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Collecting/Producing Recordings

CONTENTS

As has become clear in the previous chapters, it is essential for the CA enterprise to study recordings of natural human interaction in detail. In the present chapter, I will first discuss some general aspects of the 'research design' of CA studies. Included are discussions of sampling issues, naturalness, and the question whether any additional information, apart from recordings, should be collected or not. Then follows a treatment of some of the practical problems of producing CA data. A general discussion of 'consent' is followed by a detailed review of three overall strategies of getting recordings: copying radio and TV broadcasts; using existing recordings; or making one's own recordings. In the next chapter, I will proceed to the subsequent issues in the production of data: making transcriptions.

Research design

Although CA projects, like most qualitative inquires (cf. Ragin, 1994), do not, in general, have a strictly pre-defined 'research design', as is

Doing Conversation Analysis

A Practical Guide

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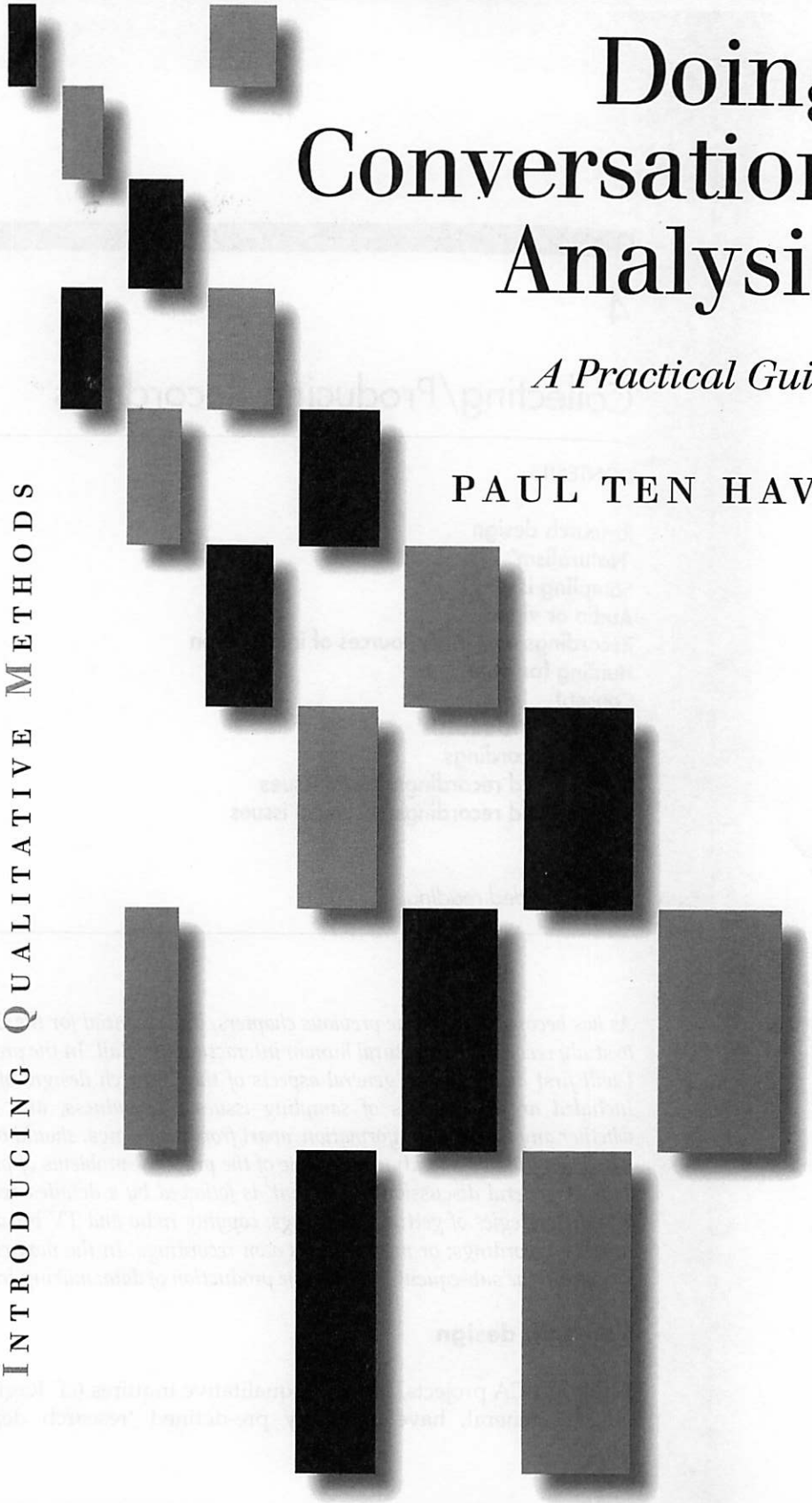
INTRODUCING QUALITATIVE METHODS



recommended for quantitative research, they are being 'designed', in a way, even if this design is not explicated. As Ragin says:

> Research design is a plan for collecting and analysing evidence that will make it possible for the investigator to answer whatever questions he or she has posed. The design of an investigation touches almost all aspects of the research, from the minute details of data collection to the selection of the techniques of data analysis. (Ragin, 1994: 191)

For CA, the general outline for research projects would at least involve the four phases of:

1 getting or making recordings of natural interaction;
2 transcribing the tapes, in whole or in part;
3 analysing selected episodes;
4 reporting the research.

As is usual for qualitative inquiries, these phases are not strictly separated. In fact, it is often advisable to proceed in a 'spiralling fashion', where earlier phases are 'inspired' by tentative efforts at later-phase work. The division of chapters in this book should not, therefore, be taken as suggesting that one first has to do all recording activities before one starts transcribing, etc. In any phase, one should at least anticipate what is to follow, and if at all possible try to do some exploratory work for the later phases.

Interwoven in the four-phase schema given above is a gradual elaboration of analytic 'questions' and 'answers'. Some projects will start with a general interest – how is talk-in-interaction organized – which will be elaborated as one goes along, especially in the analysis. Others will start off with some quite specific questions, but even these will often have to be specified when one struggles to find good answers through an analysis of the data.[1] For the moment, these issues will only interest us as they are to be taken into account in the decisions one has to make regarding data collection.

'Naturalism'

The general CA recommendation for making recordings is that these should catch 'natural interaction' as fully and faithfully as is practically possible. The term 'natural' in this expression refers to the ideal that the interactions recorded should be 'naturally occurring', that is, 'non-experimental', not co-produced with or provoked by the researcher. This preference contrasts with Harold Garfinkel's 'breaching experiments' in ethnomethodology, which produced hilarious dialogues like:

Excerpt 4.1, from Garfinkel & Sacks, 1970: 350

HG: I need some exhibits of persons evading questions. Will you do me a favor and evade some questions for me?
NW: [Oh, dear, I'm not very good at evading questions.]

Note: Square brackets do *not* indicate overlap here.

In other words, the ideal is to (mechanically) observe interactions as they would take place without research observation, but one can never really verify this (cf. Labov's [1972] remarks on the Observer's Paradox). Therefore, many researchers try to make the observation, including recording activities, as unobtrusive as possible. I will later discuss some tactics for making the activity of recording at least less imposing on the interaction being recorded. For the moment, let me just suggest that, in many cases, there does not seem to be a sharp line separating 'naturally occurring' from 'experimental' data (in the broad sense of 'researcher-produced'). The following excerpt, for instance, can be seen as presenting a piece of 'non-natural' speech, since Pam is referring to the activity of being recorded, and in so doing characterizes the situation as being an 'experimental' one, in a way.

Excerpt 4.2, from Schegloff, 1996a: 59 [Automobile Discussion 1: 01–12]

Carney: (. . . hear the same story),
Pam: ·hh Oh yeah you've gotta tell Mike tha:t. Uh-cuz they
[want that on fi:lm.
Carney: [Oh: no: here we go ag(h)[(h)ain o(h)o(h)o] ·hh=
Curt: [Huh huh huh huh.]
Gary: =I[dont thin[k it's that funny.
Carney: [O h : [:,
Pam: [I gotto go t'the
joh[n before I hear tha[t again.
Carney: [You'll like it, you' [ll rilly like it.
Curt: [You do too y[ou laugh leke hell you
Hhuh! [
Phyllis: [°ejjej huh

Schegloff does analyse aspects of this episode, however, *as* natural talk. In fact, the speakers do seem to talk in a, for them, ordinary, fashion. So, whether some piece of talk can be treated as 'natural' or not depends not only on its setting, but also on the way it is being analysed. Data that seem to be 'artificial', in terms of their content being provoked by the researcher, or the situation of being recorded, may be considered 'natural' in terms of the ways in which the participants interact while responding to this provocation.

In various types of 'applied CA', the data may 'in themselves' be 'experi-

mental', for instance as part of a research project based on interviews, while for a CA researcher, with an interest in the social organization of interview talk, these are 'natural' specimens of the phenomenon of interest.[2]

Sampling issues

In many types of social research, a major aspect of research design is to draw a useful sample of cases from a 'population' of possible cases to investigate. Ragin, for instance, defines sampling as follows:

> Sampling is the process of selecting a representative set of cases from a much larger set. Researchers sample because they often confront a wealth of potential cases and do not have the time or resources to study them all. (Ragin, 1994: 191)

Most often, methodological discussions of 'sampling' presuppose a 'factist perspective', rather than a 'specimen' approach (cf. Alasuutari, 1995, and chapter 3 above). That is, data are sought in order to represent a reality that is not directly observable. Therefore, the sample should provide a set of indicators for the population parameters to be estimated. When the evidence is used in a 'specimen perspective', however, the reality to be studied is seen to be directly observable in the specimens at hand. So, for instance, if a naturalist would like to study some aspects of the life of *Passer domesticus*, observing a few specimen sparrows will do. The investigator might take care to select cases that are not markedly exceptional, for example by observing sparrows in some places where they are quite common, but no effort at statistical sampling would seem to be needed. The specimens have to be representative not of the population of sparrows, but of the category *Passer domesticus*. In a similar fashion, instances of 'story-telling' or 'repair', or whatever, are analysed in CA in terms of their category, not of a 'population' of stories or repairs.

In his Lectures, Harvey Sacks (1984a, 1992a, 1992b, for instance 1992a: 483–8) has repeatedly discussed these matters in terms of their presuppositions regarding the phenomena one studies. If, as the corpus of CA studies suggests, the way people organize their talk-in-interaction is 'orderly', that is, based on a set of formal procedures of immense generality,[3] then it does not matter very much which particular specimens one collects to study that order. As a member of a culture, one has been exposed to a very limited and arbitrary 'selection' from the culture, which still allows one to develop the required competencies to deal with other members one encounters in a large variety of settings in an orderly fashion.

In the quote below, already used in the previous chapter, John Heritage adds an important note to these ideas:

> [. . .] CA has adopted the naturalist's strategy of building up large collections of data from as many natural sites as possible. Like a good collection of naturalist's specimens, these growing data bases contain many variations of particular types of interactional events whose features can be systematically compared. Analysts constantly seek for new variants and may focus their searches on particular settings in the expectation of finding them. (Heritage, 1988: 131)

The sparrow example is based on the assumption that the category *Passer domesticus* refers to a collection that is basically invariant, but this may not be the case for 'stories', etc. Therefore, a 'maximum variation strategy' is often proposed, as does Heritage in the quote: selecting specimens 'from as many natural sites as possible'. Sparrows may feed their young in different ways, depending on certain properties of their biotope, for instance. In a similar fashion, 'stories' may be told differently in a peer group, a doctor's office, or a radio talk show. A maximum variation strategy may be specifically fit if one is interested in story-telling as such, that is, in 'pure CA', while in some kinds of 'applied CA' one might rather prefer a deliberately restricted set of instances, for instance to a specific circumstantial category. In such a strategy, the interest is not in the activity-as-such, but in specific kinds of category- or context-bound activities. In both strategies, comparing cases is an essential operation (to which I will return). The issue here is whether the data-set will allow for direct comparisons, or whether the analysis will focus on one core category or setting, while other categories or settings will of necessity only be used in an indirectly comparative fashion, for instance by reference to presuppositions and/or findings about that category or setting. The *logic* of CA, however, in terms of data selection suggests that *any* specimen is a 'good' one, that is, worthy of an intense and detailed examination. It is focused on the specific ways in which that particular specimen has been produced as an 'orderly product'. As Sacks has suggested:

> It may be that we can come up with findings of considerable generality by looking at very singular, particular things. By asking what it takes for those things to have come off. (Sacks, 1992a: 298)

When you are interested in 'greeting', for instance, you may not need to examine '100,000 random greeting sequences' to do that.

In short:

- When you are interested in a particular class of interactional phenomena that can happen anywhere, you might select a varied set of data sources to collect instances of your phenomenon.
- When you are interested in a class of interactional phenomena that you expect to be particularly prominent in a, or some, specific setting(s), you might collect recordings from that or those setting(s).
- When you are interested in a particular setting, you will, of course, have to collect recordings made in that setting, but you might

consider catching some variation within that setting, and you might also consider data from other settings for comparative purposes.

- When you are interested in a class of interactional phenomena as it functions in a particular setting, your choice will tend to be even more restricted, but you might still strive for some variation and/or data from other settings as well.

In any case, however, and in particular if your project is an exploratory one, you might use whatever data you can lay your hands on, especially at the start of the project. Whatever choice you make, you should take the particular possibilities of a setting into account, and look for any indications of 'setting sensitivity' that the participants might demonstrate in their talk and other activities.

Audio or video

As was noted before, CA was originally developed on the basis of audio recordings only, either of telephone conversations or of face-to-face interactions. In the latter case, the analysis was inevitably 'incomplete', in the sense that the recorded interaction might have involved non-vocal exchanges, or non-vocal accompanying activities, that would not be accessible to the analyst of the audio tape. This handicap did not prevent CA's originators from developing the enterprise in a way that is still valid today. Later students of interaction, who used video materials, were able to supplement these early findings with insights concerning visual aspects of verbal interaction. In face-to-face situations, who is addressed by an utterance, or, more generally, where a participant's attention is directed at, is routinely inferable (and inferred) from the direction of his or her gaze. As far as I am aware, Charles Goodwin initiated this line of CA research, in close collaboration with Marjorie Harness Goodwin. Apart from CA's originators, Sacks, Schegloff, and Jefferson, he was most strongly influenced by Adam Kendon, who had started to study 'gaze' in natural interaction already in the 1960s, and Erving Goffman, who had been his teacher. It is, as I noted before, remarkable that 'the video branch of CA', which also prominently includes the work of Christian Heath, uses many of Goffman's concepts (cf., for instance, C. Goodwin, 1981; M.H. Goodwin, 1995; Heath, 1988).

Apart from the visual aspects of the interaction, a special argument for using video over audio applies to those settings in which core aspects of the action relate to the physical environment, the use of objects, technological artefacts, and/or the body or bodies of one or more of the participants (cf. especially Jordan & Henderson, 1995; also Suchman & Trigg, 1991). As Christian Heath remarks:

> The emergent and sequential organization of interaction is also relevant to how we might consider the contextual or *in situ* significance of visual conduct and

> the physical properties of human environments. Gestures and other forms of bodily conduct arise in interaction, people not infrequently use artefacts when talking to each other, and it is not unusual for aspects of the physical environment to become relevant within the course of social activities. (Heath, 1997: 187)

He further adds, however, that while visual conduct is part of what I would call the interactional stream, it tends not to have a neat turn-by-turn organization, as talk has been found to have overwhelmingly. This may be one reason why video analysis seems to be less used than one might expect, given its obvious importance.

However, even if one's analysis is not focused on the intricacies of the interweaving of vocal and visual conduct, it may be advisable to use video, because video recording provides a wealth of contextual information that may be extremely helpful in the analysis of interactional talk-as-such, especially in complex settings with more than a few speakers, like meetings of various sorts.

The choice between audio or video recording can also be influenced by considerations of costs, availability of equipment and relative ease of access. As Heath and Luff explain:

> There can be additional problems associated with video recording, particularly since it is more difficult to preserve the anonymity of the participants. However, it has been found that people in a wide variety of settings are often willing to allow researchers to record both the audible and visual aspects of their conduct if they are guaranteed a final veto on whether the recordings should be preserved. (Heath & Luff, 1993: 308)[4]

Recordings and other sources of information

It has become clear, by now, that conversation analysis requires access to recordings of talk-in-interaction. The question remains, however, whether these would be the only sources of information used, or whether additional data should also be collected. This is an issue that has been, and continues to be, widely and hotly debated, between CA and its critics, as well as within the CA community. This is not the place to review, let alone evaluate, these debates extensively. Therefore, the discussion below is limited to a summary overview of the various arguments and standpoints, and an enumeration of some possibilities of additional data gathering and its possible analytic usefulness.

CA's insistence on the use of audio or video recordings of episodes of 'naturally occurring' interaction as their basic data is, indeed, quite unique in the social sciences and means that some of the most common data sources are not used, or at least not as 'core data'.

These include:

- interview data, as expressions of opinions and attitudes or descriptions of scenes not witnessed by the researcher;

- observational studies, relying on field notes or coding procedures;
- idealized or invented examples, based on the researcher's own native intuitions; and
- experimental methodologies.

All of these kinds of data are seen as too much a product of the researcher's or informant's manipulation, selection, or reconstruction, based on preconceived notions of what is probable or important (Heritage & Atkinson, 1984: 2–3). Recorded data, instead, are indefinitely rich in empirical detail, which could never be produced by the imagination of anybody (Sacks, 1992b: 419–20). As Heritage and Atkinson say:

> [. . .] the use of recorded data serves as a control on the limitations and fallibilities of intuition and recollection; it exposes the observer to a wide range of interactional materials and circumstances and also provides some guarantee that analytic conclusions will not arise as artifacts of intuitive idiosyncrasy, selective attention or recollection or experimental design. The availability of a taped record enables repeated and detailed examination of particular events in interaction and hence greatly enhances the range and precision of the observations that can be made. The use of such materials has the additional advantage of providing hearers and, to a lesser extent, readers of research reports with direct access to the data about which analytic claims are being made, thereby making them available for public scrutiny in a way that further minimizes the influence of individual preconception. (Heritage & Atkinson, 1984: 4)

So recordings are CA's basic data, but, as noted above, CA researchers differ amongst themselves as to the extent to which it makes sense to use additional data as 'background information' in addition to recordings, which serve as 'focal data'. In 'pure' CA, as originally developed, even the general setting in which the recordings were made was hardly considered. Either it was left in the dark, or the data source was only mentioned in general terms, as in 'tape recorded phone calls to and from the complaint desk of a police department in a middle-sized Midwestern city' (Schegloff, 1968: 1093, n. 1).

This reliance on such a 'restricted' database has sometimes been seen as a severe limitation of the validity of CA's findings. Critiques on this point can take a variety of forms. Reference has been made to 'missing data' concerning participants, such as the usual macro-sociological variables (socio-economic status, age, gender), institutional position, and personal background. Often, critics tend to complain that the institutional context of the interaction is neglected analytically in CA (Cicourel, 1981, 1992). And others wonder why sources like interviews with participants, their comments on recordings ('member checks'), or interpretations of taped material by panels of 'judges' are not used.

To put it bluntly, explanations of what happens in any kind of interaction, institutional or not, that make reference to 'fixed' givens such as institutional identities and functions, institutionalized resources or

relationships, or whatever, are not acceptable to a CA analysis until their local procedural relevance is demonstrated (cf. Schegloff, 1991, 1992a). And even then, what may be said concerning such interactional moments is only that those properties or relationships are 'talked into being' then and there. For example, in my own work on medical consultations, I think I can show that a 'medical consultation' is only constituted during specific parts of the encounter, and by analysable means, while at other moments something like a 'conversation' or another kind of interaction is going on (Ten Have, 1989, 1991b). The same kind of reasoning may then apply to certain properties and identities that are, by members, considered to be stable within settings, such a being a patient or a physician, and also to those that are thought of as constants to any setting, such as age or gender (cf. Garfinkel, 1967: 116–85).

Given these ideas and findings, any preconceptions of properties, relationships, and occasions that are used as taken-for-granted realities in other branches of the social sciences are to be 'bracketed' in an analysis such as CA aspires to. In that sense, CA is a member of the family of 'foundational sciences', examining the pre-givens of everyday life (cf. Lynch et al., 1983: 208). It should be noted that one might even object, on these grounds, to mentionings of the institutional identities of speakers (such as 'Dr' and 'Pt'), as was done in some transcription fragments quoted earlier in this book, and massively elsewhere. The existence and relevance of such identities are, strictly speaking, to be discovered in the analysis, as products of the local practices of participants (cf. Watson, 1997, and my discussion in chapter 5).

Similar kinds of arguments can be raised against criticisms that accuse CA of unwisely neglecting other sources of data, in addition to recordings. When, for example, I present my own work on medical consultations to a non-CA audience, I am often questioned why I do not use various other kinds of information, such as interviews with participants, case records, or interpretations made by a panel of 'judges', to support my findings. My answer to the question why I do not interview the doctors and patients who are represented in my corpus of recordings is as follows. There is no way of knowing how an interpretation of an action by a participant, produced in a setting different from the original one, relates to the action so interpreted. It might make more sense to connect such interpretations to the setting in which they are made, the sociological interview, the viewing or hearing session, than to the setting referred to. It may be very hard for participants to reconstitute after the fact the moment-by-moment interweaving of meanings in interaction. They may be prone to present rather partial accounts, putting their actions in a favourable light. Furthermore, the attention of CA is directed not at uncovering hidden meanings, strategic projects, and the like, but at the meanings that actually and observably are produced in and through the interaction, in order to describe the 'technology' used to bring those about.

Similar arguments could be raised against the use of case records as a

source of information on patients' backgrounds or medical careers, and against the use of panels of lay interpreters of the recordings, to ground or confirm the researcher's interpretations. Again and again, the CA practitioner will feel that those other sources could be analysed in terms of their own productive processes (cf. Garfinkel, 1967: 186–207; Heath, 1982), but that the information which they provide should not prejudge the detailed analysis of the interactional data themselves, and should not be considered more valuable than those data on *a priori* grounds.

In the last decade, however, this 'the data are enough' perspective of 'pure CA' has been criticized from positions closer to its centre than before. A first 'stream' of critical remarks is part of a more extended critical and ambivalent assessment of CA's methods and results from an ethnomethodological perspective, and specifically from what has become known as 'the studies of work programme'. It involves a preoccupation with actually used, 'uniquely adequate' local competencies, the so-called 'haecceities' that constitute the 'just-thisness' of an activity or object (Garfinkel, 1991; Garfinkel & Wieder, 1992; Lynch, 1993: 283). As elaborated by Michael Lynch in a number of his publications (Bjelic & Lynch, 1992; Lynch, 1985, 1993; Lynch & Bogen, 1994, 1996), the ethnomethodological critique of CA is directed at the larger issue of CA's 'generality', its tendency to treat single occasions of interaction as a field in which members apply general, context-independent 'devices', 'organizations', 'machineries', etc. One implication of this position is that an analysis of an interactional episode involving a special kind of 'work' would *require* detailed knowledge of, if not outright 'competence' in, the professional activities studied (cf. Ten Have, 1990, 1995b; also Clayman & Maynard, 1995).

A second general critique of CA that is relevant here takes off from what may be called a 'culturalist' and 'humanist' perspective. It is most prominently represented by Michael Moerman in his book *Talking culture: ethnography and conversation analysis* (1988) and some subsequent papers (1990/1, 1992), who argues for a 'culturally contexted CA'. While already an anthropological fieldworker, Moerman encountered CA almost at its beginning. It seemed to resonate with some of his misgivings about traditional fieldwork methods, but the following quote expresses his ambivalence:

> But to know conversation analysis is not necessarily to love it. Its high-powered lens sacrifices range, breath, and *mise-en-scène*. Moreover, the world it has discovered is startling and strange. There are general, powerful, and intricate abstract structures and processes of human conversation that do not correspond to the social order we commonly recognize or to the cultural worlds that we admire. This can make conversation analysis seem bloodless, impersonal, and unimportant to anthropology's central concerns. Few [CA] publications connect the technical organization of conversation to richly experienced human reality. (Moerman, 1988: x)

The general idea seems to be, on the one hand, that analysing talk-in-interaction on the basis of recordings presupposes and requires a thorough

knowledge of the culture shared by the participants and taken-for-granted in their actions. Such membership knowledge can be acquired as a lay participant in the culture involved, or through participant observation using the well-known techniques of anthropology. On the other hand, the suggestion is also that one has to know the local circumstances, the interlocutors themselves even, in order to get at the meanings and intentions involved. Moerman's interests, then, go beyond 'the technology of conversation' (Sacks, 1984b: 411; 1992b: 339), because, as he says 'we never merely exchange turns of talk. In all conversation, people are living their lives, performing their roles, enacting their culture' (Moerman, 1988: 22). In his proposals for a 'culturally contexted conversation analysis', Moerman wants to combine the methods and objectives of ethnography and CA, as he sees them. His conception of CA, as John Heritage (1990/1: 302) has remarked, seems, however, to be rather limited, as an arid form of 'sequential analysis', to which ethnography should add the 'blood' it lacks.

> All actions are socially situated and all situations structured. Sequential analysis delineates the structure of social interaction and thus provides the loci of actions. Ethnography can provide the meanings and material conditions of the scenes in which the actions occur. Culturally contexted conversation analysis thus permits a description that while never complete, is sufficient for showing the nexus between cultural rules and individual intentions. (Moerman, 1988: 57)[5]

Moerman's book has stimulated a lot of debate, which I will not summarize here.[6] Instead, in the spirit of the present book, I now turn to some more practical considerations.

A third 'internal' position of critique on CA's traditional reluctance regarding non-recorded data can be found among people engaged in so-called 'workplace studies', who work with video recordings to analyse local work practices in technologically complex environments. They suggest that in order to study talk-in-interaction as part of the work in those settings, the analyst should acquire at least some knowledge of the organizational and technical purposes and procedures that the workers studied use as a matter of course. Christian Heath, who has earlier done video-based studies on the medical consultation (1986, 1988, 1992), and more recently studied a variety of technologically complex workplaces (Heath & Luff, 1991, 1996; Luff & Heath, 1992), writes in an introduction to video analysis:

> As studies of talk and interaction have become increasingly interested in more specialized forms of human activities, often arising within particular organizational or institutional domains, it has been recognized that it is necessary to augment recorded materials with extensive fieldwork. So, for example, our own studies of general practice involved a long period of non-participant observation before any recording took place in order to begin to assemble a sense of the organization of certain specialized tasks such as diagnosis, treatment and using medical records. (Heath, 1997: 190)

Switching to his recent activities, he adds:

> With the emergence of more wide-ranging studies of workplace interaction, especially those concerned with the use of tools and artefacts in complex technological environments such as control rooms and emergency centres, we have witnessed an increasing commitment to undertaking wide-ranging fieldwork alongside more focused interaction analyses (Whalen, 1995a). [. . .] It is not unusual in such studies to delay gathering recorded materials until researchers have a passing understanding of the activities in question and the various tools and technologies which feature in the accomplishment of even the more mundane activities in such settings. (Heath, 1997: 190–1)[7]

Jack Whalen, for instance, mentions in the report to which Heath refers in the quote above:

> My resources for developing this analysis include extensive field observations undertaken while working as a call-taker and dispatcher at a police and fire communications facility [. . .] for fifteen months, as well as video recordings of call-takers at work that were collected at [. . .]. The discussion is also informed by field work at other public safety dispatch facilities. (Whalen, 1995a: 187)

The paper itself presents a detailed analysis of the work of call-taking by analysing a single case using both the audio recording of a call and a video recording of the computer screen on which is visible the call-taker's work of filling in the computer-based 'form'.

My own position in these debates is a balanced one (I think!). For 'pure' CA, in the sense explained in chapter 1, as an examination of 'the institution *of* interaction as an entity in its own right' (Heritage, 1997), gathering additional information does not seem to be required in most cases, especially if one studies interaction 'from within' one's own culture and if one has enough recorded data from the same or similar participants to acquire a sense of their *emic* competencies and understandings. For studies of talk-in-interaction from a less familiar culture, one might indeed require deeper understanding of that culture, based on an extended period of 'living in', rather than focused ethnographic research.

For studies in 'applied CA', such as studies of 'the management of social institutions *in* interaction' (Heritage, 1997, again), the methodological situation seems to be quite different. For studies of certain relatively routine and standardized situations, the CA researcher might not need much more background knowledge that what he or she has as a member of society at large, depending, of course, on one's research objectives. In my own research on doctor–patient interaction in the setting of general practice, for instance, I did not gather any particular information on the setting, the doctors, or the patients. I did not seem to need such information to study the 'obvious' structures, strategies, and devices that I analysed. Had I been interested, however, in particular professional aspects of the doctor's questioning, such as the use of 'protocols', the

additional data would have been necessary. In his study of calls to a 'poison centre', Frankel (1989) needed information on the bureaucratic 'form' the call-taker was using in order to understand the particular questioning strategies observed, which seemed illogical when limiting oneself to the tape alone (cf. Whalen, 1995a, 1995b, for similar observations). In short, ethnographic research in addition to CA can be helpful to build up a knowledge base that is sufficiently similar to what a member knows to understand what is going on. One could speak of a 'virtual membership' requirement, which does not compete with the primacy of recorded data, but rather supports it.

In one of the earliest CA-based studies in and of an institutional setting, Douglas W. Maynard (1984) not only collected tapes of actual 'plea bargaining' sessions, but also undertook an extensive 'ethnographic' study of the setting in which he made his tape recordings. He spent

> three months of observing municipal court operations, interviewing public defenders, district attorneys, probation officers, and judges, and collecting data from court records. [. . .] I continued regular observations of pretrial conferences for another two months.
>
> Field notes from observations, interviews, and court records are the primary source of data for [a chapter], which described the courtroom social structure. (Maynard, 1984: 16)

While the other chapters in the book are mainly based on the recorded data, focusing ultimately on 'a discourse system for negotiation', which is quite general, the institutional ethnography is not given 'just for the context'. In fact, it plays an important part in 'understanding' what goes on in those sessions, in terms of 'framing practices' and 'institutional mandates' (see Maynard, 1984, for more details, and his 1989 paper 'On the ethnography and the analysis of talk in institutional settings').

In a recent paper, Anssi Peräkylä (1997) raises the issue of what he calls the 'inclusiveness' of tape-recorded data:

> Although tape-recorded data have intrinsic strength in terms of accuracy and public access, special attention needs to be paid to the *inclusiveness* of such data. Video or audio recordings of specific events (such as telephone conversations, medical consultations or public meetings) may entail a loss of some aspects of social interaction, including (a) medium- and long-span temporal processes, (b) ambulatory events and (c) impact of texts and other 'non-conversational' modalities of action. The potential loss can be prevented with appropriate arrangements in the data collection. (Peräkylä, 1997: 203–4)

He mentions, among other things, for (a) temporal processes, the possibility of recording a series of events, such as consultations, but one could also think of meetings (cf. Boden, 1994), where one builds on previous ones and anticipates later ones, for (b) ambulatory events, the possibility of using multiple cameras or recorders in different parts of a work setting,[8]

while (c) is mentioned in the references in this chapter to studies by Firth, Frankel, Maynard, and Whalen.

> In sum, by appropriate research design, conversation analytic studies of institutional interaction can be made more inclusive in terms of different layers of the organization of interaction. (Peräkylä, 1997: 205)

In the end, then, the choice of whether one needs additional data apart from the recordings does seem to depend, on the one hand, on one's theoretical–methodological outlook, and, on the other, on the kinds of activities one wants to study. No 'fixed' answer seems to be available, but any choice one makes should be accounted for in terms of debates and considerations like the ones given above.

Hunting for data

Like a late eighteenth-century medical researcher, looking for corpses to dissect, a conversation analyst is an habitual 'data hunter'. Using a less gruesome metaphor, Bill Davey and Karsten Gramkow Andersen, of Aalborg University, Denmark, write in a recent paper on 'Some practical and legal aspects concerning the collection of empirical data':

> It is of paramount importance that the analyst goes about his everyday life like a photographer. Just as the photographer looks at the world though an imaginary camera lens assessing potential shots, so the analyst must look for potential data sources in the world around him. (Davey & Gramkow Andersen, 1996)

In short, any option for procuring data should be considered, but practical, ethical, and legal considerations should also play a part.

When one considers the possibilities for procuring data for 'doing conversation analysis', the most obvious way of getting data would seem to be to make one's own recordings of 'natural interactions', either audio or video. There may, however, be other, less cumbersome ways of obtaining data. I will distinguish three overall strategies of procuring useful recordings:

- copying radio and TV broadcasts;
- using existing recordings;
- making one's own recordings.

Before I discuss these three strategies in greater detail, I will present a general discussion of issues of consent.

Consent

Whatever one's data source, one should always consider issues of consent. This means that one needs to consider the rights of the participants in the

interaction, and/or those responsible for the situation, and/or the owners of the recording. These rights concern three basic, often mixed, but distinguishable, rights to refuse:

- to be recorded or to give access to the situation for recording purposes;
- to grant permission to use the recording for research purposes;
- public display or publication of the recordings in one form or another.

If one uses the first or second of the three strategies mentioned above, the issue of the right not to be recorded has already been faced, explicitly or implicitly, by the person who was responsible for the original recording. It can be considered to be settled, for all practical purposes. What is still open, however, is, first, the issue of using the recording for detailed consideration in a CA research project, and, second, the possible 'publication' of the recording in one way or another.

It is a fact that many people dislike the idea that known or unknown aspects of their spontaneous actions will be considered in great detail. This is clearly evident from the reluctance of many to have their actions recorded for research purposes, as well as the mirror-reluctance of many researchers to request or even admit that they want to record their conversations, or have them recorded. And even if people do consent to being recorded, they quite often offer nervously hilarious comments on possible exposures, etc.[9] There seems to be a common-sense association, then, between a detailed consideration of actions and an unpleasant exposure, or critical assessment, of those actions. People seem to be afraid of being 'caught' off-guard. It depends on one's interests and inclinations how one handles such resistances, but they should at least be noted.

One strategy is to restrict one's data to people one does not know personally, which seems to make detailed study easier to accept for both parties, the researcher and the researched. This may be one advantage of using the first or second of the three strategies. How one goes about these things in the third strategy will be discussed in detail in the section on making recordings oneself.

As to the issue of 'publication', there are, of course, many modalities in which recordings can be made 'public' (cf. the detailed discussion in chapter 10). For the sake of the present discussion, I will consider the following:

- the use of transcripts in a restricted setting;
- the public use of transcripts;
- the use of recordings in a restricted setting;
- the public use of recordings.

When one uses transcripts, it is advisable to *always* change identifying details, such as names, addresses, etc. One might think this is unnecessary

in restricted settings, ranging from informal 'data sessions' to presentations at conferences, but I have experienced some quite embarrassing situations when this was not done and someone 'recognized' one or more of the interactants. Therefore, I have made it a routine to change identifying details as part of the transcribing process (see next chapter). In so doing, the distinction made above between restricted and public use of transcripts is no longer relevant.

With recordings, whether audio or video, the situation is different. It may be very useful, in a data session or paper presentation, to play an audio or preferably a video tape, together with showing a transcript (see chapter 10). It can help to make the phenomena under discussion more vividly 'present' as a support for the analysis. This is what I call 'use in a restricted setting', for instance among professionals, who may be supposed to be able and willing to handle any 'recognition' discreetly. There is still a risk, however, of embarrassing situations when a person is recognized. Furthermore, it is possible, especially when considering the activities of public figures like politicians, that the reception of the analysis itself is somehow contaminated by pre-existing evaluative images of those figures.

Until recently, it was hardly possible to make recordings used for CA research available with an 'unrestricted' publication of the analysis. With current advances in information and communication technologies, however, this is becoming a real issue. 'Sound bites', 'frame grabs', and 'video clips' can now be added to a research report made available to an unrestricted public on CD-ROM or the World Wide Web. When using such possibilities, the issues of privacy and consent can become especially acute.

The practical upshot of these considerations is that gaining the required consent for making and using recordings of natural interactions may be quite complicated. It may involve differentiating the recording process itself, the use of the recording for detailed research, and the public use of data excerpts in one way or another. For this last phase, one can negotiate various restrictions, whatever is necessary to gain consent. As I mentioned, making transcripts unrecognizable by a process of anonymization should be a routine procedure, which can be explained as part of the consent-gaining process. Furthermore, one can promise to confine public showing to restricted, professional audiences. In cases of real, that is, unrestricted, publication, one probably should procure separate consent, possibly by showing the relevant excerpts to the interactants themselves.

In the discussion above, I have treated the issue of 'consent' as one that should be on the agenda in the negotiations between researcher and researched. But of course these relationships are embedded in a larger arena. There is a general tendency now, most prominently in the United States, for universities and even governments to *require* formal consent procedures. Universities have 'Internal Review Boards' (IRBs) and require

all research projects that involve humans to be reviewed. In the Netherlands, such requirements are especially enforced in medical settings, such as hospitals. Their Ethical Committees will usually demand the use of consent forms for any audio or video recording.

With these considerations in mind, I will now discuss the three strategies in more detail.

Radio/TV broadcasts

The use of radio and TV broadcasts as a data source may seem strange at first, since the interactions recorded in such a way are generally considered to be rather 'artificial'. Not only may the interactions themselves be prepared or rehearsed, but the recordings may also have been edited in ways which are not discernible to the lay observer. There still may be good reasons, however, to consider such a source, depending on one's overall research goals and the characteristics of the broadcast under consideration. Using this kind of material is obviously sensible if one is interested in a particular form of 'mass media talk', as in studies of news interviews (for instance, Clayman, 1992; Greatbatch, 1988; Heritage, 1985; Heritage & Greatbatch, 1991), or aspects of the interaction in televised political debates (Atkinson, 1984a, 1984b, 1985; Clayman, 1993), TV talk-shows (Hopper, 1995) or 'talk radio' (Hutchby, 1996a, 1996b). But since people do not shift to a completely different set of interactional procedures when they know their talk is broadcast, mass media recordings can also be used for studying talk-in-interaction as such (see, for instance, Schegloff, 1978). In fact, I think that for beginning CA researchers, the mass media are a real data goldmine. At a nominal cost, one can have access to professionally recorded interactions occurring in a wide range of situations and in all major languages. One should, of course, be aware that the material may be edited in various ways, and that the fact that people know they are being recorded may influence their interactions. But at least for exercise materials, this should not be too much of a problem.

When I started doing CA research, I recorded a long series of weekly call-in radio shows, broadcast live in the Netherlands in the late 1970s, which featured informal counselling conversations between the hostess and callers about 'problems with sex and relationships'. Of course, this material had special features, some of which were more or less 'visible', such as the speakers' orientation to the fact of broadcasting, while others were hidden, such as alerts for fake calls, sometimes passed on to the hostess in the studio. But for me, the material offered an excellent training opportunity, and it helped me gain a first overall conception of how such problem-oriented talks might be organized, and some of the detailed ways in which this could be done.

Existing recordings

Another simple way of getting useful recordings is to copy existing ones. This may, again, be done if one does not have a specific interest, but would like to have 'any' recordings of natural interaction. But this strategy may be particularly useful if one does have such a special interest and if the interactions one wants to study are being recorded 'naturally', as part of routine procedures. Both Harvey Sacks and Emanuel Schegloff used existing recordings, routinely made in a suicide prevention center and a police department, respectively, for their first systematic investigations. Similarly, Alan Firth (1995b, 1995c, 1995d) has used routine recordings to study business negotiations. As he explains:

> The data reproduced in this paper were collected over a four-month period in 1989–90. The source of the data is 'Melko Dairies,' one of the largest dairy conglomerates in Denmark. As a matter of policy, the company audio records all international telephone calls made to and received from foreign clients. Invariably, international calls within the company are conducted in English. [. . .] In order to protect the anonymity of the parties concerned, all personal names, company names and public places have been changed. (Firth, 1995c: 169, note 5)

And again, Harrie Mazeland has asked a varied set of qualitative researchers who had done interviews for copies of some of the recordings of such interviews, in order to study question/answer sequences in such settings (Mazeland, 1992; Mazeland & Ten Have, 1996). For my own research on doctor–patient interaction (as in Ten Have, 1989, 1991b, 1995a), I relied entirely on recordings made for teaching purposes or other research projects, copied from tapes made by colleagues. Apart from the practical benefits, this fact also served as an extra barrier, so to speak, to protect the privacy of the participants.

Making field recordings: social issues

Making one's own recordings, or having them made at one's request, is quite often the only way to get precisely the kind of data one wants. This may be less urgent when one has a general interest, but is quite often required if one has a specific one. In other words, for those engaged in 'pure' CA, as previously defined, making recordings is often less a problem than for those in 'applied' CA. There are a number of issues related to making recordings, some strategic or tactical, others practical, technical, ethical, and legal. In this section, I will focus on the choice of the setting, the problems related to gaining access and consent, and some general practical issues. I will not go deeply into the technical aspects here, such as brand names and types of devices, etc., because these would

probably be out of date once this book is published. And I will also only roughly gloss the legal issues, since these are bound to vary from country to country, as well as from situation to situation.

As I suggested, if you are interested in talk-in-interaction in a general way, you can make recordings in 'any situation'. So you could just pick a setting that is accessible to you, and where you can either gain consent from the parties present, or get away with it without getting such consent. The most important consideration for you would probably be to set up the recording device and manage the consent in such a way that it does not interfere with the parties speaking in a 'natural' way (but see my earlier remarks on 'naturalness').

At the very start of my CA work, I recorded quite a number of my own telephone calls. This at least provided me with what is called 'one party consent', since I was consenting to my own recording, which seems to be sufficient in many situations.

Here is the start of the very first call I recorded, in which I called my father-in-law (Schrama), who offered to make enquiries about the gadget I needed to tap the phone:

Excerpt 4.3 Start of a telephone conversation[10]

1		((telephone ring))
2	O:	Schrama
3	B:	dag met Paul
3	*B:*	*hi Paul speaking*
4	O:	ja Paul
4	*O:*	*yes Paul*
5	B:	ik kom even melden dat ik eh geslaagd ben in het kopen van een
5	*B:*	*I just called to tell you I uh succeeded in buying a*
6	B:	telefoonspoel
6	*B:*	*telephone coil*
7	O:	HA hh hh
8	B:	du[s daar hoeft u uw best niet meer voor te doen
8	*B:*	*so you don't have to make any more efforts for that*
9	O:	[goed
9	*O:*	*Okay*
10	O:	oh (.) oh (.) oh (.) 'h goed gelukkig voor je
10	O:	*oh (.) oh (.) oh (.) 'h okay I'm happy for you*
11	B:	ja hoor[11]
11	*B:*	*yes okay*
12	O:	dus ik kan nou eh alle (.) akelige dingen op dit gesprek kun je-
12	*O:*	*so I can now uh all (.) nasty things on this conversation you can-*
13	O:	ik moet dus meer oppassen wat ik zeg
13	*O:*	*I have to be more careful about what I say*
14	B:	ja ik [neem het inderdaad op om te kijken of het lukt
14	*B:*	*yes I [do indeed record it to see whether it works alright*

15 O: [als ik het goed begrijp
15 O: [*if I understand it rightly*
16 O: juist
16 O: *right*

Details of this excerpt can be seen as a demonstration of some of the points I raised earlier concerning the 'feelings' people may have about being recorded. My father-in-law's cautiously formulated remarks in lines 12 and 13 clearly indicate that his awareness that I might record the call made the situation a different one for him. In 12 he starts twice with an utterance that he does not finish, only to make a full statement of the consequences hinted at in the last try in 13. Similarly, my account for making the recording, in 14, as an exercise to check whether the device worked alright, can be heard as a kind of 'mitigation' of my act of making the recording, pretending that this was not a serious attempt, but just a preparatory, throw-away one. When I later showed him the excerpt in my first published CA paper, he did not comment explicitly, but his wry smile seemed to indicate his surprise rather than pleasure.

As with many other kinds of 'deviance', later recordings produced less awkward feelings, at least in me. I stopped the habit later, however, because the fact that I was one of the participants, and that I had a lot of background knowledge on many of my interlocutors, tended to influence my analysis of such data too much and in ways that were hard to control. Some of these data have been used by others, though. Indeed, most CA researchers seem to avoid data in which they themselves participate.

There are several ways in which the making of recordings can be arranged: one can ask one of the participants to do it; one can arrange to have some technician make the recordings; or one can make them oneself, but then without participation, or at least only minimally. In the first option, one often does not have, or even seek, personal access to a setting, resulting in a database that consists mainly, or practically only, of the recorded interactions. The second option is mostly chosen in well-funded projects using technically complicated video equipment. When one selects the third option, one has a chance, and often the intention, to acquire a more extensive database, including personal experience in the setting, field notes of observations, and possibly formal interviews with participants as well. As discussed in a previous section, researchers differ in their preferences in this matter, some arguing for a pure form of CA, uncontaminated by any knowledge external to the recorded data, while others prefer analysing the data within a framework informed by ethnographically acquired knowledge. To put some flesh on the bones of this abstract debate, it may be useful to provide some examples. I therefore turn now to a summary description of two 'cases', illustrating the first and the last options, respectively. Both concern varieties of 'applied CA', where quite specific kinds of data were needed.

As an example of the first procedure, I quote and paraphrase parts of

the PhD thesis of Patricia Haegeman, on 'Business English in Flanders: a study of Lingua Franca telephone interaction'. For this project, she needed recordings of telephone conversations made in a business setting in Flanders, with native speakers of Dutch, graduates in economics, speaking English for business purposes. She describes that it was quite difficult to get the right kind of people to cooperate in such a project. She started out with graduates in Applied Economics from her university, but

> relatively few graduates agreed to cooperate and had the permission from their superiors to do so. Notwithstanding our promise to leave out confidential data, a huge number of potential subjects were lost for this reason. (Haegeman, 1996: 78)

Therefore, she had to search in a wider network of candidate participants. She succeeded, finally, in constructing a corpus of forty-nine calls recorded in seven companies, more than three hours of talk, rendered in about 6,500 lines of transcript, 148 pages. The actual process of having the recordings made is described by her as follows:

> For each tape we went to the company to hand to the participant a recorder, usually walkman-size, plus a tape and a small suction cap microphone which could be stuck to the receiver of their own phone. This procedure was chosen to disturb the normal course of affairs as little as possible. In the following days or weeks the participants recorded their English calls and contacted us when the tape (usually only one side) was full or when they thought they had recorded enough. We then collected the materials at the company. No money or presents were given: only a thank you letter was sent afterwards and the people interested received an article about other parts of the research (a questionnaire study). (Haegeman, 1996: 78)

Referring to the debate between what she calls 'hardcore CA' and a 'more ethnographic trend', she adds that she refrained from collecting additional 'background data':

> Never did we collect any ethnographic information about the company, products, (co)participants etc., nor did we conduct any post data interviews. We only know what we could not avoid getting to know: since we went to the company to take and bring back the recorder, we know the location and had some impression of the company in general. We saw the recording participant and spoke with him for five or ten minutes, mainly to show how to handle the recording equipment. All we know and wanted to know is based on the recorded conversations, some of the unavoidable facts mentioned above and the equally unavoidable knowledge we have of the world as a member of a culture. (Haegeman, 1996: 79)

Interestingly, Patricia went so far as to seek professional legal advice on her liabilities 'for the sake of completeness and, admittedly, just to be on the safe side' (Haegeman, 1996: 81). She submitted her 'case' to lawyers

who applied the then relevant Belgian law, which, of course, may differ from laws elsewhere. I will selectively render her summary of the ten-page report she received:

> The facts are split up into the recording and the transcribing, each of which involves a discussion of criminal law and one of civil law. [. . .] In any criminal charge it would be difficult to define the interaction recorded as 'private communication' on the one hand and difficult or impossible to prove that there was any 'criminal intent' on the other. [. . .] As for civil law, any plaintiff would have to prove fault, damage and the causal relation between the two. Fault: since the researcher has taken all possible measures to avoid any infringement on privacy and since the recordings were made voluntarily and free of charge by people who were informed beforehand about the aim and who decided themselves what was recorded, it would be far from simple to find the researcher guilty of any fault. Damage: since no identification is possible it will be difficult to prove that there ever was any damage suffered by any party. Thirdly, it would have to be proved that the possible infringement on privacy was the cause of the possible damage suffered.
>
> Conclusion: as long as the tapes, which may contain (ab)usable data, are safeguarded from access by any third party and as long as the transcripts delete any data which could identify any party involved, there can in principle be no problem. (Haegeman, 1996: 81–2)

These findings and suggestions may not be taken as guidelines for other situations and countries, of course, but they may serve as an indication of the sort of issues that may be involved.

My second example refers to work by a team based at the University of Giessen, Germany, including Michaela C. Goll and Christoph Meier, and their study of what they call 'telecooperation', which includes *teleconferencing*. In a recent paper,[12] they write:

> An alternative to the established procedures for investigating telecooperation [. . .] is to penetrate the 'skin' of an organization, inject a probe and actually look at how collaborative work is performed in situ. In the case of our research, the probe is our videorecording equipment, which allows us to analyze authentic and contextualized work processes in much more detail than any other procedure would afford. Where observational notes or interviews would give us accounts based on 'after-the-fact' sense-making and interpretation, audiovisual recordings provide us with a real-time version of participants' conduct and procedures. Repeatedly viewing particular episodes and rendering them as transcripts on paper in order to literally compare them side by side are powerful resources for reconstructing the systematics underlying participants' actions.

And coming to the topic of this chapter, they explain:

> However, often no attempt is made to obtain permission for audiovisual recordings of authentic work processes for fear of being rejected immediately or for fear of losing whatever rapport has been gained with representatives of an organization. We have made the likelihood of eventually gaining permission for

> such videorecordings the central criterion in pursuing access to various organizations. Of course we were not granted permission to do so right away. However, we have made clear from the very beginning that audiovisual recordings are a central component of our research procedures and that we eventually will want to make such recordings. In the case of the organization where we have observed the videoconferencing interaction reported on [in the paper], contact was pursued during a 'trial period' of three months involving participant observation in the meetings before permission to record interactions was finally obtained. Proceeding in this way we have been able to gain permission to record interactions involving various telecommunications technologies in three organizations.

In fact, Christoph Meier was sitting in on the meetings that were recorded on tape, making notes on what was happening. He was, indeed, keeping a low profile in the interaction, but he unavoidably was part of the events, looking at the points where the action was, laughing with the jokes, etc.

The recordings were made under strict conditions regarding confidentiality concerning the identity of each company and the participants, and pseudonyms were used throughout the transcripts and the analysis. Meier had to sign a contract on the protection of participants' rights and the use of materials with the company at which he recorded the videoconferencing sessions. The paper, however, does contain 'frame grabs' from the video tape, from which the participants might be identified, although the images are in fact rather vague. All publications, however, have been checked with Meier's 'sponsor' at the company, and have been agreed with.

As these examples make clear, there are many ways of organizing 'access' to data and 'consent' with recordings being made. 'Gaining access' and 'managing field relationships' are widely discussed in the older literature on ethnographic methods (cf. Bogdan & Taylor, 1975: 25–78; Douglas, 1976: 133–80; Hammersley & Atkinson, 1983: 54–104; Lofland & Lofland, 1984: 13–45; Schatzman & Strauss, 1973: 18–33). Getting consent for, and managing the making of, recordings seems to be just a special case of that general problematic. From that literature, my own experiences and that of my colleagues and my students, I would say that one should try to design a strategy and develop tactics which serve to weaken negative motivations and to strengthen positive ones for whoever has the power to keep you from making the recordings, or whose cooperation you may need to influence the disinclined, and to normalize and naturalize the recording situation. The major negative motivation one encounters is that people want to avoid 'trouble', of whatever sort.

The major troubles expected are the fear of some kind of 'exposure' (as discussed in the section on consent), disturbance of established work processes, and resistances from other parties in the field. The major strategy, then, is to neutralize these fears, by providing guaranties of anonymity, minimal disturbances of work processes, and the willingness to face other parties' resistances. The major positive motivations one can hope for

include the possibility of helping nice people such as the researcher who deserve such help and of facilitating a research project which may have some practical pay-offs, or social research in general. In this respect, students have some advantages: they seem 'innocent', or at least not too 'dangerous', and they obviously need help to finish their study obligations.

Pointing to possible practical benefits may be dangerous, since this may be seen as making promises which may turn out to be hard to fulfil. Sometimes, however, organizations may ask researchers for some kind of advice on 'communication problems' and the like. In such cases, the making of recordings of the routine handling of whatever the organization deals with can be requested as a prerequisite for such assistance. Those tapes, then, can serve the purposes of both informing the advisor and providing materials for detailed CA research, either as part of that process or independently of it. In general, whenever some kind of interaction is felt to be 'problematic', in one sense or another, it may be easier to find people willing to have it recorded for research.

In the paper quoted earlier, Davey and Gramkow Andersen suggest exploiting any 'existing networks, e.g. family and friends or clubs and associations to which you may have access'. And they continue:

> When approaching companies or institutions, try to approach them through an existing contact. Often people you know will be eager to help by recording meetings or phone calls, and they are generally prepared to overcome potential difficulties. In such cases, the informant will probably be willing to persuade the co-participants that the data will be treated confidentially. Implicit in this is that the informant obtains the consent of the co-participants and that they 'know' of the recording. (Davey & Gramkow Andersen, 1996)

When no such contacts exist, a different approach is needed:

> When approaching an area, a company or an institution with which there is no initial contact, it will be necessary to keep a higher level of formality. the approach we use can be described in three main steps. First you must present yourself and your work, then overcome any objections from potential informants and the company, and finally there is the actual recording of data. (Davey & Gramkow Andersen, 1996)

The presentation is best done in writing, using official (university) paper, (co-)signed by someone clearly identified as a person of authority, like a professor. The actual negotiations are probably best done during a personal visit, arranged by telephone. One could start the conversation presenting oneself and explaining the purpose of the research in general terms, to be followed by a more detailed sketch of the research process, including the making of the recordings. The reassurances regarding routine anonymization and minimal disturbance, mentioned before, should be emphatically stressed. Any objections raised should be taken seriously, and it depends

on one's circumstances and interests which concessions should be made if the reassurances do not do the job of gaining consent.

Davey and Gramkow Andersen add a very sensible suggestion:

> After arranging for the recording to take place, it is useful to send a resumé of the agreements reached. This gives both parties to the recording agreement something they can refer to in the future, in case of uncertainty about the conditions of data collection and use of the data [. . .]. (Davey & Gramkow Andersen, 1996)

The negotiations quite often will result in various conditions which limit the future use of the data, as suggested in the section on 'consent'. It seems best to be quite reluctant in this respect, but to respect any conditions agreed upon, since one may be taken to be legally responsible for any use not agreed upon beforehand.

Making field recordings: technical issues

As to the technical details of making recordings in 'natural settings', I will limit my discussion to a bare statement on basic requirements and a few suggestions. For any recording, whether audio or video, an obvious requirement is that the recorded talk is transcribable. It does not need to be superb hi-fi, but the words spoken should be clear and the interactional details that have been proven to be important for a CA analysis (seen next chapter) should be discernible. This may be quite difficult to realize over bad telephone lines or in noisy environments. What helps, in the case of field recordings of face-to-face interactions, is to have good microphones, independent of the recorder, and placed as close to the sound sources as is practically possible. As Charles Goodwin writes:

> Tests [. . .] showed that the main influence on sound quality, even more important than the quality of the microphone used, was the distance of the microphones. The closer the microphone, the better the sound. The best sound is obtained by actually attaching the lavaliere microphone to the speaker. Because of the quality obtained, this method is regularly used by linguists to obtain samples of speech. (C. Goodwin, 1981: 38)

This is often cumbersome and intrusive, however, therefore Goodwin decided to do it differently:

> I recorded speech by positioning a stationary microphone with the participants but not attached to them. The microphone was centrally placed and located as close to the participants as possible without being excessively intrusive. The placement that produced perhaps the best results was over the center of the group, slightly above the heads of the participants. (C. Goodwin, 1981: 39)

Note that the microphone should be hanging rather than standing, in order to avoid picking up too many noises through a table, etc.

Technically, almost perfect recording is possible, but it depends on local technical and financial resources, and interactional circumstances, how much of this ideal can be realized. Many researchers will make concessions in terms of recording excellence, in order to have a less obvious and imposing recording situation. For this it helps to acclimatize the participants to the situation, for instance in recording regularly held meetings. Then the impact of the recording situation will in all probability be less strong, and the recorded interaction quite natural.

Marjorie Goodwin, for instance, reports on the data gathering for her research on 'talk as social organization among black children', based on audio recordings, in the following terms:

> My actual method of working consisted of travelling with the children as they went about their activities while I had a [brand name and type] cassette recorder with an internal microphone over my shoulder. I began recording two months after I started fieldwork and continued for sixteen months. The children knew they were being taped, but talked directly to the machine only in the early days of recording. Because I used only the internal microphone, I never had to actively point something at the children in order to record them but could get good records of their conversations simply by staying with them. Indeed, the recorder became a natural part of my appearance, almost like a purse. Strapped over my shoulder in its black case, often over my black trenchcoat, it was seldom commented upon after the first weeks of use. (M.H. Goodwin, 1990: 22)

In many cases, however, such extended periods of fieldwork, in which recording activities can become almost 'natural', is not feasible, or maybe not even desirable (cf. the earlier discussion). In such cases, when there are only a limited number of recording situations, extensive pre-testing of the equipment is desirable, unless one is willing to accept the risk of a beautiful 'recording' resulting in a useless tape.

There are various techniques for capturing telephone sound, some based on induction, with a coil attached to the telephone externally, while others use a direct connection to the telephone wires. Davey and Gramkow Andersen discuss the possibilities for Danish telephones, which suggest that the technical possibilities strongly depend on local technical circumstances. Therefore one should make one's own local enquiries and test the device on each connection to be taped.

For video taping, one should select such a position for the camera that the recording will show not only all the participants in the required detail, but also whatever is important for understanding what is going on, such as various objects handled or referred to, computer screens viewed, etc. This may be quite a puzzle, possibly requiring more than one camera and 'split-screen' techniques for transcription and presentation purposes. Years ago I saw a tape, for instance, made by Richard Frankel, for a study of physicians' note-taking during medical consultations. If I remember correctly, it had a side-view of the two parties sitting together, but, in addition, one could see, in a small 'window', the doctor's writing hand on

the paper, taken by a camera above the desk looking down. In this fashion, one could study in detail how the talk and the writing were coordinated.

Try to use a fixed camera, on a tripod, whenever that is possible. A hand-held camera gives you more flexibility, but that introduces an element of selectivity that may frustrate you in the analysis. Furthermore, a stable image is much more comfortable to look at, especially when you have to watch a scene repetitively. And finally, a stable image is easier to handle for compression programs when you digitalize the data, which will soon become the common procedure.

Heath and Luff suggest that although one might think that a camera is an intrusive element in the setting, it may in the end influence action less than would a human observer.

> Thus, where possible, the researcher should set up the equipment prior to the events being recorded and try to avoid focusing and operating the camera equipment during the events. Of course it will be necessary to change cassettes, but normally it is possible to manage these practicalities between events. (Heath & Luff, 1993: 308)

As this chapter has tried to make clear, the process of collecting recordings, or making them oneself, needs a CA researcher's careful attention. In some cases, it can be quite simple, but in others rather complicated. This depends on circumstances, opportunities, and especially research purposes. No easy recipes can be given. One needs to adapt the considerations and suggestions provided to one's own circumstances.

EXERCISE

Make at least one audio recording of naturally occurring verbal interaction and write a detailed account of your choice of the recording strategy, the equipment and set-up used, the experience with it, including any reactions from the participants and the ultimate sound quality of the record. Alternatively, if that suits your purposes, you can choose to make a copy from the mass media (radio or TV) or to use an existing recording. In that case, you should write an account for that particular choice, referring to issues of naturalness and sampling, the particularities of the recording situation, any possible editing, etc. Conclude with an evaluation in methodological terms.

See appendix B for more extensive instructions.

Recommended reading

As noted, the practical aspects of 'producing data', in the sense of collecting or making recordings, are hardly discussed at length in the CA literature, especially as concerns

audio recordings. Therefore, I cannot provide a long list of recommended reading on this topic. Of the titles mentioned below, the paper by Davey and Gramkow Andersen is probably hard to get hold of, while it is also quite strongly oriented to the Danish situation. The Goodwin pieces are very good, and – although focused on video recording – recommended for reading by researchers using audio as well.

Davey, B., & K. Gramkow Andersen (1996) 'Some practical and legal aspects concerning the collection of empirical data'. In: K.A. Jensen & J. Steensig, eds, *Datadag*. Aarhus: ADLA (Danish Association for Applied Linguistics)

Goodwin, C. (1981) Section on 'Data' in his: *Conversational organization: interaction between speakers and hearers*. New York: Academic Press: 33–46

Goodwin, C. (1994a) 'Recording human interaction in natural settings', *Pragmatics* 3: 181–209

Notes

1. These issues will be discussed more fully in later chapters on analysing data.
2. Cf. Mazeland (1992) and Mazeland & Ten Have (1996) on qualitative research interviews, and Houtkoop-Steenstra (1995), Schaeffer (1991) and Schaeffer & Maynard (1996), for examples of a vastly growing number of CA studies of standardized survey interviews.
3. Cf. the characterizations of CA by George Psathas (1995: 2–3), quoted in the last section of the previous chapter.
4. Cf. discussions of consent and access later in this chapter.
5. I am indebted to John Heritage (1990/1: 302) for locating this quotation.
6. Cf. a collection of papers by Robert Hopper (1990/1) and a paper by Christian Nelson (1994) for further information.
7. Cf. Heath & Luff (1993: 309) for a similar statement.
8. David Frohlich has done some experiments with 'electronic shadowing' that also seem to offer a promising solution for this problem.
9. Excerpt 4.3, to be given later, provides an illustration of this phenomenon.
10. As in all non-English transcriptions, an English gloss, printed in italic, is provided in the line below the one representing the original speech (see discussion of data translation in the next chapter).
11. Any translation of a particle like 'hoor' seems to be problematic. Cf. 'demonstration 4' in chapter 6.
12. The paper, called 'Interactional dynamics of electronically mediated collaborative work: local solidarity in videoconferencing', was, at the time of writing, available on the World Wide Web on the home page of the project at: http://www.uni-giessen.de/~g312, but has been withdrawn; other reports have been made available at the site.