

METHODOLOGICAL ISSUES IN CONVERSATION ANALYSIS⁽¹⁾

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Abstract

Conversation Analysis (CA), a research tradition that grew out of ethnomethodology, has some unique methodological features. It studies the social organization of 'conversation', or 'talk-in-interaction', by a detailed inspection of tape recordings and transcriptions made from such recordings. In this paper, I will describe some of those features in the interest of exploring their grounds. In doing so, I will discuss some of the problems and dilemma's conversation analysts deal with in their daily practice, using both the literature and my own experiences as resources. I will present CA's research strategy as a solution to ethnomethodology's problem of the 'invisibility' of common sense and describe it in an idealized form as a seven step procedure. I will discuss some of the major criticisms leveled against it and touch on some current developments. Conversation Analysis is a disciplined way of studying the local organization of interactional episodes, its unique methodological practice has enabled its practitioners to produce a mass of insights into the detailed procedural foundations of everyday life. It has developed some very practical solutions to some rather thorny methodological problems. As such it is methodologically 'impure', but it works.

Interests and practices of Conversation Analysis

Most practitioners of CA tend to refrain, in their research reports, from extensive theoretical and methodological discussion. CA papers tend to be exclusively devoted to an empirically based discussion of specific analytic issues. This may contribute to the confusion of readers who are not familiar with this particular research style. They will use their habitual expectations, derived from established social-scientific practice, as a frames of reference in understanding this unusual species of scientific work. But a CA report will not generally have an a priori discussion of the literature to formulate hypotheses, hardly any details about research situations or subjects researched, no descriptions of sampling techniques or coding procedures, no testing and no statistics. Instead, the reader is confronted with a detailed discussion of transcriptions of recordings of (mostly verbal) interaction in terms of the 'devices' used by its participants.

Some of the early articles reporting CA work, such as Schegloff & Sacks (1973), did include some explanations of the purposes of CA, however. And more recently, a growing number of introductory papers and chapters has been published that present an accessible overview of CA's theoretical and/or methodological position and/or substantive findings ⁽²⁾. An important addition to this literature is an edited collection of fragments from Harvey Sacks' unpublished Lectures that deal with methodological issues in CA (Sacks, 1984 a).

The 'methodology' that is presented in these sources is, however, rather different in character from what one can read in the established methodological literature. There are hardly any prescriptions to be followed, if one wants to do 'good CA'. What one does find are summary descriptions of practices used in CA, together with some of the reasons for these practices. What is given may be called, in the terminology of Schenkein's (1978) introduction, a 'Sketch of an analytic mentality'.

The basic reasoning in CA seems to be that methodological procedures should be adequate to the materials at hand and to the problems one is dealing with, rather than them being pre-specified on a priori grounds. While the essential characteristics of the materials, i.e. records of streams of interaction, and the general purposes of study, i.e. a procedural analysis of those streams, sets broad limits to what an analyst can

responsibly do, it leaves the researcher with ample room to develop his ⁽³⁾ own best fitting heuristic and argumentative procedures.

Conversation Analysis may then be conceived as a specific analytic trajectory which may be used to reach a specific kind of systematic insight in the ways in which members of society 'do interaction'. In their introduction to a collection of research papers, Heritage & Atkinson (1984) write:

The central goal of conversation analytic research is the description and explication of the competences that ordinary speakers use and rely on in participating in intelligible, socially organized interaction. At its most basic, this objective is one of describing the procedures by which conversationalists produce their own behavior and understand and deal with the behavior of others. A basic assumption throughout is Garfinkel's (1967: 1) proposal that these activities - producing conduct and understanding and dealing with it - are accomplished as the accountable products of common sets of procedures.

(Heritage & Atkinson (1984):1

The idea is that conversations are orderly, not only for observing analysts, but in the first place for participating members (Schegloff & Sacks, 1973: 290; Sacks, 1984 a: 22). This orderliness is seen as the product of the systematic deployment of specifiable interactional methods - 'devices', 'systems', an 'apparatus' - that are used by members as solutions to specifiable organizational problems in social interaction. These methods have a double-faced characteristic: on the one hand they are quite general, while on the other they allow for a fine-tuned adaptation to local circumstances; in the terminology used by Sacks et al (1978), they are both 'context-free' and 'context-sensitive'.

CA researchers insist on the use of audio- or video recordings of episodes of 'naturally occurring', that is non-experimental, interaction as their basic data. This insistence is quite unique in the social sciences and means that some of its most favoured data-sources such as: 1) interview-data as expressions of opinions and attitudes or descriptions of scenes not witnessed by the researcher, 2) observational studies relying on fieldnotes or coding procedures, 3) idealized or invented examples based on the researcher's own native intuitions, and 4) experimental methodologies, are not used in CA. 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.

(T)he 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. The transcriptions made after these are to be seen as a convenient form to represent the recorded material in written form, but not as a real substitute (Psathas & Anderson, 1990). By making a transcription, the researcher is forced to attend to details of the interaction that would escape the ordinary listener. Once being made, transcripts provide the researcher with a quick access to a wide

range of interactional episodes, that can be inspected for comparative purposes. Furthermore, as noted, transcripts are being provided with their analysis as an essential part of CA's research reports, giving the reader a way of checking the analysis presented, that is not available with other methods.

It is to be stressed, however, that transcriptions cannot represent the recordings in their full detail. They are always and necessarily selective. The system used in CA is specifically designed to reveal the sequential features of talk. As the system has developed over the years, more and more details of the actual sequential production of talk-in-interaction has been added to the basic 'text', written in standard orthography. From its inception in the work of Harvey Sacks in the 1960s, this development has mainly been the work of Gail Jefferson, whose sensitivity and precision in the rendering of interactional details seems to be unmatched by anyone in the field (4).

Here follows an example of a fragment of a medical consultation, transcribed by her (5).

AP(1):[4]

13 Dr: Come'n sit down, (.) Missiz Sampson,=

14 Pt: =Yes

15 Dr: Ah

16 (0.3)

17 Dr: ``t`hhhh |^I |vdon't think we've |^met before |^h'v we

18 (1.0)

19 Pt: Well I've had this: u-sore throat on'n off, for weeks no:w.=

20 Dr: =|^Oo dear.

21 Pt: En I've got a cough- writs- it's- I've been you know choking

22 you know'n I'm[coughin]g- I'm getting no relief from=

23 Dr: [Mm|^hm,]

24 Pt: coughing it's just tau- choking that (.) [() back]of=

25 Dr: [``h h h h h h]

26 Pt: =my[()

27 Dr: [Do you bring any |vphlegm up when you |vcough.

28 (0.7)

29 Pt: Well- (0.2) e-yesterday I managed to be sick'n I di:d you

30 know,? but normally: i[t-

31 Dr: [But you: vomited then,

32 Pt: Ye:s u[h huh

33 Dr: [Mm.

As was noted above, the choice of details to be included in a transcript is to a large extent based on the insights developed within CA, especially those concerning the sequential organization of talk (c.f. the overlaps in various places in the above transcript). But, at the same time, many researchers in CA emphasize that transcriptions should not be made with a specific research problem or hypothesis in mind. The ideal would be to have a large corpus of very detailed transcripts that can be used to locate and analyze specific phenomena. For reporting purposes one could then use selected simplified transcripts (c.f., for instance, Maynard, 1984: 21). In this way, a practical compromise may be reached in the dilemma between precision and readability (Heritage & Atkinson, 1984: 12).

The fact that the transcription system is especially focussed on representing the sequential organization of talk is in accord with CA's basic conception of talk-in-interaction. At any moment, the future development of the interaction - what in transcripts is to the right and below what is noted - is to a large extent open and yet to be produced by the interactants themselves, in ways that are not in the hands of anyone of them. Talk-in-interaction is factually always 'closed to the left' and 'open to the right'. While an openness to interactional negotiation may be rather obvious when one considers future developments, it is, in a sense, also true for the present and the past. It can be demonstrated in many cases that, for the participants themselves, what an utterance 'means' and what it 'does' - its semantic and pragmatic import - is not fixed, once it is produced, but is liable to be defined and redefined, in short negotiated, in utterances following it. These essential properties of conversation are consequential for the study of its organization. The fact that

speakers understand an utterance by reference to its turn-within-sequence character provides a central resource for both the participants and the overhearing analyst to make sense of the talk.

Heritage & Atkinson (1984): 7

Taking a specific utterance as a point of reference, we can say that while it will be oriented to the state of the interaction as it has developed so far, and will display that orientation in one way or another, the utterance following it will do the same. So, while utterance 1 may display a specific conception of what's going on, utterance 2 will contain a similar or different conception, after which the speaker of utterance 1 may react to that in utterance 3, possibly with another version (c.f. Schegloff & Sacks, 1973: 297-8). In fact, in 'repair sequences' such a negotiation structure is often quite clear. But also in less outspoken cases, such processes of interactional negotiation and accommodation are going on continuously (c.f. the meaning of 'sick' in line 29 of the transcript above).

As was suggested in the last quote, these circumstances are an important resource for the conversation analyst, or, as I will argue later, one major 'support strategy' for CA. The analyst should always compare his readings of the meaning of an utterance with the readings demonstrated in utterances following the target one. No definite 'prove' is provided, however, for, as Heritage and Atkinson (1984: 11) do not fail to note, understandings, and negotiations about understanding, will in many cases not be easily visible on the conversational surface. Subsequent utterances are not to be seen as an unproblematic window on co-participants' minds.

Many critics of CA, however, seem to suggest that such is the basic objective of CA. To my mind, the interpretation of the meaning of utterances for participants is not an end in itself, but one possible means to an end, which is the analysis of conversational organization. The earlier noted fact that many papers in CA do not contain programmatic explanations, has as one possible effect that readers are not informed on the specific research objectives and strategies of CA. Such a lack of background knowledge may be one reason why criticisms of CA often make the impression of just missing the point of what CA is all about. For this

reason, it seems worthwhile to pursue my explications of CA's basic objectives, before dealing with some of these criticisms. The best way to do so, to my mind, is to connect CA's practice back to the position of ethnomethodology, from which it has been developed. Before I will do that, I will give one more quote, this time from a transcribed lecture of Harvey Sacks (6)

The gross aim of the work I am doing is to see how finely the details of actual, naturally occurring conversation can be subjected to analysis that will yield the technology of conversation.

The idea is to take singular sequences of conversation and tear them apart in such a way as to find rules, techniques, procedures, methods, maxims (a collection of terms that more or less relate to each other and that I use somewhat interchangeably) that can be used to generate the orderly features we find in the conversations we examine. The point is, then, to come back to the singular things we observe in a singular sequence, with some rules that handle those singular features, and also, necessarily, handle lots of other events.

Sacks (1984 b):411

Methodological strategies in ethnomethodology

Ethnomethodology proposes the study of social order as it is constituted in and through the socially organized conduct of the society's members (7). Harold Garfinkel has derived the problem of social order and the notion of membership from Talcott Parsons' theory of action. But the way in which he has tackled it is mainly derived from the phenomenological tradition, especially 'the constitutive phenomenology of the natural attitude' as conceived by Alfred Schutz. In ethnomethodology, whatever is the case for members may be studied in a procedural fashion. The central idea is that members are continuously, in every moment of their waking life, engaged in establishing what may be reasonably assumed to exist, by connecting whatever presents itself to their attention with elements of their stock of knowledge. This knowledge consists, as Schutz has argued, of typifications and recipes, such as action-types, person-types and course-of-action types (c.f. Schutz, 1962). Members demonstrate their competence by showing that and how they know what is the case by connecting 'indexical particulars', context-specific information, in a reasonable manner with generally available knowledge, 'what any competent member knows'. So by fitting 'cases' to 'types', a reasonable world is constituted (Garfinkel, 1967, especially p. 78).

Since ethnomethodology has an interest in the procedural study of common sense as it is used practically, it is faced with a peculiar methodological problem. This may be glossed as 'the problem of the invisibility of common sense'. Members have a practical rather than a theoretical interest in their constitutive work (8). They take common sense and its constitutive practices for granted, unless some sorts of 'trouble' make attention necessary. So an early strategy of Garfinkel was to 'breach' expectations in order to generate this kind of trouble (Garfinkel, 1967: 35-75). For ethnomethodology, common sense practices are the topic of study, but those practices are also, unavoidably, used as a resource for any study one may try to undertake. Without the use of common sense, its object of study would be simply unavailable, because it is constituted by the application of common sense methods, such as 'the documentary method of interpretation' (Garfinkel, 1967: 76-103). So the problem for ethnomethodology is how common sense practices and common sense knowledge can lose their status as an unexamined 'resource', in order to be a 'topic' for analysis (c.f. Zimmerman & Pollner, 1971). Formulated in this way, it is a double-faced problem: on the one hand a problem of minimizing the unexamined use of common sense, and on the other that of maximizing its examinability. This double-sided problem seems to be in principle unsolvable, one is bound to lose either the resource or the topic. So what one has to do is to find practical solutions, which are unavoidably compromises. I will presently suggest a typology of the solutions that have been tried in ethnomethodology so far.

The first strategy is especially prominent in Garfinkel's early work (1967). This strategy consists of the

close study of sense-making activities in situations where they are especially prominent. Such situations are those in which sharp discrepancies, between on the one hand existing expectations and/or competencies, and on the other practical behavioral and/or interpretive tasks, necessitate extraordinary sense-making efforts by members. Such situations may occur naturally - as in the case of a 'transsexual' studied by Garfinkel (1967: 116-85) - or they may be created on purpose - as in the 'breaching' experiments, mentioned before.

In order to escape some of the practical and ethical problems generated by such experiments, a second strategy was developed. In this the researcher studies his own sense-making work by putting himself in some kind of extra-ordinary situation. This may be a situation where routine sense-making procedures are bound to fail, or where one has to master a difficult and unknown task, or where one is instructed by a setting's members to see the world in a way that is natural for them but not for oneself. Mehan & Wood (1975) use the expression 'becoming the phenomenon, while Schwartz & Jacobs (1979) recommend strategies of becoming The Stranger or The Novice. Out of many possible examples I would like to mention David Sudnow's (1978) study of becoming a jazz piano player, and Lawrence Wieder's (1974) study of his being instructed in the use of 'the Convict Code' as a general interpretive and explanatory device in a half-way house for paroled addicts.

The third strategy is the one that most resembles traditional fieldwork. It consists of closely observing situated activities in their natural settings and discussing them with the seasoned practitioners, in order to study the competences involved in the routine performance of these activities. To further this close study, or to be able to study these activities after the fact, recording equipment may be used. Examples of this kind of study can be found in Garfinkel's (1967) work on juries and coroners, Zimmerman's (1969) study of case-workers in a welfare agency, and Lynch's (1985) research on laboratory scientists.

The fourth strategy is the one I have already described as CA's way. It involves the study of ordinary practices by first mechanically recording some of their 'products', by the use of audio or video equipment. These recordings are then transcribed in a way that limits the use of common sense procedures to hearing what is being said and noting how it has been said. The transcriptions are used to locate some 'orderly products'. It is the analyst's task, then, to formulate a 'device' which may have been used to produce that 'product' and phenomena like it (c.f. Sacks, 1984 a).

In actual practice, these strategies tend to be combined in various ways. In examples of the first three types, a tendency exists to use literal quotes from what was said by the research subjects, as in Garfinkel's (1967) reports of his 'experiments', while in more recent studies recordings and transcripts tend to be used, as in Garfinkel et al (1981) and Lynch (1985). There is a major difference, however, between the first three strategies - ethnomethodological studies in the stricter sense - and the fourth - CA. In the first set, specific circumstances are created or sought out, where sense-making activities are more prominent and consequently easier studied. In this way ethnomethodology displays a strategic preference for the extra-ordinary ⁽⁹⁾. In contrast to this, CA tends to focus on the utterly mundane, the ordinary chit-chat of everyday life. While in ethnomethodology the 'visibility problem' is - in part - solved by the creation or selection of 'strange' environments, in CA this 'estranging' task is performed by the recording machine and the transcription process.

The general idea laying behind the use of these strategies is thus to evade as far as possible the unthinking and unnoticed use of common sense that seems to be inherent in empirical research practices in sociology. The ethnomethodological critique of these practices comes down to the objection that idealized and de-contextualized 'reconstructions' of social life, made by the research subjects and/or the researcher, are studied instead of that life in its own situated particulars ⁽¹⁰⁾. So ethnographers may be said to study their own fieldnotes as an unexamined resource for their study of a community's life. Or researchers using interviews study the responses they have recorded as an unexamined resource for their study of opinions and unobserved activities. In both cases the situated 'production' of those materials is not given systematic

attention in its own right. The focus of such studies is on either individuals or collectivities. In contrast to such a 'methodological individualism' or 'collectivism', ethnomethodology and CA subscribe to a position that Karin Knorr-Cetina (1981) calls 'methodological situationalism'.

The above critique, however, can also be turned against ethnomethodology and CA themselves. Although the 'unthinking' use of common sense may be minimized, it cannot be banned, but this fact is not often acknowledged. I will now discuss two cases where ethnomethodological writers have discussed this problem quite frankly. The first of these is Don Zimmerman's 'Preface' to Wieder's (1974) study.

Zimmerman points to the general, sensible and unavoidable use of what he calls 'idealizations' in the natural and social sciences as well as in everyday life. Idealizations are selective, abstract and logically coherent constructions that are used to collect phenomena in terms of selected features judged to be relevant from a specific, for instance theoretical, point of view. Although he acknowledges the success of this procedure in the natural sciences, he sees certain drawbacks in its use in the social sciences: 'a necessary consequence is the suppression of whole classes of data'. He specifically objects to the use of such idealizations that ignores the fact that idealization is a feature of the social life studied itself.

Thus, ethnomethodologists would contend that these idealizations in the human sciences have ignored the fact that idealization occurs naturally within the domain of scientific theorizing (which is, after all, done from within the world) and takes place as well within the domain of everyday life - in the form of common-sense typifications (...). For ethnomethodology then, 'idealization' (of either scientific or common-sense form) is a phenomenon for study, not a resource (...). Though ethnomethodologists must themselves idealize their phenomena in some fashion when pursuing an analysis, their approach differs from current constructive theorizing in that their idealizations attempt to incorporate the view that, from the outset, societal members recognize and accomplish the orderly structures of their world (...) via the use of idealizations. The phenomena of interest, then, are what Schutz (1962) refers to as second-order phenomena, namely members' idealizations of their own and others' behavior (...) social reality consists of the common-sense, practical activity of everyday 'idealizations' of the social world and activities within it (...). For ethnomethodologists, idealizations (or rational constructions) of the social world must be recognized as also having the features of being 'done from within the world' and being 'part and parcel of that world', i.e., what Garfinkel (1967) calls 'reflexive features'.

Zimmerman in: Wieder (1974):22-3

So idealizations are always and unavoidably used, in ordinary life as well as in the sciences. The point is to recognize this and to take it into account in one's own idealizing practices. How this is to be done is less clear, however. My second case throws some light on this from a CA perspective.

As a solution to the vexed problem of the relation between the shared cultural knowledge (members' knowledge) that the sociologist possesses and the analytic apparatus that it is his responsibility to produce, I propose the following:

A. The sociologist inevitably trades on his members' knowledge in recognizing the activities that participants to interaction are engaged in; for example, it is by virtue of my status as a competent member that I can recurrently locate in my transcripts instances of "the same" activity. This is not to claim that members are infallible or that there is perfect agreement in recognizing any and every instances; it is only to claim that no resolution of problematic cases can be effected by resorting to procedures that are supposedly uncontaminated by members' knowledge. (Arbitrary resolutions, made for the sake of easing the problems of "coding", are of course no resolution at all for the present enterprise.)

B. The sociologist, having made his first-level decision on the basis of members' knowledge,

must then pose as problematic how utterances come off as recognizable unit activities. This requires the sociologist to explicate the resources he shares with the participants in making sense of utterances in a stretch of talk. At every step of the way, inevitably, the sociologist will continue to employ his socialized competence, while continuing to make explicit what these resources are and how he employs them. I see no alternative to these procedures, except to pay no explicit attention to one's socialized knowledge while continuing to use it as an indispensable aid. In short, sociological discoveries are ineluctably discoveries from within the society.

Turner (1971):177

What Turner suggests is that ethnomethodological research is done in two phases. In the first the researcher uses his own membership knowledge to interpret his materials, while in the second he analyzes this interpretation from a procedural perspective (11). The four strategy-types, discussed above, differ in the way in which they produce their materials. But always the study of these materials can be seen as organized in these two phases of membership interpretation and procedural analysis. In Wieder's (1974) book on a half-way house, for instance, the first part is devoted to an ethnographic study of the setting from which the concept of a Convict Code emerges, while the second deals with the ways in which this Code is used as a daily interpretive and explanatory device.

A model of CA's research practices

These ideas can be used to build an idealized model of CA's research practices.

1. As a first step, the production of the materials to be analyzed is 'delegated' to a machine which is 'ordered' to record whatever can be heard or seen by its receptors. Although recordings are in certain ways 'selective', this selection is not idealizing in nature, in the sense given by Zimmerman in the quote given above. There is, of course, a human factor in the decision to record a particular conversation involving these participants in that setting at that time. In general, Conversation Analysts are rather easygoing about these decisions and their consequences. As long as the recording sounds 'natural' it is considered to provide useful data (12). This primary data-base can be made more accessible by transcription, but it remains 'for ever' available in its original form.
2. From the recordings, transcriptions are made in a manner that is, as I described earlier, a practical compromise given various objectives, considerations and circumstances. In making a transcription one inevitably uses one's membership knowledge, but one tries to restrict this to understanding what is being said and hearing how it was said. Both one's informal understanding of what utterances 'mean' and one's analytic interests will tend to 'predispose' one to certain hearings, but one can try to check one's hearing systematically against the recording. One can also have a transcript checked by others. Ideally, the researcher should make the transcriptions he will use in research himself. When already available transcripts are used, they should be checked against the recordings. With experience and time everyone can make a reasonably acceptable transcription, which provides a useful although always incomplete representation of what was said and how it was said.
3. The episodes to be analyzed can be selected from the transcripts on the grounds of a variety of considerations. One can select a particular set of circumstances, such as consultation openings, as in the fragment quoted before. Or one can spot the presence of an interesting 'candidate phenomenon' (as 'discussions' about the meaning of lay term such as 'sick'). Or one can be intuitively intrigued by some materials. Sometimes conversationalists seem to succeed particularly well in bringing off something - Jefferson calls these 'virtuoso moments' - and these may provide good starting points. The episode will generally consist of one or more sequences, in which an interactant initiates an action and (the) other(s) react(s) to it.

4. The researcher, then, tries to make sense of the episode, using mainly his common sense, as Turner emphasized in part A of the last quote. This knowledge is in principle procedurally similar to the one used by the interactants themselves in recognizing and producing the episode under consideration. This interpretation is specifically directed at a typification of what the utterances that make up the sequence can be held to be 'doing' and how these 'doings' interconnect.

5. In a next step, the researcher tries to explicate the interpretation, previously produced on common sense grounds, as Turner indicate in part B of the quote. A reasoning is constructed that arguably leads to the typifications given, specifying its empirical grounds, i.e. the details of the episode under consideration and its analytic resources, the common sense knowledge used. In other words, in this phase of the analysis the researcher uses both the details of the interaction and his own membership knowledge as a resource for the study of the knowledge used by the participants in bringing off the sequence under consideration. In this way, these are made available for further analysis.

6. This analysis can be elaborated in various ways. As was explained above, a major resource for supporting an analysis of a particular utterance is inspecting its sequelae, subsequent utterances and sequences. Participants' analyses of the target utterances and sequences may be displayed there. Participants can, for instance, refer to an episode, implicitly or explicitly, much later in the interaction, and in so doing give a hunch as to how they have 'heard' it.

7. Another support for a particular analysis can be found in the continuation of the analytic process in which the current episode and its analysis will be compared to other instances. As became clear in the Sacks quote above, an analysis in CA is always comparative, either directly or indirectly. The idea is that the devices used to recognize and produce a particular instance are similar to those used in many others. The common sense used in step 4 stems, as Schutz has argued, from one's own previous experiences and from those of others, passed on through example and explications in language. And both in this step and the following ones previous analytic results as well as those of others will also play a part. Comparison with similar or dissimilar cases, either implicitly or explicitly, is an important resource for what is called 'single case analyses', which focusses on the explication of one particular episode. But it is the major strategy in many other CA projects, the so-called 'collection studies', in which 'collections' of instances that are relevantly similar or different are systematically compared. And there are also papers that use a design somewhere in between these types (13).

In the fragment quoted before, the single episode may be first inspected on its own, but even then particular 'hearings' will tend to be based on general expectations about what is likely or 'normal' in the circumstances of a consultation, i.e. formulating complaints, or giving a response to a particular type of utterance, such as mentioning that one hasn't met before. With these as a background, we can see that the patient's 'Well I've had this: u-sore throat on'n off, for weeks no:w.' (line 19) corresponds with the first, but not the second. We might use an observation such as this as a starting point for further analyses, taking examples from other consultations and considering the findings from earlier analyses of such occasions (c.f. Heath, 1981; Ten Have, 1980, 1987).

It should be stressed, then, that the seven step scheme represents an idealization that may not be agreed to by other Conversation Analysts. The formulation of step 4 and the separation of this and the following steps may be especially debatable. Many researchers will tend to collapse steps 4, 5 and 6. I would argue, however, that it may be a good thing to give step 4 separate attention, in the sense that one tries to explicate the local meanings of utterances and sequences independent of the analytic interests of the project one works on (14). To my mind, then, this scheme represents some of the basic intellectual moves of CA, although the order suggested here is, of course, not strictly adhered to in actual practice. Researchers will, for instance, often return to 'earlier' phases. And step 7, when taken in its explicit form, obliges the researcher to recycle steps 3 through 6 for each particular instance used in the comparison.

Starting from step 4, a local common sense interpretation of an episode, the scheme suggests that this interpretation is considered again and again in subsequent steps, explicating and possibly revising it. The ultimate goal, as I said earlier, is not to argue for the best possible interpretation, but to formulate the means used by the members in their situated interactions: 'devices', 'the apparatus' or 'the technology of conversation'.

CA's 'restricted' database.

As I have indicated above, CA tends to use a very restricted data base, i.e. recordings of naturally occurring interactions. This is often seen as a severe limitation of the validity of its findings. From a CA point of view, however, it is rather a strong point for analytic results, if they are built up solely from recorded data. Critiques on this point can take a variety of forms. Reference has been made to 'missing data' concerning participants, as the usual macro-sociological variables (SES, 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). And others wonder why sources like interviews with participants, their comments on recordings, or interpretations of taped material by panels of 'judges' are not used.

I will take up the 'institutional' issue first. To understand CA's position on this, we should go back to CA's beginning in the early work of Harvey Sacks and Emanuel Schegloff. In that early phase those scholars were working on material from institutional settings, such as calls to an emergency psychiatric facility or to the police in cases of disaster. But these studies showed that participants in such unusual institutional circumstances were using interactional devices that were quite commonplace. Such devices, then, might also, or perhaps even better, be studied in less dramatically pre-defined circumstances, such as unremarkable conversations between equals.

The choice of 'conversation' has been presented as rather arbitrary (Schegloff & Sacks, 1973: 289-90), but, with the wisdom of hindsight, it may be judged to have been a lucky one. Ordinary conversation seems to be of the utmost importance for social life, both to 'old' and 'new' members, it is the bedrock for intersubjective understanding, and also a kind of 'technological reservoir' for whatever kind of more 'formal' or 'restricted' social life (15). Seen in this light, it has been a wise decision to concentrate, at least for a certain amount of time, on the most ordinary conversations as materials for analysis. Its commonplaceness has been an asset rather than a deficiency, since there is no obvious, pre-given functional significance to prejudge what is happening in the data.

In later developments of CA, however, we see that many members of later generations have turned again to the analysis of interactions in 'institutional' settings (16). Pre-trial conferences, court hearings, news interviews, medical encounters, classroom interactions and political rallies have been among the objects for these analyses. What these researchers have done is, to say it in a few words, to use the repertoire of conversational devices, described by the first generation of CA mostly on the basis of ordinary conversation, to explore how this repertoire is restrictively used by members to constitute episodes of 'institutional' life, recognizable as such to both members and analysts (c.f. Atkinson, 1982). As Heritage has formulated it, when he summarized his discussion of these kinds of analyses:

it is within these local sequences of talk, and only there, that these institutions are ultimately and accountably talked into being. (...) the details of little, local sequences which at first seemed narrow, insignificant and contextually uninteresting, turn out to be the crucial resources by which larger institutionalized activity frameworks are evoked. Such institutional contexts are created as visible states of affairs on a turn-by-turn basis. It is ultimately through such means that 'institutions' exist as accountable organizations of social actions.

Heritage (1984: 290)

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 (17). And even then, what may be said concerning such 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 analyzable means, while at other moments something like a 'conversation' or another kind of interaction is going on (Ten Have, 1989 a). The same kind of reasoning may then apply to certain properties and identities that are, by members, considered to be stable within settings, such as being a patient or a physician, and also to those that are thought of as constants to any setting, such as age or gender (c.f. 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 (c.f. Lynch et al, 1983: 208). It should be noted that one might even object, on these grounds, to mention the institutional identities of speakers (i.e. 'Dr:' and 'Pt:') as was done in the transcription fragment quoted earlier in this paper. The existence and relevance of such identities are, strictly speaking, to be discovered in the analysis, as products of the local practices of participants.

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'm often questioned why I do not use various other sources of information, such as interviews with participants, case records or the 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 that are represented in my corpus of recordings, is as follows. There is no way to know 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 favorable light. Furthermore, the attention of CA is not directed at uncovering hidden meanings, strategic projects, and the like, but in 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 analyzed in terms of their own productive processes (c.f. Heath, 1982), but that the information which they provide should not prejudice the detailed analysis of the interactional data themselves, and should not be considered more valuable than those data on a priori grounds.

CA's own interpretive practice

Arguments like those above could, of course, be turned against CA itself, which may be considered as being just another meaning producing context and one as strange to the original setting as the others mentioned above. These arguments do, indeed, represent a serious problem for CA, the problem of how to account for its own reflexive contributions to its results. This problem cannot be solved in principle, but only in practice. One can only try to create circumstances and design procedures that seem to further the chances of producing a valuable reading of what happened. What is valuable, however, depends on the kind of analysis one is engaged in. And the practical interests of former participants might very well be to 'explain' what was done with a rationalizing motive, rather than with a detached interest in understanding

the episode as a whole. Furthermore, the interpretive frame, used by a CA researcher, is different from that of either a participant or another lay observer. Borrowing from the terminology developed by Anthony Giddens (1984), we may say that while members are assumed to be 'practically' competent in producing ordinary interaction, the transformation of this knowledge into the 'discursive' kind seems to require special knowledge, special motives, and a lot of time. All of these are not available in hearing or viewing sessions with participants or independent lay 'judges'.

In terms of Alfred Schutz (1962), the natural attitude of members is at odds with the scientific attitude taken by researchers, also and especially when the first is the object of the second, as is the case in all kinds of phenomenologically oriented research, including CA. For those kinds of reason, Harold Garfinkel and Harvey Sacks (1970: 345) have recommended an attitude of 'ethnomethodological indifference' as a 'procedural policy'.

In the CA view, as explained earlier, a general practical competence is assumed to be available to members (c.f. Garfinkel & Sacks, 1970:342). The same competence is used by CA researchers, but in a different manner and for different reasons: to explicate it as part of a scientific enterprise. There just does not seem to be any other way than the painstaking analysis of detailed records to do this job.

What has been contested by critics relatively close to CA's position, such as Aaron Cicourel (1981) and Michael Lynch (1985), however, is the availability of the practical competence used by participants to the overhearing conversation analyst who does not have an intimate knowledge of the participants and their circumstances. CA seems to assume a rather general 'conversational' competence, while these critics focus on specialized setting- or task-bound competences. As Lynch has formulated this criticism, while the existence of a general competence, involved in the production of 'conversation' seems to be granted, the application of CA results and concepts to the study of episodes of 'shop talk', divorced from a more comprehensive investigation of their original setting, would reveal only the 'conversational' aspects of that talk, not those that are constitutive of the unique character of the specific tasks and setting. To quote some of Lynch's formulations:

the wholesale application of results from studies of conversation to investigations of specific work situations provides an easy way to produce analytic findings while leaving the specific and substantive character of the work being done-in-and-through the conversation unexplicated. In elaborating upon the situated use of generic conversational 'structures' the analyst recapitulates sociology's extrinsic interest in work by using a corpus of analytic 'structures' from 'society in general' (or 'ordinary conversation' in this case) to account for specific instances of conduct. Such a practice guarantees an analytic distance from the work's detailed accomplishment as a specific feature of those settings. (...) The 'just what' of the work specifically eludes any account which subsumes an analysis of shop talk within a general conversation analytic.

Lynch (1985: 9)

So what Lynch objects to is to treat any episode of interactional talk as 'conversation', since for instance talk-as-part-of-work, i.e. 'shop talk', might well involve competences that are not discernible for researchers who do not know 'the shop', but have to rely on recordings of interaction only.

Seen from this perspective, it is interesting to note that most CA studies of 'institutional' interaction have dealt with a very specific type, namely professional-lay interactions. This sub-genre might be considered to occupy a kind of intermediate position between on the one hand 'conversation' and on the other 'shop talk'. These encounters can be studied, therefore, from either a professional perspective, taking studies of 'shop talk' in the profession concerned as a starting point, or from a lay perspective, from the point of view of 'conversation'. It is this last line that has been taken mostly by CA studies of various sorts of professional-lay interaction (c.f. Atkinson, 1982; Frankel, 1984; Heritage, 1985; the discussion in Heritage, 1984: 280-90; also, Ten Have, 1991).

An important theme in this research is the identification of what makes these interactions different from conversations. By comparing these encounters with what is known about conversations, one takes the general competences of the lay participant as a starting point for the analysis. Therefore the focus is on those aspects of the encounters in which they are most different from conversations, for instance on the party-specific or 'asymmetrical' distribution of various sorts of utterances. It could very well be that a CA-type study that would start from the professional side, taking 'shop talk' as its starting point, could produce very different kinds of results, for instance focussed on the specific profession involved, professional strategies, etc.

In other words, the amount of 'ethnographic' background knowledge the CA researcher chooses to gather and use in the analysis is related to the aspects of the interaction one wants to explore. In his investigation of 'plea bargaining', for instance, Douglas Maynard (1984) did an ethnographic study of pretrial conferences in a municipal courtroom setting, in order to ground his analysis of a corpus of tape recorded negotiations. That analysis itself, however, produced a typical CA kind of result, a sequential model that was followed in more or less extensive form in all of his cases. So here the specifics of professional work itself proved to be analyzable in a CA manner, but the researcher had to know quite a bit about that work and its setting before he could do the analysis. The result of this work, however, the sequential model, is quite formal and promises to have a much larger relevance than the single municipal court studied.

In a similar fashion, one could raise the problem of 'cross cultural' CA. When CA is grounded in the general competence of the researcher, would that make research in different cultural settings impossible? In actual practice, most CA is done by researchers studying material 'from their own culture'. The anthropologist Michael Moerman, however, has studied interactions in a radically different culture, that of Thailand. His studies have produced results that are largely compatible with CA studies of 'western' interactions. But, reflecting on his work, Moerman (1988) has argued for what he calls a 'culturally contexted conversation analysis', a CA that is deeply informed by an ethnographic study of the culture in which the interactions studied are embedded. I think his work suggests that CA can produce results that are quite 'universal' on the level of formal structures and mechanisms, but needs to be grounded in an intimate knowledge of the culture from which the interactions are produced. But whether this knowledge is gained by membership or by ethnography seems to be less important, a practical matter.

Returning to Lynch's remarks, one can say that they display a kind of ambivalence in recent ethnomethodologists' perspective on CA. On the one hand, CA's 'results' are respected, but on the other hand, there are some misgivings as regards the way in which these have been gained. Compare the following on CA's reliance on tapes and transcripts:

I cannot claim for conversational analysis that it is so situated with respect to its phenomenon that it does not exploit the analytic 'distance' constituted in its use of tape-recordings and transcripts of 'ordinary conversation'. Additionally, the relation of conversational analysis relation to 'original conversations' is such that it provides an 'overhearer's' version of any conversation when the interpretation of utterances is from the perspective of how such utterances stand for 'anybody' who might hear them rather than just those persons in the conversation (though a familiarity with the particular situation is gained and often comes to 'inform' the analysis). The rehearability of tapes is additionally a resource for a skilled development of 'hearing' by the analyst, and this 'hearing' develops in detailed ways which are not identical to any 'first hearing' of tapes, especially by naive practitioners of conversational analysis. How this is symmetric with the 'hearing' of original participants in the conversation is open to question.

And he adds:

I mention these features of the analytic distance of conversational analysis not as a way of

finding faults to be corrected, since those features are identical with the resources for finding 'news' about conversation which is of such detail and complexity as to be unavailable without the use of tapes, systematic collections of 'instances', etc. All of these analytic operations mark conversational analysis as, in many respects, a constructive analytic enterprise, though with other such enterprises there is a strong claim for the discovery of features of conversation which in some manner 'show' members' activities as 'oriented' to them.

Lynch (1985): 196: n. 17

In short, while recognizing the gains made by CA, it is seen as dangerously close to the 'constructive' practices that ethnomethodology criticizes in conventional social science. Earlier in his book Lynch had called CA a 'double edged' resource for his work. Its 'analytic distance' is, on the one hand objected to since it relies on an 'overhearer's perspective', and is in that respect not true to the phenomenological reality of the lived interactional streams it studies, but, on the other hand its results bring us 'news' about the accomplishment of conversation that does not seem to be available in other ways.

In an interview with Bennetta Jules-Rosette (1985) Harold Garfinkel has made some remarks that go in the same direction. For instance, after he has stated that there are profound compatibilities in a focus on a procedural analysis and local production, he adds:

Mais je me demande comment cette généralité peut être documentée et détaillée. Les scripts me mettent mal à l'aise. (...) Oui, les transcriptions. Je suis sceptique quant à l'utilisation d'une théorie des signes pour traiter un phénomène comme objet théorisé. (...) Une théorie des signes est utilisée pour analyser un objet théorisé qui est produit, reconnu et compris par et pour ses membres seulement et entièrement in situ, dans des détails cohérents, inévitablement pertinents, et en tant que tels. Une interprétation de la signification des signes est donnée afin de rapporter le signe-objet aux intérêts de l'interrogation. Mais est-ce bien cela que vous voulez? Ne faut-il pas plutôt rechercher la cohérence contrôlable, produite localement et de façon endogène, des détails identificateurs de l'objet?

Jules-Rosette, 1985: 38

What these remarks suggest is that the strategy taken by CA, while starting from the same problematic as other kinds of ethnomethodology, is, by its reliance on a reification of its object through the 'overhearing' of tapes and the construction of transcripts, restricted in its study of conversational streams as situated practices. Instead, a sequential/structural representation is studied from a position of 'anybody'. This suggests that some proponents of ethnomethodology take a more principled position as regards the methodological dilemma's that I have discussed before, than the one taken in CA, which might be seen as a more compromising and practical one.

Quantification and codability (18)

From a phenomenological perspective, then, CA can be seen to be dangerously close to becoming just another form of 'constructive analysis', that is a type of analysis that constitutes its own object without taking this into account. This is especially acute when we consider the possibilities of CA being developed into a quantitative enterprise, or, more generally, the possibilities of 'coding' interactional phenomena.

As has been said earlier, CA aims to analyze devices and competences at a quite general level, available to 'anybody', practices that are relatively 'context-free', although capable of a delicate 'context-sensitivity' (Sacks, et al 1978). Seen from this perspective, it is quite rational that many CA studies are not limited to an extensive discussion of one or a few fragments of talk, but take on the systematic examination of larger collections of instances. When one reads these kinds of studies, one is struck by the very frequent (!) use of various kinds of quantifying expressions, such as 'routinely', 'regularly', 'frequently', 'a substantial number', 'often', 'generally', 'recurrent', 'comparatively rare', 'commonly', 'massively recurrent' and 'absent'. In this

way, the discussion of specific instances is given a wider relevance as an exemplary treatment of something that is typical or atypical in some sense. Usually, however, the quantitative information is kept relatively vague: the primary focus is still on the quoted fragments themselves.

In contrast to this common type of CA 'quantification', we may note a number of studies which, while claiming some sort of relationship to CA, have put their focus on qualification itself, in the sense of presenting their major findings in terms of percentages and tables. Best known examples of this kind are found in a series of studies by Candace West and Don Zimmerman on gender-based differences in interactional behavior, especially interruption (starting with Zimmerman & West, 1975; c.f. West & Zimmerman, 1985). The same approach has been taken by West in her later studies of medical consultations, focussing on interruptions and the distribution of various kinds of questions among the participants (West, 1984). In a similar vein, Richard Frankel (1984) has reported research on physicians' use of 'the third turn option', after a patient has answered a question.

What is remarkable about these studies is that while they also discuss some excerpts qualitatively, they tend to base their counts of instances on specified 'objective' criteria. For example, West (1984: 55), provides an 'operational definition' of an interruption, referring to her earlier work with Zimmerman:

An interruption is an initiation of simultaneous speech which intrudes deeply into the internal structure of a current speaker's utterance; operationally, it is found more than a syllable away from a possibly complete unit-type's boundaries.

Zimmerman and West, 1975: 113-115

And in Frankel (1984: 157), we find a similar kind of definition in:

For purposes of analysis, all third turns containing one or more contrast-class terms, e.g. good-bad, right-wrong, true-false, etc. were coded as Evaluation sequences. Similarly, all third turns containing one or more neutral terms were coded as Acknowledgement sequences.

Although these definitions, especially the first one, presuppose some analytic capabilities - to be able to discern unit-type boundaries, for instance - they tend towards the kind of coding instructions one finds in conventional quantitative sociology. Such an approach implies that the analysis of some kinds of objects can be specified in such a way that it can be responsibly delegated to a 'clerk' or a 'machine'.

Similar problems of 'codability' are encountered when one uses certain kinds of computer programs to 'assist' in the analysis of conversational materials (Ten Have, 1989 b). These programs allow a limited number of characters to 'code' an item such as an utterance or an episode. This limitation forces the analyst to document his analysis of instances in a word or two or in an acronym. He should use a restricted repertoire of codes in a consistent manner. In this way the specific properties and meanings of the separate instances are unavoidably lost. My own suggestions are to build up such an enterprise 'from below', so to speak, that is by explicating one's analyses in separate accounts, linked to the codes, in order to be able to reflect later on issues of consistency, shifted meanings, etc, rather than use stipulated top-down definitions. In other words, one could develop 'user routines' to allow for reflexive examination as a strategy to counter premature interpretive closing.

What is basically at issue is whether one can ignore the sequential environment of interactional items, or rather how that environment can be implicated in any kind of coding. In CA, the expression 'sequential environment' refers in the first instance to the immediately preceding and following utterances. The meaning of anything done or absent in conversation depends ultimately on the 'slot' in which it is found or from which it is missing (c.f. Schegloff, 1968; Schegloff & Sacks, 1973). But the larger environment can also be important, for instance in medical consultations the phase of the encounter (Ten Have, 1987, 1990, 1991). In West's (1984) study of patient initiated questions, no attention was given to the phase in which

these were put forward, although in my material this seems to be decisive for their acceptability (Ten Have, 1991). Frankel (1984) has simplified this problem by restricting his material to the first few minutes of the encounter. In coding transcripts for computer assisted analysis, I have proposed to include codes for the sequential environment as part of the 'routine'. In short, whether one codes in order to investigate distributions or for purposes of quick retrieval in a data base, the coding should be sensitive to shifting meanings, especially in relation to shifting environments.

These problems can be partially illustrated by considering some aspects of the fragment quoted at the beginning of this paper. It is clearly from the opening phase of the encounter, so the contributions of the physician in the part covered by lines 19-33 can be analyzed as instances of 'doing receiving an opening complaint'. Most of them start in overlap with the patient and would be classified as 'interruptions' by West. This terminology suggests a certain aggressiveness, a tendency to force the patient to speak within the frames of relevance established by the physician. When these contributions are analyzed more deeply, however, and related to their 'targets', they seem rather supportive of the patient's telling and relatively 'late' in coming. For instance, the question in line 27 refers to the 'coughing' in 24 and invites the patient to expand her story on that point. The fact that it does 'interrupt' seems to be as much a product of the patient's hurried way of telling, as it is of the doctor's 'impatience' (c.f. also his 'announcing' inbreath in line 25). Similarly, the utterance in line 31, 'But you: vomited then,', relates to the preceding 'sick'n I di:d' and is even 'invited' by the 'you know,?', but the patient does not provide him with an open space to follow up on it. In short, operational definitions, even when they are sequentially oriented, may obscure rather than enlighten the delicate negotiations that are going on in everyday interactions.

Discussion

Reviewing the methodological issues discussed in this paper, we can conclude that CA's position is marked by a tension between what we might call 'interpretation' and 'analysis'. 'Interpretation', here, refers to the effort to formulate the relatively unique meaning an utterance, an action or an episode seems to have for participants and/or researchers, while 'analysis' is used to indicate efforts to isolate aspects, mechanisms and procedures that are relevant to a range of cases. CA may be defined, then, as the enterprise of analyzing interpretations in interaction.

CA's history illustrates this point. In the beginning the focus was mostly on textual or verbal aspects of what is done in interaction. Gradually, other aspects were added to this base, mostly in relation to the sequential organization of interaction, for instance points of overlap, audible breathing and intonational phenomena. This can be clearly seen by comparing transcripts included in earlier and later studies (c.f. Jefferson, 1985). Special problems are connected with the analysis of non-vocal phenomena in direct interaction, such as facial expressions and body movements. A frequent objection to CA is that one cannot decide the meaning of 'words' without a consideration of what is called 'non-verbal' behavior. This objection assumes that one can only say something sensible about meanings when one considered a situation in toto. CA's counter-argument seems to be that, while recognizing that meaning is depending on a moving Gestalt, one can analyze the contribution of specific details or classes of phenomena to that Gestalt separately. So one can start with the most accessible aspects of what is done in conversations, the speaking of words, and pursue the analysis of less easily isolatable ones later, when one has learned more about the whole organization through the first. Thus the start with verbal aspects is not a principled choice, but a practical one. A growing collection of CA studies of non-vocal phenomena, including prominently Goodwin (1981) and Heath (1986), shows that these can be included in the CA framework very well. But in this field also, the research has started with the most accessible aspects of the interactional stream, the most 'transcribable', such as gaze direction.

What lies behind the ways in which CA researchers deal with the tensions between 'interpretation' and 'analysis' is a definite 'work ethic'. Practitioners of CA are less given to philosophical reflection than to hard work. The ability to produce analytical results, empirically based findings about basic procedures of 'doing being human', is one of its main attractions. The solution of CA's basic problems, which stem from the way

it has developed from its basic problematic by way of a strong empirical commitment, is to be found in those same practices. And it is bound to be a practical one. It is in the hope of elucidating this process that this reflective paper has been written.

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* * *

Appendix: Transcript notation

The symbols used in the transcript are explained here in a summary fashion. For a fuller version see Atkinson & Heritage (1984): IX-XVI

Overlapping utterances are marked by [and sometimes]

= connects 'latched' utterances

Intervals in and between utterances are given in seconds (0.3), a (.) is a small untimed pause.

A colon marks an extension of the sound it follows

Punctuation marks are used to indicate intonation: a period a stopping fall in tone; a comma continuing intonation; a question mark a rising inflection.

A dash marks an abrupt cutoff.

Arrows indicate marked rising (|[^]) or falling (|^v) shifts in intonation.

Underlining indicates emphasis.

A degree sign (°) marks a softer passage of talk.

Audible aspiration is indicated with hhh, inhalation by °hhh.

Empty parentheses enclosed unidentifiable speech.

* * *

NOTES

1. This is an electronic version of a paper published originally in the *Bulletin de Méthodologie Sociologique*, Nr. 27 (June): 23-51. Some references have been updated, but apart from that, the text is the one published. A first version was read in a session on 'Issues in Qualitative Data Interpretation', Research Committee 33, Logic and Methodology in Sociology, International Sociological Association, XIth World Congress of Sociology, New Delhi, August 1986.

2. These include, in order of publication, Schenkein (1978: 1-6), Bergmann (1981), Levinson (1983: 284-370), Heritage (1984: 232-92), Heritage & Atkinson (1984), Heritage (1989), and Lee (1987).

3. For convenience, whenever no particular person is indicated, I use the gender of the writer to decide the choice of personal pronouns; no exclusion of anyone is intended.

4. C.f. Jefferson, 1985 for her progressively refined rendering of laughter and its gains. For more extend discussion of transcription practices, comparing approaches from CA, Social Psychology and Linguistics, see Section 3: Transcription procedures, in Roger & Bull, 1989. See also Psathas & Anderson, 1990.

5. Transcription conventions are summarized in an appendix.

6. No discipline or scientific school is one man's invention, but the contribution of the late Harvey Sacks (he died in 1975) approaches such a position. His transcribed lectures, that have now been published in full (Sacks, 1992), are a continuing resource for CA researchers.

7. My treatment of ethnomethodology will be sketchy. For more elaborate expositions see Heritage (1984, 1987). Livingston (1987) uses exemplary discussions of topics rather than intellectual history to explicate its perspective. The basic source is Garfinkel (1967).
8. C.f. Giddens (1984) on 'practical' versus 'discursive' consciousness.
9. This seems less so for the third type. There is a tendency, though, for selecting settings in which fact-production is a major task, as in the examples quoted earlier. A similar tendency is discernable in the related work of Aaron V. Cicourel.
10. For that reason those analyses are called 'constructive' (Garfinkel & Sacks, 1970).
11. A similar model for ethnomethodological research has been developed by Ilja Maso (1964).
12. It seems that most people who know they are being recorded will get used to that idea rather quickly. Furthermore, the kind of details that interest CA are not the ones that can be consciously controlled very easily (c.f. Ten Have & Komter, 1982).
13. Examples of these various types in the work of Sacks and Schegloff are, for 'single case analyses': Sacks, 1974, 1978, Schegloff, 1987; for 'collection studies': Sacks, et al. 1978 (1974), Schegloff, 1968; and for mixed types: Schegloff, 1978.
14. Maso (1987) advocates such an analysis independent of the major research questions as a general strategy in qualitative research.
15. See Heritage (1984: 238) for a more extensive argumentation along these lines. One might also consider the fact that participation in an ordinary conversation seems to be one of the human activities that seems the most difficult to simulate on a computer (c.f. McTear, 1987).
16. After the original publication of this paper, two edited collections have been published which contain both discussions of these matters and exemplary papers, one edited by Boden & Zimmerman (1991), and another by Drew & Heritage (1992).
17. C.f. Schegloff (1991), who uses the expression 'procedural consequentiality'.
18. After this paper was published, Schegloff (1993) has produced some very interesting 'reflections' on the issue of quantification in CA.

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