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Ideas and Evidence in CA Research

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While in the first two chapters, CA was mostly discussed 'in its own terms', in this chapter I will further explicate the basic methodological features of CA in terms of some of the recent general literature on qualitative inquiry. I hope that this 'externalist' approach will be helpful to locate the specific characteristics of CA more clearly.

CA's 'image'

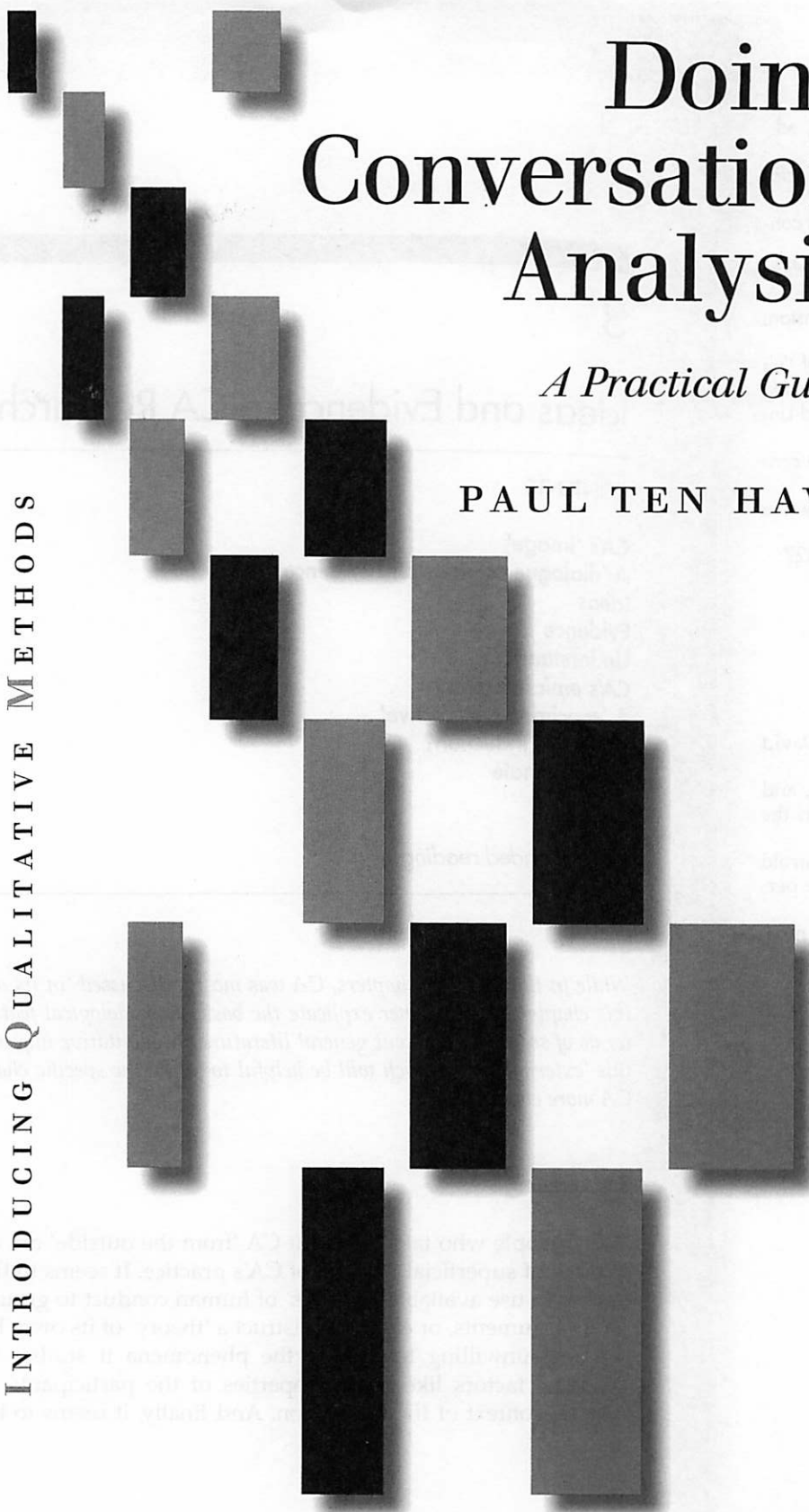
Many people who take a look at CA 'from the outside' are amazed by a number of superficial features of CA's practice. It seems to them that CA refuses to use available 'theories' of human conduct to ground or organize its arguments, or even to construct a 'theory' of its own. Furthermore, it seems unwilling to explain the phenomena it studies by invoking 'obvious' factors like basic properties of the participants or the institutional context of the interaction. And finally, it seems to be 'obsessed'

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with the details of its materials. These impressions are not too far off the mark, but the issue is why CA refuses to use or construct 'theories', why it refuses interaction-external explanations, and why it is obsessed with details. The short answer is that these refusals and this obsession are necessary in order to get a clear picture of CA's core phenomenon, the *in situ* organization of conduct, and especially talk-in-interaction. So CA is not 'a-theoretical' but it has a different conception of how to theorize about social life. For the longer answer, I will use, as announced, some core notions from current literature on 'qualitative social research'.

A 'dialogue of ideas and evidence'

I will start with some ideas taken from Charles Ragin. In his book *Constructing social research: the unity and diversity of method* (1994), he explores both what unites the various kinds of social research and the character of their marked differences. What unites social research is that it involves efforts to construct 'representations of social life' in a scientific way. He discusses various conventional ideas about the demarcation of social research from other kinds of 'telling about society', like journalism, or documentary film making, which he finds lacking in generality. Neither a special definition of its object, 'society' (or 'people doing things together' in Howard Becker's phrase), nor a special 'language' (the so-called 'variable language') or 'the scientific method' (i.e. testing hypotheses about relations between variables) seems to do the job of demarcation convincingly.

The distinctiveness of the social scientific way of telling about society is most apparent [in] representations of social life produced by social scientists for social scientists [. . .].

Briefly, social scientific audiences expect social scientific representations:

- to address phenomena that are socially significant in some way,
- to be relevant to social theory, either directly or indirectly,
- to be based on or incorporate large amounts of appropriate evidence, purposefully collected, and
- to result from some form of systematic analysis of this evidence.

[. . .]

Ultimately, it is their strong grounding in ideas and evidence that makes these representations especially relevant to social scientists. (Ragin, 1994: 23)

It is clear that CA is in the business of studying aspects of 'social life', in the sense of 'people doing things together'. That conversation analysts 'address phenomena that are socially significant in some way' may be a more contentious claim. Indeed, the study of the ordinary chit-chat of informal interaction is quite often considered to be 'irrelevant' from a societal point of view. And while many kinds of 'institutional talk' may be considered to be worth studying, the aspects chosen by CA may be not. Indeed, as a student of doctor-patient interaction, I have often heard that

people considered this to be an 'important' topic, followed by the suggestion that my research might be 'useful' to teach doctors, or more generally to ameliorate medical care. I tend to react a bit hesitantly to such common-sense suggestions, but I will return to the issue in a later chapter. When CA researchers take a stand on the issue of 'social significance' at all, either they tend to refuse the implied assignment 'be significant', or they take it in a very general fashion, as the possibility to contribute to the basic understanding of social life. Emanuel Schegloff, for instance, has included the following statements in his World Wide Web presentation:

For me, direct interaction between persons is the primordial site of sociality. I am interested in exploring what we can learn about any of social science's traditional concerns through the detailed naturalistic study of interaction. In the course of pursuing this goal through the close study of (audio and/or video) recorded episodes of all manner of naturally occurring interaction, it has turned out that we can also discover previously unrecognized concerns for social science, and ones which appear to be central to the organization of conduct in interaction and of persons' experience of it. This mode of studying interaction ends up as an instrument for studying a broad range of topics in sociology and related disciplines.¹

What is expressed here points at once in the direction of 'learning about social life' and in that of a kind of re-fashioning, or, as it has been called in recent ethnomethodology, a 're-specification' of 'social science's traditional concerns'. The latter hope or promise can be seen as a kind of fulfilment of Ragin's next 'expectation', quoted above, 'to be relevant to social theory, either directly or indirectly', although in an 'indirect' fashion. The issue of 'theory' in CA will be given an extensive treatment in a moment.

The other two 'expectations', formulated by Ragin, 'to be based on or incorporate large amounts of appropriate evidence, purposefully collected', and 'to result from some form of systematic analysis of this evidence' do not seem to pose serious 'problems' for CA. The 'large amount of evidence' requirement, in fact, is taken to be satisfied for qualitative research by 'incorporating a lot of in-depth information about a limited number of cases'. CA's 'obsession with detail', then, can be seen to represent one of the legitimate choices a scientist has in fulfilling his or her obligations to ground the representation strongly in the evidence under consideration.

In his discussion of the variety of research types, Ragin concedes that social researchers pursue widely variant goals and follow very different strategies. A major part of his book is devoted to extensive discussions of three different 'research strategies', which are characterized as follows:

- 1 *Qualitative research* on the commonalities that exist across a relatively small number of cases.
- 2 *Comparative research* on the diversity that exists across a moderate number of cases.
- 3 *Quantitative research* on the correspondence between two or more attributes across a large number of cases (covariation). (Ragin, 1994: 33, cf. 48-50)

For instance, a qualitative researcher, like an ethnographer, might do an intensive study of the social life of a relatively small group of people, studying many aspects of it using a variety of methods with an eye on its uniqueness. Survey researchers, on the other hand, study tables based on simple answers to relatively superficial questions produced in thousands of standardized interviews, in order to disentangle the complex statistical relationships between variables like gender, education, and political preference. Intermediate between the two is Ragin's third type, comparative research, which he defines in a specific way as studying 'configurations of similarities and differences across a limited range of cases', focused on patterns of causal conditions leading to specific effects (cf. Ragin, 1987).

Apart from its usefulness as an overview, I take it that the major point of this schema is that these various kinds of research should be discussed and evaluated in their own terms, that is, a survey should not be judged on grounds relevant to an ethnography and vice versa. CA can be seen as a kind of 'qualitative' inquiry, focused 'on the commonalities that exist across a relatively small number of cases'. In any case, it absolutely does not deal with 'the correspondence between two or more attributes across a large number of cases', with a focus on 'variables and relationships among variables in an effort to identify general patterns of covariation' (Ragin, 1994: 190).

Ragin's discussion of distinctions between various kinds of social research is embedded in a general conception, summarized in a useful 'Simple Model of Social Research' (cf. figure on p. 57). He starts off from the following observations:

Social research, in simplest terms, involves a dialogue between ideas and evidence. Ideas help social researchers make sense of evidence, and researchers use evidence to extend, revise, and test ideas. The end result of this dialogue is a representation of social life – evidence that has been shaped and reshaped by ideas, presented along with the thinking that guided the construction of the representation. (Ragin, 1994: 55)

Because the 'distance' between abstract and general 'ideas' and concrete and specific 'evidence' tends to be a large one, his model specifies some mediating structures, called 'analytic frames' and 'images', between the two. 'Analytic frames' are deduced from general ideas and focused on the topic of the research, while 'images' are inductively constructed from the evidence, but in terms provided by an analytic framework. The researcher's core job is to construct a 'Representation of Social Life', combining analytic frames and images in a 'double fitting' process called 'retroduction' (a combination of deduction and induction).

Ideas and evidence interact through images and analytic frames. [...] Think of analytic frames as a detailed sketch or outline of an idea about some phenomenon. Ideas are elaborated through analytic frames. Frames constitute ways of seeing the things they elaborate. [...]

Images, by contrast, are built up from evidence. [...] To construct images, researchers synthesize evidence – they connect different parts or elements of the things they study in order to create more complete portraits based on some idea of how these parts are or could be related. Initial images suggest new data collection paths. (Ragin, 1994: 58)

One can say that the various traditions in social research differ from each other in the kinds and contents of their leading ideas, in the character of the evidence used, and in the manner in which the dialogue of ideas and evidence takes form in their practices and public presentations.

Ideas

CA tends to be very sceptical of the existing repertoire of abstract and general ideas about human conduct, and especially of those about action, language use, and verbal interaction (cf. Heritage, 1995: 396–7). Established ideas in these areas are considered to be misleading, not doing justice to the complexities of human interaction, because they tend to be too individualistic and rationalistic, and in any case too simple. They do play a ‘negative’ role, however, as simplifications to be avoided.²

While in many traditions the starting point for any project is to deduce an analytic framework from the general repertoire of ideas, ideally codified in a systematic theory, such deductions are treated with suspicion in CA. This does not mean that CA does not have any ‘ideas’ or ‘analytic frames’, but rather that these are produced in a different, one could say more ‘inductive’, manner.³ As I already indicated in chapter 1, it makes sense, in this respect, to differentiate the early phase of CA, leading to a coherent research tradition, and its later, secondary, elaboration. It is clearly the work of Harvey Sacks and his co-workers, partially documented in Sacks’ *Lectures on conversation* (1992a, 1992b), to have built CA’s analytic frameworks, centred on basic concepts like turn-taking, sequencing, repair, preference, etc. Against the background of a wide-ranging reading in a number of disciplines, confronting the details of the evidence, Sacks et al. have constructed not only *images* of the interactions studied, but also the analytic *instruments* with which to make these constructions. Within a decade, a conceptual repertoire was formed that is still used today. Although Sacks and Schegloff were primary in developing this repertoire, their students, including especially Jefferson and Pomerantz, have also made important contributions, as have members of later generations.

This general scepticism in regard to traditional conceptual ideas and distinctions is one of the main differences between Erving Goffman’s work and CA, despite some obvious similarities. Goffman ultimately works to develop a broad conceptual schema that provides a neat ordering of the phenomena of interest. He does use various kinds of ‘evidence’, but in a

rather loose, illustrative manner (as in Goffman, 1981; cf. Schegloff, 1988a). His basic inspirations, however, tend to be found in the sociological tradition, especially Durkheim (cf. Burns, 1992; Manning, 1992, among others). Furthermore, Goffman's writings use a complex set of rhetorical 'tricks' to produce a curious mixture of surprise and recognition in his readers. As Rod Watson has noted:

[. . .] Goffman's work is pattern-elaborative: It involves the subsuming of a huge variety of seemingly discrete phenomena under the aegis of, initially, a single metaphor. His work thus establishes a homologous pattern in an immense range of images. This approach capitalizes on an element of surprise and on the capacities of readers as active pattern detectors, in order to achieve a fresh view of what typically are overfamiliar phenomena. (Watson, 1992: 4)

Indeed, a number of writers have commented on Goffman's literary style to the effect that he creates particular and, for sociology, rather unusual 'effects' by using 'incongruity', irony, etc. (cf. Fine & Martin, 1995; Lofland, 1980; Watson, 1992). Goffman's perspective, then, positions him as an onlooker, an outsider to the interactions he observes among ritually obsessed persons trying strategically to project a favourable image of themselves. CA, on the other hand, uses a rather 'flat' style to analyse the technology-in-use for talk-in-interaction, based on a detailed inspection, rather than an ironic gloss of talk's work. Some CA researchers, however, have used concepts derived from Goffman, such as 'involvement', 'participation framework', 'participation status', and the like, especially those who work with video-data (cf. Goffman, 1981; C. Goodwin, 1981; M.H. Goodwin, 1990; Heath, 1986, 1988, etc.), as well as some of his observations. And all recognize their debt to Goffman in having opened up the field of interaction as a proper object of sociological study.⁴

The above contrasts and considerations should not be taken to imply that CA is indeed 'a-theoretical' in its overall orientation. I would maintain that it is, rather, strongly theoretical, but it deals with 'theory' in a different way compared to conventional social science. It is clear from the development of CA, as it can be traced in Sacks' Lectures, that its originators were widely read in a large number of disciplines, including not only various branches of social science, but also parts of philosophy, linguistics, the classics, literary studies, etc. (cf. Schegloff's introduction to Sacks, 1992a, and Silverman, 1998: chap. 2). Although the aspect of CA as a critical dialogue with a wide range of theoretical approaches to human conduct is generally not stressed, it should not be denied either. In many ways, then, the major difference between CA and other efforts to understand human life is one of *theoretical style*. What CA tries to do is to explicate the inherent theories-in-use of members' practices as *lived orders*, rather than trying to order the world externally by applying a set of traditionally available concepts, or invented variations thereof.

Evidence

Various aspects of CA's approach come together in the way it deals with its evidence. The preference for recordings of natural interactions is, on the one hand, based on the experience that such evidence is extremely 'rich', 'inexhaustible' really, in the complexity of its details, which could never have been imagined (Sacks, 1992a: 419–20). On the other hand, CA's scepticism regarding established generalized ideas about human conduct leads to the rejection of evidence that is at least partly co-constituted by such ideas, as in observations, interview materials, or documents. The verbal accounts participants might produce regarding their own conduct are rejected also, at least as primary data on the interactions accounted for. Experience shows that participants may not afterwards 'know' what they have been doing or why, and furthermore tend to justify their behaviour in various ways. Such explanations may be interesting in their own right, as 'accounting practices', but are not accorded any privileged status in the analysis of the original interaction. CA tries to analyse conduct 'in its own setting', so to speak (cf. Heritage & Atkinson, 1984).

As I already indicated in chapter 1, such recordings are not used on their own by just repeated listenings or viewings. What has been recorded is 'transcribed' using a set of conventions developed by Gail Jefferson. Transcripts are unavoidably incomplete, selective renderings of the recordings, focusing at first on the text of the verbal stream, and adding various kinds of particularities of the ways in which the words were spoken later. The purpose of a CA transcription is to make *what* was said and *how* it was said available for analytic consideration, at first for the analyst who does the transcribing, and later for others, colleagues and audiences. Transcribing recordings gives the analyst a 'feel' for what has been recorded, it helps to highlight phenomena that may be later considered in detail. And, of course, transcripts are used to communicate the evidence to an audience. In so doing, such an audience acquires a kind of 'independent access' to the data being analysed. But even when a recording is being played as well, transcripts help to highlight specific phenomena and create a 'shared focus' among audience and analyst. So, while the tapes contain the primary material on which the analysis is to be based, it is elaborated, clarified, and explicated by the transcripts.

When one compares the various conventions for transcribing verbal interaction (cf. Edwards & Lampert, 1993), it is very clear that each system has its own theoretical and methodological 'bias'. For CA transcripts using the conventions developed by Gail Jefferson, the prime function is to note sequential phenomena in much more detail than is necessary for other kinds of approaches. The basic technique is to visualize on paper the timeline of the interactional stream, and to place each participant's contribution in relation to those of others. The space occupied by the letters in the printed words is taken as a visual image of the length it took to produce the corresponding sounds during the interaction. And the details

added to the textual transcript serve to make this picture more complete and exact, noting pauses, overlapping, slower, faster, latched, or stretched speech. As Jefferson (1985b) makes clear regarding one particular kind of phenomenon (laughter), the system evolved in response to the emerging analytic needs and insights.

For the vocal part, most conversation analysts follow the Jefferson conventions. Details not covered by that system, especially visual details when video recordings are used, can be added to that vocal 'base-line' according to the needs of the analyst (Heath & Luff, 1993). In this way, CA's transcription system, while recognizing Gail Jefferson's foundational contribution, has become a kind of collective property, a 'language' that has evolved from the collectivity's experience (cf. also Psathas & Anderson, 1990, and the extensive discussion in chapter 5).

Understanding

In terms of Ragin's overall model of research, one could say that transcripts function as a kind of mediation between the raw data, the recordings, and the to-be-constructed *images*. For the latter, no exact equivalent is available in CA, since the data are not combined or summarized before they are confronted with the analytic frames. What comes closest to Ragin's *images* are the 'understood' transcripts that are sometimes produced, with a column reserved for explicative *glosses* accompanying the transcript data themselves, as in the following example:

Excerpt 3.1, from Frankel, 1984: 153; earlier quoted without glosses as excerpt 1.2

[turn designed for agreement]	Pt: This- chemotherapy (0.2) it won't have any lasting effects on havin' klds will it?
[silence]	(2.2)
	Pt: It will?
[preference reversed]	Dr: I'm afraid so

The point at this moment is not whether this method of adding 'glosses' to transcripts is useful, but to stress that an analyst works on a transcript in an 'understanding' fashion, before and during his or her more strictly 'analytic' elaboration. Inevitably, a member uses his or her 'membership competencies' to understand what the interaction may be all about and how the various details may function in that respect. As Sacks et al., in their classic paper on 'turn-taking' (1978: 44-5), have stressed, the interactants' own 'displays' of their understanding of the utterances, often discernible in their next uptake, are a preferred resource for checking those

understandings. But the researcher's own comprehension, 'as a member', so to speak, is also and inevitably involved. This basic methodological insight has been very well expressed in an early paper by Roy Turner:

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)

The ways in which these understandings and explications can be woven into the analytic process will be discussed more fully in the chapters on analytic procedures. For the moment, I just remark that 'understood transcripts' are the basic *images* a CA researcher works with, and that such 'understanding' involve the use both of members' knowledge and of analytic insights and concepts.

In Ragin's model, the ultimate analysis is characterized as a process of *double fitting* of the images and the analytic frame, leading to a representation of (an aspect of) social life. In CA the analysis results in a collection of relatively abstract statements on the 'procedures' or 'devices' that participants have used in the specific cases analysed. This can be done on the basis of one case, in a *single case analysis*, or on the basis of a larger set of cases, in a *collection study*, but one can also use various kinds of combinations, such as one focal case and a series of less fully analysed supportive or contrastive cases.⁵

Ragin suggests that, especially in qualitative research, the process of *double fitting* involves changes in both the *images* and the *analytic frame*. For the analytic frameworks of CA, I have suggested above that these have developed, and continue to be developed, over the cumulative series of CA studies, rather than in just the individual studies on their own.

CA's *emic* interests

In anthropology, a distinction is often made between what are called *emic* and *etic* descriptions, concepts, or meanings. These terms were developed by Kenneth Pike:

It proves convenient – though partially arbitrary – to describe behavior from two different standpoints, which lead to results which shade into one another. The *etic* viewpoint studies behavior as from outside of a particular system, and as an essential initial approach to an alien system. The *emic* viewpoint results from studying behavior as from inside the system. (Pike, 1967: 37)

These terms were constructed by analogy with 'phonetic' and 'phonemic' from linguistics, but they have a wider relevance. *Etic* categories are in principle universal. They can be formulated prior to any particular analysis, to be applied afterwards to cases at hand. *Emic* categories, on the other hand, are focused on one culture in particular and are 'discovered' during investigation into that particular culture.

Descriptions or analyses from the *etic* standpoint are 'alien' in view, with criteria external to the system. *Emic* descriptions provide an internal view, with criteria chosen from within the system. They represent to us the view of one familiar with the system and who knows how to function within it himself. (Pike, 1967: 38)

The last clause in this quote resonates with the notion of 'membership knowledge', inevitably used in an understanding of conversational materials, as discussed above. I would say, therefore, that CA is interested in an *emic* social reality. This does not exclude the use of a 'technical vocabulary', as in sequential organization, adjacency pairs, etc. What it does mean is that such a vocabulary refers to members' knowledge-in-use, that is, members' methods or 'the procedural infrastructure of interaction'.

I should add, however, that the *emic/etic* contrast might be taken to suggest a rather particularistic version of 'culture' which tends to be disputed within CA. A concept like 'culture', if used at all, tends to be used in an 'enabling' rather than a 'limiting' fashion, that is, the competencies of 'members' are stressed, rather than any opaqueness to outsiders of 'a culture'.⁶

The actual terms *emic* and *etic* are rarely used in CA writing, but the repeated stress on taking a members' perspective can, I think, be fruitfully characterized in these terms. In a rare passage where these concepts are discussed in a CA context, Charles Goodwin (1984: 243–4) offers a critique of the way 'emic categories' are investigated in 'contemporary work in the ethnography of speaking, and disciplines such as folklore that draw from it', which depend on 'linguistic labels' used by participants or examples of a category produced at the request of the researcher. And he writes:

Pike (1967), in his seminal expansion of the etic/emic distinction beyond the scope of phonology, defined emic analysis in terms of how phenomena are utilized within specific systems of action, not with labels recognized by informants. In the present chapter [C. Goodwin, 1984: 225–46], structures that participants attend to within a strip of talk (for example, 'background' as opposed to 'climax' segments of a story) have been specified, not by questioning the participants, but rather through study in detail of the actions they perform as the talk itself emerges. In addition to revealing some of the internal organization of multi-unit turns, such an approach embeds relevant structural units within the activity systems that give them meaning, and demonstrates how participants use this structure as a constitutive feature of the events they are engaged in. (Goodwin, 1984: 243–4)

In short, as I emphasized before, CA is interested in the procedural infrastructure of situated action, rather than in the categories of 'action-as-talked-about'. Therefore, CA's interest in an *emic* reality should be understood in this sense of implicated procedures of talk-in-interaction.

A 'specimen perspective'

In his *Researching culture: qualitative method and cultural studies* (1995), Pertti Alasuutari has made a useful distinction concerning the ways in which (qualitative) researchers conceive the analytic function of the data they use. When questionnaires or interviews are used, these are mostly put into what he calls 'a factist perspective':

The central characteristic of the factist perspective is that it makes a clear-cut division between the world 'out there', on the one hand, and the claims made about it, on the other. This means that the act of putting things into words, or the interactive situation in which words are uttered and all kinds of signs are produced, are not considered as useful material because they are not facts about the world 'out there'. [. . .] The characteristics of language and of the situation are only taken into account as possible noise in the channel through which information about the world is conveyed, or as distortions in the lens through which the reality is observed [. . .].

A second characteristic of the factist perspective is that it makes sense to reflect on the truthfulness of the given information or the honesty of the informant. [. . .]

A third characteristic of the factist perspective is a pragmatic or common-sense notion of the truth and reality the researcher is after when conducting the interviews or studying other sources. One wants to find out about the actual behaviour, attitudes or real motives of the people being studied, or to detect what has happened. The factist perspective is, in fact, easy to recognize: we are dealing with it if it is possible to suspect that the informant or other source may lie, mislead or be dishonest. (Alasuutari, 1995: 47)

It may be clear, by now, that CA is *not* taking a 'factist perspective' on its research materials. It represents, rather, the alternative perspective that Alasuutari discusses: the 'specimen perspective'.

What is meant by the specimen perspective? Unlike data seen from the factist perspective, a specimen as a form of research material is not treated as either a *statement about* or a *reflection of* reality; instead, a specimen is seen as *part of* the reality being studied. Therefore, honesty is an irrelevant concept to be used in assessing the material. A specimen may be badly representative of the whole, or it may be technically bad, but it cannot lie. (Alasuutari, 1995: 63)

As stated before, CA studies are (transcripts of) recordings of episodes of naturally occurring interaction. These are, then, to be considered as *specimens* of their kind, and not, in a *factist* vein, as either *statements about* (as 'testimonies') or *reflections of* (as 'indexes') a reality 'out there'. It seems important to clearly distinguish these perspectives, both in discussions about CA and in one's own CA practice. From a CA point of view, one can indeed discuss whether an excerpt at hand is an 'instance' of a class, such as 'assessing non-present persons' or 'patients questioning physicians', but not whether the participants 'mean what they are saying', or even whether an action on display in the data is a 'reflection' or 'index' of, say, 'medical power'.

John Heritage (1988) has used a similar metaphor for CA's collection of data:

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

This image of CA research as a naturalist's hunt for interesting specimens suggests an attitude of a charming *naïveté* not only as regards data collection, but also concerning data analysis, which has been disputed from an ethnomethodological point of view (cf. Bogen, 1992; Lynch, 1993: 203-64; Lynch & Bogen, 1994). I will return to this issue in the next section.

A logic of induction?

From Ragin's overall model, and the treatment of CA as a kind of qualitative inquiry which is distrustful of *a priori* 'theorizing', one might infer that CA's basic approach is an 'inductive' one. This has indeed been suggested in some publications, but it may also be disputed. In any case a clarification is in order.

Ragin characterizes induction in the following terms:

Induction is the process of using evidence to formulate or reformulate a general idea. The process of constructing images (via the synthesis of evidence) is mostly

inductive. Generally, whenever evidence is used as a basis for generating concepts, as in qualitative research, or empirical generalizations, as in quantitative research, induction has played a part. (Ragin, 1994: 188)

John Heritage (1988), in the sequel to the quote in the previous section, gives the following overall sketch of CA's analytic treatment of its 'specimens', calling its procedures 'inductive':

Once possessed of a corpus of data, CA operates in the first instance using inductive search procedures. An analyst who is interested, for example, in how invitations are accepted or rejected will begin by building up a collection of invitations and will attempt to establish regularities in the organization of positive and negative responses to them. At the core of this task is the demonstration that these regularities are methodically produced and oriented to by the participants as normative organizations of action. (Heritage, 1988: 131)

An essential part of this search for, and specification of, 'regularities' is the so-called 'deviant case analysis'. Within qualitative methodology in general, it is often stressed that 'negative' or 'deviant' cases should be treated seriously. This is especially so in the strategy called 'analytic induction' which was originally used to arrive at 'universal' statements, for which any kind of 'negative' finding was a serious challenge (cf. McCall & Simmons, 1969: 196-215). In more recent years, the concept of 'analytic induction' is used in a less strict sense 'to refer to any systematic examination of similarities that seeks to develop concepts or ideas' (Ragin, 1994: 93).

Analytic induction is a technique used primarily by qualitative researchers to access commonalities across a number of cases and thereby clarify empirical categories and the concepts that are exemplified by the cases included in a category. It is a 'double fitting' of ideas and evidence that focuses on similarities across a limited number of cases studied in depth. (Ragin, 1994: 183)

A number of writers, including David Silverman (1985: 122; or 1993: 128) and John Heritage (1995: 399) have used the concept of 'analytic induction' to describe CA's general approach to data. Silverman uses Schegloff's (1968) study of telephone openings as a major example of this strategy as used in CA. As will be recalled from chapter 2, Schegloff first formulates a 'distribution rule for first utterances', and then finds that one case out of 500 does not fit, because it is the caller rather than the called who speaks first. He then reworks his analysis to come up with a 'deeper' one that fits with all cases at hand. So this is a clear example of 'deviant case analysis'. In Heritage's characterization, this general strategy is specified for CA in the following way:

This means taking cases where the established pattern is departed from *and showing the ways in which the participants, through their actions, orient to these departures*. (Heritage, 1988: 131; emphasis added)

And he continues:

If both dimensions of the analysis can be adequately accomplished, then the empirical task of showing that a particular normative organization is operative in interaction (that is, underlying both the production of and reasoning about a particular social action or sequence of actions) will have been achieved. Beyond this point, there is the theoretical task of specifying the role which the organization that has been discovered plays in the communicative and social matrix of interaction. (Heritage, 1988: 131)

I take it that Heritage in this quote specifies three consecutive phases of an analysis:

- 1 establishing a regular pattern of (inter)action;
- 2 describing the normative orientations of participants, as demonstrated in 'deviant cases'; and
- 3 providing a functional specification of the organization, discovered in 1 and 2, in the wider matrix of interaction.

One could suggest, I think, that it is the first phase of this process that best fits with more traditional concepts of 'induction'. Recall that in his general definition of the concept, quoted above, Ragin mentioned two functions of induction: the generation of 'empirical generalizations', especially in quantitative research, or 'concepts' in qualitative investigations. Phase 1, establishing regularities, would be similar to the first of these, while phases 2 and 3 are more interpretive, analytic, or theoretic, rather than empirical operations, although based on, inspired by, and referring to empirical materials. In other words, I am suggesting that there are two sides to a CA analysis, the first being more strictly empirical in a behavioural sense, the second oriented to what has been called the explication of the *endogenous logic* that provides for the sense of the (inter)actions, as part of a lived moral-practical order (Coulter, 1991: 38-9; Jayyusi, 1991: 242-3).

It will also be clear that the first, 'behavioural', phase is best served by a strategy of 'collection studies', while the second ultimately depends on intense 'single case analyses' of particular, 'telling' instances. The order of such phases in actual projects may vary, of course. One may, as Heritage's quote suggests, start with a collection, and later analyse the deviant cases in depth. But one may also begin with whatever one has at hand, or with what Gail Jefferson in a data session once called some 'virtuoso moments', episodes that strikes the observer as being carried out in a particularly felicitous manner, or with two cases that seem to be instructively contrastive. In short, there is not one royal way to select instances (cf. related discussions in chapters 4 and 6). In a certain respect, there is tension between the 'two sides' of CA. But I do think that both are needed to produce adequate findings. The art of CA is how to combine the two in one project, or over a series of projects.

In Ragin's overall scheme, 'induction' stands for a bottom-up move, from the evidence to the ideas, while 'deduction' refers to a top-down treatment of data in terms of pre-established ideas. I have earlier suggested that in its first phase CA's conceptual apparatus was developed in its originators' struggle with the data, while in its second phase this apparatus is generally available as an established repertoire. This means that in a general sense 'induction' has, to a certain extent, given way to 'deduction'. The danger in this situation is that less talented, insightful, or sensitive practitioners may be tempted to 'apply' the established concepts in a mechanistic fashion, as 'coding instruments'. The concept of 'preference', for instance, as developed by Sacks (1987) and elaborated by Pomerantz (1984) and others, has been codified in schemes like those of Levinson (1983: 332-45) or Heritage (1984a: 265-80) which may seem to suggest such a 'mechanistic' treatment. In other words, the temptation is to use CA's previously established concepts and findings as law-like or even 'causal' rules, whereas one should, I would maintain, see them as descriptions of possible normative orientations of participants, available for various usages as *they* see fit. Any instance of talk-in-interaction is built on routines of various sorts, but it is, at the same time, a unique achievement here and now.

CA's rationale

In order to understand CA's 'refusals and obsessions', referred to at the beginning of this chapter, one needs a solid understanding of CA's purpose in its own terms. In a recent booklet, George Psathas has provided a useful summary characterization of CA, which I quote below:

Conversation analysis studies the order/organization/orderliness of social action, particularly those social actions that are located in everyday interaction, in discursive practices, in the sayings/tellings/doings of members of society.

Its basic assumptions are:

- 1 Order is a produced orderliness.
- 2 Order is produced by the parties in situ; that is, it is situated and occasioned.
- 3 The parties orient to that order themselves; that is, this order is not an analyst's conception, not the result of the use of some preformed or preformulated theoretical conceptions concerning what action should/must/ought to be, or based on generalizing or summarizing statements about what action generally/frequently/often is.
- 4 Order is repeatable and recurrent.
- 5 The discovery, description, and analysis of that produced orderliness is the task of the analyst.
- 6 Issues of how frequently, how widely, or how often particular phenomena occur are to be set aside in the interest of discovering, describing, and analyzing the *structures*, the *machinery*, the *organized practices*, the *formal procedures*, the ways in which order is produced.

- 7 Structures of social action, once so discerned, can be described in formal, that is, structural, organizational, logical, atopically contentless, consistent, and abstract, terms. (Psathas, 1995: 2–3)

This last point formulates the ultimate purpose of CA. It tries to provide analytic descriptions of the organization of (inter)action, abstracting from the 'contents' of those (inter)actions. Therefore, CA has been characterized by Schwartz and Jacobs (1979) as a kind of 'formal sociology', as have been the sociology of Simmel, ethnomethodology, and the approach taken by Erving Goffman. One might also speak of a procedural approach to social life. Note that Psathas' points 3 and 6 express CA's refusals regarding 'theory' and 'explanation', as discussed above. Point 4, order being 'repeatable and recurrent', grounds the search for 'regularities, but points 1 and 2 stress the notion that the order is produced *in situ* again and again. In short, even in this summary statement, CA's *creative tension* is discernible. The 'obsession with detail' noted in the first section of the chapter functions to keep this tension alive, and to ward off the temptations of a repeatable coding science.

EXERCISE

Design an exploratory CA study.

Now that you have, hopefully, acquired a basic understanding of CA, think of a possible *project* in which you could try to get a hands-on acquaintance with CA's basic practices as these will be explicated in the following chapters. The project would have to be exploratory in nature, i.e. provide you with opportunities to change it when you learn more from the book, additional readings, your own explorations, and, possibly, discussions with others. A first question would be what *kind of data* you want to record and why, audio or video, informal interaction or institution-based talk, etc. A second issue would be if you already have a *preliminary interest* in some aspect of the interaction, for instance discussions, stories, interruptions, or whatever. If you have certain *expectations* regarding the kinds of phenomena to be encountered in the kind of data you are planning to record, you might want to put these on paper, not as hypotheses to be tested, but as presuppositions to be 'bracketed', so to speak. This procedure is based on the assumption that if you formulate your expectations beforehand, it is easier to set those aside as issues to be dealt with later, while you first take a look at the data 'with fresh eyes'. Finally, you should think about ways in which you would *do* the analytic explorations themselves, what kind of *analytic strategy* you think would be useful. The overall purpose of the exercise is to ask you to *think for yourself* about these issues *before* you read my detailed suggestions in the following chapters.

See appendix B for extended instructions.

Recommended reading

There are not too many longer introductions and overviews of CA, in the strict sense used here, as yet. The best I know is:

Psathas, G. (1995) *Conversation analysis: the study of talk-in-interaction*. Thousand Oaks, CA: Sage (Qualitative Research Methods 35)

A recently published one is:

Hutchby, I., & R. Wooffitt (1998) *Conversation analysis: principles, practices and applications*. Oxford: Polity Press

There is also a book centred on Harvey Sacks:

Silverman, D. (1998) *Harvey Sacks and conversation analysis*. Key Contemporary Thinkers. Cambridge: Polity Press

An interesting discussion of CA methodology in terms of 'traditional' methodological concepts is provided by:

Peräkylä, A. (1997) 'Reliability and validity in research based on tapes and transcripts'. In: D. Silverman, ed., *Qualitative research: theory, method and practice*. London: Sage: 201–20

Notes

1. From: WWW: The UCLA Department of Sociology: Faculty Members, Research Interests and Publications (http://www.sscnet.ucla.edu/soc/fac_list.htm).
2. Within CA, 'speech act theory' is often used as the chosen opponent (cf. Heritage & Atkinson, 1984: 5); in related work we find similar contrasts (cf. Button et al., 1995; Suchman, 1987).
3. The concept 'induction' is used here in a rather loose manner; I will return to its usability for CA later in this chapter.
4. These remarks do not suffice, of course, to draw a clear picture of a very complicated relationship. See especially Schegloff (1988a; in Sacks, 1992a: xxiii–xxiv), Watson (1992), and remarks in Psathas (1995: 9–11).
5. The issue of 'sampling', which might be raised in this context, is rarely dealt with in CA. This is defensible in the light of the 'exemplary' use of cases in CA, corresponding to the 'specimen perspective', discussed in the next section. I will take it up, however, in the next chapter. See Benson & Hughes (1991: esp. 126–32) for a discussion concerning the irrelevance of established methodological considerations for ethnomethodological studies.
6. A core contribution to this debate is Michael Moerman's: *Talking culture: ethnography and conversational analysis* (1988); see also: Hopper (1990: chap. 4) and, on the related issue of 'foreign language data', Wagner (1996).