldstein reports that this technique produces results closer to the "natural contexts" of folklore performance than to those explicitly structured by the fieldworker ("artificial contexts").

Two major methodological shortcomings remain. First, Goldstein and others have successfully identified some of the limitations on the usefulness of the interview for collectors. They also have a sense of the effects of the researcher's presence on the form and content of what is collected, whether in "natural" or "artificial" situations. Like ethnographers, however, folklorists have seldom gone beyond a commonsensical perspective on the interview. This leads them to misconstrue the nature of the interview as a speech event and thus the status of the data it yields. This prompts Goldstein (1964:104) to argue, for example, that the interview "supplies the collector with an insider's view of the individual, his culture, and his folklore" and of the way in which the informant conceptualizes and orders this knowledge (1964:109, 123). Discourse generated by interviews is structured, however, by the communicative norms of this type of speech event and by the role of the interviewer.

This lack of sophistication with respect to the nature of the interview and the role of the interviewer prevents folkloristic methodologists from providing their readers with clear guidelines for assessing the role of these factors in the generation and interpretation of interview data. This hiatus is all the more important because "Interviewing is the most common field method employed by folklore collectors" (Goldstein 1964:104).

A second major problem is tied to the concept of "context." Although the new generation of folklorists have laudably pointed to the importance of the social and linguistic setting in which materials are collected, this has not led to the development of a sophisticated view of the nature of contextual components. As Cook-Gumperz and Gumperz (1976) argue, the context of a speech event is not simply the sum total of elements present at the time it emerges. Contexts are not given, a priori, before the event begins. Contexts are interpretive frames that are *constructed* by the participants in the course of the discourse. The presentation of a checklist of elements in the social and physical setting that are seen as constituting "the context" is thus theoretically misdirected, as is the notion that "the collector has a clear duty to place the total situation of record as he observes it" (MacDonald 1972:410). Analysts would be better advised to look closely at the *form* of the performance in order to see how the participants are providing each other with signals as to the situational elements relevant to the meaning of what they are saying. The common practice of observing the "context" in "natural" performances and recording the texts in interviews thus creates a dangerous chasm between text and context (cf. Briggs 1985a).[1]

Oral history

In turning to oral historical interviewing, the same basic methodological schizophrenia is encountered. We find, on the one hand, a number of manuals that describe the way in which oral historians generally design and implement their interviews and interpret their findings. The authors generally include some tips as to how interviews are best undertaken. These pertain to techniques for establishing rapport, expressing interest in the interviewees' memories, avoiding "loaded" questions, and the like in addition to suggestions regarding tape-recording and transcribing interviews (cf. Baum 1971; Davis, Back, and MacLean 1977; Garner 1975; Hoopes 1979; Ives 1974; Moss 1974; Neuenschwander 1976; Shumway and Hartley 1973; Sitton, Mehaffy, and Davis 1983).

These discussions simply assume, however, that both the authors and their readers know what interviews are, how they work, and their compatibility with the process of articulating one's experiences. They similarly eschew any serious concern with the fact that the products of interviews are dialogic texts that are largely structured by the interviewer. Several authors have argued, for example, that oral historians must compensate for "biases" on the part of either interviewer or respondent that reduce the reliability and validity of interview data (cf. Cutler 1970; Hoffman 1974). As I argue in the section on sociology, conceptualizing the interview process in terms of the way specific "biases" can "distort" the data ultimately succeeds in further obscuring the real problem – the dialogic, contextualized nature of all discourse, including interviews.

A number of works that have appeared recently do take up some of these issues (see, for example, Allen and Montell 1981; Friedlander 1975; Joyner 1975, 1979; Thompson 1978). These writers help dispel the notion that oral historians collect, even in ideal terms, reflections of historical events. Thompson has articulated the point well. Speaking of social statistics, written documents, published sources, and oral history interviews, he notes: "They all represent . . . the *social perception*

of facts; and are all in addition subject to social pressures from the context in which they are obtained. With these forms of evidence, what we receive is *social meaning*, and it is this which must be evaluated" (1978:96; emphasis in original). Joyner (1979:48) echoes Thompson's concern with context, arguing, "What is necessary is a full description of the context in which the testimony was taken." The point has similarly been made that interviews impose different kinds of constraints on speech than "ordinary" conversation does (Allen and Montell 1981:40–44). Some of these insights were anticipated by Vansina's (1965) study of the social relations that condition the transmission of historical tradition in Africa.

Such writers offer a new level of methodological sophistication to the discipline. Their efforts have not, however, succeeded in resolving two basic problems. First, the nature and significance of context in oral history interviews is not sufficiently appreciated. The task of examining the context is equated with a description of the physical and social setting of the interview. As Cook-Gumperz and Gumperz have argued, however, contexts are not simply extralinguistic givens that surround the spoken word. Context rather enters discourse as "perceptual cues which must be actively and continuously monitored in the course of the interaction" (1976:8). The point is thus to look into the verbal (and nonverbal) exchange itself for clues as to how the participants are drawing on their surroundings in interpreting each other's remarks.

Some of the most significant contextualization cues are so-called fillers and crutch words. Nearly all writers, however, urge researchers to delete these cues (cf. Davis et al. 1977:39–40; Thompson 1978:198). Ives (1974:49) and Thompson (1978:179) even suggest that the interviewer should avoid responding with "uh-huh" or "yes" for fear of cluttering the tape and transcript. In spite of recent efforts to call attention to the importance of context, the oral historians' own procedures are robbing their works of contextual information.

This problem reveals a deeper issue. The goal of oral history is to elicit information about past events. Researchers have noted the selectivity of memory. Yet a lack of awareness is apparent with respect to the fact that oral history interviews produce a *dialogue* between past and present. Interviewees interpret the meaning of both the past and the present, including the interview itself. Each query presents them with the task of searching through their memories to see which recollections bear on the question and then fitting this information into a form that will be seen as answering the question. Oral history interviews are thus related to the present as systematically as to the past. This is not to say that oral historians are entirely unaware of this

process; great strides have been made in recent years toward becoming more sensitive to these issues. This growing awareness has, however, largely failed to stimulate researchers to thoroughly rethink the way they structure interviews and analyze interview data in light of the bifocal, synthetic nature of oral history.

Sociolinguistics

I am using the term "sociolinguistics" in a broad sense to refer to research on the ethnography of communication (Gumperz, Hymes), the quantitative study of linguistic variation (Labov, Sankoff), and the macrosociology of language (Fishman). The labels and boundaries associated with work in these areas are problematic because leading practitioners have all expressed some ambivalence regarding the term "sociolinguistics." Practitioners are concerned with the description and analysis of language use and the way it relates to cultural and social patterns. Quite different approaches to these problems are present in the field, and these discrepancies motivate contrastive views on methodological issues.

The field of sociolinguistics presents us with a paradox of a rather different sort when it comes to methodology than was the case with folklore or oral history. On the positive side, sociolinguists have provided us with systematic comparative data on language structure and use. Researchers have emphasized the diversity that is evident in forms, contexts, functions, and speech event types as well as in the way these phenomena are viewed in different communities. A crucial move for the development of sociolinguistics was Hymes's (1971a) emphasis on the study of sociolinguistic competence; this involved the expansion of Chomsky's (1965, 1968) stress on formal features and rules to include the communicative norms that underlie their use. Superficial attempts to correlate social–cultural and linguistic patterns are increasingly giving way to studies of the subtle ways in which alternations in linguistic features (from phonetic changes to changes from one language to another) are intimately related to shifts in interactional patterns. In spite of its theoretical difficulties, Austin's (1962) work on the performative force of language has stimulated much research on the dynamics of this process.

Another major effect of sociolinguistic research was to provide new sources of rapprochement between linguists and practitioners in such areas as education, social–cultural anthropology, folklore, sociology, and psychology. The point has been made by Cicourel (1964, 1974a, 1982b), Grimshaw (1969), Hymes (1969a, 1971b, 1972), Labov (1972a,

1972b), and others that the success of social-scientific research depends on awareness, generally implicit, of the ways in which the groups in question use language. Unless these sociolinguistic patterns move, at least in part, from the role of means to that of ends of research, the errors that are lodged in our own ethnocentric conceptions of linguistic processes may be built unknowingly into the results. Just as importantly, these practitioners have stressed the need to examine the results of educational, anthropological, folkloristic, sociological, and psychological research in developing a broader understanding of linguistic patterns. The present study is an attempt at deepening and systematizing both of these lines of attack.

Social-scientific research draws on a host of different types of communicative events. With its emphasis on speech acts and on the communicative norms that underlie them, sociolinguistics holds tremendous potential for revealing the nature of the social-scientific research enterprise. The beginnings of this process are apparent in the work of a number of leading researchers.[2] Hymes (1969a, 1971b) points out to political scientists and social anthropologists the fact that their methodologies rely fundamentally on linguistic processes; the devotion of greater attention to the communicative dimensions of data acquisition is thus requisite to methodological adequacy and a means of enhancing one's comprehension of the data. Grimshaw (1969) similarly encourages his readers to see "language as obstacle and as data in sociological research." Irvine (1978) argues that the elicitation of cross-cultural psychological data in artificial environments may lead to gross distortion. Gumperz (1972) notes that "the interview setting . . . is often formal and contrived and almost always quite different from the settings within which people usually interact." He thus urges sociolinguists to utilize a broader range of elicitation techniques. Labov (1966, 1972b, 1972c) proposes an expansion of the range of elicitation techniques that are included in the interview in order to collect a more diverse and controlled range of data.

This research offers important insight into research interviews. Its major limitations emerge from the interview's being generally treated in passing, not as a primary research focus. Albert's study of speech patterns of the Barundi provides a case in point. Her data were taken from "informants' verbalizations, spontaneous or by elicitation" and "the comparison of the interview protocols of a variety of informants" (1972:74–5). In her conclusions, Albert noted that "For Barundi (and possibly for a number of similar societies in Africa and the Mediterranean countries), rules of procedure for data gathering included: (1) avoiding direct questions, except in such matters as asking which road leads to Kitega or Nyabikere" (1972:98). Realization of the difficulties

involved in obtaining information through direct questioning did not induce her, however, to question her own use of elicitation and interview protocols or to propose an alternative methodology for interviews. Indeed, she includes "systematic questioning" in her list of suggested methods for obtaining "a comprehensive view of speech behavior" (1972:104).

This case is not isolated – the insights that have emerged have failed to produce a systematic critique of sociolinguistic methodology. In fact, sociolinguists are relatively conservative, methodologically speaking. Practitioners rely primarily on familiar ethnographic and linguistic methods. Grimshaw (1974) has summarized the most common data-collection procedures under four headings: (1) the observation of "natural" speech in "natural" settings; (2) the observation of "natural" speech in contrived settings (i.e., experimental situations or those organized by the researcher); (3) the elicitation of speech through direct inquiries; and (4) the use of historical and/or literary materials.[3]

This is not to say that no methodological progress is apparent. Chomsky's reliance on the linguist's utterances and intuitions have been rejected in favor of data from native speakers. Tape- and video-recording has greatly increased the precision of data collection, and a great deal of attention has focused on the range of contexts in which recordings are made. The problem is rather that these methodological insights have not stimulated systematic sociolinguistic investigations of the speech events that researchers use in collecting their data. Proposals for methodological progress are still based to a high degree on preconceptions regarding the communicative underpinnings of observation[4] and interviewing; neither of these activities has formed the primary subject of in-depth sociolinguistic research. These insights have not even produced greater interest in methodological issues.[5]

An obvious and important exception is provided by Labov's work on speech variation in New York City (1966, 1972a, 1972b, 1972c). Labov has devoted considerable attention to methodological issues; this springs, at least in part, from his interest in combining elicitation techniques from linguistics, sociological survey methodology, and sociolinguistic concern with the contextual dimensions of speech. Labov's central methodological innovation consists of an attempt to increase the diversity of speech acts recorded in interview settings.

Interviews were structured in such a way as to produce five classes of data. "Casual speech . . . the every-day speech used in informal situations, where no attention is directed to language" (1966:100), emerged "spontaneously" before and after the interview and during interruptions, and during asides. Labov also obtained "casual speech" (Style A) by eliciting childhood rhymes and emotionally charged personal

narratives. A second type (Style B) consists of "careful speech"; he argues that the formality of the interview setting induces the interviewee to focus greater attention on his or her own speech. Labov also increased the formality of the situation even more by asking interviewees to read short texts (Style C) and word lists (Style D). The interviews provided data on the socioeconomic characteristics and linguistic attitudes of each interviewee in addition to data for each of the contexts on five phonological variables (1966:137).

Labov's work is praiseworthy for the seriousness that is attached to methodological issues and for his attempt to base methodological innovations on the communicative norms of interviewees.[6] The value of his contribution is marred, however, by a number of crucial assumptions. Labov is centrally concerned with the way in which "the formal interview itself defines a speech context in which only one speaking style normally occurs, what we may call *careful speech*" (1966:91; emphasis in original). Several problems are apparent here. Interviews are quite complex and multifaceted speech events. Their communicative properties are highly reflective of the specific type of interview and of the relationship that is established between the involved parties. Discourse analysts and ethnomethodologists have similarly shown that what Labov terms "casual speech . . . the every-day speech used in informal situations, where no attention is directed to language," is much more formally and functionally complex and varied than Labov's definition would suggest.

Labov's reformulation thus ultimately relies on commonsensical assumptions regarding the nature of both interviews and "ordinary" discourse rather than on a systematic investigation of their communicative underpinnings. His distinction between "careful" and "casual speech" is similarly based on a naive concept of "formality"; as Irvine (1979) has argued, our judgments regarding the "formality" of a speech event reflect a broad range of frequently contradictory criteria. As Wolfson (1976) points out, Labov evinces little understanding of the way in which the unique features of interviewing shape the form and content of the sum total of utterances that constitute it, even in the course of "spontaneous," emotionally charged responses.

The commonsensical basis of Labov's view of interviewing is apparent in the way he uses the interview "to gather the information which is the ostensible subject of the questions being asked" (1966:137). As his focus shifts away from phonological variables, he is no longer concerned with the role of the context and style. In other words, he seems to think that the referential content of the responses can be interpreted without serious consideration of the role of the interviewer, the setting, and the formal features of the question. This appears all the more

questionable because Labov places a great deal of importance on questions dealing with "subjective evaluation and linguistic attitudes" (1966:137). We are, however, not presented with any evidence that such data are less sensitive to contextual variation than phonological features: The stability of the referential content is simply assumed.

Sociology

Compiling a summary of the literature on methodology in sociology is a daunting task indeed. Sociologists as a whole take methodological issues seriously. Sociologists are often heard to criticize other social scientists as being insufficiently "rigorous." Classes in methodology are required in most sociology programs, even at the undergraduate level. The literature on methodology is enormous; a comprehensive survey would constitute a monograph in and of itself, and a long one at that. A substantial amount of research has focused on methodology, particularly on interviewing. Good critical studies of the interview have been emerging since the time of the classic study by Hyman et al. (1954), *Interviewing in Social Research.*

This should not be taken to mean, however, that the basic methodological issues that confront the discipline have been solved. Contradictions are certainly apparent here as well. The literature on methodology ranges between introductory manuals that codify received interviewing techniques and sophisticated critiques that point to basic underlying problems. Beyond the limitations of the literature on methodology itself lies the fact that many of its most significant findings have yet to be incorporated into interviewing practice.

Types of sociological research. Sociologists rely on a wide range of research activities. *Field research* or *participant observation* was utilized extensively in the pre–World War II period, particularly by members of the Chicago School of sociology; it played a central role in such classic sociological investigations as Lynd and Lynd's (1929) *Middletown,* the study by Warner and his associates of Yankee City (Warner and Lunt 1941), and Whyte's (1943) *Street Corner Society.* After a long lapse in interest, observation has gained prominence again in recent years (cf. Brogdan and Taylor 1975; Johnson 1975; Schatzman and Strauss 1973). As Riley and Nelson (1974:6) note, the term "observation" is used in sociological parlance to include a wide range of activities that revolve around "watching the behavior of the group, listening to its members, and noting its physical characteristics." The designation sometimes includes combinations of interviewing and observation, while it is also

used to distinguish between research that includes interviewing and that which does not. Some sociologists have gone so far in attempting to circumvent the problems inherent in interviewing that they have developed techniques, termed "inobtrusive measures," in which the observer cannot be seen by the subjects (cf. Webb et al. 1966).

With respect to interviewing, sociologists make a basic distinction between *standardized* and *nonstandardized* interviews. The former type involves the use of a common set of questions with all respondents, whereas the latter does not. Standardized interviews are further categorized as *scheduled,* where both the wording and order in which the questions are asked is specified, or as *nonscheduled.*[7] In the latter case, the interviewer is free to present the questions in the way in which it seems most suitable for each interviewee. Data obtained through the use of scheduled interviews are thus much more amenable to statistical analysis than those from unscheduled or nonstandardized interviews. Scheduled interviews can be either of the *fixed-alternative* or *closed-ended* type, in which case the respondent chooses from a predetermined list of answers, or *open-ended,* where the range of possible answers is not specified. The actual list of questions that is used in an interview is generally termed a *schedule.* It thus contrasts with a *questionnaire,* a list of questions the subject reads and completes without the assistance of the researcher.[8]

Sources on methodology

Some basic categories of sources can be distinguished in the vast literature on interviewing. A number of manuals present introductions to interviewing and, in some cases, related research techniques. Some of the most widely used are those by Babbie (1973), Backstrom and Hursh (1963), Bailey (1978), Denzin (1970), Gorden (1975), Hoinville and Jowell (1978), Kahn and Cannell (1957), Kerlinger (1964), Richardson, Dohrenwend, and Klein (1965), and Simon (1969). A number of collections of essays on methodology have appeared (e.g., Riley 1963; Franklin and Osborne 1971).

Other works deal with particular types or aspects of interviews. Gorden (1969) suggests that these can be classified with respect to whether they focus on the type of respondent or the type of interviewer.[9] In the first case, we find works that concentrate on the aged (Kastenbaum and Sherwood 1967), parents (Langdon 1954), children (Rich 1968), elites (Dexter 1970), potential employees (Turner 1968), and other groups. Sources that treat particular types of interviewers often deal with the use of interviews in such professions as

nursing (Bermosk and Mordan 1964), medicine (Froelich and Bishop 1969), psychiatry and counseling (Davis 1971; Erickson and Shultz 1982; Labov and Fanshel 1977; Sullivan 1954), journalism (Sherwood 1969), and social work (Fenlason et al. 1962; Garrett 1942).

With few exceptions, these works fail to provide a critical perspective on interviewing. Most authors simply assume that they and their readers already know what interviews are all about. These writers thus rely on their status and that of their audience as members of a society in which interviews are an established speech-event type. The presumption seems to be that native speaker status enables the authors to forgo the need to examine the nature of the interview as a communicative event. The underlying argument seems to be as follows: "We all know what interviews are and why they are important. We will thus simply assume this knowledge and go on to present techniques for improving the quality of interviews or to explicate the nature of specialized forms of the interview."

The "bias" theory

It is, of course, nearly de rigueur to include a consideration of interview problematics. This generally comes under the aegis of examining the way in which interviewer-induced *bias* can reduce the validity and reliability of the findings. The inclusion of such considerations certainly helps to alert readers to certain types of problems. Discussions of bias emerge, nevertheless, from a highly problematic theoretical premise. The claim is that the influence of one or more of a range of independent variables, such as the age, gender, race, political views, personality, or interactional style of the researcher and/or interviewee, can "bias" responses to questions. The assumption here is that if you could strip the interview situation of all of these factors, the "real" or "true" or "unbiased" response would emerge. This ideal response is sometimes referred to as the "individual true value," abbreviated as the ITV (cf. Brenner 1981b). The thrust of the argument is that the researcher should attempt to ensure, insofar as is possible, that none of these factors has any special effect on the data. In analyzing the responses, such distortions should be factored out.

Becoming sensitive to the role of such factors is certainly laudable. Two implications of this line of reasoning are less so. First, this approach leads most practitioners to believe that if no particular source of "bias" is present or if such overt "distortions" have been accounted for, the researcher can treat these data as if they were a direct reflection of the interviewee's thoughts. In other words, once the problem of

"bias" has been treated in this way, one can forget that the statements were made in the course of a particular interview. This facilitates the comparison of data from different interviews, interviews with different persons, and data collected through "participant observation" and interviews.

A second implication involves the generally implicit assumptions that lie at the base of this picture of interviewing. The "bias" theory reflects Durkheim's (1938) notion that social facts exist independently of the observer and can be perceived from without. It is similarly based on individualistic and positivistic assumptions regarding, in Karp and Kendall's (1982:251) terms, the stability and observability of social facts. A response lies within the interviewee, and the problem simply consists of extracting it from her or him as directly as possible. The truth value of an informant statement is measured vis-à-vis its correspondence to the "real" object "out there," as somehow grasped "objectively," independently of the manner in which it is communicated to the researcher. Note the way in which such advice encapsulates our own folk epistemology, conceiving of "the truth" as being singular, unequivocal, and semantically transparent, once it has been identified. It goes without saying (or does it really?) that this reassuringly places the researcher in the position of final arbiter of what is "correct" and "objective." It also strongly biases the analyst in favor of responses that seem to bear a direct relationship to the "reality" in question. Statements whose meaning is clearly affected by the situation in which they are uttered are deemed less reliable.

One of the difficulties encountered by this approach is that an individual's knowledge and attitudes emerge from a complex web of relations with other human beings. Interviewees do not draw, even ideally, on a fixed idea or feeling in answering a question, but connect questions with some element or elements of a vast and dynamic range of responses. Because the interview is itself a social interaction, it provides another impetus for generating new reactions. Moreover, the response is often less a selection of one element of this complex whole than an index, in Peirce's (1932: 2.305) sense of the term, of the relation between conflicting or competing elements (Cicourel 1974c:20). The goal of getting the "individual true value" for each question thus greatly oversimplifies the nature of human consciousness. As Dean and Whyte (1958:38) have pointed out, the interviewer's task is thus not that of fishing for "the true attitude or sentiment," but one of interpreting the subtle and intricate intersection of factors that converge to form a particular interview. The social situation created by the interview does not simply constitute an obstacle to the respondent's articulation of his or her beliefs. Like speech events in general, it shapes the form and content of what is said.

This becomes apparent in, inter alia, the way in which responses comment metacommunicatively on the interview situation itself (see Chapters 3 and 5 of this volume).

This is not to say, however, that interviews are just like any other speech event. As Wolfson (1976) has argued, interviews constrain the presentation of many types of forms and of certain topics and alter the manner in which observed features are presented. One rule takes precedence over others – the need to adapt the form and content of the information in order to make it apparent that it provides an answer to the question. In interview-elicited narratives, for example, features such as the conversational historical present tense (e.g., "And so he says to the guy . . .") are absent (Wolfson 1976, 1979). Similarly, as Wolfson (1976:192) notes, "there is often elaboration and emphasis on the specific part of the story which answers the question that has been asked." The interview is thus probably the last place where one should forget that the statements were made in a particular context.

The emergence of a critical understanding of the interview

A number of sources have appeared that provide us with important insight into the limitations of the interview as a research method. Their contribution lies in two key areas.

Reliability and validity. First, these works have clarified the thorny issues that surround the important problems of reliability and validity. These two concepts are discussed in most works on methodology, because they provide the benchmarks by which data analysis and collection are measured. "Reliability" refers to the probability that the repetition of the same procedures, either by the same researcher or by another investigator, will produce the same results. "Validity" refers to the accuracy of a given technique, that is, the extent to which the results conform to the characteristics of the phenomena in question.

Hyman et al. (1954:20–1) pointed out that very few studies in the methodological literature were concerned with validity. Emphasis was rather placed on decreasing inter-interviewer variation, that is, in reducing the extent to which inter-interviewer differences affect the reliability of data. As Hyman et al. note, this presents a strong force for methodological conservatism. It also suggests that such efforts toward methodological reform may have no positive effect whatsoever on the degree to which procedures enhance our understanding of the questions they investigate.

The work of Hyman et al. and others has increased the degree to

which questions of validity are taken seriously, although a bias toward studies of reliability is still apparent (Gorden 1969:6). The degree to which such work has affected interview practice is, however, far from clear. Cicourel provides us with insight into why this is the case. He argues that Hyman and his associates fell short of resolving the dilemma due to their failure to question the premise that underlies the reliability versus validity issue – the idea that procedures can be designed that will be both reliable *and* valid (Cicourel 1964:93).

Cicourel (1982a) raises the question of *ecological validity*. This concept pertains to the degree to which the circumstances created by the researcher's procedures match those of the everyday world of the subjects (cf. Neisser 1976). The problem is that competing demands are placed on the researcher. Standardization, a crucial device for promoting reliability, leads interviewers to attempt to present each question in exactly the same manner to each respondent. Bailey (1978:171) even suggests that "the interviewer's inflection and intonation should be the same for each respondent."

This raises two issues. First, true standardization would be achieved if the *meaning* of the question were the same for each respondent. This leads writers such as Gorden (1969:61) to suggest that differences in social backgrounds between respondents will force the interviewer to change the question wording in order to maintain the validity of quantitative measures. The problem runs deeper than this, however, because, as Cicourel (1974c:20) and Dexter (1970:144) note, interviewees respond not simply to the wording of the question but to the interview situation as a whole.

A second issue emerges from the fact that each interview is a unique social interaction that involves a negotiation of social roles and frames of reference between strangers. As Hyman et al. (1954:80–1) show, "bias" seldom appears as the interviewer's imposition of her or his own ideological slant on the respondent. "Bias," meaning inter-respondent and inter-interviewer differences in the presentation of questions and the perception of responses, *is rather an interactional resource* that is used in accomplishing the task at hand.

As this task becomes more onerous – for example, if the schedule is long and complex, questions are repeated, or the material is socially sensitive – it becomes more difficult to obtain answers. The negotiation process becomes more critical at this stage, and the interviewer must draw upon a wider range of conversational resources in narrowing the gap between the standardized questions and the background knowledge and communicative norms of the interviewee. Interestingly, writers are divided on the issue as to whether or not this is a good thing. (Compare, for example, Bailey 1978 with Gorden 1969.) Cicourel's point is that such divergences emerge from the fact that reliability and validity are incompatible goals.

The concept of context and the nature of interpretation. This realization has led to a deeper understanding of the role of the context in interviews. As noted earlier in this chapter, many writers conceive of "the context" as being the sum total of physical, social, and psychological stimuli that exist at the time of an interaction. This definition conveniently allows the analyst to decide what counts as "the context" on the basis of his or her own assessment of the situation. It also dichotomizes the analysis of "contextual variables" from that of the verbal components of the discourse. Given the fact that responses address the total situation, this dichotomization is methodologically problematic.

Brenner (1978, 1980, 1981a, 1981b), Cicourel (1974c, 1981, 1982a, 1982b, 1985), Dexter (1970), and Mehan (1979) have challenged this separation. They argue that the context is a phenomenological construct that is created jointly by the participants. Not only are contexts not simply situational givens, they are continually renegotiated in the course of the interaction. The words of the interviewer and interviewee do not simply occur within this frame; along with nonverbal components, they are the very stuff of which the context is constructed. Each utterance thus reflects this ongoing process, just as it contributes to it. As Cicourel (1974c:88) notes, "the actor's remarks in the interview, even when termed spontaneous, are often the product of a carefully monitored kind of presentation."

This immediately calls into question the naive concept of the interviewer as the medium through which the respondents' attitudes and beliefs are conveyed to the reader. The interviewer rather stands as a co-participant in the construction of a discourse. This view also challenges received procedures for the interpretation of interview data. Researchers generally draw on their a priori, commonsense understandings of the meaning of the questions; these are then used as frames for the interpretation of responses. If questions and responses rather constitute small-scale models of interviewer–interviewee interaction, this mode of analysis will radically distort their significance.

Unresolved issues

In a nutshell, then, the sociological literature on interviewing consists on the one hand of myriad studies that presuppose and accordingly reify the very nature of the phenomena in question. On the other hand, a limited number of studies have revealed some of the basic

problems that underlie interview techniques. This work has not succeeded, however, in producing a methodological reformulation that would tell practitioners how they can best cope with these problems.

One of the most pressing needs in this regard is a more detailed analysis of the ways in which these difficulties emerge in interview data and a method for identifying them. Most professional interviewers will not be satisfied by general statements regarding the problematic nature of their enterprise. Methodological reform will also require the presentation of concrete ways in which these findings can translate into procedure.

Another central goal is the development of a greater understanding of the nature of the interview as a communicative event. As Wolfson (1976) has argued, the interview is a unique speech event that is patterned by a complex array of communicative features, many of which are not shared by "ordinary" conversation. The precise nature of the norms that underlie interviewing are, however, still very poorly understood. As Grimshaw (1969) and Cicourel (1964, 1974c, 1982a, 1986) have argued, interviewing must be seen as a research subject in its own right and not simply as a useful tool.

Finally, Cicourel (1974c) cuts to the very heart of the matter when he notes that the linguistic and sociolinguistic background of the interviewee may differ from that of the researcher. Given that the two parties are generally separated by lines of class, ethnicity, and/or cultural background, this is more than a remote possibility. This problematizes the notion that the questions and responses mean the same things to interviewee and interviewer. Cicourel (1974c:19) rightly suggests that the only solution to the dilemma is learning about the commonsense understandings and the sociolinguistic background of both parties.

What is needed is a means of exploring the nature of the communicative norms brought into play in the interview. A clearer grasp on the norms that are presupposed by the interview vis-à-vis those characteristic of the respondents' speech communities is a prerequisite for progress in these areas. One major limitation of the studies mentioned in the previous section is that they fail to draw on data on a wide variety of speech events in characterizing the background knowledge the interviewee brings to the encounter. If interview data alone are used in this capacity, the argument becomes circular.

The roots of methodological conservatism

The work of Brenner, Cicourel, Dexter, Grimshaw, Hyman, Mehan, Wolfson, and others certainly provides a basis for revamping our approach to the subject. Why, then, have the past few decades produced

almost no changes in the way in which most practitioners use interviews in their research? Interview techniques not only continue to reign as the major tool in social research but are still used in a highly uncritical fashion. This lack of awareness of the issues raised by the use of interviews in social-scientific research in general has been noted by Dexter (1970:157):

> Professional interviewers have for the most part assumed without analysis the nature of the process in which they are engaged. Until that process is itself viewed as problematic, something to be analyzed and explored, we will not be ready to determine what it records and measures, let alone how it can be used to draw valid inferences, etc.

The ensuing decade has witnessed few responses to Dexter's challenge.

Some of the major obstacles to progress along these lines can be linked to basic, recurrent crises in social-scientific and linguistic theory. An exploration of this complex web of theoretical and methodological issues must be based, however, on a fuller understanding of the status of the interview as a communicative event, and I will thus postpone it to the last chapter. One factor promoting stagnation, of course, is that most practitioners see no reason to question their own methodology, given the uncritical acceptance of the role of interviewing in research manuals, in social-scientific and linguistic writings, and in modern society as a whole.

Even if practitioners recognize the need for methodological change, however, accepted canons of interview technique render it nearly impossible for them to do so. The development of a more sensitive approach to the use of interviews is obstructed by a number of fundamental contradictions in the communicative norms that guide such encounters. These revolve around the way in which the communicative norms of the interview and those of the interviewees' speech communities place competing demands on the researcher.

As I argue in Chapters 3 and 4, the interview presupposes a set of role relations, rules for turn-taking, canons for introducing new topics and judging the relevance of statements, constraints on linguistic form, and so on. This effects a displacement of many of the norms that guide other speech events in the local community. Even in the most "unstructured," "open-ended" interviews, the interviewer has a great deal more control over the development of the discourse, and the respondent is primarily confined to answering the questions. Indeed, as many writers have argued, interviews are not *supposed* to be conversations.

The natives are not all willing, however, to permit this substitution of imported for indigenous modes of interaction. The greater the distance between the cultural and communicative norms of researcher and consultants, the more likely it becomes that this hiatus will generate interpersonal tension and misinterpretation in interviews. This

commonly leads to difficulties in inducing the respondent to answer the question, producing seemingly irrelevant or incomplete replies or even silence. If the social roles and communicative patterns of the interview stand in opposition to basic moral values and/or patterns of interaction, it can bring the interviewing process to a halt. Interviewers attempt, either consciously or unconsciously, to avoid such friction by drawing on the everyday sociolinguistic norms of the respondents in creating a "friendly atmosphere." Such rapport-building speech events include introducing oneself, making "small talk" before and after the interview and perhaps between questions. It is often necessary to permit respondents to "wander off the point" and provide "irrelevant" information at times, that is, to permit a bit more egalitarian distribution of the control over the interaction.

The interviewer is thus subjected to conflicting pressures. She or he expects to be able to keep the interaction within the confines of the interview. The very success of that interview depends, however, on the researcher's capacity for allowing native communicative routines to work their way into the interview situation. This ambiguity also provides conflicting bases for evaluating interviews. Does the ability to maintain a focus on the research interests of the interviewer and his or her plan for the interaction constitute success? Or do the best interviews emerge from a more egalitarian cooperation in which both fieldworker and consultant contribute to the interview agenda and to the form of the discourse? The dilemma also raises the familiar issue of reliability versus validity. The interviewer's attempts to increase reliability by standardizing the presentation of the questions thwarts her or his ability to achieve ecological validity.

These contradictions emerge from the same underlying problem. Our refusal to examine critically the communicative norms of the interview in light of those implicit in the interviewees' metacommunicative repertoires has forced these two sets of sociolinguistic patterns into a relationship of unreconcilable opposition. Unfortunately, methodologists of the interview have failed to illuminate the nature of these contradictions, let alone attempt their resolution. This has clearly hampered their ability to assist researchers in escaping a sort of methodological "double bind" (cf. Bateson et al. 1956) inherent in the received research methodology.

Plan of the book

Dismal as this portrait of the state of interviewing in the social sciences and linguistics may seem, the situation is far from hopeless. This book

seeks to demonstrate that we already possess the tools to place methodology on a solid footing. The main thing that is needed is a new point of departure. My contention is that investigating the metacommunicative repertoire of the group in question is the necessary starting point for research. This thesis cannot be argued in vacuo, that is, without grounding the argument in the analysis of the communicative patterns observed in a given speech community. This work thus focuses on the way in which a group of Spanish-speakers in the United States exchange messages about their own cultural and linguistic system. An analysis of *Mexicano* metacommunication provides me with a basis for arguing the incompatibility of standard interview techniques with the native system and for demonstrating the richness of these metacommunicative routines themselves as a source of linguistic and anthropological data. Having placed the interview in its social and communicative context, I am able to look more deeply into the theoretical and political underpinnings of the received methodology.

Chapter 2 sets the stage for this analysis by providing background on *Mexicano* society. The work is predicated on the notion that linguistic patterns cannot be understood independently of social and cultural patterns and vice versa. Most particularly, comprehension of a number of central *Mexicano* genres that are collectively termed 'the talk of the elders of bygone days' is predicated on knowledge of the profound socioeconomic transformation that overtook the *Mexicano* residents of northern New Mexico around the turn of the century. Losing their subsistence base forced *Mexicanos* into a much more intimate and economically dependent relationship with 'the outside world' and partially displaced timeworn patterns of social interaction.

Chapter 3 focuses directly on the interview. The analysis points to the ways in which interviewers commonly pose questions in such a way that interviewees cannot answer them. The focus is on the communicative components of the interview situation that can obstruct the process of obtaining responses and interpreting their meaning. The data are drawn from the communicative blunders I committed in research with *Mexicanos* in northern New Mexico.

My goal, however, is not simply to identify and describe such problems. The development of greater methodological sophistication is also contingent on examining their cultural and communicative roots. A basic thesis of this work is that such illumination will only follow from awareness of the native metacommunicative patterns in the interviewees' speech communities. Chapter 4 thus traces the way in which *Mexicanos* acquire metacommunicative competence. I argue that moving through the various levels of skill involves a progression from observation to imitation to performance; the competence of individuals is

continually reassessed on the basis of their control over appropriate speech forms and the pragmatic force of their words. This analysis points to the potential of native metacommunicative routines as sources of socio-cultural and sociolinguistic data. A comparison between the norms presupposed by these events and those involved in interview-centered research is used in isolating the sociolinguistic bases of some of the most fundamental problems introduced by placing too much reliance on interview techniques.

In Chapter 5, I utilize the preceding analysis of what is likely to go wrong in interviews in developing a proposal for setting them straight. I argue that fieldwork should begin with an investigation of native communicative patterns, particularly those pertaining to metacommunicative routines. The focus then shifts to detailing the way these results can be used in shaping research design, in periodic evaluations of the effectiveness of the methodology, and in interpreting interview data.

Chapter 6 suggests that the problems inherent in interviews are of much more than "purely methodological" significance. I try to show that the popularity of interview techniques is based on the fact that they encapsulate our folk theories regarding the nature of social reality and of communicative processes. Interviewing has thus enabled us to retain some of the basic preconceptions that we have striven so hard to banish from the domain of explicit theory. Our inability to come to terms with methodological issues thus greatly hinders theoretical advancement, just as it points out that we have not been entirely successful in extirpating a number of rather disturbing political implications of the research process.

2. The setting: Mexicano society and Córdova, New Mexico

The data were collected in Córdova, a community of about 700 inhabitants in the mountains of northern New Mexico. The residents are *Mexicanos*, with the exception of one recent Mexican immigrant, two middle-aged Anglo-Americans who have married Córdovans, and occasionally a few transient Anglo-American youths. *Mexicanos* are descendants of primarily Spanish and Mexican citizens who settled in New Mexico and southern Colorado during the seventeenth, eighteenth, and nineteenth centuries. Their ancestry includes a significant Native American element, but the *Mexicanos* consider themselves to be culturally Hispanic.

Córdova lies in the extreme southeast corner of Rio Arriba County (see map, Figure 1). The foothills area that includes the community is bordered on the east by the Sangre de Cristo mountain range; Córdova lies at the base of the highest peak in this section of the range, the Truchas Peak, which stands at 13,102 feet of elevation. The foothills extend some fourteen road miles to the west, where they are severed by the area's major water course – the Rio Grande. Córdova is situated in a small enclosed canyon, the Quemado Valley. The valley, which runs roughly east and west, is 6,800 feet above sea level. The surrounding hillsides can boast only sparse range grasses, cacti, and small trees, primarily piñons and junipers. The scarcity of arable land and the harshness of the environment are compounded by the scant and variable rainfall – an average of ten to fifteen inches per year (Maker, Folks, Anderson, and Link 1973:6–7). Elevations above 8,000 feet are characterized by more precipitation, denser stands of range grasses, and tall conifers (yellow and ponderosa pines, spruce, Douglas fir, aspen, etc.).

Most of the Córdovans' houses are built on the hillside that extends from the north bank of the Rio Quemado to just below the mesa above. Residents note that the selection of this location was motivated by the requirements of their defensive posture vis-à-vis nomadic tribes and in order to reserve the valley land for agriculture. All of the

the U.S. Forest Service, a job that involved extensive interaction with English speakers. Mr. Córdova often reads English-language newspapers. Interestingly, Lina Ortiz de Córdova, Federico Córdova's wife, who is monolingual, responded to my questions in the same fashion as the Lópezes.

Mr. Córdova had not only acquired the phonological, syntactic, and semantic systems of American English, but he had mastered its conversational structure as well. He had such a sophisticated idea of the interviewing process that he even thought to make sure that the tape recorder had been turned on before beginning his account. (The one stipulation he imposed in helping me was that I was to give him a copy of the final publication.) Why, then, was the latter interview so painless and so "successful"? Even though Mr. Córdova and I spoke in Spanish, the interview was bilingual, since the frame of reference and the conversational structure we used did not emerge from conversational patterns that related Córdovan youths and elders. Fortunately, this is not true of the sections of the interactions in which Mr. Córdova "wandered off the point," i.e., gained control of topical selection. Here the tape recordings reveal the same richness of metacommunicative routines and rhetorical structure that characterize the pedagogical dialogues with the Lópezes.

My research methods thus dictated the imposition of my own conversational norms on my consultants. The gap between American English discourse structure (especially interview techniques) and patterns that are characteristic of speakers of New Mexican Spanish is sufficiently wide that my initial position of communicative hegemony was successful only with a bilingual consultant. Fortunately, the Lópezes and other elders were so consistent in their refusal to allow me to structure our pedagogical interactions in keeping with my own "instincts" that I was forced to develop an alternative methodology. In Chapter 5, I systematize this process and attempt to show how it can be applied to interviewing in general.

5. Listen before you leap: toward methodological sophistication

I must admit to having painted a critical picture of the state of interviewing in the social sciences and linguistics. I initiated the discussion by pointing to a number of serious flaws in the literature on interviewing and by relating the persistence of crucial theoretical problems to a lack of methodological sophistication. Chapter 3 pointed to some of the procedural problems that can impede interviewing and can create serious problems in analyzing the data. I argued in Chapter 4 that native metacommunicative routines can inform the use of interview techniques in a given culture as well as provide precisely the types of data that are crucial for many problems in social scientific research.

It would thus be far from surprising if the reader were to have gained the impression that I am attempting to convince researchers to stop interviewing altogether. Indeed I am not. Interviews are highly useful tools for exploring a host of problems. As noted in Chapter 1, the theoretical and methodological insights that have emerged from such fields as the ethnography of communication, conversational analysis, language acquisition research, and other fields have provided us with the skills necessary for conducting and analyzing interviews in a more appropriate fashion.

Similarly, I am not arguing that greater methodological sophistication can only be gained through becoming a sociolinguist or at least developing more interest in the communicative dimensions of the interview than in the problems under study. This is hardly an all-or-nothing affair. It would be unrealistic to expect survey researchers who work with large populations to fully investigate the communicative norms of all potential respondents. Such a proposal would be seriously counterproductive, because it would lead most practitioners to dismiss these criticisms on the grounds that they could never satisfy them. It would also serve to widen the gap between researchers who lack interest in the ethnographic and linguistic communicative knowledge that underlies their data-collection techniques and practitioners who study communicative processes but lack interest in broader social issues.

This chapter is designed to forestall this conclusion by presenting some practical proposals for incorporating the study of native metacommunicative routines into interview-based research. I will propose a four-phase approach to conducting interviews and interpreting the results. My thesis is that any type of interviewing will be plagued by serious procedural problems if it is not based on sensitivity to the relationship between the communicative norms that are presupposed by the interview and those that are more broadly characteristic of the population under study. This need cannot be addressed in the same fashion, however, by the fieldworker who works by herself or himself in a small community as the research team studying a large and diverse sample. I will accordingly provide some sense as to how my suggestions can be taken up in large-scale survey research.

Phase 1: learning how to ask

Adequate applications of interviewing techniques presuppose a basic understanding of the communicative norms of the society in question. Obtaining this awareness should accordingly constitute the first item on researchers' agenda. In the case of fieldwork, the first weeks or months of a researcher's field stay are generally devoted to gaining an initial acquaintance with the native community and, in some cases, to learning the language. This is an ideal time in which to observe such simple facts as who talks to whom, who listens to whom, when people talk and when they remain silent, what entities are referred to directly and which are referred to indirectly or signaled nonverbally, and the like. An essential question is: What are the different ways in which people communicate? Hymes (1972) and others have outlined the possible types of variation, and a number of descriptions of local verbal repertoires are available (e.g., Abrahams 1983; Albert 1972; Gossen 1974; Sherzer 1983). Sherzer and Darnell's (1972) "Outline Guide for the Ethnographic Study of Speech Use" sketches the issues that might be raised. This is not to say that one must be a sociolinguist to conduct interview-based research. As I argued in Chapter 1, sociolinguistics itself is hardly free from methodological naiveté. The point is rather that overcoming procedural problems is predicated on developing a broader understanding of communicative processes.

The goal at this stage is to gain a sense of the range of social situations in relation to the types of speech events that can take place in each. Learning the rules that relate the two is crucial. One way to facilitate this undertaking is to conduct an intensive analysis of selected

speech events, as was illustrated in Chapter 4. For each major segment of the life cycle, select a frequently observed speech event. It is useful to take some events that are of special importance to native speakers and others that are unremarkable. It may also be necessary to take gender, social class or caste, and other factors into account in choosing examples for intensive analysis. Each event type should be observed a number of times; I strongly recommend the tape-recording of at least one instance.

These examples should be analyzed with two objects in mind. First, the fieldworker should ascertain the meaning of the event for the participants. Particularly if one's linguistic competence is still incomplete, help can be sought in transcribing, translating, and interpreting the episode. Such exegesis should hardly be confined to obtaining literal, referential meanings. The point is to discover the linguistic and social–cultural knowledge that underlies the ability to participate in and interpret such events. Second, once an array of such events has been analyzed, the data can be compared, attempting to discern the basic norms that underlie specific communicative patterns.

Several issues merit special attention. Metacommunicative features provide particularly important clues for the fieldworker. As I argued in Chapter 4, certain linguistic forms point to the speaker's view of basic social–cultural processes and of the ongoing speech event. As Silverstein (1985) has argued for quotation-framing devices, metacommunicative features often index the interpretation that the speaker ascribes to the utterance. Developing an ability to read such metamessages provides the analyst with the ability to base his or her interpretation on the participants' ongoing process of sorting out the meaning of what they are saying and hearing.

It is also important to learn how speakers frame queries. What are the proper linguistic forms for different types of questions? How do noninterrogative forms serve as questions in some contexts? Who can ask questions of whom? Obviously, it is terribly important for the fieldworker to discover the negative cases – what types of questions are inappropriate in what circumstances? It is also crucial to study the acquisition process, for example, the appropriate means of learning these rules. In order to become a good interviewer, the researcher will have to develop some degree of *competence* in these sociolinguistic patterns.

Applying this component of the methodology to large-scale survey research presents a real challenge. It would be difficult to document the sociolinguistic repertoire of potential respondents in a large sample that is stratified along the lines of class and ethnicity and that covers a

substantial geographic area. It would be hard in any case to convince most funding agencies that a large amount of money should be allocated for research that is preliminary to an exploration of the ostensive goals of the study.

There are a number of ways in which researchers can, however, gain greater awareness of communicative patterns in the population in question within the temporal and monetary constraints faced by most survey projects. First, sociolinguists have now conducted studies in urban environments; these range from microanalyses of small groups or specific situations to macro studies of the relationship between linguistic and social–cultural features of large populations. Labov, for example, has explored speech patterns in New York City from the level of narrative construction by members of specific youth gangs to broad correlations of phonological and syntactic features with such variables as class, ethnicity, education, and so on (1966, 1972a, 1972b). Fishman (1964, 1966; Fishman, Ferguson, and Das Gupta 1968) has analyzed sociolinguistic patterns on an even larger scale. Some dimensions of communication in modern society, particularly in educational settings, have formed the subject of fairly extensive ethnographies of communication. Obviously, not every problem and community have been studied, but the literature can be most useful in giving researchers a sense of the range of sociolinguistic variation they are likely to encounter once the interviewing has begun.

Researchers are also well advised to conduct a limited amount of sociolinguistic fieldwork on the native metacommunicative routines that relate to the focus of the survey. The idea here is to interact with members of the population in a variety of situations, particularly those in which the relevant matters are likely to be broached. It might be possible, for example, to tape-record a public meeting where pertinent issues are raised. A careful analysis of the transcript will reveal some of the ways in which such topics are appropriately introduced in formal settings.

An example of how a modicum of research on metacommunicative routines can improve interview techniques is provided by research I conducted with Sherolyn Smith in Gallup, New Mexico (Smith and Briggs 1972). Our task was to provide the City of Gallup with data that would enable planners to gauge how a neighborhood facility center then under construction could best meet the needs of area residents. A survey instrument was administered to a 10 percent random sample.

The instrument had been pretested and revised. No effort was made, however, to conduct preliminary research on the ways in which residents would discuss such topics in other contexts. The situation was complicated by the fact that the population of Gallup is ethnically quite complex, consisting primarily of Zuni and Navajo (Native American), Mexican-American, Black, and Anglo-American residents.

One question was designed to elicit information on the range of services in the facilities center that the respondent and her or his family would use, if available. In conducting the interviews, I noted that the numbers were much lower for Navajo respondents than for members of the other groups. These data seemed to lend themselves to the interpretation that Navajo residents were less interested in using the services than were the other residents.

Fortunately, I began conducting informal ethnographic research with Navajo and Mexican-American residents. After spending a minimal amount of time with Navajo families, I learned it was deemed highly inappropriate to speculate on the behavior or beliefs of others. The danger here is that such talk might be seen as a usurpation of the individual's own decision-making power, which would be construed as an attack on the person's integrity (cf. Kluckhohn and Leighton 1946: 302, 309–10). Speculating on the preferences of one's spouse and children would accordingly be deemed extremely rude. Rather than do so, Navajo respondents would estimate which services they themselves were likely to use. The use of a probe to obtain data on other family members generally yielded statements such as "No, I don't think so." The data thus reflected a gap between the presuppositions of the questions and the conversational maxims of native speakers of Navajo rather than a lack of interest in these services.

Simply using the question in a pretest did not expose the problem, because the pretest did not provide information on Navajo metacommunicative norms. The lack of such insight introduced a clear source of bias into the data, and it placed Navajo respondents in an uncomfortable position. The point is that the investment of a minimal amount of time in discovering these communicative patterns *before* designing the instrument would have circumvented the problem.

Phase 2: designing an appropriate methodology

Most practitioners have at least some idea as to the problem they plan to investigate and the research methods and methodology they will use before inaugurating their research. It is nearly always necessary to modify both to some degree in the course of the research. Freilich (1970) recommends to anthropologists that the "active" phase of research, that which is focused on the fieldworker's own interests, be preceded by a period of passive research. The passive phase serves as a guide for reformulating plans for the active. Such changes are, however, generally

undertaken on an ad hoc basis. The difficulty here seems to be that modifications of research methods have heretofore been seen as responses to specific obstacles. As Freilich (1970:25) puts it:

> For example, a strict sampling may not be possible if local customs prohibit [the anthropologist] from interviewing particular people or groups; if the subject matter central to the project's goals is too sensitive to be researched, due to the internal problems of the system being studied; or if important informants do not cooperate with the researcher because of his nationality, race, sex, or religious affiliation.

Such specific circumstances do need to be borne in mind. But this stopgap approach falls far short of an adequate adaptation to local social–cultural and communicative norms.

I suggest that systematic data collection should be guided by systematic examination of the best methods for conducting research on the chosen problem in the society in question. I see two considerations as being particularly important here.

First, the results of Phase 1 should inform an in-depth investigation of the points of compatibility and incompatibility between interview techniques and the local metacommunicative repertoire. This will suggest which topics can be explored in the course of interviews and which social situations are appropriate for interviewing. Again, the negative results are equally important – what issues will have to be explored by other means. This examination will also assist the researcher in selecting the most suitable interview techniques and in modifying them in order to increase their compatibility with local communicative practices. As was suggested in Chapter 3, such an exercise will help the researcher avoid the procedural problems that threaten rapport, disrupt interviews, and greatly confound the analysis of interview data.

Second, as I argue in Chapter 6, interview techniques rely primarily on the referential or descriptive function of language and on knowledge that lies within, in Silverstein's (1981a) terms, the limits of awareness of speakers. This means that interviews will be totally ineffectual in dealing with some topics, and they certainly will exclude important facets of those subjects that can be treated in interviews. It is thus crucial to design a methodological plan in such a way that interview data are systematically supplemented with other types of information whenever possible.

I pointed out in Chapter 4 that a close analysis of native metacommunicative routines can provide rich data on problems of interest to social scientists. These routines, rooted in the society's communicative patterns and closely tied to the social context of the interaction, are less likely to be idealized or decontextualized than are responses to interview questions. Accordingly, they are less subject to the imposition of the researcher's own categories and presuppositions on the data. This recommendation is hardly unprecedented, because writers who focus on methodology often note that interviews should be supplemented by observation (Johnson 1975; Langness and Frank 1981:50; Pelto and Pelto 1978:74; Riley and Nelson 1974; Spradley 1979:32; Webb et al. 1966; Whyte 1943:29–30; Williams 1967:28).

My recommendation goes beyond this basic principle. I am rather proposing a *systematic* integration of a wide range of metacommunicative routines into research methodological guidelines. I would also like to suggest that the process of selecting these routines and determining their role in the research be based on a preceding analysis of the society's communicative patterns.

One methodological concern that is generally seen as relatively minor weighs quite heavily in this type of analysis. The abbreviatory nature of notes taken during or after interviews or other interactions may preserve a good deal of the *referential content* of the utterances, but the *form* will prove elusive. On the other hand, tape-recording interviews and other events is quite important. This enables the researcher to conduct a detailed study of the form of the discourse in these events. One of the most important issues I have raised is that formal features, from the smallest details to the largest structural units, index the metacommunicative properties of the speech. The sensitive researcher may be able to discern some of the metacommunicative features; such properties are, however, extremely subtle, and most are not consciously accessible in the course of an event. Tape recordings, on the other hand, can be reviewed time after time, transcribed closely, and can be presented to one's consultants for comment.

Tape recordings are also interpretively open-ended, like any text in the native language. As the researcher's social–cultural and linguistic competence grows, new dimensions become apparent. New theoretical understandings can similarly be applied to the original recordings to see if they can resolve persistent problems. Notes are frozen at the level of competence possessed by the researcher at the time of their writing, and they are much less useful in exploring new theoretical orientations.

The situation with videotaping is less clear, in my opinion. I have used it, and quite successfully, I think, during my two most recent field stays. I formerly based my hypotheses about the nonverbal correlates of speech events on my memory of the most salient gestures, body postures, and so on. The videotapes show the nonverbal components in detail, and this has greatly added to my understanding of the contextualization of the verbal forms. I have also had much better results in

eliciting commentary from participants with video rather than audio recordings. Consultants greatly enjoy seeing themselves on their own television sets, and they often become quite voluble. One elder became nearly ecstatic while viewing the tape of the conversation we had just completed, and commented in detail on the historical and cultural bases of his statements. After the tape was over, he noted 'This is a very important day for me, Carlos. I had never even heard the sound of my own voice before now.'

On the other hand, video equipment is vastly more intrusive than a small cassette tape recorder with built-in condenser microphone. I find that the presence of the video camera often gives me, as the researcher, much more control over the interaction. Although awareness of the recording equipment decreases over time, the participants do not become oblivious, as witnessed by references to the presence of the camera. This enhanced self-consciousness can lead to a shaping of one's behavior in accordance with the image one wishes to project. Speakers thus focus more on monitoring the referential content of their words; this frequently inhibits the use of very context-sensitive forms, such as proverbs.

This process of accommodating interactional patterns to the presence of the camera does not render the data invalid or useless. The point is not to attempt to eliminate the effects of the researcher's own presence, a fruitless and theoretically unsound goal. The impact of the video equipment on the speech event can provide fascinating insights with regard to which facets of communication lie within the limits of awareness and the conscious control of natives. Nevertheless, videotaping should be carefully complemented by audiotaping and observation. As is the case with tape-recording, awareness of local communicative norms will help the researcher gauge when it will be appropriate to record and how taping is likely to affect the interaction.

Phase 3: reflexivity in the interviewing process

Once the interviewing has begun, this sketch of the local communicative economy should inform periodic checks on the effectiveness of one's interviews. A good means of undertaking such an evaluation is to analyze a selected interview in detail; a tape-recorded example is a far more reliable source for this task than a reconstruction. Some revision of the Jakobson–Hymes model of the communicative event, such as the one I presented in Figure 2, provides a good starting point for initiating such evaluations. Each of the components – interviewer, respondent, audience, message form, reference, channel, code, social

roles, interactional goals, social situation, and type of communicative event (along with key, genre, and other factors that prove to be important to one's own situation) – should be examined in terms of their role in shaping the meaning of what is said by both parties.

This analysis of the manner in which the researcher's and the consultants' conversational norms are juxtaposed in the interview will increase the former's awareness of the conversational loci of procedural problems. It will enable the investigator to discern where she or he has misconstrued the meaning of the responses, thus heading off possible errors in the interpretation of the data. Likewise, periodic evaluations will enable the researcher to progressively reduce the scope of the difficulties that procedural problems pose for the success of the research. This awareness can permit researchers to avoid the faux pas that reduce the coherence of the discourse and render their interviewees less willing and able to respond.

Going over selected interviews with consultants can be quite useful. Such assistance can be obtained by soliciting aid in transcribing and/or interpreting the interview. I have learned a great deal by turning on a tape recorder while I replay a videotape of an interview or other speech event with the participants. They frequently go into great detail with respect to why they made a given statement, why it is true, how others would disagree, and so on. My experience suggests, however, that the interviewees themselves are less likely to point out the ways in which the researcher has violated the norms of the speech situation or misconstrued the meaning of an utterance than are persons who did not participate in the initial interview.

Microanalyses of interviews will in turn provide a new source of comparison with data from other communicative events. Paying attention to the different ways in which topics are addressed in different social situations will help round out, so to speak, impressions derived from a given means of data acquisition. Once again, such comparisons will enable the researcher to see more clearly where interviews will produce gaps in the data. Analysis of the interviews and their juxtaposition with metacommunicative material from other events will permit ongoing revisions in research plans. It may be necessary to explore a wider range of speech events or to change one's mode of participation in them in order to obtain information on certain topics. It might, for example, be wise to ask a native or a co-researcher of the opposite sex to record a given event if one's presence is precluding certain types of discussion. I also think it is particularly important to look over as many of the research results as possible about three months before completing the study. This will minimize the possibility that major hiatus will plague the interpretation and write-up of the materials.

Phase 4: analyzing interviews

If I were to try to put my finger on the single most serious shortcoming relating to the use of interviews in the social sciences, it would certainly be the commonsensical, unreflexive manner in which most analyses of interview data are conducted. As Cicourel (1974c:22) has put it, "questions and answers are presumed to possess 'obvious' significance." It is simply assumed that different responses to roughly the same question are comparable. The usual practice thus consists of extracting statements that pertain to a given theme, event, symbol, or what have you from field notes or transcriptions. These responses are then juxtaposed, yielding a composite picture of things that seem to go together in the eyes of the researcher on the basis of referential, decontextualized content.

With respect to anthropological fieldwork, this technique used to serve as a starting point for analysis when ethnographers were urged to file their field notes directly into categories provided by Murdock et al.'s *Outline of Cultural Materials* (1950) or the Royal Anthropological Institute's *Notes and Queries on Anthropology* (1951). Some researchers now index their field notes and/or transcriptions in terms of major dates, events, names, and the like; they then feed this information into a computer. At the push of a button, the machine accomplishes their decontextualization for them automatically.[1]

The development of a more sophisticated approach to the analysis of interview data is imperative. The communicative blunders described in Chapter 3 point to the complexity of the interview process and to the many factors that can give rise to procedural problems. Yet the goal of analysis cannot simply be to control for or eliminate such problems. This approach would preserve the fallacy that underlies the "bias" research on the interview I critiqued in the first chapter. Interviews are cooperative products of interactions between two or more persons who assume different roles and who frequently come from contrasting social, cultural, and/or linguistic backgrounds. A mode of analysis that envisions interview data as, even ideally, a direct outpouring of the interviewees' thoughts or attitudes obscures the nature of the interview as a social interaction and a communicative event. Such a perspective also misses the point that the interview situation itself is a rich source of data if it is viewed as an object of analysis as well as a research tool.

One of the major findings that emerged from an analysis of my own communicative blunders is that *the communicative structure of the entire interview affects the meaning of each utterance.* To cite one instance: My initial interviews with Silvianita and George López were strained and relatively unproductive, and it was many years before I was able to appreciate why this had been the case. I had never considered the possibility that the Lópezes might not accept my definition of our interactions as interviews. The Lópezes viewed these sessions as pedagogical encounters between two elders and a young person with little knowledge of the community, *Mexicano* culture, or New Mexican Spanish. Even after I published a volume on the Lópezes and other carvers (1980), the couple told their visitors that I had come to learn how to carve. (They noted that I had indeed become a proficient carver but then, for some reason, had given up the work.)

My initial questions met with responses that seemed superficial or irrelevant or with a strident 'Who knows!' Overlooking their perception of our relationship would lead me to believe that they were fairly ignorant of the history of their family and its carving. What they were really trying to get across, in fact, was that I had to learn to respect them as elders as well as to discover which questions were relevant to them and the basic cultural assumptions that underlie the answers.

A different sort of example is provided by my misinterpreting statements when I did not take the speaker's interactional goals into account. For instance, my question, 'Were there any *ricos* here in bygone days?' emerged from my desire to collect data on the history of social inequality in the community. Mr. Trujillo was willing to help me satisfy my need for such information. But his answer also addressed his own desire to induce me to internalize basic *Mexicano* values of religiosity and corporatism. This anecdote exposes a very general phenomenon. The unifunctional utterance, one that accomplishes only one communicative function, is rare, at least in conversation. Statements nearly always relate to two or more features of the communicative situation, such as distinct interactional goals, at the same time. If one considers each "answer" only in the context of the preceding question, then a great deal of meaning will be lost.

What is needed is a means of interpreting interview data that will assess the manner in which each statement fits into this communicative web and will thus have the best chance of yielding an adequate interpretation of its meaning. I thus propose a two-step process, one that begins with the structure of each interview as an interactional whole and then proceeds to the identification of the metacommunicative properties of the individual utterances.

The structure of the interview

I will draw on the Jakobson–Hymes model of the communicative event (see Figure 2) in framing my remarks on the structure of the interview as a whole. The model simply serves as a heuristic device in assessing the range of elements that *might* be of importance in a given interview. Researchers will certainly have discovered by this point, however, that some of the Jakobson–Hymes components may play a relatively insignificant role in any particular interaction or perhaps in a given speech community as a whole. Similarly, elements that do not figure in the analytic model may prove crucial. Researchers should have developed a good working sense of each major type of speech situation regarding the range of components that should be checked and of the range of communicative functions they can convey.

Perhaps the most basic maxim to be followed is that the interview must be analyzed as a whole before any of its component utterances are interpreted. This process can proceed much more quickly and adequately if the researcher takes relatively detailed notes on each interview. Once the interview is over (and generally after returning to one's residence), the investigator should note important facts that will not appear on the record of the interview itself, be it a tape recording or a video recording or a set of notes. Detailed notes on the setting, participants, time of day, ongoing social or ritual events, and so forth should be complemented by the researcher's perceptions of the interaction. This procedure may be impractical when the project focuses on quantitative analysis of data from a large survey. The inclusion of even minimal contextual information at the end of the schedule would, however, greatly facilitate interpretation of the statistical patterns.

In beginning the analysis, compare such notes with the transcript (if available). What major themes were stressed in each participant's statements? How was each reacting to the interview and to the other participants? As argued in Chapter 3, it is particularly important to look for possible divergences in interactional goals, perceptions of the nature and purpose of the interaction, and the like. If these cues are missed, they are likely to lead the researcher to misconstrue his or her consultants' remarks.

A second step is to map out the linear structure of the interview. Many interviews proceed from informal conversation to introductory statements and/or questions, to broad questions, to more detailed questions, and then return to informal dialogue before the participants shift to other activities or the researcher leaves. Significant interactional units may also be segmented by the arrival or departure of

participants, movements from one topical focus to another, activities (such as cooking, eating, or working) occurring simultaneously, and the like. This sketch should indicate major changes in key, tone, or genre.

The initial stage of the analysis thus consists of identifying the components of the interview and interpreting their communicative functions. It may be useful to plot the most important features on a series of sheets of paper. A visual representation is helpful in discerning the outlines of the communicative forest from amid its many trees. However one may approach it, a synthesis of the components and functions is the next step. As Jakobson (1960), Mukařovský (1977a, 1977b), and others have argued, the meaning of an utterance or other sign is tied to the *interaction* of its constituent components and their functions. Even if a response appears to be oriented toward the referential function – providing information on the topic specified in the question – its meaning is dependent as well on the coexisting communicative functions. The interview is a gestalt produced by the interaction of all these parts. In assessing the role each element plays in this process, consider the manner in which the functioning of each component is affected by the roles of the others. For example, code-switching between Spanish and English is affected by the competence of the participants in each language, the social relationship between them, the topics under discussion, the social situation (e.g., formal vs. informal, ritual vs. everyday), the genre and key of the discourse, and so on.

Interpreting individual utterances

We are now in a position to be able to address the needs of the researcher who is really not interested in the interview qua speech event, but in the bearing of a series of responses on the topic at hand. The proposed mode of analysis provides both a head start and some insurance for the interpretation of individual statements. Having identified the utterances that address the subject in question, the analyst can focus on ascertaining how the specific utterances fit into the broad communicative outlines that have been sketched for the interview as a whole. As Agar and Hobbs (1982) have shown, the meaning of a response may emerge from its relation to utterances at any point in the preceding discourse. A few hours of auditing tapes, reading notes and transcripts, and thinking about the interview places one in the best position for discerning the broader significance of the responses. This greatly decreases the danger of coming up with narrow or erroneous interpretations.

Metacommunicative features. Two concepts, metacommunication and contextualization, provide excellent keys to the interpretation of individual statements. In studying the metacommunicative properties of utterances, we are examining their capacity for simultaneously commenting on communicative processes (including the interaction itself) and indicating a referent.[2] This task has been stimulated in recent years by the advent of ethnopoetics. Hymes (1981) in particular has shown how a close analysis of the *form* of oral literature provides a sounder basis for interpretation than deductions based on content alone.

This leads me to the proposition that speech, whether contained in interviews, myths, or "natural" conversations, provides an ongoing interpretation of its own significance. This interpretation is conveyed mainly in stylistic terms. Thus, if the analyst pays close attention to how a statement is made, he or she will find clues to the interpretation the speaker wishes to attach, so to speak, to the words. These stylistic cues can be (and usually are) embedded in any part of the message form, including its visual (gesture, gaze, proxemics, etc.), prosodic (intonation, loudness, stress, vowel length, phrasing, pitch, etc.), and verbal dimensions. Lexical selection, pronominalization, verb tense and aspect, the operation of optional syntactic transformations, and the like enable speakers to choose between referentially equivalent forms that will convey entirely different messages about the topic in question.

Both the range of stylistic devices within individual languages and the variation between languages preclude offering any simple formulas for discerning the interpretations embedded in texts. But the researcher will already have two useful tools for interpreting utterances. First, the researcher can draw on her or his analysis of the overall structure of the interview. This should provide a good sense of the range of factors that shape specific statements. Here the exceptions prove the rule: The key to the meaning of individual utterances often lies in their *departure* from the communicative norms of the conversation as a whole. Sudden changes in prosodic features, lexical range, or other stylistic elements frequently point to the presence of a new interpretive frame, such as sarcasm or joking. Likewise, many metacommunicative devices function similarly to the conversational metasigns described in Chapter 3 in that they serve to articulate the relationship between individual utterances and the overall structure of the discourse. Having this broader frame in mind is the best insurance against overlooking the presence of these forms and the ways in which they shape the meaning of responses.

A second tool for discerning the metacommunicative properties of interview responses will already be in the researcher's hands at this

point. The findings from Phase 1 of the research will have attuned the investigator to a wide range of metacommunicative forms and functions used in that speech community. The analysis of native metacommunicative routines is particularly useful in this regard. Our ability to interpret the role of metacommunication in interviews is frustrated by the nature of the interview as a communicative event. Interviews are attractive in that they present the possibility of gathering a mass of data on topics selected by the researcher in a short amount of time. Researcher and interviewee implicitly agree to foreground the referential function of language and to suppress most of the stylistic and social constraints that normally impinge on transmission of information on these topics (i.e., as they are conveyed in ritual, production, etc.).

This does not engender a total dearth of metacommunicative elements; it does, however, greatly reduce the degree to which they rise into consciousness, particularly that of the interviewer. When it comes to native metacommunicative routines, however, this bias toward the referential coding and the decontextualization of forms is generally absent. In transferring awareness of the role of metacommunicative elements from the latter realm to the former, we increase our chances of perceiving the role of these processes.

Contextualization. I have argued that discourse contains features that signal (generally implicitly) how messages are to be read. This led me to suggest ways of enhancing our ability to read the interpretation embedded in the text. These procedures are designed to reduce our tendency to propound interpretations that have little basis in the text itself. This does not mean, however, that "reading" texts, be they interviews or anything else, is a mechanical process that draws on the interpreter's consciousness as a mere scanning instrument. The basic task is still the same: trying to figure out what the devil that person was trying to get across. The procedure is similarly analogous – examining the myriad details of what is said and done in order to connect them in such a way that the interpreter feels relatively confident that she or he has made sense of the discourse.

Looking for metacommunicative elements enables the researcher to base his or her interpretation on what the speaker is saying not only about "the world out there" but also about the researcher's own words and the manner in which the utterances as a whole relate to the circumstances of their production. This does not, however, guarantee that the interpreter will have identified all of the metacommunicative features and grasped their communicative functions. Is there no way of rechecking one's perceptions against the text, that is, asking the speakers if we have understood them?

Taken literally, the notion is absurd. The researcher can, of course, go back to the interviewee and ask if the interpretation is correct. This can produce interesting data on native textual analysis or literary criticism, but it hardly solves the problem. Human introspective capacities do not necessarily extend to recalling exactly what one was intending to say at some point in the past. Likewise, most metacommunicative features are not fully conscious, and speakers are unlikely to have perceived them at all (cf. Gumperz 1982:131–2; Silverstein, 1979, 1981a). In any case, taking a tape or transcript back to the interviewee(s) creates *another* speech event, and its contextual elements will shape the consultant's remarks along other lines. What is needed is some means of rechecking one's perceptions against those of the participants at the time.

One can do precisely that, if in a slightly roundabout manner, while analyzing the conversation. Participants are constantly exchanging implicit messages as to how they perceive the speech event and how they want their utterances to be interpreted. They are also continually checking to see if their perceptions are shared by the other participants. This process has been captured by Cook-Gumperz and Gumperz (1976) under the aegis of "contextualization." They argue that communicative contexts are not dictated by the environment but are *created by the participants in the course of the interaction*. Similarly, contexts are not conditions that are fixed at the beginning of an interaction, remaining stable until its termination.

Cook-Gumperz and Gumperz suggest it is accordingly necessary for speakers to provide contextualization cues to signal which features of the social and linguistic setting provide frameworks for interpreting their remarks. Participants monitor each other's words and actions in order to see how their interlocutors perceive the context, and this is particularly useful to researchers in their efforts to assess the validity of their own interpretations.

A variety of types of signals are used by co-conversationalists in ascertaining whether or not their perceptions of the communicative event are shared. Some of these are explicit, such as when we ask, "Are you being sarcastic?" "Is that a joke?" "Do you really mean that?" and the like. Although such queries generally present themselves as responses to the ambiguity of the preceding utterance, this is not always the case. As I noted above, *Mexicano* elders continually interject interrogatives such as *¿ves?* 'do you see?' *¿sabes cómo?* 'do you know what I mean?', *¿no?* 'no?' or 'really?' in the course of pedagogical dialogues to assess the comprehension of their pupils. Conversational uses of proverbs and other genres feature an elicitation

of the listener's comprehension of and agreement with the speaker's point as a central component of the performance (Briggs 1985a).

It is, however, far more common to use implicit messages, features that hide, so to speak, behind the referential context of what is said, in contextualizing utterances. Specialists in nonverbal communication have conducted a great deal of research on the way speakers use visual signs in providing interpretive frames for verbal messages (cf. Birdwhistell 1970; Hall 1959, 1966, 1977; Kendon 1972, 1973, 1977, 1978; Kendon, Harris, and Key 1976; Scheflen 1965, 1966). Interlocutors use visual contact to provide a near-constant means of monitoring the contextual cues of their fellow participants. Speakers draw on a wide range of signals, including extending one's hands with cupped, upturned palms, shrugging the shoulders, and lifting the head and/or eyebrows, to elicit indications of comprehension and agreement. They shape their utterances from moment to moment in keeping with both solicited and impromptu responses from their listeners. A look of boredom may prompt a reassessment of the relevance of one's remarks, while a visual sign that the hearer is confused often elicits an elaboration of material that had been presupposed, together with a repetition of the utterance. The value of the visual track as a means of assessing the meaning speakers attach to their words provides a strong incentive for videotaping at least some interviews.

The conversational analysis group has identified a host of devices that enable co-conversationalists to coordinate their turns at talk (cf. Duncan 1973, 1974; Duncan and Niederehe 1974; Jefferson 1972; Sacks 1967; Sacks, Schegloff, and Jefferson 1974). "Huh?", "right?", "yes?", "okay?", "you know?", "see?", and so forth have traditionally been viewed as mere fillers, phatic signals used to keep the channel open until we think of something to say. Research has shown, however, that they provide the person who dominates the floor with a great deal of feedback with respect to the manner in which her or his interpretations of the interaction are shared by the other participants.

This process is requisite to adequate comprehension in dialogue. As Gumperz (1982a, 1982b) and others have shown, interpretive frames are often divergent, leading co-conversationalists to misjudge their interlocutors' intents. Gumperz (1982a) has shown that such miscommunication frequently occurs in interactions between members of different classes and/or ethnic groups within a single society, and interviewers are hardly exempt from this process. Researchers similarly encounter difficulties in communicating with members of another society or a different group within their own society using a pattern of interaction possibly unfamiliar to the latter. Discerning such malcomprehension is impor-

tant for two reasons. First, it is important that the researcher does not simply preserve the misunderstandings of the interview, both his or her own and those of the interviewees, in the course of the analysis. If the respondent did not understand the question and the analyst does not realize this, the meaning of her or his "answer" will be distorted.

The second reason involves the value of these "errors" as sources of data. As scholars have long noted with respect to metaphor and ambiguity (cf. Fernandez 1972, 1974, 1977; Ricoeur 1977; Sapir and Crocker 1977), disentangling cases in which interpretive frameworks are not fully specified or are shared only in part can provide powerful insights into the nature of social–cultural and communicative norms. Some of the most interesting situations emerge when the participants realize that something has gone awry. This usually invokes procedures for renegotiating a common frame. (See Jefferson [1972] on "side sequences" and Churchill [1978] on mechanisms for repairing procedural problems.)

These moments provide particularly fruitful means of comprehending interpretive frames: Calling the contextualization process into question brings it much closer to the surface of consciousness. Both referential and other communicative functions are brought to bear reflexively on the task of interpretation. Researchers may thus profit from paying close attention to the way their consultants check to see if they share a common interpretation of the meaning of what is being said and how they deal with situations in which this is not the case.[3]

Studying the manner in which participants in interviews monitor each other's interpretive frames still does not guarantee the analyst that his or her account is correct and/or exhaustive. It and the other steps outlined in this chapter do, however, enable researchers to base their interpretations as much as possible on those of the respondents. The technique leads the analyst away from literal, narrowly referential meanings and toward grasping the broader pragmatic significance of what is said. The procedure helps the investigator avoid the errors in interpretation that result from differences in communicative as well as basic social–cultural norms between researcher and consultants. The preceding discussion of malcomprehension points to the way in which greater methodological sophistication can turn interviewing pitfalls into important sources of data. In a word, developing interview techniques that fit the metacommunicative norms of the society in question provides a basis for overcoming a number of the problems that have diminished the depth and the accuracy of social-scientific research.

Presuming that the researcher has now grasped the significance of the interview data, the question then becomes one of the best way to

present these findings. Obviously, this process follows from the proclivities and the research interests of the individual. I would like to argue, however, for the importance of describing not only the content of respondent statements but their interpretive framework as well. Since the metacommunicative dimensions inform the investigator's analysis, readers must be provided with at least a sketch of such features if they are to be in a position to judge the interpretation competently.

One way to answer this challenge is to provide substantial excerpts from the transcripts, either in the text or in appendixes. It is important to resist standard editorial policies and the urgings of many manuals[4] that prescribe deletion of both the interviewer's questions and all back channel cues from the transcripts. As the reader will certainly have gathered from the preceding pages, this method does expose the role of the researcher, including his or her ungrammatical sentences, faux pas, and general naiveté. Now that arguments for the obligation of the practitioner to account for her or his own contribution to the data-collection process are becoming more prevalent and more forceful,[5] however, the mask of "scientific objectivity" no longer provides such an effective means of avoiding this kind of exposure.

6. Conclusion: theoretical quagmires and "purely methodological" issues

The preceding pages have hardly eschewed theoretical issues. Thus far, however, theory has been used primarily as a means of highlighting the problems inherent in interview techniques, exploring their theoretical roots, and pointing the way to methodological progress. Such discussion is not, in and of itself, sufficient to show that the adoption of a critical perspective on interviewing is requisite to theoretical advances in the social sciences and linguistics. But the lack of a critical perspective on interview techniques is tied to a number of fundamental theoretical obstacles.

My thesis is that methodological shortcomings have both emerged from and in turn reinforced these theoretical quagmires. The problem is that the goals of social-scientific and linguistic research lie beyond the confines of this highly circumscribed process. The only way to break this pattern is to raise methodological questions from the inferior status they currently enjoy, explore the interpenetrations of theoretical and methodological problems, and revise methodology in the light of theory *and vice versa.*

A number of important theoretical advances have been made in the last two decades. Scholars from diverse disciplinary and theoretical perspectives have moved away from an emphasis on static structures and codes as abstracted from human conduct. Research has focused increasingly on the way codes relate to messages, or structure to action, and the manner in which the system is transformed through use. Social–cultural anthropology, for example, has moved away from viewing culture as monolithic and static toward analyzing the way in which cultural systems are instantiated in individual events by concrete persons (cf. Crick 1976; Geertz 1973). As the authors of a recent assessment of the field put it, symbolic anthropology is based on the assumption that "the constant tension between individual experience and the collective means for expressing and interpreting that experience is the dynamic relationship by which culture comes to be and through which it is constantly changing" (Dougherty and Fernandez 1981:413).

Linguistics has taken a similar turn. Saussure (1959) deemed the units of *langue,* the abstract entities in language that hold constant between contexts, the real objects of linguistic inquiry. The role of context in determining the meaning of signs was accordingly relegated to the periphery. The Chomskyian revolution in syntax and ethnoscientific approaches to semantics advanced this concern with ideally context-independent units and their relations in linguistic systems to a new level of abstraction.

Now few practitioners adhere to Saussure's rigid dichotomy between *langue* and *parole,* and even fewer are willing to accept his banishment of the latter from the sphere of serious scholarly study. Many linguists have similarly rejected Chomsky's (1957, 1965) emphasis on "competence" apart from "performance" and his reliance on introspective data. Hymes (1964, 1972, 1974a), Fishman (1964; Fishman, Ferguson, and Das Gupta 1968), Labov (1966, 1972a, 1972b), and others elevated the study of *parole* 'speaking', to the status of a major field of linguistics – sociolinguistics. A concern with close analyses of conversations and various kinds of texts has stimulated the growing areas of conversational and discourse analysis. The application of such techniques to oral literatures has given rise to ethnopoetics (cf. Hymes 1981; Tedlock 1983). Practitioners have become increasingly aware that the meaning of what is said is shaped by accompanying prosodic and nonverbal cues; a priori assumptions regarding the nature of the relationship between communicative modes is giving way to collaborative research between specialists in these different areas.

The most encouraging facet of these developments is their relationship to advances in other disciplines. A number of philosophical movements have contributed insights into questions of meaning and communication. "Speech act" theorists, particularly Austin (1962) and Searle (1969; 1979; 1983), have heightened awareness of the formulaic and performative character of speech. Phenomenological and hermeneutic traditions have the increased sophistication of textual interpretation (Gadamer 1975, 1979; Rabinow and Sullivan 1979; Ricoeur 1979, 1981).

In sociology, the work of Goffman (1959; 1974, 1981) and the ethnomethodologists, particularly Garfinkel (1967, 1972) and Sacks (1967, 1974), has greatly influenced the study of communication and social behavior in general. Cicourel (1964, 1974a, 1982a, 1982b, 1983) has challenged social scientists to pay more attention to the linguistic underpinnings of their research and to make explicit the ethnographic background information that informs their analysis. These approaches echo Schutz's (1962) concern with the phenomenological underpinnings of the reality of everyday life. These writers have stimulated a

great deal of research on the nature of the interpretive activities through which social structures are created and understood. This work, along with that of Gouldner (1970) and others, has challenged sociologists to be more explicit about the nature of their own interpretive procedures.

Social scientists and linguists have increasingly looked to literary criticism for insights into the process of textual analysis (see, for example, Bakhtin 1981; Bloom et al. 1979; Culler 1975; Derrida 1976, 1978; Jameson 1971, 1972; Kristeva 1980; Vološinov 1973; Williams 1977). Folkloristic research on genre and performance has expanded the range of speech acts examined by linguists and anthropologists and has pointed to the importance of oral textual traditions (cf. Abrahams 1983; Bauman 1975; Ben-Amos 1976; Glassie 1982; Paredes and Bauman 1971). This convergence is apparent in the growing tendency of practitioners to abandon narrow disciplinary lines in favor of exploring broader approaches to research problems.

Unfortunately, this major theoretical reorientation has not produced a corresponding methodological revolution. Although interviews constitute the central mode of data collection in the social sciences and linguistics, they are probably the least understood. Only occasionally have interviews formed the focus of theoretical analyses or ethnographic descriptions. Scholars have begun to compile descriptions of native metacommunicative routines and to demonstrate their usefulness as sources of sociolinguistic and ethnographic data. Because few practitioners have used research on specific routines in exploring the nature of metacommunicative competence as a whole, this body of research has not generated a rethinking of the way we communicate with our consultants. Cicourel, Dexter, and others have pointed to the problems inherent in received techniques (see Chapter 1). Nevertheless, their calls for research on the communicative nature of interviews and for the incorporation of such findings into research methodology have, by and large, gone unheeded.

Our inability to translate theoretical insights into solid methodological reformulations must be examined. Why does the gap between theory and methodology appear to be so large? Why are we so reluctant even to examine these questions? I suggest that these methodological shortcomings are not arbitrary or inexplicable. I also believe that the apparent hiatus between theoretical and methodological concerns is illusory. It is rather the case that we have banished a number of fundamental preconceptions, like Freudian neuroses, from the realm of conscious theory, only to re-create them on a subconscious methodological level.

In Chapter 1, I related our uncritical acceptance of interview techniques to their ubiquity in social-scientific and linguistic research as well as in the native linguistic communities of scholars. I now want to advance the proposition that the received perspective on interviewing emerges primarily from two basic assumptions. I contend that interview techniques are prima facie expressions of our underlying, generally unstated theories of communication and of reality. If we look just below the surface of interviews, we encounter the same persistent, problematical preconceptions whose existence as conscious theoretical models has come under increasing attack. Discovering this connection is requisite to achieving a better grasp on the nature of the interview and to making theoretical headway.

Referential bias and the problem of awareness

One set of preconceptions emerges from our folk views of language. As Silverstein has argued, scientists and philosophers who study language operate under the influence of their own "linguistic ideologies, . . . sets of beliefs about language articulated by the users as a rationalization or justification of perceived language structure and use" (1979:193). These ideologies draw on the conscious beliefs that native speakers have formulated with respect to the nature of language and language use. One of the most interesting facets of Silverstein's work is his demonstration that the relationship between language structure and native linguistic awareness is both selective and patterned. His thesis is that certain elements will be accessible to native exegesis, whereas others will evade conscious formulation and explication. These are, in his terms, unavoidably referential, surface segmentable, and relatively presupposing (1981a:5–7). These features are, I want to argue, encapsulated in the interview.

Unavoidable referentiality entails, in Jakobson's (1960:353) terms, the predominance of the referential or descriptive function of language. Native speakers will thus see the meaning as resting primarily on the ability of a stretch of speech to designate one or more aspects of their cultural universe and to convey a proposition about such. "Mary is a brave girl" thus delimits a specific human object and suggests that she falls within a recognized cultural category, that of "brave girls." The referential function focuses the attention of speaker and hearer on the referential *object,* not on the speech act itself. On the other hand, the referential content of Chomsky's (1957:15) famous example, "colorless green ideas sleep furiously," is both avoidable and beside the point. Rather, it directs our attention to the ability of grammatical systems to generate utterances that conform to syntactic rules but are

devoid of sense. Similarly, as Bloch (1975a) has pointed out, political rhetoric is frequently lacking in new informational content.[1] Nevertheless, such discourse can be extremely effective if this lack of emphasis on content is coupled with a clever use of stylistic features to create the desired effect on the audience. (I owe this point to Ronald Reagan.)

The axis of *surface segmentability* revolves around the way in which forms are analyzed. The meaning of "surface segmentable forms" accrues to units such as affixes, lexemes, and phrases that are susceptible to traditional syntactic and semantic analysis. In other words, speakers will be able to identify specifiable stretches of the words as they are spoken in decoding their meaning. In the above example, "Mary" is decodable as a designation for a specific individual, "is a brave girl" makes a statement about that person. Each component of the predicate phrase is in turn analyzable for its contribution to the meaning of the utterance as a whole. On the opposite end of this continuum we find stylistic features. Intonational changes, for instance, necessarily overlap in production with the referential content of speech. The meaning of the sentence "John is a very brave fellow," uttered with a "sarcastic" tone of voice, would be interpretable by native speakers. They would, however, have more difficulty identifying the elements of the utterance that signaled the fact that its overt meaning was precisely the opposite of what the speaker sought to convey.

A third axis of comparison, *relative presupposition,* entails the manner in which the context enters into the meaning of what is said. The most highly presupposing forms are interpretable only when certain facts concerning the context of utterance are apparent to all participants before the act of speaking. Use of the token "she," for example, presupposes a shared understanding of the fact that a nonpresent female is the subject of discourse. Lacking such knowledge, the hearer will be forced to ask "Who are you talking about?" A sudden shift in the formality index of speech, on the other hand, can change the social context of the conversation itself. When a status superior changes from formal to familiar forms or both interlocutors shift from the standard language to a local dialect, for example, this alters the manner in which the participants interact. Relatively presuppositional forms are thus opposed to their *creative* counterparts. The latter are not present before the time of utterance, are brought into being by the speech act itself, and themselves effect a change in the social situation.

Silverstein's argument is that linguistic forms that are unavoidably referential, surface segmentable, and relatively presupposing are the most readily available to consciousness. Interestingly, these are precisely the areas in which the interview is strongest. Interviews foreground the referential function. Both researcher and respondent implicitly agree to amass as much information about a given topic as possible. We accordingly see the meaning of interview data as emanating from the contribution of the surface forms to this propositional knowledge. Similarly, all statements are ideally decodable vis-à-vis the information shared by all participants in addition to that made available to them in the course of the interview. These canons pertain to interpretation as well as production, because the interview frame alerts the participants that messages will be decoded (and should be decodable) via the contribution of surface segmentable units to propositions under specifiable (presuppositional) conditions.

Interestingly, invocation of the interview frame also selects negatively for metacommunicative events that are less surface segmentable and more creative, and whose meaning hinges less on reference. This description closely fits nearly all of the *Mexicano* metacommunicative routines I presented in Chapter 4. Here stylistic features, such as tonality, pitch register, stress, phrasing, laryngeal constriction, Spanish/English code-switching, and so on, convey crucial information regarding meanings that are not decodable vis-à-vis business-as-usual referentiality. The less segmentable and referential messages provide the juxtaposition of clarity with indirectness that is the sine qua non of more advanced rhetorical strategies. The emphasis on creativity is obvious; speakers use their words to transform the parameters of the interaction itself.

The reluctance of researchers to pay more serious attention to native metacommunicative routines is also linked to standard modes of analyzing data. I argued in Chapter 4 that *Mexicano* metacommunicative routines, such as eliciting repetitions from young children, political rhetoric, and scriptural allusions, present native analyses of basic *Mexicano* cultural premises. These analyses are, however, not immediately apparent in the referential content of the surface forms. Discerning them entails learning how value-laden statements are embedded in formulaic (and in many cases folkloric) expressions. Similarly, the relationship between the referential content of the words and what is really being said is generally indirect. Such forms are also pregnant with indexical features that point to details of the ongoing context. Here we are more aware that limiting one's interpretation to the referential content of the surface forms constitutes taking the joke (or what have you) literally. It is harder to delude ourselves into thinking that the meaning of the interaction is independent of the context in which it was articulated.

But this is precisely what we do with interviews. Researchers ask for conscious models of social–cultural and linguistic events and processes – "Tell me what you . . ." Because we are drawing on knowledge that lies within the limits of native awareness, the data are rich in forms that are unavoidably referential, surface segmentable, and rela-

tively presupposing. The natives presumably also sing, dance, pray, feast, labor, and so forth about these issues. Such events generally convey this information indirectly, embedding it in elaborate metaphors and in the minute details of specific interactions. Interviews, on the other hand, are designed to extract this social–cultural or linguistic information from the contexts in which it is usually conveyed.

As Silverstein (1981a) has argued, submitting such knowledge to introspection leads to the transformation of the signs actually used in such contexts into maximally referential, segmentable, and presupposing expressions. In other words, relying on interviews allows us to accomplish an initial decontextualization of the data even before we begin the analysis. We are, in effect, asking the natives to reduce the information to precisely the type of forms that fit our native-speaker bias for unavoidably referential, surface segmentable, and relatively presupposing forms.

Overreliance on interviews thus frustrates theoretical progress in two ways. First, interviews suppress precisely the types of data needed to elucidate current theoretical concerns. Many researchers have sought, for example, to counter the structuralists' concern with codes as abstracted from use by studying what makes specific messages unique. Interviews reduce the variation between referentially equivalent messages and focus instead on the way messages elucidate the code. Analyses that center on interview data will thus frustrate efforts to appreciate the role of social and linguistic context, variation, performance and performativity, poetic structure, and the like.

Second, scholars from a wide range of disciplines have urged their colleagues to reject timeworn modes of analysis that isolate the referential content of what is said and then extract that content from its social and linguistic context (see, for example, Bauman 1975; Geertz 1972, 1973; Hymes 1974a, 1981; Silverstein 1976). But interviews provide us with forms that are maximally referential, surface segmentable, and presupposing. This leads us to believe that we can analyze responses simply in terms of a referential analysis of surface forms in keeping with explicit presuppositions. In other words, interview data lull us into being content with business-as-usual interpretive techniques.

A major theme of the present work has been that this practice is fraught with danger. Interview data are indeed decontextualized with respect to the social behavior they describe. But interview responses are equally contextualized; their form and content are shaped to fit the exigencies of the interview situation. This reversion to unsophisticated modes of analysis unconsciously undermines our attempts to expand analytic techniques to reflect theoretical advances, just as it jeopardizes the accuracy of our findings.

In short, recent trends in theorizing have made us increasingly skeptical of the bias toward referential, context-independent signs that is part and parcel of our own native theory of communication. The methodological underpinnings of accepted research techniques are, however, still based on these preconceptions. This leads us to place little reliance on speech events that confront us with linguistic forms that challenge the premises of our native theory. Rather, we concentrate on obtaining masses of data that precisely fit our vision of what talk is all about. Analysis then rejects the course charted by our own theoretical understandings in favor of following the dictates of our linguistic ideology. Research thus ultimately serves to reinforce our preconceptions rather than to draw on our consultants' understandings in broadening our horizons and deepening our comprehension.

Reflexivity

A second major obstacle to methodological progress is our implicit ontology, particularly our theory of social reality. Karp and Kendall (1982) have neatly characterized the degree to which our perspective on fieldwork follows "from behaviorist, reductionist, and naturalist premises, namely, that the object of anthropological inquiry is the 'stuff out there' and that that 'stuff' has as two of its primary attributes *stability* and *observability*."[2] In other words, if social facts are, as Durkheim (1938) would have it, like rocks, we need not concern ourselves with the effects of our actions (including the asking of questions) on our consultants' behavior, or with the fact that our perceptions are mediated by our own personal, cultural, and conceptual orientations. This conception neatly excuses the social scientist from the task of examining his or her own role in the process.

Articulating such claims has become highly unfashionable, and many practitioners have admitted the importance of reflexivity, or reflection on one's relation to the research situation (cf. Babcock 1980; Ruby 1980, 1982). Theorists have similarly replaced the objectivity-*versus*-subjectivity question with an awareness of the fact that social scientific investigations entail intersubjectivity, the creation of a psychological link between two or more minds. The role of social–cultural, theoretical, and personal predilections in the perception, interpretation, and translation of data is now well known.

Karp and Kendall's (1982) discussion of anthropological research provides a case in point. They argue that fieldwork does not simply involve learning to think like the natives. It is rather a means of learning to provide a rapprochement between native meanings and the re-

quirements of anthropological discourse. The last two decades have witnessed the emergence of an anthropological genre in which ethnographers detail their actions in and reactions to the field situation (see, for example, Belmonte 1979; Berreman 1962; Crapanzano 1977, 1980; Dwyer 1982; Rabinow 1977, 1982; Ruby 1980, 1982). If discussion of one's relation to the fieldwork process is treated as a task that is independent of interpreting the data, however, the status quo of linguistic and ethnographic reporting will be preserved. The problem of translating awareness of the need for reflexivity into procedures for systematically analyzing the effect of the fieldworker's presence on the data has scarcely been discussed.

Our reliance on interviews and our uncritical approach to them is partly responsible for this lack of progress. When we begin an interview, our questions direct the attention of the participants *away* from the ongoing social situation. We collaborate in using the referential power of speech to transport researcher and interviewee(s) alike to another place and perhaps another time. Ordinarily, the researcher will have played no role in the events or processes under discussion. This facilitates our false consciousness of objectivity – the illusion that the object of scrutiny is "out there" and operates independently of our actions; we accordingly excuse ourselves from the need to consider our relation to it.

Nothing could be farther from the truth. This collectively created portrait of "the real world" is produced to satisfy the goals of an interaction in which we have a leading role. Given the fact that the researcher plays the dominant interactional role in interviews, her or his participation must be assessed in analyzing each datum that emerges from this setting. Decontextualizing what is said by focusing exclusively on the referential content of responses irrevocably separates the act of self-examination from the task of interpreting the meaning of what is said. As was the case with Humpty-Dumpty, no methodological sleight of hand can ever reunite them.

Politics and methodology

What, then, does it mean to rely primarily on interviews in one's research and to analyze the data in the received fashion? I have argued that interviews constitute remarkably clear encapsulations of our theories of communication and of social reality. I have also tried to show that many speech communities possess quite different norms with respect to what constitutes knowledge and how one goes about acquiring it. In the case of anthropological fieldwork, for example, recall that the

standard modus operandi is to seek out individuals who are particularly good at answering our questions and to "train the informants to conceptualize cultural data in the frame of reference employed by the anthropologists" (Pelto and Pelto 1978:72). In other words, rather than learn the natives' means of acquiring information, we commonly impose our communicative norms on our consultants. This practice amounts to *communicative hegemony*.

Given the degree to which interview techniques form powerful encapsulations of our folk views of life and language, what we are exporting is not simply an approach to research. The formula is classic: members (primarily) of the middle and upper middle classes of dominant, Western societies enter into other communities. Once there, they impose a priori notions of the most efficient means of accomplishing their goals. The goals themselves are not intrinsically malignant; I firmly believe in the merits of seeking a deeper understanding of ourselves and of human conduct in general. But the ways in which we go about doing it indicate that many of our efforts are fraught with the same contradictions inherent in more exploitative journeys to other lands. This suggests that communicative hegemony is a rather more subtle and persistent form of *scientific colonialism* (cf. Galtung 1967; Hymes 1969b).

This process holds grave consequences for research within our own society as well. Let us take an example that is of both immediate and central concern – research on gender roles and gender-based discrimination.

Carol Gilligan's *In a Different Voice* has received much scholarly and popular attention, and it has greatly stimulated discussion of the ways in which social scientific models contain sexist biases. She specifically argues that "theories formerly considered to be sexually neutral in their scientific objectivity are found instead to reflect a consistent observational and evaluative bias" (1982:6). She suggests that women speak "in a different voice," in other words, that "women perceive and construe social reality differently from men" (1982:171). Men, she argues, tend to view development in terms of separation and achievement, whereas women stress attachment. For women, this places awareness of the "reality of interconnection" with other individuals in interpersonal relationships at the core of perceptions of self and other (1982:172). She also notes that women are more likely to base their decisions on "a contextual judgment, bound to the particulars of time and place" (1982:58–9).

It thus comes as a surprise to learn that the "three studies [which] are referred to throughout this book and reflect the central assumptions of my research . . . all . . . relied on interviews and included the

same set of questions" (1982:2). Gilligan's conclusions regarding the importance of context and interpersonal relationships in women's experiences never moved her to question the advisability of using interviews as the sole source of data acquisition in her research. It should be stated in her defense that her choice of open-ended interviews may well have been motivated by the desire to avoid even more highly constrained encounters, such as questionnaires or experimental procedures. If so, however, this concern would seem to have led her to pay careful attention to the role of the interview as a social encounter in shaping the interview texts. Unfortunately, this is not the case. Gilligan analyzed the interview data in a highly decontextualized fashion; the reader does not even learn in a given instance whether the interviewer was female or male.

In the case of the abortion-decision study, for example, the twenty-nine participants ranged in age from fifteen to thirty-three, and they were quite diverse in terms of social class and ethnicity. The women were encouraged to participate in the study by clinic counselors for essentially therapeutic reasons (1982:3). It seems quite likely that some of the women perceived the interviews as therapy, others, as a contribution to research, while others may simply have viewed them as bureaucratic preliminaries to getting an abortion. These differences suggest that the interviews may have constituted vastly different types of communicative encounters in the eyes of the respondents; the role of such factors is, however, not explored (cf. Constantinople 1984:16; Nails 1983).

There is one exception. An eleven-year-old girl and boy named Amy and Jake both comment on a hypothetical situation in which a man must steal to save his wife's life. In a fascinating analysis, Gilligan notes that "the different logic of Amy's response calls attention to the interpretation of the interview itself" (1982:31). The answers initially suggest that the boy is more advanced in terms of moral development. Gilligan notes, however, that "it immediately becomes clear that the interviewer's problem in understanding Amy's response stems from the fact that Amy is answering a different question from the one the interviewer thought had been posed."

Gilligan thus comes face-to-face with the sort of problems inherent in interview techniques. Her argument with respect to the more contextual and interpersonal nature of women's responses should have led her quickly away from placing such heavy reliance on interview data or at least led her to analyze such data contextually. Rather than seize on her insight into Amy's response and radicalize her research methodologically, Gilligan reverts to a decontextualized mode of analysis.

My goal in criticizing Gilligan's methodology is not to undermine the

importance of her research. I am certainly in agreement with the conclusion: "Among the most pressing items on the agenda for research in adult development is the need to delineate *in women's own terms* the experience of their adult life" (1982:173; emphasis in original). I am, rather, concerned with the way that an uncritical acceptance of the methodological status quo reintroduces the very sorts of ontological and political biases that are being called into question. If her analysis is correct, then one could hardly find a less effective mode of capturing this "voice" than traditional interview-based research. Gilligan's methodology severs the data from a set of contextual factors and an interpersonal relationship that has profoundly affected the respondent's self-expression – the interview situation itself.

Research on gender roles provides a striking example of the methodological conservatism that characterizes social-scientific research. Feminist writers have addressed the crucial task of exposing cases in which research has added "scientific" legitimacy to gender-based discrimination. This has involved a radical critique of taken-for-granted concepts based on sexist assumptions. The same critical spirit has not fostered a comparable examination of the assumptions that underlie our reliance on naive interview techniques.

I have tried to show that interviews provide a particularly effective means of assuring oneself in advance that the discourse inscribed in the course of the research will be filtered and codified in keeping with predominant Western institutions and ideologies. Because interviews constitute powerful encapsulations of the societal status quo, sole reliance on interviews and decontextualized modes of analysis provide faulty means of collecting data that are less colored by "a consistent observational and evaluative bias." An exclusive reliance on interviews and decontextualized modes of analysis places feminist research in the unusual position of drawing female as well as male researchers into a relationship of communicative hegemony vis-à-vis women in order to understand women's experiences. I would accordingly add another item to the agenda for gender-related research – the need to listen to the way that women ordinarily articulate their experiences to other women.

My argument is that the close relationship between methodology and theory is paralleled by an equally intimate connection between methodology and politics. Just as interview techniques contain hidden theoretical and ideological assumptions, they are tied to relationships of power and control. The same patterns of inequality emerge from the relationship between controlling and subordinate groups within societies, between "developed" and "underdeveloped" societies, and be-

tween interviewers and interviewees. This leads me to suggest that our reluctance to examine the assumptions that underlie interview techniques and to adopt more socially, culturally, and linguistically sensitive research techniques is rooted, at least in part, in the desire to hold on to a rather comfortable position in a number of unequal relationships.

Most researchers will deny the existence of a political dimension to their methodological choices. Regardless of their possible incompatibility with the metacommunicative norms of the population in question, interviews will still be seen by many as providing the most rational and efficient means of acquiring large bodies of information that bear on the issues of greatest moment to social-scientific inquiry. After all, it will be argued, such research is undertaken in order to address "our" questions, not "theirs." But there is a hidden irony here. Given the state of interviewing practice, we often learn more about our own preconceptions and communicative norms than about the daily-life issues of the group in question. It is the objectivist ideology that underlies interview techniques rather than an empirical assessment of their strengths and weaknesses that leads us to believe we are capturing features of our subjects' behavior and belief.

The prevalent ideology of interviewing precludes us from ever seriously addressing the ecological validity problem raised in Chapter 1. Our methodology thus ultimately seems to hurt "us" more than it hurts "them." The opposite is true, however, in those cases in which the failure of our efforts to impose communicative hegemony, say on inner-city ethnic children, prompts us to label them cognitively, linguistically, and/or socially "deficient." In the case of inner-city ethnic children, this fallacy has often been legitimized by "scientific" research and enshrined in educational policy (cf. Labov 1972a).[3]

That the political dimensions of methodological issues are seldom examined or discussed should come as no surprise. We gather our data in encounters that focus on the topics under discussion rather than on the research encounter itself. We further this process in our analysis of interview data. By failing to consider the effects of the interview situation on responses, we circumvent the vital process of examining our own contribution to the generation of the data. Focusing on what the natives say and do thus keeps us from having to ask tough questions with regard to the effect of our actions on the data, and on the people we are studying.

If communicative hegemony were to be made explicit and propounded as a theoretical position, few scholars would find it acceptable. As an implicit methodological premise, however, it encounters little resistance. Methodology is, after all, "purely methodological," and it is not a valid object of serious study. A historian friend of mine

once quipped that theory for historians is like underwear: It's all right to have it, just as long as it doesn't show. This sort of repression seems to be directed at methodology in linguistics and the social sciences. But it prevents us from coming to terms with the fact that the received methodology acts as a hidden filter, blocking our ability to hear what "they" are saying while allowing the comforting sound of our own preconceptions about language and life to be echoed in the data. Our neglect of methodological questions and refusal to examine our role in generating the data preclude any departures from the status quo.

The path from theoretical obstacles to methodological shortcomings thus forms a tight circle. Only by considering methodology in the light of theory and pondering the theoretical baggage hidden on the methodological plane will we finally be able to chart a new course. But where are we to begin? Because we have spent so much time avoiding these questions, the first steps will not come easily. What we need is a specific, concrete focus for our initial efforts. I submit, by way of conclusion, that the most fruitful point of departure is learning how to ask.