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ANALYZING TALK AND TEXT

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The linguistic character of field data is most obvious in the case of texts and interviews. Even if our aim is to search for supposedly "external" realities (e.g., class, gender, power), our raw material is inevitably the words written in documents or spoken by interview respondents. Moreover, although observational data should properly include descriptions of contextual aspects of social interaction (what Stinson, 1986, calls "the sociology of space and place"), much of what we observe in formal and informal settings will inevitably consist of conversations.

Of course, if our data are transcripts of audiotapes, then we come face-to-face with how talk organizes the world. Although talk is sometimes seen as trivial ("mere" talk), it has increasingly become recognized as the primary medium through which social interaction takes place. In households and in more "public" settings, families and friends assemble their activi-

ties through talk. At work, we converse with one another and have accounts of our activities placed in dossiers and files. As Heritage (1984) argues, "The social world is a pervasively conversational one in which an overwhelming proportion of the world's business is conducted through the medium of spoken interaction" (p. 239). Indeed, what Heritage calls "the world's business" includes such basic features as telling news, deciding if one should commit suicide, and children's learning how to converse with their mothers (see Sacks, 1992a; Silverman, 1998b, chap. 1).

Yet a curiously prelinguistic sensibility pervades much social research. This is most obvious in quantitative researchers' preference for "operational definitions" that arbitrarily define the meaning of (linguistically mediated) phenomena. Less apparently, an inattention to participants' talk-in-interaction is shown by those qualitative researchers who claim to have direct

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access to the external "realities" mentioned earlier. These "realities" are often simply "read off" interview respondents' answers or transcripts of talk, with little or no reference to whether (and how) they are made reference to by the speakers. So, for instance, our "knowledge" that the speaker is a woman or that the setting is, say, a medical consultation is of no analytic relevance unless we can demonstrate the relevance of these features in the actions of the parties concerned (see Schegloff, 1991).

By contrast, 20th-century thought has resisted such researchers' assumptions that words are simply a transparent medium to "reality." From Saussure (1974), the Swiss linguist, we learn that signs derive meaning from their relation to other signs. From Wittgenstein (1968), the philosopher, we understand that the meaning of a word derives largely from its use. Consequently, as Wittgenstein puts it,

when philosophers use a word—"knowledge," "being," "object" (etc)...—and try to grasp the essence of the thing, one must first ask oneself: is the word ever actually used in this way in the language-game which is its original home?

What we do is to bring words back from their metaphysical to their everyday use. (para. 116)

Wittgenstein's critique of some philosophers of, of course, be turned upon those quantitative researchers who arbitrarily construct "operational definitions" of phenomena without ever studying the "language-game" in which a phenomenon has its everyday home. However, what Wittgenstein is saying constitutes an equally relevant critique of qualitative research that claims to discover social "realities" undressed by participants.

As Garfinkel (1967) implies, both quantitative researchers' scientism and qualitative researchers' claims for "empathic understanding" are deeply commonsensical, because both trade off the capacity of societal members to "see through" appearances to an underlying reality. In this sense, there is a strong similarity between social researchers who claim to be able to access some social structure or emotion "behind" their data and, say, TV sports commentators who tell their audiences what sportspeople are "feeling." Both parties use (as a tacit resource) what

portunity for an authentic gaze into the soul of another, or even for a politically correct dialogue in which researcher and researched offer mutual understanding and support. The rhetoric of interviewing "in depth" repeatedly hints at such a collection of assumptions. Here we see a stubbornly persistent romantic impulse in contemporary sociology: the elevation of the experiential as the authentic—the selfsame gambit that can make the TV talk-show or news interview so appealing.

Such qualitative researchers share survey researchers' assumption that interview responses index some external reality ("facts" or "events" for the latter group and "feelings" or "meanings" for the former). Both groups build into their research designs various devices to ensure the accuracy of their interpretations. So you can try to ensure that you have accurately depicted such realities and experiences by such measures as intercoder agreement and computer-assisted qualitative data programs. And you can check the accuracy of what your respondents tell you through other observations. Let us call this a *realist* approach to interview data.

An alternative approach treats interview data as accessing various stories or narratives through which people describe their worlds (see Holstein & Gubrium, 1995, 1997). This *narrative* approach claims that, by abandoning the attempt to treat respondents' accounts as potentially "true" pictures of "reality," we open up for analysis the culturally rich methods through which interviewers and interviewees, in concert, generate plausible accounts of the world (e.g., Gubrium, 1993; Voysey, 1975).

I am aware that many readers of this volume will favor the former approach. In this light, I want to give an example of how one realist interview study was eventually driven in a narrative direction. Jody Miller and Barry Glassner (1997) describe a study involving in-depth, open-ended interviews with young women (aged 13 to 18) who claim affiliation with youth gangs in their communities (Miller, 1996). These interviews followed the completion of survey interviews administered by Miller. Here is how Miller and Glassner (1997) describe the purposes of each form of data:

While the survey interview gathers information about a wide range of topics, including the individual, her school, friends, family, neighborhood, delinquent involvement, arrest history, sexual history, and victimization, in addition to information about the gang, the in-depth interview is concerned exclusively with the roles and activities of young women in youth gangs, and the meanings they describe as emerging from their gang affiliation. (p. 105)

Let us focus on the data that Miller obtained in her in-depth interviews. This is one example:

Describing why she joined her gang, one young woman told Miller, "well, I didn't get any respect at home. I wanted to get some love and respect from somebody somewhere else." (p. 107)

Here is another respondent's explanation of why she joined a gang:

I didn't have no family. . . . I had nothin' else. (p. 107)

Another young woman, when asked to speculate on why young people join gangs, suggested:

Some of 'em are like me, don't have, don't really have a basic home or steady home to go to, you know, and they don't have as much love and respect in the home so they want to get it elsewhere. And, and, like we get, have family members in gangs or that were in gangs, stuff like that. (p. 107)

Let us assume that you have gathered these data and now want to begin analysis. Put at its starkest, what are you to do with them?

In line with the realist approach, using programs such as the Ethnograph or NUD*IST (see Weitzman, Chapter 30, this volume), you may start by coding respondents' answers into the different sets of reasons they give for participation in gangs. From these data, two reasons seem to predominate: "push" factors (unsupportive families) and "pull" factors (supportive gangs).

Moreover, given the availability of survey data on the same respondents, you are now in a position to correlate each factor with various background characteristics that they have. This seems to set up your research in good shape. Not only can you search for the "subjective" mean-

♦ Interviews

For the qualitative-minded researcher, the open-ended interview apparently offers the op-

ings of adolescent gangs, you can relate these meanings to "objective" social structures.

The realist approach thus has a high degree of plausibility to social scientists who theorize the world in terms of the impact of (objective) social structures upon (subjective) dispositions. Moreover, the kind of research outputs that this approach seeks to deliver are precisely those demanded by "users" in the community, who seek immediate practical payoffs from social science research.

Calling their approach a "methodology for listening," Miller and Glassner (1997) are thus centrally concerned with "seeing the world from the perspective of our subjects" (Glassner & Loughlin, 1987, p. 37). In this respect, they share the same assumptions about the "authenticity" of "experience" as other realists and, therefore, fail to detect culturally (and locally) specific elements in several "personal" tales (see Gubrium, 1993; Voysey, 1975).

However, say we are not entirely satisfied by the apparent plausibility of realism. How can the narrative approach kick-start data analysis? Miller and Glassner (1997, pp. 103-104) suggest that one way to begin is to think about how respondents are using culturally available resources in order to construct their stories. They refer to Richardson's (1990) suggestion that "participation in a culture includes participation in the narratives of that culture, a general understanding of the stock of meanings and their relationships to each other" (p. 24).

How, then, can the data above be read in these terms? The idea is to see respondents' answers as *cultural stories*. This means examining the rhetorical force of what interviewees say as "interviewees deploy these narratives to make their actions explainable and understandable to those who otherwise may not understand" (Miller & Glassner, 1997, p. 107).

In the data already presented, Miller and Glassner note that respondents make their actions understandable in two ways. First, they do not attempt to challenge public views of gangs as bad. But, second, they do challenge the notion that the interviewee herself is bad. However, Miller and Glassner (1997) note that not all their respondents glibly recycle conventional

of an activity (e.g., being a male football fan) represented by what people say to the researcher, to each other, to caregivers, and so on (Gubrium, 1975/1997)?

This raises the important methodological issue of whether interview responses are to be treated as giving direct access to "experience" or as actively constructed "narratives" involving activities that themselves demand analysis (Holstein & Gubrium, 1995; Silverman, 1993). Both positions are entirely legitimate, but you need to justify and explain the position you take.

Is your analytic position appropriate to your practical concerns? Some ambitious analytic positions (e.g., hermeneutics, discourse analysis) may actually cloud the issue if your aim is simply to respond to a given social problem (e.g., living and coping in a community of elderly people, students' views of evaluation and feedback). If so, it might be simpler to acknowledge that there are more complex ways of addressing your data but to settle on presenting your research as a *descriptive* study based upon a clear social problem.

Do interview data really help in addressing your research topic? If you are interested in, say, what happens in school classrooms, should you be using interviews as your major source of data? Think about exactly why you have settled on an interview study. Certainly, it can be relatively quick to gather interview data, but not as quick as, say, gathering texts and documents. How far are you being influenced by the prominence of interviews in the media (see Atkinson & Silverman, 1997)?

In the case of the classroom, couldn't you observe what people do there instead of asking them what they think about it? Or gather documents that routinely arise in schools, such as pupils' reports, mission statements, and so on? Of course, you may still want to do an interview study. But, whatever your method, you will need to justify it and show you have thought through the practical and analytic issues involved in your choice.

Are you making too-large claims about your research? It always helps to make limited claims about your own research. Grandiose claims about originality, scope, or applicability to social problems are all hostages to fortune. Be careful in how you specify the claims of your approach. Show that you understand that it constitutes one way of "slicing the cake" and that other approaches, using other forms of data, may not be directly competitive.

Does your analysis go beyond a mere list? Identifying the main elements in your data according to some theoretical scheme should be only the first stage of your data analysis. By examining how these elements are linked together, you can bring out the active work of both interviewer and interviewee and, like them, say something lively and original.

♦ Texts

To introduce a separate section on "texts" now begins to look a little artificial. After all, to treat an interview as a narrative can mean looking for the same textual features as researchers working with printed material. Indeed, the mere act of transcription of an interview turns it into a written text. In this section, I use *text* as a heuristic device to identify data consisting of words and images that have become recorded without the intervention of a researcher (e.g., through an interview).

Of course, every way of seeing is also a way of not seeing. As Atkinson (1992, p. 459) points out, one of the disadvantages of the coding schemes used in both interview and text-based analysis is that, because they are based upon given sets of categories, they furnish "a powerful conceptual grid" from which it is difficult to escape. Although this grid is very helpful for organizing the data analysis, it also deflects attention away from uncategorized activities.

In part, Atkinson's critique vitiates the claims of many quantitative researchers in their attempts to produce reliable evidence about large samples of texts. Their favored method is "con-

tent analysis," in which the researchers establish a set of categories and then count the number of instances that fall into each category. The crucial requirement is that the categories are sufficiently precise to enable different coders to arrive at the same results when the same body of material (e.g., newspaper headlines) is examined (see Berelson, 1952).

The meat of the problem with content analysis (and its relatives) is not simply Atkinson's point about overlooked categories, but how analysts usually simply trade off their tacit members' knowledge in coining and applying whatever categories they do use. For instance, in a lecture given in the 1960s, Harvey Sacks compared the social psychologist Bales's (1950) tendency to produce immediate categories of "interaction process" with the relatively long time taken by experienced physicians to read the output of electroencephalographs. According to Sacks (1992b), one should not "categorize . . . as it comes out" (p. 28). Indeed, as we shall see shortly, our ability to categorize quickly is properly treated as a research topic rather than a research resource.

By contrast, in some qualitative research, small numbers of texts and documents may be analyzed for a very different purpose. The aim is to understand the participants' categories and to see how these are used in concrete activities such as telling stories (Propp, 1968; Sacks, 1974), assembling files (Cicourel, 1968; Gubrium & Buckholdt, 1982), and describing "family life" (Gubrium, 1992). The theoretical orientation of these qualitative researchers makes them more concerned with the processes through which texts depict "reality" than with whether such texts contain true or false statements. As Atkinson and Coffey (1997) put it:

In paying due attention to such materials, however, one must be quite clear about what they can and cannot be used for. They are "social facts," in that they are produced, shared and used in socially organized ways. They are not, however, transparent representations of organizational routines, decision-making processes, or professional diagnoses. They construct particular kinds of representations with their own conventions. (p. 47)

The implications of this are clear:

We should not use documentary sources as surrogates for other kinds of data. We cannot, for instance, learn through records alone how an organization actually operates day-by-day. Equally, we cannot treat records—however "official"—as firm evidence of what they report. . . . That strong reservation does not mean that we should ignore or downgrade documentary data. On the contrary, our recognition of their existence as social facts alerts us to the necessity to treat them very seriously indeed. We have to approach them for what they are and what they are used to accomplish. (p. 47)

What does it mean to approach texts "for what they are"? Potential examples are semiotics, ethnographically oriented narrative analysis, and discourse analysis. In crude terms, semiotics treats texts as systems of signs on the basis that no meaning ever resides in a single term (see Silverman, 1993, pp. 71-80), narrative analysis focuses on how accounts artfully use local cultural resources (e.g., Atkinson & Coffey, 1997; Denzin, 1990; Holstein & Gubrium, 1997), and discourse analysis focuses on how different versions of the world are produced through the use of interpretive repertoires, claims to "stakes" in an account (see Potter, 1997), and constructions of knowing subjects (Prior, 1997).

Following my interest in ethnomethodology's focus on members' methods, I will sketch out a less familiar example—Sacks's account of membership categorization analysis (see also Silverman, 1998b; Watson, 1997).

Membership Categorization Analysis

Like some contemporary ethnographers concerned with narratives, Sacks (1992b) believes the issue is not to second-guess societal members, but to try to work out "how it is that people can produce sets of actions that provide that others can see such things . . . [as] persons doing intimacy . . . persons lying, etc." (p. 119). Given that many categories can be used to describe the same person or act, Sacks's task was "to find out how they [members] go about

choosing among the available sets of categories for grasping some event" (p. 41). So Sacks does not mean to imply that "society" determines which category one chooses. Instead, he wants to show the active interpretive work involved in rendering any description and the local implications of choosing any particular category. Whether or not we choose to use Sacks's precise method, he offers an inspiring way to begin to analyze the productivities of any text.

As we have already seen, "coding" is not the preserve of research scientists. All of us "code" what we hear and see in the world around us. This is what Garfinkel (1967) and Sacks (1992a) mean when they say that societal members, like social scientists, make the world observable and reportable. Put at its simplest, this means that researchers must be very careful how they use categories. For instance, Sacks (1992b) quotes from two linguists who appear to have no problem characterizing particular (invited) utterances as "simple," "complex," "casual," or "ceremonial." For Sacks (1992a), such rapid characterizations of data assume "that we can know that [such categories are accurate] without an analysis of what it is [members] are doing" (p. 429).

At this point, the experienced researcher might respond that Sacks has characterized conventional research as overly naïve. In particular, most researchers are aware of the danger of assuming any one-to-one correspondence between their categories and the aspects of "reality" that they purport to describe. Instead, following Weber (1949), many researchers claim that they are simply using hypothetical constructs (or "ideal types") that are to be judged only in relation to whether they are *useful*, not whether they are "accurate" or "true." However, Sacks (1992a) was aware of this argument:

It is a very conventional way to proceed in the social sciences to propose that the machinery you use to analyze some data you have is acceptable if it is not intendedly the analysis of real phenomena. That is, you can have machinery which is a "valid hypothetical construct," and it can analyze something for you. (p. 315)

By contrast, the "machinery" in which Sacks is interested is not a set of "hypothetical constructs." Instead, Sacks's (1992a) ambitious claim throughout is "to be dealing with the real world" (p. 316). The "machinery" he sets out, then, is not to be seen as a set of more or less useful categories, but as the *actual* categories and mechanisms that members use.

Let us take a concrete example. In two of Harvey Sacks's lectures, he refers to a *New York Times* story about an interview with a U.S. Navy pilot about the pilot's missions in the Vietnam War (see Sacks, 1992b, pp. 205-222, 306-311). Sacks is especially interested in the story's report of the pilot's reported answer to a question:

How did he feel about knowing that even with all the care he took in aiming only at military targets someone was probably being killed by his bombs?

"I certainly don't like the idea that I might be killing anybody," he replied. "But I don't lose any sleep over it. You have to be impersonal in this business. Over North Vietnam I condition myself to think that I'm a military man being shot at by another military man like myself." (Sacks, 1992b, p. 205)

Sacks invites us to see how the pilot's immediate reply ("I certainly don't like the idea") shows his commitment to the evaluational scheme offered by the journalist's question. For instance, if the pilot had instead said, "Why do you ask?" he would have shown that he did not necessarily subscribe to the same moral universe as the reporter (and, by implication, the readers of the article) (Sacks, 1992b, p. 211).

Having accepted this moral schema, the pilot, as Sacks shows, now builds an answer that helps us to see him in a favorable light. The category "military man" works to defend his bombing as a category-bound activity that reminds us this is, after all, what military pilots do. The effect of this is magnified by the pilot's identification of his coparticipant as "another military man like myself." In this way, the pilot creates a pair (military man/military man) with recognizable mutual obligations (bombing/shooting at the other). In terms of this pair, the other party cannot properly complain, or, as Sacks (1992b) puts it, "there are no complaints to be offered on their part

about the error of his ways, except if he happens to violate the norms that, given the device used, are operative" (p. 206).

Notice also that the pilot suggests, "You have to be impersonal in this business." Note how the category "this business" sets up the terrain on which the specific pair of military men will shortly be used. So this account could be offered by either pair-part. However, as Sacks (1992b) argues, the implication is that "this business" is one of many where impersonality is required, for "if it were the case that, that you had to be impersonal in this business held only for this business, then it might be that doing this business would be wrong in the first instance" (p. 206).

Moreover, the impersonality involved is of a special sort. Sacks points out that we hear the pilot as saying not that it is unfortunate that he cannot kill "personally," but rather that being involved in this "business" means that one must not consider that one is killing persons (p. 209). However, the pilot is only *proposing* a pair of military man/military man. In that sense, he is inviting the North Vietnamese to "play the game" in the same way a child might say to another, "I'll be third base." However, as Sacks (1992b) notes, in children's baseball, such proposals can be rejected: "If you say 'I'll be third base,' unless someone else says 'and I'll be . . . another position, and the others say they'll be the other positions, then you're not that thing. You can't play'" (p. 307).

Of course, the North Vietnamese indeed did reject the pilot's proposal. Instead, they proposed the identification of the pilot as a "criminal" and defined themselves as "doing police action." As Sacks notes, these competing definitions had implications that went beyond mere propaganda. For instance, if the navy pilot were shot down, then the Geneva Conventions about his subsequent treatment would be properly applied only if he indeed were a "military man" rather than a "criminal" (p. 307).

Unlike more formalistic accounts of action (Mead, 1934; Parsons, 1937), Sacks's analysis shows us the nitty-gritty mechanisms through which we construct moral universes "involving

body of data with which to work. So, although it may be useful initially to explore different kinds of data (e.g., newspaper reports, scientific textbooks, magazine advice pages), you should usually do this only to establish the data set with which you can most effectively work. Having chosen your data set, you should limit your material further by taking only a few texts or parts of texts (e.g., headlines).

♦ Transcripts

The three types of qualitative data discussed here all end up in the form of some kind of text. In interviews, researchers usually work with written transcripts. Similarly, audiotapes of naturally occurring interaction are usually transcribed prior to (and as part of) the analysis.

The two main social science traditions that inform the analysis of transcripts of tapes are conversation analysis (CA) and discourse analysis (DA). For an introduction to CA, see ten Have (1998); on DA, see Potter and Wetherell (1987) and Potter (1997).² In the rest of this chapter, I will deal with two more practical issues: (a) the advantages of working with tapes and transcripts of naturally occurring talk and (b) the elements of how to do analysis of such tapes.

Why Work With Tapes?

The kind of phenomena I deal with are always transcripts of actual occurrences in their actual sequence. (Sacks, 1984, p. 25)

In contemporary philosophy, Sacks was attracted to speech-act theory, which, like him, treats talk as an activity. However, Austin (1962) and Searle (1969) did not study actual talk but worked with invented examples and their own intuitions about what it makes sense to say. Sacks (1992b), on the contrary, notes:

One cannot invent new sequences of conversation and feel happy with them. You may be able to take "a question and answer," but if we have to extend it very far, then the issue of whether somebody would really say that, after, say, the fifth utterance, is one which we could not confidently argue. One doesn't have a strong intuition for sequencing in conversation. (p. 5)

The earlier ethnographers had generally relied on recording their observations through field notes. Why did Sacks prefer to use an audio recorder? Sacks's answer is that we cannot rely on our recollections of conversations. Certainly, depending on our memories, we can usually summarize what different people said. But it is simply impossible to remember (or even to note at the time) such matters as pauses, overlaps, and inbreaths.

Now, whether you think these kinds of things are important or not will depend upon what you can show with or without them. Indeed, you may not even be convinced that conversation itself is a particularly interesting topic. But, at least by studying tapes of conversations, you are able to focus on the "actual details" of one aspect of social life. As Sacks (1992b) puts it:

My research is about conversation only in this incidental way, that we can get the actual happenings on tape and transcribe them more or less, and therefore have something to begin with. If you can't deal with the actual detail of actual events then you can't have a science of social life. (p. 26)

Tapes and transcripts also offer more than just "something to begin with." In the first place, they are a public record, available to the scientific community, in a way that field notes are not. Second, they can be replayed and transcriptions can be improved, and analyses can take off on different tracks unlimited by the original transcript. As Sacks (1992b) told his students:

I started to play around with tape recorded conversations, for the single virtue that I could replay them; that I could type them out somewhat, and study them extendedly, who knew how long it might take. . . . It wasn't from any large interest in language, or from some theoretical formulation

of what should be studied, but simply by virtue of that; I could get my hands on it, and I could study it again and again. And also, consequently, others could look at what I had studied, and make of it what they could, if they wanted to disagree with me. (p. 622)

A third advantage of detailed transcripts is that, if you want to, you can inspect sequences of utterances without being limited to the extracts chosen by the first researcher. For it is within these sequences, rather than in single turns of talk, that we make sense of conversation. As Sacks (1992b) points out:

Having available for any given utterance other utterances around it, is extremely important for determining what was said. If you have available only the snatch of talk that you're now transcribing, you're in tough shape for determining what it is. (p. 729)

There remains the potential charge that data based mainly on audio recordings is incomplete. We see Sacks's response to this issue when a student asks a question about "leaving out things like facial expressions" from his analysis (1992b, p. 26). Sacks at once concedes that "it would be great to study them [such things]. It's an absence." Nonetheless, he constructs a two-part defense of his data.

First, the idea of "completeness" may itself be an illusion. Surely, there cannot be totally "complete" data any more than there can be a "perfect" transcript? Second, Sacks (1992b, pp. 26-27) recognized some of the undoubted technical problems involved in camera positioning and the like if one were to use videos. These are the very issues that have been addressed, if not resolved, by more recent work based on video-recorded data (e.g., Heath, 1986, 1997; Heath & Luff, 1992). Rather, as always in science, everything will depend on what you are trying to do and where it seems that you may be able to make progress. As Sacks (1992b) puts it, "One gets started where you can maybe get somewhere" (p. 26).

It should not be assumed that the preparation of transcripts is simply a technical detail prior to the main business of the analysis. The

convenience of transcripts for presentational purposes is no more than an added bonus. As Atkinson and Heritage (1984) point out, the production and use of transcripts are essentially "research activities." They involve close, repeated listenings to recordings that often reveal previously unnoted recurring features of the organization of talk. Such listenings can most fruitfully be done in group data sessions. As described by Paul ten Have (1998), work in such groups usually begins with the members listening to an extract from a tape with a draft transcript and agreeing upon improvements to the transcript. Then

the participants are invited to proffer some observations on the data, to select an episode which they find "interesting" for whatever reason, and formulate their understanding or puzzlement, regarding that episode. Then anyone can come in to react to these remarks, offering alternatives, raising doubts, or whatever. (p. 124)

However, as ten Have makes clear, such group data sessions should be rather more than anarchic free-for-alls:

Participants are, on the one hand, *free* to bring in anything they like, but, on the other hand, *required* to ground their observations in the data at hand, although they may also support them with reference to their own data-based findings or those published in the literature. (p. 124)

Analyzing Tapes

As with any kind of data, the analysis of tapes and transcripts depends upon the generation of some research problem out of a particular theoretical orientation. Like the writing of field notes, the preparation of a transcript from an audio- or videotape is a theoretically saturated activity. Where there is more than one researcher, debate about what you are seeing and hearing is never just about collating data—it is *data analysis*.

But how do you push the analysis beyond an agreed transcript? The temptation is to start at line 1 of your transcript and work your way

down the page, making observations as you go. However, the danger of proceeding in this way is that your observations are likely to be ad hoc and commonsensical. Moreover, if you are committed to an approach (like CA or DA) that looks at how the participants coproduce some meaning, then beginning with a single utterance gets you off on the wrong foot.

How else can you proceed? Jennifer Mason (1996) suggests that you can formulate a research topic in terms of different kinds of puzzles. Identifying a puzzle can also be a way to kick-start the analysis of a transcript. Once you have found your puzzle, the best method is often to *work back and forth* through your transcript to see how the puzzle arises and is resolved. This implies a strongly inductive bent to this kind of research. It follows that any research claims need to be identified in precise analyses of detailed transcripts. It is therefore necessary to avoid premature theory construction and the "idealization" of research materials that uses only general, nondetailed characterizations. Heritage (1984) sums up these assumptions as follows:

Specifically, analysis is strongly "data-driven"—developed from phenomena which are in various ways evidenced in the data of interaction. Correspondingly, there is a strong bias against *a priori* speculation about the orientations and motives of speakers and in favour of detailed examination of conversationalists' actual actions. Thus the empirical conduct of speakers is treated as the central resource out of which analysis may develop. (p. 243)

In practice, Heritage adds, this means that it must be demonstrated that the regularities described can be shown to be produced by the participants and attended to by them as grounds for their own inferences and actions. Further, deviant cases, in which such regularities are absent, must be identified and analyzed.

However, the way in which CA obtains its results is rather different from how we might intuitively try to analyze talk. It may be helpful, therefore, if I conclude this section by offering a crude set of prescriptions about how to do CA (Table 31.1) and a list of things to avoid in

TABLE 31.1 How to Do Conversation Analysis

1. Always try to identify sequences of related talk.
2. Try to examine how speakers take on certain roles or identities through their talk (e.g., questioner/answerer or client-professional).
3. Look for particular outcomes in the talk (e.g., a request for clarification, a repair, laughter) and work backward to trace the trajectory through which a particular outcome was produced.

SOURCE: Silverman (1998a).

TABLE 31.2 Common Errors in Conversation Analysis

1. Explaining a turn at talk by reference to the speaker's intentions (except insofar as such intentions are topicalized in the conversation).
2. Explaining a turn at talk by reference to a speaker's role or status (e.g., as a doctor or as a man or woman).
3. Trying to make sense of a single line of transcript or utterance in isolation from the surrounding talk.

SOURCE: Silverman (1998a).

doing CA (Table 31.2). If we follow these rules, the analysis of conversations does not require exceptional skills. As Schegloff (1992) puts it in his introduction to Sacks's collected lectures, all we need to do is "begin with some observations,

then find the problem for which these observations could serve as . . . the solution" (p. xlviii).

This means that doing the kind of systematic data analysis that CA demands is not an impossibly difficult activity. As Harvey Sacks (1992a) once pointed out, in doing CA we are only reminding ourselves about things we already know:

I take it that lots of the results I offer, people can see for themselves. And they needn't be afraid to. And they needn't figure that the results are wrong because they can see them. . . . [It is] as if we found a new plant. It may have been a plant in your garden, but now you see it's different than something else. And you can look at it to see how it's different, and whether it's different in the way that somebody has said. (p. 488)

Wittgenstein (although mentioned only twice in Sacks's lectures), and his concern for assembling reminders of what we know already, is clearly relevant. Wittgenstein (1968) writes: "The aspects of things that are most important for us are hidden because of their simplicity and familiarity" (para. 129). Now Wittgenstein, of course, is referring to what is hidden from philosophers. But the same issue often arises for social scientists—to whom things can be "hidden because of their simplicity and familiarity."

◆ Conclusion

In a sense, nearly all qualitative research touches upon talk and text. In this chapter, however, I have resisted such inclusiveness in favor of a much more strictly defined version of appropriate ways of responding to the linguistically mediated character of qualitative data.

Although my own approach derives from Garfinkel's and Sacks's concern with members' methods, I have nonetheless tried to avoid adopting a "take it or leave it" approach. In particular, I have highlighted points of contact with a wide range of other approaches, including narrative-based ethnography, discourse analysis, and semiotics.

political context disconcerting (see Wetherell, 1998). Equally, CA specialists question the validity of some DA researchers' appeals to their own sense of context (see Schegloff, 1998).

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■ Notes

1. In Garfinkel's (1967) sense, *reflexivity* refers not to self-questioning but to how context is constituted through interaction (see Gubrium & Holstein, Chapter 18, this volume).
2. The difference between DA and CA is a matter for debate. Some DA researchers find CA's refusal to engage directly with cultural and

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