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PART VI VALIDITY

13 Reliability and Validity in Research Based on Tapes and Transcripts

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As Kirk and Miller (1986: 11) and Silverman (1993: 144) point out, the issues of reliability and validity are important, because in them the *objectivity* of (social scientific) research is at stake. The aim of social science is to produce descriptions of a social world – not just any descriptions, but descriptions that in some controllable way correspond to the social world that is being described. Even though all descriptions are bound to a particular perspective and therefore represent the reality rather than reproduce it (Hammersley, 1992), it is possible to describe social interaction in ways that can be subjected to empirical testing.

There is no single, coherent set of 'qualitative methods' applicable in all analysis of texts, talk and interaction. Rather, there are a number of different sets of methods: different ways of recording and analysing human activity and the use of symbols. Insofar as these various methods claim an epistemic status different from mere common sense, insofar they claim to report more than the research subjects' own descriptions of their circumstances, the question of objectivity is relevant for all these methods.

In research practice, enhancing objectivity is a very concrete activity. It involves efforts to assure the accuracy and inclusiveness of recordings that the research is based on as well as efforts to test the truthfulness of the analytic claims that are being made about those recordings. These concrete efforts, however, take different shapes according to the type of recordings on which the research is based. Questions that arise in the context of ethnographic field notes, for example, are different from questions that arise in the context of written texts. Field notes can be produced so as to be focused on particular issues or, alternatively, they can be produced so as to include as wide a range of events as possible (Hammersley and Atkinson, 1983: 150-1). In the analysis of written texts such a question does not arise: the researcher cannot control the focus of a given text that is used as data. (But the researcher can, of course, select the range of texts that he or she uses.)

This chapter will deal with issues of reliability and validity in research based on tapes and transcripts, and, in particular, in conversation analysis (CA). I will focus this discussion on one specific type of qualitative research

only mainly because, as it was just pointed out, the questions of reliability and validity take a different form in different qualitative methods. The second reason for focusing on this specific variant of qualitative research is the fact that there are no accessible discussions available on issues of validity and reliability in conversation analytic studies.¹ This does not mean, however, that questions of validity and reliability have been addressed in conversation analytic research practice. In fact they are addressed more there than in many other types of qualitative research. But what has been lacking is a general student-oriented discussion about validity and reliability in conversation analytic research. The purpose of this chapter is to make a contribution in that direction.

Although the discussion in this chapter focuses on a specific type of qualitative research (conversation analysis), the basic issues raised here are relevant in the context of any qualitative method. Therefore, readers who are not primarily interested in conversation analysis are encouraged to treat this chapter as an *example* of the kinds of considerations that need to be addressed by any qualitative researcher. Even though the specific questions and answers concerning validity and reliability are different in other qualitative methods, the basic concerns are the same.

The argumentation presented in this chapter concerns primarily conversation analytic research on *institutional interaction* (Drew and Heritage, 1992; Drew and Sorjonen, 1997). As John Heritage points out in his contribution to this volume, there are two different kinds of conversation analysis going on today: 'The first examines the social institution of interaction as an entity in its own right; the second studies the management of social institutions [such as corporation, classroom, medicine etc.] in interaction' (p. 162). The first type of conversation analysis focuses on what is called 'ordinary conversation': informal talking among friends, family members, or the like. The latter one focuses on verbal interaction between professionals and clients or amongst professionals.

The methodological constraints facing these two types of conversation analysis are partially overlapping and partially different. In this chapter, the primary focus is on the latter type of conversation analysis. Hence, I will discuss issues such as the use of written documents along with the conversational data, and the criteria for validating claims about the relevance of an institutional context of interaction. These issues concern the analysis of institutional interaction, not the study of ordinary conversation. Some other issues that I will discuss, however (such as deviant case analysis), are also applicable to the analysis of ordinary conversations.

The aim of all conversation analytic studies (both on ordinary conversation and on institutional interaction) is to produce descriptions of recurrent patterns of social interaction and language use. CA is particularly rigorous in its requirement of an empirical grounding for any descriptions to be accepted as valid. In this respect, CA differs from some other forms of discourse analysis (Fairclough, 1992; Jokinen et al., 1993; Parker, 1992) and social constructionism (Gergen, 1994) which emphasize more the

'openness' of any language use to different interpretations and hence underline more the active contribution of the researcher in 'constructing' the descriptions that she or he produces about language use.

Reliability

Kirk and Miller define reliability as 'the degree to which the finding is independent of accidental circumstances of the research' (1986: 20). In ethnographic research, the reliability of research results entails 'whether or not (or under what conditions) the ethnographer would expect to obtain the same finding if he or she tried again in the same way' (1986: 69).

In the context of ethnography, as Silverman (1993: 146–8) also points out, checking the reliability is closely related to assuring the quality of field notes and guaranteeing the public access to the process of their production (cf. also Hammersley and Atkinson, 1983: 144–61). In conversation analytic research, tapes and transcripts are the 'raw material' comparable to ethnographers' field notes. Accordingly, the quality of tapes and transcripts has important implications for the reliability of conversation analytic research.

Concerns of reliability as a reason for working with tapes and transcripts

Working with tapes and transcripts eliminates at one stroke many of the problems that ethnographers have with the unspecified accuracy of field notes and with the limited public access to them. According to Harvey Sacks, realizing the potential of tape-recorded materials actually gave a crucial impetus to the creation of conversation analysis:

It was not from any large interest in language or from some theoretical formulation of what should be studied that I started with tape-recorded conversation, but simply because I could get my hands on it and I could study it again and again, and also, consequentially, because others could look at what I had studied and make of it what they could, if, for example, they wanted to be able to disagree with me. (1984: 26)

Tape recordings and transcripts based on them can provide for highly detailed and publicly accessible representations of social interaction. Therefore, Kirk and Miller's suggestion that in qualitative research 'issues of reliability have received little attention' (1986: 42) does not apply to conversation analytic research. CA claims part of its justification on the basis of being free of many shortcomings in reliability characteristic of other forms of qualitative research, especially ethnography.

Securing maximum 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.

Temporal processes Conversation analytic research has brought a new kind of temporality into the central focus of sociological analysis: sequential organization of interaction operates in and through the relative timing of actions. As ethnographic research has repeatedly shown, however, local social worlds are also organized in terms of longer temporal spans (let alone the historical time focused on in the classical macro-sociological works of Durkheim, Weber and Marx). In hospitals, for example, management of chronically ill or dying patients involves complex trajectories shaped in and through the evolving daily actions of staff and patients (Glaser and Strauss, 1968; Strauss et al., 1985; Sudnow, 1967). Similarly, in social services, the recognition of events such as child mistreatment involves long-span processes with a multitude of agents and their negotiations at different sites (see Dingwall et al., 1983).

In the research based on tape recordings of single encounters, there is a risk that some of these longer-term temporal processes will be lost from sight. To prevent this from happening, longitudinal study designs can be used. Heritage and Lindström (1996), for example, report research based on recordings of six consecutive visits by a health visitor to a mother who had recently given birth. Their analysis focuses on the ways in which the mother progressively discloses morally problematic material, and the ways in which the health visitor manages these disclosures.²

Ambulatory events People move about in doing things. In Goffman's terms, the participants in any face to face interaction are 'vehicular entities, that is, human ambulatory units' (1983: 7). For anyone who has acted as a participant or as an observer in a hospital setting, this must be obvious. The ward round, for example, is, from the professionals' point of view, a single event with a number of alternating sub-groups of patient participants; this event moves about in patients' rooms and in the corridors of the ward.

The whole richness of ambulatory interaction can hardly be encapsulated using a stationary video camera, say, in one patient room. By collecting ethnographic data along with the tape recordings, the researcher can capture some aspects of ambulatory events. A good example of the fruitful combination of ethnographic and tape-recorded data is provided by Goodwin (1994, 1995; for a more abstract discussion, see Silverman, 1994). Moreover, by the use of multiple cameras, recordings can be made that are both comprehensive and accurate. Multiple cameras also need to be used when the interaction involves multiple sites which are connected using

technical means, such as monitors or telephones. Charles and Marjorie Goodwin, for example, have used multiple cameras in recording the work of the crew in an oceanic research vessel (Goodwin, 1995) and the activities of air-traffic controllers (Goodwin and Goodwin, 1997). Similarly, Heath and Luff (1997) have analysed convergent activities in London Underground Line Control Rooms.

Documentary realities Written documents (and their production and use) are important for social life, as a domain of signification of its own, or, as Dorothy Smith (1974, 1990) puts it, as a 'textual reality'. Written documents also constitute a domain which is in contact with the domain of spoken interaction and which in some events organizes some aspects of it. As Firth (1995: 205–11) has recently shown, in international business communication, written messages (communicated through telex and fax) relate in many ways to the organization of the telephone conversations between the trade partners. Therefore, full understanding of some of the institutional activities conducted by telephone is not possible without the analysis of the prior written messages that inform their production. Similarly, in medical settings the content and the ordering of some of the questions that the professionals pose to the clients can be strongly influenced by clinical forms that the professionals need to fill in. Thus, even though every question has its local interactional management which can be observed in the tape recording, the logic of the questioning as a whole may not be derivable from the vocal events only. Therefore, it is important that the conversation analyst carefully collects and uses all the relevant written documents, along with the collection of tape recordings (for further examples of this, see Maynard, 1996; Whalen, 1995).

Different aspects of social organization 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 action. However, it also needs to be pointed out that *conversation analytic studies do not aim at describing all aspects of social organization*. (This is, of course, true concerning any other methodology as well.) The organization of verbal interaction in face to face encounters and telephone conversations is the domain in which adequate conversation analytic studies can rightly claim superior reliability, and this is indeed the home base of CA methodology. In studies that focus primarily on other aspects of social organization (such as textual, pictorial or technological realities) other methods may be more suitable.

Improving the reliability of CA in its own field

The claim of superior reliability in studies of face to face interaction needs to be justified, however, in each single piece of conversation analytic

research. The key aspects of reliability involve *selection of what is recorded*, *the technical quality of recordings* and *the adequacy of transcripts*.

Basic *selection of what is recorded* arises, of course, from the research problem. But after this has been done (i.e. when the researcher has decided to tape-record encounters in a specific setting such as classroom, doctor's surgery, educational counsellor's office, or the like) the researcher still has to make some very consequential choices. The most important choice is *how much to record*.

There is a limit to how much data a single researcher or a research team can transcribe and analyse. But on the other hand, a large database has definite advantages. As the analysis of data in conversation analytic studies usually progresses inductively, the researcher normally does not know at the outset of the research what exactly the phenomena are that he or she is going to focus on. Therefore, it may turn out that she or he wants to analyse events that do not occur very many times in each single recording. For example, delivery of the diagnosis in medical consultations is such an event: there are consultations where no diagnosis is delivered, and in many consultations, it is done only once (cf. Heath, 1992; Peräkylä, 1995b). In order to be able to achieve a position where he or she can observe *the variation of the phenomenon* (such as the delivery of the diagnosis) in any reliable way, the researcher needs a large enough collection of cases. Therefore, he or she may need to have access to a relatively large database. In practice, a large portion of the data can be kept as a resource that is used only when the analysis has progressed so far that the phenomena under study have been specified. At that later stage, short sections from the data in reserve can be transcribed, and, thereby, the full variation of the phenomenon can be observed.

The technical quality of recordings is a decisive issue: if something is lost from sight or remains inaudible in the tapes, there is no way of recovering it. It may be extremely frustrating to have some badly recorded sections of events that at a later stage of the research turn out to be of primary importance for the analysis. This kind of frustration can be minimized by already at the planning stage of the research paying enough attention both to the quality of the equipment and to the arrangements of recording. The crucial aspects of quality include the sound (quality and location of microphones) and the inclusiveness of the video picture (the location and the type of the lens of the camera(s)). (For further details, readers are advised to consult Goodwin's [1992a] thorough treatment of the topic.)

The adequacy of transcripts is equally important: even though in a proper analysis of data the tapes need to be listened to and watched, at least the selection of what is analysed in detail is usually done on the basis of the transcripts only. The quality of transcripts in research on naturally occurring interaction seems to vary greatly. Not only are the details of intonation and prosody sometimes omitted, but what is more problematic, whole utterances (especially in multi-party situations) can be missing from

transcripts in studies that otherwise have been seriously and adequately designed and conducted.

Transcription is a skill that can only be acquired through long enough training. It is extremely useful if an experienced transcriber can supervise a beginner. This is most easily done by the more experienced one correcting some of the beginner's transcripts. In fact, the correction of transcripts is useful for anybody preparing transcripts: another researcher can always hear some of the things that one has not noticed. Correction by colleagues also enhances a culture of shared practices in measuring pauses, intonation, and so on.

It is advisable to include many aspects of vocal expression in the initial transcripts (for conversation analytic transcription conventions developed by Gail Jefferson, see the Appendix to this volume and Atkinson and Heritage, 1984: ix-xvi). A rich transcript is a resource of analysis; at the time of transcribing, the researcher cannot know which of the details will turn out to be important for the analysis. After the analysis has been accomplished and the results are published, however, some of the special notation not used in the analysis can be left out. 'Simplified' transcripts can make the reception of the analysis easier, especially if the audience is not specialized in conversation analysis.

In sum, reliability of observations in conversation analytic research (as in any other empirical method) can only be achieved through serious effort. The method itself does not guarantee reliability. In conversation analytic studies, proper attention needs to be paid to the selection and technical quality of recordings as well as to the adequacy of the transcripts.

Validity in conversation analytic research

The validity of research concerns the interpretation of observations: whether or not 'the researcher is calling what is measured by the right name' (Kirk and Miller, 1986: 69; cf. Altheide and Johnson, 1994; Silverman, 1993: 149-66).

In the discussions about validity, especially in the context of quantitative research, there is an underlying background assumption about a separation between the 'raw' observations and the issues that these observations stand for or represent. Responses to questionnaires, for example, can be more or less valid representations of underlying social phenomena, such as the respondents' attitudes or values (cf. Alkula et al., 1994). Conversation analysis is in stark contrast to this kind of approach: the core of its very aim is to investigate talk-in-interaction, not as 'a screen on which are projected other processes', but as a phenomenon in its own right (Schegloff, 1992a: xviii). This commitment to naturalistic description of the interaction order (Goffman, 1983) and the social action taking place within that order (cf. also Sacks, 1984) gives a distinctive shape to the issues of validation in

conversation analysis. These include the *transparence of analytic claims*, *validation through 'next turn'*, *deviant case analysis*, *questions about the institutional character of interaction*, and finally, *the generalizability of conversation analytic findings*.

The transparence of analytic claims

In *Tractatus Logico-Philosophicus*, Wittgenstein pointed out that philosophy, rightly understood, is not a set of propositions but an activity, the clarification of non-philosophical propositions about the world. The method of this activity is complex because the 'knots' in our thinking are complex, but the results of philosophy are simple (see Kenny, 1973: 18, 101–2). A similar kind of paradox between the complexity of method and the simplicity of results is characteristic of conversation analysis, too.

The results of (good) conversation analytic research exhibit, in a positive manner, what Kirk and Miller (1986: 22) called *apparent validity*: once you have read them, you are convinced that they are transparently true. A conversational activity called 'fishing' may serve as an example. Anita Pomerantz showed in a paper published in 1980 how participants in a conversation can indirectly 'fish' for information from one another by telling what they themselves know. Descriptions of events displaying their producer's 'limited access' to the relevant facts may work as a device for inviting the other party to disclose his/her authorized version of the same issues (assuming, of course, that the other party is in a position of having privileged access to the relevant facts). Such dynamics are at work in cases like the following:

- (1) B: Hello::
 A: HI:::
 B: Oh:hi:: 'ow are you Agne::s,
 → A: Fi:ne. Yer line's been busy.
 B: Yeuh my fu (hh)- .hh my father's wife called me
 ..hh So when she calls me::, .hh I can always talk
 fer a long time. Cuz she c'n afford it'n I can't.
 hhhh heh .ehhhhhh
 (Pomerantz, 1980: 195)

In Extract 1 above, the description based on a limited access to relevant facts given by A (marked with an arrow) works as what Pomerantz called 'a fishing device', successfully eliciting B's insider's report in the next turn. By telling her observations about the line having been busy, A makes it relevant for B to disclose to whom she was talking.

The description of an activity like 'fishing' tends to 'ring a bell' as soon as anyone stops to think about it. 'Fishing' is something in which everybody has participated in different roles. But until Pomerantz's article, this activity has not been described formally. The results of Pomerantz's analysis are very simple. Her argument is transparently true, or, in Kirk and Miller's (1986) terms, it has a genuine 'apparent validity'.

But just as in Wittgenstein's philosophy, 'although the *result* . . . is simple, its method cannot be if it is to arrive at that result' (Wittgenstein, 1975: 52). In conversation analysis, the complexities of the method involve other kinds of issues of validation.

Validation through 'next turn'

As Sacks et al. pointed out, research on talk-in-interaction has an inherent methodological resource that research on written texts lacks: 'Regularly . . . a turn's talk will display its speaker's understanding of a prior turn's talk, and whatever other talk it marks itself as directed to' (1974: 728). In other words, in the unfolding of the interaction, the interactants display to one another their interpretations of what is going on, especially of what was going on in the immediately preceding turn of talk (Heritage and Atkinson, 1984). From this fact arises a fundamental validation procedure that is used in all conversation analytic research:

But while understandings of other turn's talk are displayed to co-participants, they are available as well to professional analysts, who are thereby afforded a proof criterion . . . for the analysis of what a turn's talk is occupied with. (Sacks et al., 1974: 729)

At the beginning of this chapter, it was pointed out that conversation analysis differs from those forms of discourse analysis and social constructionism which emphasize the open-endedness of the meaning of all linguistic expressions. Now we can see the reason for this: even though the meaning of any expression, if considered in isolation, is extremely open-ended, any utterance that is produced in talk-in-interaction will be locally interpreted by the participants of that interaction. In the first place, their interpretation is displayed in the next actions after the utterance. Hence, any interpretations that conversation analysts may suggest can be subjected to the 'proof procedure' outlined by Sacks et al.: the next turn will show whether the interactants themselves treat the utterance in ways that are in accordance with the analyst's interpretation.

Therefore in Extract 1 shown above, the utterance produced by B in lines 5–8 provides a proof procedure for the interpretation suggested by Pomerantz concerning A's turn in line 4. (What Pomerantz suggested was that 'telling my side' [what A did in line 4] can operate as a 'fishing device', which indirectly elicits an authoritative version of the events from the interlocutor.) And as we see, Pomerantz's interpretation passes the test: in lines 5–8, B gives her first-hand account of what had happened.

In much everyday conversation analytic work, things are not as nice and simple as in Extract 1: the next turns may be ambiguous in relation to the action performed in the preceding turn. However, the 'proof procedure' provided by the next turn remains the primordial criterion of validity that must be used as much as possible in all conversation analytic work.

Deviant case analysis

By examining the relations between successive turns of talk, conversation analysts aim at establishing *regular patterns* of interaction (Heritage, 1995). The patterns concern relations between actions (such as the relations between 'telling my side' and 'giving an authoritative report' in the case of 'fishing' described above). After having established a pattern, the analyst's next task is to search for and examine *deviant cases*: cases where 'things go differently' – most typically, cases where an element of the suggested pattern is not associated with the other expected elements.

The deviant case analysis in CA closely resembles the technique of 'analytic induction' often used in ethnographic studies (see Hammersley and Atkinson, 1983: 201–4; Silverman, 1985: 111–15; 1993: 160–2). For the analyst, those cases that do not fit the inductively constructed pattern are deviant. Rather than putting aside these discrepant cases, the analyst is encouraged to focus particular attention on them.

In her well-known paper on 'fishing', Pomerantz (1980: 186–7) presents a deviant case in which a description of events displaying its producer's 'limited access' does *not* lead the other party to disclose her authorized version of the event:

- (2) A: . . . dju j'see me pull us?=
 1→ B: =.hhh No:. I wz trying you all day. en the line
 wz busy fer like hours
 2→ A: ohh:::;, oh:::;, .hhhhh We::ll, hh I'm g'nna
 c'm over in a little while help yer brother out
 B: Goo [:d
 A: [.hhh Cuz I know he needs some he::lp,
 ((mournfully))
 B: .hh Ye:ah. Yeh he'd mention' that tihday.=
 A: =M-hm,=
 3→ B: .hhh Uh:m, .tlk .hhh Who wih yih ta:lking to.
 (Pomerantz, 1980: 186–7)

In Extract 2 above, B reports her experience about A's line having been busy (arrow 1). In terms of the interactional pattern identified by Pomerantz, this kind of telling should make relevant a subsequent disclosure of the details of the event by the other, more knowledgeable party. In the extract above, however, this does not happen. Instead, A shifts the topic in her subsequent turn (arrow 2). Therefore, within the framework of the analysis of 'fishing', we can consider Extract 2 as a deviant case.

In a recent paper, Clayman and Maynard (1994) have outlined three different ways that deviant cases can be dealt with.

(1) Sometimes deviant cases can be shown to exhibit the interactants' orientation to the *same* considerations and normative orientations that produce the 'regular' cases. In those cases, something in the conduct of the participants discloses that they, too, treat the case as one involving a

departure from the expected course of events. If the deviant cases show this kind of property, they provide *additional support* for the analyst's initial claim that the regularities found in the first phase of the data analysis 'are methodically produced and oriented to by the participants as normative organizations of action' (Heritage, 1988: 131).

Extract 2 above is an example of this type of deviant case. After A has failed to respond to B's initial 'fishing' turn by an authorized report of the events, B asks directly to whom A had been talking (arrow 3). Through her question, she openly requests the information which the fishing device (arrow 1), according to Pomerantz's analysis, solicited indirectly. This shift to open information seeking after an unsuccessful 'fishing' attempt indirectly confirms B's initial orientation to the 'fishing' as a device which can be used in indirect solicitation of information.

(2) Clayman and Maynard (1994) point out, however, that there are also deviant cases that cannot be integrated within the analysts' construction of the participants' orientations that normally produce the regular cases. In dealing with these cases, the analyst may need to change his or her construction of the participants' orientations. A classical example is Schegloff's (1968) analysis of a single deviant case in his corpus of 500 telephone call openings. In this single case, unlike the other 499, the caller spoke first. The analysis of that single case led Schegloff to abandon his initial hypothesis (according to which there is a norm obligating the answerer to speak first) and to reconceptualize the very first moves of telephone calls in terms of the adjacency pair 'summons (telephone ringing)–answer'. In the deviant case, the answerer didn't produce the relevant second pair part, and, accordingly, the caller reissued the summons by speaking first.

(3) There are also, however, deviant cases which cannot be integrated either into the existing or into a reconceptualized hypothesis concerning the participants' orientations (Clayman and Maynard, 1994). In these cases, an explanation can be sought from the individual contingencies of the single case. Normative orientations or strategic considerations other than those that usually inform the production of the pattern may be invoked by the participants in single cases, and these other orientations or considerations may explain the deviance. My study on the delivery of diagnosis in primary health care (Peräkylä, 1995b) provides an example. The study focused on the ways in which doctors display the evidence for the diagnostic conclusions that they deliver to the patients. A pattern was found in which the doctors delivered the patients' diagnosis *without* verbal reference to the evidential basis of the diagnosis *only* in sequential positions where the evidence had just been made observably present through the physical examination or the examination of medical records. In two single cases, however, the diagnosis was delivered without that kind of presence of evidence. But observable individual contingencies that made these cases 'different' from the rest of the data were found: in these cases the diagnosis was delivered *for the second time* during the same consultation. The

evidential basis had been present in the first announcements of the diagnosis, and the reconfirmation could be made without such basis.

In sum, deviant case analysis constitutes a central resource for testing of hypotheses in conversation analytic work. Therefore, the researcher should consider the deviant cases not a nuisance, but a treasure. The meticulous analysis of those cases gives impetus, strength and rigour to the development of the analytic arguments.

Validity of claims concerning the institutional character of interaction

In both qualitative and quantitative research, a central dimension of validity involves the correspondence between a theoretical paradigm and the observations made by the researcher. 'Construct validity' is a term that is often used in this context (Carmines and Zeller, 1979: 22-6; Kirk and Miller, 1986: 22). It involves the relations between theoretical concepts and the observations that are supposed to represent those concepts. As it was pointed out above, the primary emphasis that CA places on naturalistic description de-intensifies the relevance of many ordinary concerns of construct validity. However, the recent expansion of conversation analytic research on institutional interaction (see Heritage, Chapter 11 in this volume; and Drew and Heritage, 1992) has reinvoked the need to consider the relation between observations and concepts also in conversation analytic studies.

In conversation analytic research on institutional interaction, a central question of validity is this: what grounds does the researcher have for claiming that the talk he or she is focusing on is in any way 'connected to' some institutional framework? The fact that a piece of interaction takes place in a hospital or in an office, for example, does not *per se* determine the institutional character of that particular interaction (Drew and Heritage, 1992: 18-21). Institutional roles, tasks and arrangements may or may not be present in any particular interactions; they may or may not be present at particular *moments* in particular interactions. If they are, the conversation analytic programme presupposes, their presence is observable to the participants and the analyst alike.

Two basic criteria for the validity of claims concerning the institutional character of talk have been outlined by Schegloff (1987, 1991, 1992b). The first criterion concerns the *relevancy of categorization*. There are indefinitely many aspects of context potentially available for any interaction: we may categorize one another on the basis of gender, age, social class, education, occupation, income, race, and so on, and we may understand the setting of our interaction accordingly. In the momentary unfolding of interaction, Schegloff argues, 'the parties, singly and together, select and display in their conduct which of the indefinitely many aspects of context they are making relevant, or are invoking, for the immediate moment' (1987: 219).

Awareness of this 'problem of relevance' requires the professional analyst to proceed with caution. There is a danger of 'importing' institutional context to data. The professional analyst may be tempted to assume, without going into the details of data, that this or that feature of talk is an indication of a particular context (such as 'medical authority' or 'professional dominance') having affected the interaction. Such stipulation for context may, Schegloff (1991: 24-5) argues, result in the analysis being terminated prematurely, so that the inherent organization within the talk is not thoroughly understood. Phenomena which in the beginning may appear as indications of the workings of an 'institutional context' may in a more thorough examination turn out to be primarily connected to the organization and dynamics of talk which can be even better understood without reference to the 'institutional context'.

Another key issue addressed by Schegloff (1991, 1992b) involves what he calls *procedural consequentiality of context*. He argues that it is not sufficient to say that a particular context is oriented to 'in general' by the participants in interaction, but, instead, it has to be shown how specifiable aspects of the context are consequential for specifiable aspects of the interaction. The goal is to make 'a direct "procedural" connection between the context . . . and what actually happens in the talk' (Schegloff, 1991: 17). What is said, when it is said, and how, and by whom, and to whom, may invoke the context; the goal of the conversation analytic research is to explicate exactly how the things said brought forward the context.

Schegloff's emphasis on the procedural consequentiality of the context has an important corollary. If a piece of research can pin down specific procedural links between a context and talk-in-interaction, it is likely that these observations are not only relevant in terms of analysis of detailed organization of interaction but also that they contribute to the understanding of the context *per se*. Standard social scientific understandings of professional and other contexts are often based on rough generalizations concerning the professionals' tasks, clients' roles and the relations between the two (cf. Hak, 1994: 472). Conversation analytic research goes far beyond such generalizations. Thus, for example, the studies of Heath (1992), Maynard (1991a, 1991b, 1992) and myself (Peräkylä, 1995b) on the delivery of diagnostic news have involved not only a detailed description of the specific practices found in medical consultations, but also a specification of a central aspect of that context, namely the dimensions and character of medical authority.

These two fundamental concerns of conversation analytic research on institutional interaction constitute a validity test for the claims concerning the institutional character of interaction. 'Relevancy of categorization' and 'procedural consequentiality of context' are something to be demonstrated by the researcher. In demonstrating them, the researcher will focus on particular phenomena in interaction, such as lexical choice, turn design, sequence organization and overall structural organization (Drew and Heritage, 1992: 29-45; Heritage, Chapter 11 in this volume). Where the

workings of context will be found in a single piece of research cannot be predicted in advance. This unpredictability arises from the inductive character of the conversation analytic enterprise; it causes both the fundamental difficulty and the exceptional fascination of conversation analytic research.

Generalizability of conversation analytic findings

The final dimension of validity of conversation analytic (and any other) research concerns the generalizability of the research findings (Pomerantz, 1990; cf. Alasuutari, 1995: 143–57). Due to their work-intensive character, most conversation analytic studies are necessarily based on relatively small databases. How widely can the results, derived from relatively small samples, be generalized?

This character of the problem is closely dependent on the type of conversation analytic research. In studies of ordinary conversation, the baseline assumption is that the results are or should be generalizable to the whole domain of ordinary conversations, and to a certain extent even across linguistic and cultural boundaries. Even though it may be that the most primordial conversational practices and structures – such as turn-taking or adjacency pairs – are almost universal, there are others, such as openings of telephone calls (see Houtkoop-Steenstra, 1991; Lindström, 1994; Schegloff, 1986), which show considerable variation in different cultures. This variation can only be tackled through gradual accumulation of studies on ordinary conversation in different cultures and social milieus. But let us focus now on the study of institutional interaction, where the problem is posed in different terms.

In some (advanced) studies of institutional interaction, explicit comparisons between different settings are made. Miller and Silverman (1995), for example, applied the comparative approach in describing talk about troubles in two counselling settings: a British haemophilia centre counselling patients who are HIV-positive and a family therapy centre in the US. In particular, they focused on similarities in three types of discursive practices in these settings: those concerned with trouble definitions, trouble remedies and the social contexts of the clients' troubles.

It is likely that as the databases and analyses on institutional interaction gradually accumulate, studies like Miller and Silverman's will become more common. The comparative approach directly tackles the question of generalizability by demonstrating the similarities and differences across a number of settings. For the time being, however, most of the studies on institutional interaction are more like case studies.

Case studies on institutional interaction are based on data collected from one or a few sites only. The number of the subjects involved in such studies can be relatively small. If the professionals involved in the study are theoretically oriented, they may use one theory only, and hence their style may be different from the style that could be found at sites where different

theories are applied.³ For these reasons, it is important to ask whether the results presented in such studies are in any way generalizable. Does everything that is said in case studies on institutional interaction apply exclusively to the particular site that was observed, or do the results have some wider relevance?

In terms of the traditional 'distributional' understanding of generalizability, case studies on institutional interaction cannot offer much. Studying one or a few sites only does not warrant conclusions concerning similarities in the professionals' and their clients' conduct in different settings. In that sense, case studies on institutional interaction have a very restricted generalizability.

However, the question of generalizability can also be approached from a different direction. The concept of *possibility* is a key to this. *Social practices that are possible*, that is, *possibilities of language use*, are the central objects of all conversation analytic case studies on interaction in particular institutional settings. The possibility of various practices can be considered generalizable even if the practices are not actualized in similar ways across different settings. For example, in my study on AIDS counselling in a London teaching hospital (Peräkylä, 1995a), the research objects were specific questioning practices used by the counsellors and their clients. These practices, arising from the Milan School Family Systems Theory, include 'circular questioning' (eliciting one party's description of his or her mind by first asking another party to give his or her account of it), 'live open supervision' (asking questions in such a manner that the delivery of the question is done in two stages, via an intermediary) and 'hypothetical future-oriented questioning' (questions about the patient's life in a hypothetical future situation). These very practices were to a large extent developed in the particular hospital that my data were from, and it is possible that they are not used anywhere else exactly in those specific ways that were analysed in my study (see Peräkylä and Silverman, 1991, for some observations on the wide variety of approaches in AIDS counselling in Britain). Hence my results cannot be directly generalizable to any other site where AIDS counselling is done.

However, the results of my study can be considered descriptions of questioning techniques that are possible across a wide variety of settings. More specifically, the study involves an effort to describe in detail how these questioning techniques were made possible: what kind of management of turn-taking, participation frameworks, turn design, sequence organization, and so on, was needed in order for the participants to set up scenes where 'circular questioning', 'live open supervision' and 'hypothetical future-oriented questioning' were done. The study showed *how* these practices are made possible through the very details of the participants' action.

As possibilities, the practices that I analysed are very likely to be generalizable. There is no reason to think that they could not be made possible by any competent member of (at least any Western) society. In this

sense, this study produced generalizable results. The results were not generalizable as descriptions of what other counsellors or other professionals do with their clients; but they were generalizable as descriptions of what any counsellor or other professional, with his or her clients, *can* do, given that he or she has the same array of interactional competencies as the participants of the AIDS counselling sessions have.

Conclusion

At the beginning of this chapter, I pointed out that the specific techniques of securing reliability and validity in different types of qualitative research are not the same. The aim of this chapter has been to give an overview of the imperatives faced and solutions found in conversation analytic research, especially when such research focuses on institutional interaction. At a more general level, however, the considerations of validity and reliability in conversation analysis are similar to those in any other kind of qualitative research: all serious qualitative research involves assuring the accuracy of recordings and testing the truthfulness of analytic claims.

In terms of the division of qualitative methods into three main 'branches' suggested by Silverman (1993), it seems that the specific constraints facing CA are closer to those of observational research than those of text analysis. The questions about the quality and inclusiveness of recordings, for example, arise in both, and deviant case analysis is also used in both.

Kirk and Miller (1986: 21, 42) point out that in conducting and assessing qualitative research (particularly ethnography), the primary emphasis has usually been laid on validity rather than on reliability, whereas in quantitative research the emphasis has been on the opposite. Put in simple terms, this may imply that qualitative research is well developed in terms of validity and underdeveloped in terms of reliability. I hope to have shown in this chapter that this is not the case with conversation analysis: CA can be considered a serious attempt to develop a method for the analysis of social action that is able to combine concerns of validity with those of reliability.

A serious concern about the reliability of observations was at the very core of the initial motivation of Harvey Sacks in beginning the line of research that we now call conversation analysis. The reliability of tape recordings remains an inherent strength of CA – but as I pointed out earlier in this chapter, tape recording *per se* does not suffice as a guarantee of the reliability of the observations. The researcher needs to pay attention both to the technical quality and inclusiveness of tape recordings and to the interplay of spoken language with other modalities of communication and social action.

The main procedures of validation of the researcher's analytic claims in all conversation analytic research include the analysis of the next speaker's interpretation of the preceding action, and deviant case analysis. In conversation analytic studies which focus on institutional interaction, new

dimensions of validation have also arisen. The next dimension of the claims concerning the relevance of a particular action, and the issue of generalizability of these claims.

Notes

I wish to thank David Silverman, John Heritage and John Heritage on the earlier versions of this chapter.

1. For general discussions on the method of conversation analysis (Silverman 1995; Pomerantz and Fehr (1997); Wootton (1989).
2. For temporality in organizations, see also Bode (1994).
3. On analyses of theoretically informed work, see Heritage (1991) 'Theoretically oriented brief family therapy'; Buttny (1993: 66–84), 'The organization of systems theory'; and Coupland et al. (1994), who describe a psychiatric outpatients clinic operating within the framework of institutional interaction.

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PART VII SOCIAL PROBLEMS

14 Addressing Social Problems through Qualitative Research

Michael Bloor

This chapter explores two case studies which provide illustrative details of two different but related approaches for researchers who wish to address social problems and who are also sceptical of the possibilities of extensive influence on the policy-making community. Both of the approaches aim to influence practitioners rather than policy-makers and both link particularly well with qualitative research methods. In the first case study, an ethnographic research project is viewed as an analogue or partial paradigm of successful practitioner work, in this case outreach work among male prostitutes: in effect, the ethnography may be viewed as a demonstration or pilot outreach project. In the second case study, ethnographic work provides the material and the stimulation for practitioners to evaluate and revise particular facets of their own service provision.

It was rather a shock for me to read in Carey's (1975) social history of the 'Chicago School' of sociology that in the 1920s the foremost practitioners of the foremost school of sociology were divided about how sociological knowledge should be applied: Should it be used to influence policy-makers? Or (and here lay the surprise) should sociologists intervene in social problems directly as consulting professionals, like clinicians or architects? I was vaguely aware that some hundred years earlier Auguste Comte had proposed a similar priestly cadre of sociologists to direct society along enlightened (and Enlightenment) paths. But the realization was somehow monstrous that, as late as the 1920s and contemporaneously with, say Eliot's 'The Waste Land' and a hundred dystopian diatribes, my intellectual forebears could hanker after the power to re-engineer social life and institutions to their nostrums. It was the absence of that power, rather than humility, which thwarted them: in Carey's analysis (1975: 71–94), it was the lack of the kind of institutionalized authority which medicine exercises over a lay clientele, rather than any acknowledged deficiency in knowledge or in technical competence, which determined the path along which sociology would develop. Sociologists eventually opted to set out their stalls as scientists