One perspective on *Conversation Analysis: Comparative Perspectives*

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Preamble

The title of this book is *Conversation Analysis: Comparative Perspectives,* a title which appears to offer a well-defined promissory note about the nature of its contents.[1] And so it does. There is an introduction which sets out some central characteristics of conversation-analytic (CA) work, and briefly reviews the history of comparative analysis in anthropology – together with some of the problems confronted in the course of that history. A number of the substantive chapters that follow report work that is comparative in its very nature; most of the authors of chapters in which this is not the case go out of their way to set their respective topics in comparative context – either by including data from other language/ culture settings or by reviewing (some of) the literature which sets their work in a comparative framework. For a readership that is (I suspect) largely drawn from the so-called "social" or "human" sciences – anthropology, linguistics and applied linguistics, communication, psycholinguistics and cognitive science, social psychology, and sociology, this is what one would expect such a volume to provide … from its *sub*-title.

But its main title should make relevant as well other dimensions of comparison than the linguistic and cultural ones, and, before settling down to address what is actually in this book, I would like to use my bully pulpit to call to mind other "comparative perspectives" that ought to figure importantly in CA work, or at least be taken into account, even when they do not figure centrally. Like what?

Comparative speech-exchange systems

The final section of the turn-taking paper (Sacks *et al.* 1974: 729–731), it may be recalled, was titled "The Place of Conversation Among the Speech-Exchange Systems." It began like this:

The use of a turn-taking system to preserve one party talking at a time while speaker change recurs, for interactions in which talk is organizationally involved, is not at all unique to conversation. It is massively present for ceremonies, debates, meetings, press conferences, seminars, therapy sessions, interviews, trials etc. All these differ from conversation (and from each other) on a range of other turn-taking parameters, and in the organization by which they achieve the set of parameter values whose presence they organize.

   Such a sort of comparative investigation of the speech-exchange systems available to members of a single society, conceived of in terms of differential turn-taking systems, has barely been looked into by us. However, certain striking arrangements may be noted, if only to suggest the possible interest of this area.   (Sacks *et al.* 1974: 729)

A footnote at the end of the first of these paragraphs read: "Nor is the feature unique to a particular linguistic or social community. It is evidently exhibited in conversation, meetings, etc. in societies whose languages and systems of social organization differ quite drastically," with a reference to Albert (1964) and a cross-reference to another footnote about cultural variation which ended thus:

Finally, the cross-cultural question, as we understand it, asks how the structures on which we report vary across languages (lexically or syntactically conceived), or language communities, or across social organizations etc. – structures which are thereby cast as more basic ones. That ordering is not at all clear to us. We do find that aspects of turntaking organization may vary in terms of other aspects of the sequential organization of conversation. And, as we suggest in the final section of this paper, there are various turn-taking systems for various speech-exchange systems, e.g., conversation, debate etc.   (Sacks *et al.* 1974: 700, note 8)

The remainder of that final section would almost certainly be worded differently now than it was then, although its substance might not be all that different. But the sentence, "Such a sort of comparative investigation of the speech-exchange systems available to members of a single society, conceived of in terms of differential turn-taking systems, has barely been looked into by us," is, with a

few exceptions, only slightly off base. There has been some work
on the turn-taking organization of "traditional" classrooms (e.g.,
McHoul 1978), and of broadcast news interviews (e.g., Greatbatch
1988; Heritage and Greatbatch 1991), but little if any pursuit of
the program of comparative studies suggested in the 1974 paper
(but see Drew 2003; Schegloff 1987a: 218–228). As the amount of
recorded data has grown over the past several decades, the mater-
ial is (in principle) in hand for the systematic study of the turn-
taking organization of meetings, classrooms of different sorts,
ceremonies of different sorts, etc. More often than not, however,
those working with such data either ignore the issue of its diffe-
rence from conversation or are apologetic for the difference (with
the data then treated as if it *were* conversational). What is needed
is explicit registering of the speech-exchange system instantiated
in the data and an exploitation of the opportunity to do the sort
of comparative analysis envisioned over thirty years ago. How do
the characteristics of turns vary (if they do) in different turn-taking
organizations? How are sequence organization or the organization
of repair affected, given that they are implemented in or through
the turns which are differently organized in different turn-taking
organizations? Even more important will be the issues we cannot
even conjure up now without having described the different turn-
taking organizations and without having examined the data com-
paratively – all *without* varying the language materials and culture
that inhabit the interactions.

"Multi-modality"

I have put "multi-modality" in quotation marks because the term
is at times employed (or "deployed") as intendedly contrastive with
other approaches to the study of interaction, including CA. One
consequence is that proponents of "multi-modality" may treat the
results of CA work as having little, if any, bearing on, or utility for,
studies of interaction based on video data, at the same time that
such analyses may focus on body-behavioral components of the
data under examination without technical attention to the talk
that is ongoing at the same time.

It is, of course, true that much of the early work in CA was done on
telephone conversation and co-present interaction with audio-only

data. In part this was because we lacked the financial support and technical knowledge to exploit the then-primitive resources of video-recording; in part, telephone data appealed because the parties could not see one another, and we could thereby finesse the issue of what we were missing in not having video for co-present interaction. As soon as we could, and with the help of Chuck and Candy Goodwin, we began to rely increasingly on video data. One surprising finding is worth mentioning. We might well have found, once we started working with video data, that much of what we thought we had learned from just audio data had to be thrown out, or at least limited in its applicability to telephonic data. In fact, that did *not* happen! Although our understanding of how inter-action works was greatly enriched by the video data and continues to be greatly enriched by it, very little that we had arrived at had to be discarded. Ironically, currently ongoing work (e.g., Rossano, 2009) is more likely to find that early work on video-accessible aspects of interaction (for example, how gaze in interaction works) needs revision for having been insufficiently informed by CA findings than the opposite!

All that said, it might be productive to undertake research on interactional practices in a fashion that compared interactions with different affordances, starting with the most basic: audio only (telephones or other voice-only modes of interacting) and audio-video recording. Some of Gail Jefferson's more recent papers have documented her findings with exemplars from both telephonic and video-taped data sources, and these might serve as a model. However, her practice served to show the adequacy of her account for data from both modalities; it would be as informative to have work which showed phenomena or practices which differed systematically in the two modalities.

Once we have gained some experience with this sort of comparative study of interaction, we might then explore more radically different data sources; for example, fully synchronous data, in which each party can access the productions of the other(s) with no delay in real time (as in ordinary interaction) compared with asynchronous data, with time lags of various degrees (from long-distance video-conferencing to great time gaps between contributions of different parties).

Numbers

We know that the number of participants matters to the workings of talk-in-interaction – not least of all to its turn-taking practices, and through them to much else (though not everything – not, for example, to word selection) in interaction. When two are involved (whether persons or parties, cf. Schegloff 1995a), the next speaker is likely to be the one not speaking now; add a third, and there is then an issue of turn allocation/selection; add a fourth and the necessary ingredients for a schism into two conversations are present. Add another two or three, and, if schism does not occur, it is not unlikely that two or three will become the main protagonists with the others becoming audience; or else an agreement may be reached about an informally improvised alternative turn-taking organization – like "going around the table" with each one in turn offering a contribution. But these are all informal characterizations, collected in a career of watching interaction. Where are the studies that focus in a systematic way on what happens when an interaction adds an additional person, and then another, and then loses one, etc.? Holding the pre-present or remaining personnel constant, the conditions are there, it would appear, for compelling comparative inquiry, though there is surely much to be learned from comparing interactions with differing numbers of participants without these special features. It requires only people with training who are prepared to cast a wider comparative net that has heretofore been the case.

Age grading

The relevance of age to talk-in-interaction is most prominent at either end of the life-span trajectory – how "adults" talk to and interact with "children" and "the elderly" (including age variation within those categories);[2] how "children" talk to and interact with "adults" (including age variation within those two categories, as well as differential categorization of recipients as age increases); and how "children" talk to and interact with "children," and the finer sub-categorizations that may be relevant there (cf. the quickening pace of CA work on "very young children" in Kidwell 2005; Kidwell and Zimmerman 2006, 2007; Lerner and Zimmerman

2003; Lerner *et al.* forthcoming; Wootton 1994; Zimmerman 1999). This is at the heart of how the newborn come to be the adults whose interaction has so far monopolized our attention, and comparative analysis will surely be part of the story.[3]

So much for what this volume is *not* about. In what follows, I mean to take up two themes. The first picks up a thread in the Editor's introduction to the volume, suggesting a possible bearing of CA on a problem with past anthropological accounts of diverse cultures. The thread will lead us from that source to a position recently staked out by Nick Enfield (2007) that brings the issue from the past into the present and is relevant to how comparative CA work is best to be advanced. That discussion will in turn lead to a second: critical and programmatic "takes" on some practical dos and don'ts that, from my point of view, are critical to progress in this area that can advance the larger project in which CA is engaged.

Comparative analysis: One CA perspective

In his useful introduction to this volume, Jack Sidnell introduces CA in several ways, one of them being by contrast with structuralism, which, he writes (p. 8),

essentially undermine[s] the very possibility of comparison. Structuralism, taken to its logical conclusion, reveals the particularity of any system (e.g., of pronouns, or verbal tenses) and the elements of which it is composed. Since each element is defined by its relation to all the others, each element is a unique outcome of the particular system in which it is embedded. The result, as is well-known, are accounts which are wholly hermetic and incapable of being compared to one another.

Although Sidnell is referring here and elsewhere in his introduction to structuralism in linguistics, anthropology too went through such a stage. The anthropological counterpart to linguistic structuralism aimed to examine all elements of a culture for how they figured in the culture as a whole, as its own gestalt. One way of understanding this phase of anthropological theory and analysis is to see in it an effort to free anthropology from the colonial auspices under which it had come to maturity (or adolescence), and make it no longer answerable to the cultural commitments and socio-political and institutional arrangements of the several imperial

powers. Anthropological and linguistic fieldworkers could now address each culture in its own, autonomous terms.

The middle decades of the twentieth century saw a confrontation between what were then termed "emic" vs. "etic" approaches (cf., e.g., Harris 1968). These terms derived from the linguistic contrast between phonemic and phonetic analysis, the former being language-internal and language-specific differentiations of sound which constituted differences of meaning within a language; the latter being, by contrast, objectively differentiated and standardized systems of discriminable sound that transcended the boundaries of particular languages and allowed them to be compared. If the earlier drift toward treating cultures and their languages as organic wholes freed them from pejorative contrast with Eurocentric cultural stances, the drift back toward etic, cross-cultural and cross-language approaches provided the leverage for critique of societal features that were found distasteful, even abhorrent, by reference to putative ethical and political standards with a claim to universal standing and relevance.

What does this capsule disciplinary history have to do with the chapters of this volume? It has to do with one central feature of CA work, and that is its commitment to get at the workings of talk-and-other-conduct-in-interaction *for the participants* – its commitment to ground its claims about what is going on in some strip of interaction, its claims about the practices at work in that strip of interaction, its claims about the organizations of practice that provide at the most fundamental and constitutive level for the integrity and working of interaction (like turn-taking, sequence organization, the organization of repair, etc.) – to ground these claims *in the demonstrable orientation and understanding of the parties to the interaction as displayed in their consequent conduct*. This is in many ways an "internalist," emic stance – although (as Sidnell notes) one that does not preclude comparative work but rather invites and structures it.

In what is in many respects a precursor to this volume, the cross-cultural, conversation-analytically focused *Person Reference in Interaction* (edited by N. J. Enfield and T. Stivers, 2007), Nick Enfield takes up a rather different position, which is worthy of attention and (in my view) contestation – a view with a backdrop in the brief history I have recounted in the preceding paragraphs. So bear with me while I try to convey the position Enfield puts forward

with supporting text from Celia Kitzinger, and a counter to that position. It will, I promise, have a bearing on the ensuing discussion of what comparative studies in CA should look like … and not.

Much of the *Person Reference* volume took up one or both of two themes in the CA treatment of practices for referring to persons. One of these themes was the account in Sacks and Schegloff (1979) of the bearing of two preferences in selecting a reference term for a person – a "recipient-design" preference for choosing a recognitional reference if possible (one designed to enable its recipient to figure out who that they know is being referred to), and a preference for minimization (using a single reference form, if possible). The other theme was the account in Schegloff (1996a) of the key resources for referring to persons in English and other languages, and, in particular, the status of default or unmarked forms for "referring *simpliciter*" – that is, using a form that does, and is understood by co-participants to be doing, nothing but simply referring to the person it identifies, the contrast being to other terms which can be used to identify and refer to that person but which do something(s) else in addition to referring (see, for example, Stivers' contribution to the same volume (2007).

Unlike some others among the linguists/anthropologists represented in that volume (e.g., John Haviland 2007: 232, whose view was that there is no referring *simpliciter* among the Tzotzil with whom he has worked, only "referring *dupliciter*"), Enfield *does* find default reference forms doing referring *simpliciter* and departures from such forms which do additional work in his examination of person reference in

Lao, a Southwestern Tai language of Laos, Thailand and Cambodia […] To refer to a person in conversation, a Lao speaker has many possible alternative formulations to choose from. The complexities of this set of alternatives concern distinctions of social hierarchy, as defined by (classificatory) kinship and other factors that determine relative position of individuals in social structure.   (Enfield 2007: 98–99)

Enfield provides exemplars of both "the default option" (referring *simpliciter*) and departures from it. As for the former (Enfield 2007: 105, emphasis supplied):

In informal, familiar, village conversation in Lao, the default way to formulate initial reference to a person is to use the person's first name prefixed by the form that appropriately denotes the referent's social position

relative to the speaker […] As long as the prefix is of the appropriate level, the "prefix-plus-name" formulation is the default option, and its deployment in interaction *will pass without special notice.*

Several pages later, Enfield provides an instance of departure from this default option. Here I will omit the details of the instance (the key element of which is that the referred-to person – Daaw – lives abroad and is being looked to for a donation of funds to renovate a local temple) in favor of Enfield's summary (2007: 109, emphasis supplied):

The formulation of this reference to Daaw, the younger sister and daughter-in-law, is pragmatically marked, signalling that the speaker is doing something more with this utterance than merely establishing reference to this person. The content of the marked formulation *provides the information needed to figure out just what this special action is* (Stivers 2007). By referring to her own younger sister Daaw as "mother's younger sister," Keet both casts herself in a lower-than-normal position (i.e., as niece), and casts the referent in a higher-than-normal position (i.e., as aunt).

Enfield's account very nicely captures referring *simpliciter,* on the one hand, and doing "more than just referring" on the other. The passages to which I have called special attention with italics refer to how these different practices of referring will be understood *by the parties to the interaction.* In the first, "will pass without special notice" refers to passing without special notice by *the Laotian co-participants;* and, in the second, "provides the information needed to figure out just what this special action is" refers to providing *the interactional interlocutors* with the needed information. Here, then, there is a welcome convergence between Enfield's undertaking and CA's undertaking; so far, so good.

But not quite enough for Enfield. He wants to go beyond what parties to the interaction demonstrably are oriented to (a limitation he correctly attributes to CA and names the "Members-Only-Filter," 2007: 113–115), and include whatever an external analyst can point to as "available in the talk" as part of "what is getting done," even if there is no evidence that the participants are oriented to it – for example, the hierarchical character of the resources available for referring to persons in Lao. He writes (2007: 114):

But however we define it, availability alone isn't enough for the Members-Only Filter. Even when something is explicitly available in the talk (such

as the hierarchical differentiation of social relations encoded in Lao person-reference title prefixes), if interactants aren't demonstrably "oriented to" it – that is, displaying some kind of awareness, attention, recognition – it is said to be irrelevant to the organization of the interaction, and therefore to its description and analysis. In the case of person reference, default formulations are said to be doing nothing but referring because they are not explicitly "oriented to" by participants. In this view, it's not enough that the content in question is merely made available.

I think this is a good characterization of the CA position (at least as I understand it), as long as one thing is made clear, and that is the term "because they are not explicitly 'oriented to' by participants." No one is requiring that the participants say things like "I see that you have used a hierarchical reference term of the sort appropriate to the referent and the occasion;" the requirement is not for "explicitness" in that sense (nor does Enfield suppose otherwise, as have some others). CA's only requirement is that there be evidence in the ensuing talk that the practice in question has made a difference in the ensuing interaction. If the analyst wants to claim that something is real, it must be demonstrably, analyzably real in its consequences *in the interaction*. As long as that would satisfy Enfield's point about "explicitly 'oriented to' by participants," I can live with the attribution. Enfield does not want to live with that constraint; he wants to go further.

What is "further"? In a section titled "What Remains Unseen Depends on Where Your Blind Spot Is," Enfield writes (2007: 115):

In the Lao system of person reference, while overt specification of kinship and other hierarchical social relations are unmarked or default in pragmatic terms, they are overtly marked both formally and semantically. These markings make explicit a person's hierarchical position relative to others in the social network, an important principle in Lao speakers' cultural understanding of personhood and society. A members-only filter would reject any claim that speakers are "doing" anything in social-interactional terms by using these socially hierarchical forms, on the grounds of a lack of "orientation."

Enfield, on the other hand, proposes to include something like "doing social hierarchy" in his account of what is getting done in interaction when person A uses the default term for B while talking to C, even though (as noted above) such a reference "*will pass without special notice*." For whom then, we may ask, is this "doing

social hierarchy" being done, and who is doing it? It appears, at first glance at least, to be being done for the outside observer, relative to whose more egalitarian system of reference this reference is hierarchical. Do we not have here an echo of the past confrontation between emic and etic inquiry?

In support of his position, Enfield invokes work from within the CA community – Celia Kitzinger's paper, "Speaking As a Heterosexual: (How) Does Sexuality Matter for Talk-in-Interaction" (2005a), which he understands to be "an important challenge to this [i.e., CA] stance" (2007: 115). In this paper, Kitzinger has examined a broad range of empirical materials drawn upon in CA writing over the past thirty to forty years, and describes a number of practices of person reference which can be taken to embody and convey the heterosexuality of the person referred to, the speaker of the talk, or its recipient. As summarized in the abstract (Kitzinger 2005a: 221):

this article analyzes the conversational practices through which cointeractants, in the course of accomplishing other activities, routinely produce themselves and each other as heterosexual. These practices include heterosexual topic talk and person reference terms: husband and wife; in-law terminology; identification of the other with reference to their spouse; the production of heterosexual "couples"; and the use of locally initial proterms.

The import of these practices is underscored by contrast to the experience of gays and lesbians in interaction who may find (to cite an account of an exchange between an insurance salesman and a prospective client who happens to be a lesbian in Land and Kitzinger 2005, and recounted by Enfield, 2007: 115–116) that a reference to "self and spouse" is subsequently recast as "your husband," which in turn gets corrected by her to "It's not my husband, it's my wife." "By contrast," Enfield writes (2007: 115–116), "heterosexual speakers run little risk of their sexuality being foregrounded when they reveal it to interlocutors in exactly the same simple ways (e.g., gender of names, pronouns, words like *wife*.)" And then (Enfield 2007: 116), "There is a clear parallel between the apparent invisibility of the heterosexist assumption in English person reference [...] and the apparent invisibility of social hierarchy in Lao person-reference kin titles." Both of them, Enfield seems to be proposing, are as much "things that are getting done in interaction" (my words, not

Enfield's) as the ones conversation analysts appear to privilege, and he cites the peroration of Kitzinger's paper (2007: 116):

As analysts, we might want not to take a member's perspective on this but rather to treat the interactant's everyday world as problematic … [W]e might ask what is happening when nothing special is happening: … when presumed ordinary experiences are treated as ordinary – what is happening THEN, how is THAT done, and what kind of a world must we be living in that these things run off smoothly?

> (Elisions are in Enfield's text, not Kitzinger's; emphases
> on "then" and "that" are in Kitzinger's text.)

By now, the potential bearing of this issue on comparative inquiry in CA should be apparent, even if not yet fully explicit. In what follows, I offer several considerations that seem to me crucial in each reader's or prospective-researcher's addressing this issue.[4]

First, Kitzinger's position is not fully represented by the peroration as Enfield cites it, and its problematicity as a CA stance is acknowledged by her. Here is the whole of the paragraph cited by Enfield, with the elided portions italicized (Kitzinger 2005a: 259):

As analysts, we might want not to take a member's perspective on this but rather to treat the interactant's everyday world as problematic *and to explore how, and in the service of what other actions, their heterosexuality is assumed and deployed. We might notice that heterosexuality is available to them as a resource; we might consider what kind of world they are reflecting and reproducing in their talk. More broadly,* we might ask what is happening when nothing special is happening: *when the second is in a preferred relation to the first; when the yes–no question is followed by a yes–no answer; when the recognitional referent is recognized or the nonrecognitional referent is treated as adequate; when the punchline of the story is promptly and properly received and the second story is treated as fitted to the first;* when presumed ordinary experiences are treated as ordinary – what is happening THEN, how is THAT done, and what kind of world must we be living in that these things run off smoothly?

I would note first that the questions here framed as ones "we might ask" have in fact been asked by conversation analysts and at least partially answered, to wit:

- "when the second is in a preferred relation to the first": Pomerantz 1984; Sacks 1987; Schegloff 1988, 2007b;
- "when the yes–no question is followed by a yes–no answer": Raymond, 2000, 2003;

- "when the recognitional referent is recognized or the nonrecognitional referent is treated as adequate": Sacks and Schegloff 1979; Schegloff 1996a;
- "when the punchline of the story is promptly and properly received and the second story is treated as fitted to the first": Jefferson 1978; Sacks 1974, 1978, 1995.

I know that Kitzinger knows this literature, as do many of the readers of this book, so how are we to understand the paragraph from her paper? How is the embedded, powerful presumption of heterosexuality (and of status hierarchy in the case of Enfield's project) different from all these other domains that Kitzinger has proposed as exemplary? If they are different, what is the bearing on how inquiry is to be conducted? We will return to these issues in a moment.

Before that, however, it is worth noting that Kitzinger is herself not unaware of the departure that this part of her program of studies (though not others) represents from the otherwise common practices of CA research. Early in the same paper (2005a: 223–224), Kitzinger is projecting the contributions she means this paper to be making to CA:

In analyzing the construction of normative heterosexuality in ordinary talk, I am also contributing to CA in two ways. First, a recurrent concern of CA is with the deployment of "membership categories" (e.g., Sacks 1995: Lecture 6) and "person reference forms" (e.g., Schegloff 1996a) in ordinary conversation. Family and kinship terms are among the categories whose use Sacks began to explore – most famously in "the baby cried, the mommy picked it up" discussion (Sacks 1972b) but also at various locations scattered throughout his work, including an interrogation of the use of inferences attached to "wife," "sister," and "child" in a counselling call (Sacks 1995: 116) and the (hypothetical) deployment of (heterosexual) "couple" inferences to disguise otherwise stigmatized identities (Sacks 1995: p. 593). The analysis presented here builds on that work and adds to it an exploration of the way in which person reference forms (such as "my husband/wife" or "her mother-in-law") also make available – at

a→ least to a recipient for whom such things matter – the inference of that person's heterosexuality; an inference not usually overtly oriented to as such by speakers or recipients in the data set under consideration, but that nonetheless shows us, as analysts, the production of normative heterosexuality as an ongoing, situated, practical accomplishment.

Second, in focusing on an interactional feature not oriented to as such

b→ by the participants themselves, I adopt an unusual analytic strategy

(however, – see Sacks 1995: 175–187) and one I would advocate for those interested in uncovering the kinds of social worlds on which (as I show) the practices and actions of speakers depend, and that they reproduce in their talk. As I have suggested elsewhere (Kitzinger 2000, 2005a, 2005b; Land and Kitzinger 2005), it is precisely the fact that sexist, heterosexist, and racist assumptions are routinely incorporated into everyday conversations without anyone noticing or responding to them as such that constitutes a culture. How is it, for example, that an unquestioned set of mundane heterosexual assumptions regularly surface in talk in which participants do not notice (or orient to) their own heterosexual privilege; and precisely, how does this failure to orient constitute and reconstitute a heterosexist culture? These questions "uncover the practical reasoning through which the taken-for-granted world is accomplished (and resisted) – the resources members have for sustaining a social world in which there are 'women' and 'men,' 'heterosexuals' and 'homosexuals,' 'normal people' and the rest of us" (Kitzinger, 2000, p. 173). As conversation analysts, we can avoid "going native" – that is, rather than taking for granted the heterosexual kinship system displayed and deployed by social members, we can treat the language through which heterosexuality is displayed with the
c→ same "outsiders'" curiosity that has animated the analysis of the subcultural argot of pickpockets (Maurer 1964), drug addicts (Agar 1973), or dance musicians (Becker 1963). We can interrogate it for what it shows us about the local production of a culture.

Note, then, at Arrow A, Kitzinger's offering "an exploration of the way in which person reference forms (such as 'my husband/wife' or 'her mother-in-law') also make available – *at least to a recipient for whom such things matter* – the inference of that person's heterosexuality." Who is this "recipient for whom such things matter?" Not the co-participants, not the researcher whose commitment is to inquiry about how interaction is made to work *by its participants,* for there is no accessible evidence that this has affected the parties to the interaction (it may, of course, have done so – even perhaps profoundly, but this has not been introduced into the interaction). The "recipient for whom such things matter," it appears, is an outside investigator for whom it matters for some reason … yet to be specified.

Note next, at Arrow B, Kitzinger's observation that this undertaking is a departure from ordinary CA research practice, offering as an off-setting authorization one of Sacks' lectures, from winter 1970. I understand the key to this citation as relevant to Kitzinger's project to be as follows. Sacks has been discussing a phone call from one "Estelle" to her friend "Jeanette"; Estelle had driven past

Jeanette's workplace – a large and upscale general store – in the late afternoon and had seen policemen outside, at least one with his gun drawn, and also someone she refers to as "a colored lady," whom she suspects as having been implicated in "the trouble." She has called Jeannette either to find out what that was all about, or, suspecting that Jeannette may have had the day off, to report the incident and prompt Jeannette to find out the details. Near the end of his discussion, Sacks proposes the following (1995: II, 184–185):

When Estelle interprets the events, she interprets them so as to find how, that the cops were there involves that they were legitimately there. And we can notice that at least nowadays [recall that this was winter 1970] that's become kind of a distributional phenomenon, i.e., whereas Estelle is able to use the presence of the cops to find what was going on – where the cops belonged there, others might see the same scene with the same parties by reference to that the cops were doing something which they had no business doing. That is, if this took place in a black neighborhood, watched by black people, the "very same scene" would perhaps turn into, for the perception of the parties, an altogether different phenomenon. There are places where the cops can count on the presence of two of their cars to provide for their visible, legitimate presence, such that others will then search the scene to find what the cops might be doing that they should be doing, and, e.g., pick up on that someone is "trying to get into the entrance where the silver is" [as Estelle had conjectured to Jeannette] or that they can imagine a killer is in there, though they of course can't see into the store. Whereas there are others who will not at all see the events in that way, but, seeing two cops on the scene, may now look to see what kind of bother the cops, by being on the scene, are producing – as compared to what kind of bother they are properly responding to.

How then is this discussion relevant to Kitzinger's project? It presents an alternative framing of a scene, not one actually entertained by the parties (or at least not introduced into the talk), that transforms the understanding that the reader (and, originally, the students in the class) may bring to the actually produced talk.[5] Is Kitzinger's reframing of the invisibly heterosexist talk as blatantly heterosexist not a similar reframing? If Sacks can do it …

Finally, at Arrow C, Kitzinger writes "we can treat the language through which heterosexuality is displayed with the same 'outsiders' curiosity that has animated the analysis of the subcultural argot of pickpockets" – "Outsiders!!" That's the key. That explicates the reference at Arrow A to "at least to a recipient for

whom such things matter" – an outsider. That was the basis of Sacks' reframing and reimagining the scene from the perspective of people of color – outsiders. And that's the bridge connecting Enfield's argument to Kitzinger. As he writes in a footnote (2007: 116), "One has to be a member of another culture to 'see' the social hierarchy so ubiquitous in Lao person reference [...] I'm not aware of a *stigmatized sub-culture* among Lao speakers for which such social hierarchical assumptions are problematic" (italics added) – outsiders. And that is the position occupied by virtually all anthropologists relative to the people and culture that they study – a position from which is made visible aspects of what is going on that may not be visible to the insiders, but who are "recipient[s] for whom such things matter," and who have the technical and academic authority to make that "mattering" stick. (By contrast, with but a few exceptions, CA work on languages/cultures other than [American] English has been done by native speakers of those languages – in this volume, Bolden, Egbert, Golato, Hakulinen, Hayashi, Heinemann, Lindström, Sorjonen, and Wu. What is both striking and [to me] mysterious is how these colleagues were able to absorb and be trained in CA on a language other than their own, distill the analytic apparatus from the language in which it was presented to them, and then bring it to bear with such effectiveness on interaction in their mother tongue.)

So, what is the problem? The problem is that, once one goes outside the world of the participants in interaction, and outside what is demonstrably relevant to their conduct, as a constraint on the terms of inquiry and disciplined accounts, we find ourselves again in a world of competing perspectives, of differing capacities in which "such things matter" underwritten by this social theory or that, this category-membership or that, this political position or that, etc.[6] I have taken this matter up at length elsewhere, together with several rounds of exchange with articulate proponents of the more traditional view (Schegloff 1997a, 1998b, 1999a, 1999b; Wetherell 1998; Billig 1999a, 1999b). Perhaps someone has found another solution to this conundrum to replace the stance that has figured centrally and pivotally in CA work: using the terms of the interaction (what the parties are demonstrably – though not necessarily explicitly – oriented to) to constrain disciplined analysis of it; or perhaps such an alternative is just around the corner.

There are, obviously, other worthwhile research programs with other analytic goals, in which this problem does not arise, or in which it takes a different form – in which, indeed, it does not present itself as a problem but as a goal. But for CA comparative analysis, where juxtaposition with other ways of talking, understanding, acting, and interacting are precisely the issue, it seems to me especially important *not* to allow the terms of one culture, language or set of preoccupations to set the terms for framing another; this is the *emic* side of the coin. For the *etic* side of the coin, we look to achieved CA findings about apparently omnipresent organizational issues and contingencies of interaction, and the practices of conduct and organizations of such practices (turn construction and turn-taking, sequence organization, practices of repair, of formulating and referring to persons, places, etc., of action-formation, etc.) which can be formulated in more abstract ways that transcend different particularized embodiments in different languages and cultures but which accommodate their specifications, for the promise of defensibly underwriting comparative analysis (Schegloff 2006).

CA comparative analysis: Some dos and don'ts

Introduction: Different orders of comparison

Although the title of the book announces that its central theme is the special perspective afforded CA by comparative analysis (and vice versa), "comparative analysis" turns out to mean, or refer to, quite different orientations from chapter to chapter.

In several of the chapters, the authors introduce us to what turns out to be pretty much the same practice in two languages. For example, in the chapter by Egbert, Golato, and Robinson addressing materials in German and English, this is a practice by which recipients of a prior turn initiate repair on an indexical reference to an object of some sort in the prior turn, in both languages using the same form for repair initiation as is used for a less specifically targeted repair initiation ("what" and "was denn," respectively), but (in both languages) delivering it with downward rather than upward prosody. And in the chapter by Hayashi and Yoon, working with Japanese and Korean materials, they describe the

deployment of minimal response tokens after a co-participant has responded to *them* with a minimal response token. In these studies, we have the *same practice* in *two different languages*.

In several of the chapters, the authors examine two closely related, but subtly differentiated, practices of talk-in-interaction in the same language and explore what different environments of occurrence, what different stances, or what different interactional outcomes characterize these practices. For Wu, what is involved are two quite similar forms of other-initiation of repair in Mandarin, forms which are used and are understood to implicate different kinds of trouble and can embody differently inflected negative stances by their speaker toward the speaker of the trouble-source turn.

Sorjonen and Hakulinen examine several differently implemented ways of responding to, and ostensibly agreeing with, a preceding assessment, while differentiating the second from the prior assessment with which it is agreeing by embodying differing degrees to which the responders approach what is being assessed from the same point of view as the prior assessor. And Heinemann, working with Danish materials (though comparing them with English and German), examines two practices for "treating a question as inapposite" because what was asked was taken by the recipient to be already known to the asker – practices which adumbrate different sources and positionings of the targeted question. In these studies, then, we have *two or more related practices* in *the same language*.

Several of the chapters focus on materials from a single language and address themselves to a single practice or a single "operator" whose deployment may vary but which does the same job – the targeted practice being related to specific features of the host vernacular language, and being set in comparative perspective by a literature review or invocation of the readers' familiarity with the relevant literature on English-language materials. So Lindström focuses on what she calls a "curled *ja*" in Swedish, treating its prosodic distinctiveness as a criterial feature; it is placed in turn-initial position after a sequence-initiating turn by another, and projects a dispreferred response-to-come, juxtaposing this with similarly-positioned "well" in English conversation. Sidnell explores a usage in other-initiated repair in two creole-speaking Caribbean communities, adapted to the absence of grammatical verb inversion

to form "yes"/"no" questions. The practice involves an *if*-prefaced repeat of the targeted trouble-source turn as a way of showing that the one initiating repair heard that the trouble-source turn was a "yes"/"no" question and is checking that it was heard/understood correctly. Sidnell sets the examination of this practice in the context of a set of repair practices in these Caribbean communities that is otherwise quite like that described for English-language interaction. Finally, Bolden, working with Russian conversational data, shows that the discourse marker *-ta* serves as a resource for positioning the talk in which it is deployed as disengaged from the just preceding talk it would ordinarily be understood to address, and for showing (by the word it is attached to) what pre-prior talk it is meant to locate as its source, and shows as well how this practice gets strategically deployed or not deployed. So, in these chapters, we have a *single practice* in a *single language*, explicating the distinctive affordances or problems of the language involved, the whole phenomenon set in some relationship to other languages, without, however, examining materials in the comparison languages.

I have done none of these chapters justice with these brief characterizations! Although varying in ambition and in execution, they all range from solid and worthwhile to remarkably subtle and extraordinary in insight and scope. Virtually all of them invite examination for the ways in which distinctive features of the target language(s) implicate distinctive constraints or affordances when compared with another language (most often, English), but none of them, I think, is quite what one would think of as overtly and distinctively "comparativist" (as compared to "comparative") in design and realization. The remaining two chapters, by Fox, Wouk, Hayashi, Fincke, Tao, Sorjonen, Laakso, and Hernandez on the one hand (henceforth Fox *et al.*) and Rossano, Brown, and Levinson, on the other, are clearly meant to be "comparativist" in their very conception.

Even a cursory examination of the chapters by Fox *et al.* and Rossano *et al.* respectively reveals a striking contrast with the other chapters in this volume, namely, the decisive role of quantitative and statistical representation and treatment of data. Indeed, in the Fox *et al.* chapter, it is virtually the only form of data presented to the reader. It is to these two contributions to this volume that the rest of this chapter is given over.

Various research trajectories.

How does a piece of CA research get done? Although there are many tales, each with its own particulars, most are variants of a few underlying prototypes.[7]

1. The conversation-analyst notices a bit of conduct – a manner or practice of speaking, or a bit of bodily behavior (a shift of posture, a form of gesture, a redirection of gaze) – and asks "What is that doing here?," "What is that being used to do in this context?," and "What is the evidence for some one or more candidate solutions to these questions?" Someone has started with what might be a building block and has asked what it was being used to build and how to ground candidate solutions in evidence.

2. The conversation-analyst notices what seems transparently to be a recognizable action A – complaining, hedging, joking, disagreeing, etc., – and other participants respond in ways that display that they have indeed understood it as A; and the analyst asks what it is or was about that talk, what practice of talking and other conduct has issued in the constitution of a recognizable Action A and its recognition of it as A by co-interactants? Here someone has started with a data-internally attested interactional product and asks what were the building blocks and practices of conduct that provided for that interactional outcome?

    In both of these scenarios, the analyst starts early on checking hunches, guesses, attractive possible solutions, by assembling a collection of instances that readily initially present themselves as additional "cases" or "exemplars" of the object the analyst is tracking, and these serve to support or redirect the analysis, or subvert the initial supposition that there was something orderly here. In both of these scenarios, the paths are supposed by the analyst to be leading to new findings – things that had previously not been "a thing" at all.

3. But another way of starting takes as its point of departure some proposal by another colleague who has been pursuing a line of inquiry of the sorts described in the preceding paragraphs; it seeks to explore that proposal in a different "environment" – a different sequential locus, a different speech-exchange system, or a different language or culture. In the present volume,

we have Federico Rossano pursuing a line of inquiry on gaze direction in interaction most associated with work by Adam Kendon (1967, 1973, 1978, 1990), Charles Goodwin (1979, 1980, 1981), and Marjorie Goodwin (1980), collecting data in a different linguistic/cultural setting in Italy and finding results quite different from theirs. These findings are presented in fuller measure in his dissertation, but in the chapter in this volume with co-authors Penelope Brown and Stephen Levinson, the authors focus on one specific aspect of gaze – speaker and recipient gazing or not at one another while a question sequence is being initiated – in three very different linguistic and cultural environments, and ask how variable (if variable at all) this practice is in these decisively contrasting environments, and how are we to understand this.

And, increasingly, there is the possibility of using what is systematically known but not yet fully explicated to target and pursue and further specify a phenomenon. So, for example, one could start with a well-theorized and empirically grounded systematics for turn-taking for conversation, and then, from what is known, pursue a further explication of, e.g., types of turn-constructional units (TCUs) (in the same language or in different languages);[8] or take the systematics of the organization of repair as the basis for further elaboration and explication of its components, e.g., on varieties of other-initiated repair sequences. Here one starts not from one or more discrete observations about a recognizable action getting done or a describable practice being deployed, but, rather, from an already sketched domain of such actions, or practices, or both; one extends the scope of the analysis to further actions or practices or relations between them, or sets about to explore what is surmised to be a comparable organization of practices in another environment, language, or culture. In this volume, we have the chapter by Fox *et al.*, focusing not on other-initiated repair but on *self*-initiated *self*-repair, and, in particular, on the locus of its initiation, and doing so in seven quite different languages.

Our examination of these two chapters provides an opportunity to reflect on the doing of comparative research of the language/culture variety *under the auspices of, and with the resources of, CA*. The last phrases are important to stress. In order to be instructive, the discussion that follows will, of necessity, need to

be critical. The work will be analyzed and evaluated *from a CA perspective;* research practices that are criticized from this perspective may be suitable and robust for other modalities of research. The preoccupation of the paragraphs and pages to follow will be the contingencies of comparative CA research – how to do it, how to do it *well* and what to avoid, using these two chapters as cases in point. And, of course, what may be good or bad for CA may *not* be for neighboring communities of analytic practice.

A research trajectory for comparative studies in CA

What might a preliminary list of instructions for doing comparative CA studies – and for presenting comparative CA studies – look like? How should it start? Here is one try.

1. State explicitly your understanding of what the target phenomenon or practice is, and, if possible and/or relevant, replicate it in its original environment in data of your own (i.e., not in the data sources in which it was originally investigated).
2. Ask whether the same features that constitute the phenomenon or practice can plausibly be expected to hold in the new environment(s) in which you will be examining it/them; if not, say what should be taken as recognition criteria in the new environment.
3. Describe the ways in which the new environment(s) is like, or different from, the environment(s) in which the target of inquiry has previously been examined, and assess whether there is a robust basis for comparability.
4. Specify what makes the phenomenon or practice to be examined of interest, and what is to be gained by pursuing it/them in *these* different environments.

In what follows, I take up the Fox *et al.* and Rossano *et al.* chapters with these candidate instructions/criteria in mind.

Fox et al., "A Cross-Linguistic Investigation of the Site of Initiation in Same-Turn Self-Repair"

I must confess that I found the chapter by Fox *et al.* quite problematic. As the project on which it is based was vetted, approved, and

funded by the Linguistics Panel of the National Science Foundation, I take it that this is a worthy project in one or more varieties of linguistics, whatever frailties beset it (in my view) as a CA undertaking in comparative analysis. Let me state briefly an initial set of problematic points, and then treat at greater length some issues that may be endemic to work of this genre, though not necessarily insuperable. The issues with which I begin are all related to the four elements previously sketched for starting a comparative CA inquiry, beginning with an explicit account of the target of the inquiry.

The target of inquiry

The chapter starts off problematically. Its first paragraph reads as follows:

Same-turn self-repair is the process by which speakers stop an utterance in progress and then abort, recast or redo that utterance. While same-turn self-repair has become a topic of great interest in the past decade, very little has been written on the question of where within a word speakers tend to initiate repair (the main exceptions being Schegloff 1979, and Jasperson 1998). And, to our knowledge, no work has been done on this question from a cross-linguistic perspective.[9]

To an uninformed reader, or to an informed reader not yet alerted to possible trouble, this may appear straightforward enough, but it is not. Having announced the domain as "same-turn self-repair," the specific focus is formulated as "where within a word speakers tend to initiate repair." This formulation presupposes that same-turn repair is initiated within a word, but, at least for English, this is only partially true. Some same-turn repair is initiated within a word by a cut-off (e.g., glottal or dental stop) or sound stretch; but some same-turn repair is initiated while no sound was being produced with "uh" or "uhm," or with "y'know"; some is initiated by silence where a next element of the turn was projected to start; some has no specifiable initiation at all. Let me expand on this a bit.

What does "the site of initiation of same-turn self-repair" refer to? Technically (and this is as important a place to be technical as there is!), what is initiated by the initiation of a same-turn self-repair is what we call "the repair segment." The repair segment can be brief – no more than the initiation and the repair operation itself,

or it can be extended – most elaborately in some word searches.[10] Among the things that can happen between the initiation and the repair itself (if, indeed, there *is* a repair itself) are a number of other bits of conduct, including many that could themselves have served as repair initiations (as noted in the preceding paragraph).

So "initiation of same-turn self-repair" is not a stand-alone thing you can look for. It is designed to be part of a larger unit in which it is doing a particular job – as the term "initiation" conveys. The form it can take varies; in English, same-turn repair initiation gets done with a glottal or dental stop, one or more sound stretches, silence, "uh" or "uhm," "y'know," (all of which occur in the data cited in Fox *et al.* 1996, two of whose three authors are among the authors of the chapter in this volume) and others, and we do not yet know for English what, if anything, the differential import of these alternative forms of initiation may be. Do they, for example, project different types of repair operation to come? Does the selection between them affect where they are to be launched? For example (as suggested above), if within a word, then cut-off or sound stretch; if not, then silence or "uh(m)"? Wouldn't one want to know these things about English before launching a cross-linguistic analysis of "where within a word speakers tend to initiate repair"?

Once we have registered seriously what "repair initiator" means, we have to check whether the object being studied and coded is actually such a thing – a member of that class. Returning to the earlier work by two of the authors of this paper (cited in the preceding paragraph),[11] we find the term being applied to instances of the sort of practice that *could be* repair initiators, but which are preceded in quite close proximity by other such practices. This means that, although it "could be" a repair initiator, in such a case it is *not* … because the repair segment has already been opened by another, prior repair initiator. Here are some exemplars taken from Fox *et al.* (1996) (with the numbers as assigned in that text, on the indicated page):

(8)   B: She said they're usually harder <u>mar</u>kers 'n I said
              wo::wuhh huhh! ˙<u>hhh</u> I said theh go, I said there's-*
              there's <u>three</u> courses a'ready thet uh(hh)hh//hff
        A: °Yeh
        B: I'm no(h)t gunnuh do well i(h)n,
                                   (p. 190)

(10)   K: <u>Plus</u> once he got- (0.8) some* u:m (1.3) he got some battery
             acid on:(0.2) on his trunk er <u>so</u>mething
                                 (p. 190)
(44)   K: .hh So I'm going to start just- very simply [with-*]
             with number one

                                                                (Fox *et al*.)

The asterisks are used by Fox *et al*. to mark what they take to be
the repair initiation, but arguably the repair initiation in Extract
8 comes three words earlier, at "go" (after which the TCU is
re-begun); in Extract 10 it comes at "got-" (a cut-off followed by
silence); and in (44) it comes at "just-" (a cut-off). In instances
such as these, the talk at – and following – the asterisk is *not initi-
ating* the repair segment; the initiation occurred earlier. The result
is that one's confidence is subverted that the bits of data coded in
the Fox *et al*. chapter in the present volume *were in fact initiating
repair,* even if in some other environment they could be used to
do so. The issue here is: What exactly is the object of inquiry and
how is it operationally understood "*positionally*" (i.e. turning on
where it is) – and here this issue arises in the "starter" language or
culture, before the issues of comparative inquiry have even been
raised. For determining the object of the inquiry, readers need as
direct access as is possible to the phenomenon.

   Another concern about the target of inquiry is *compositional*.
Early in the fourth section of their chapter, the authors write:

Repair that is initiated before a word is recognizably complete is thus
word-disruptive. During the final sound of a word, speakers can produce
either word-disruptive or word-preserving cut-off: in word-preserving
cut-off, the cut-off is done softly, and the final segment is not shortened
or perturbed in any way, while in word-disruptive cut-off the cut-off is
louder, the final segment is typically shortened, and glottalization may
be begin during a vowel preceding the final segment in a consonant-final
word.   (Fox *et al.,* pp. 73–74)

The puzzle here is the phrase "and the final segment is not short-
ened or perturbed in any way"; if the "cut-off" leaves the word
unperturbed, in what sense is this a repair-initiation?

   And a third concern about the target of inquiry is *interactional;*
nowhere is there any suggestion about how this matters interaction-
ally. What turns on where exactly the repair initiator goes? What
turns on it *for the parties to the interaction,* not for the researchers.

Consider, for example, their Extract 4. They write, "Extract (4) shows initiation in the final sound of the pronoun *she;* the pronoun is replaced with a full noun phrase (*this girl*):

(4)   HG

Hyla: and **she- this girl's** fixed up on a da- a blind da:te.

Here is the sequence in which it occurs, taken from the original transcript. Hyla is recounting the characters and plot line of the play (*The Dark at the Top of the Stairs*) that she and Nancy are going to see that evening – a plot line she knows because she had seen (and "loved") the movie, and Nancy had not. The extract presented here starts well into Hyla's introduction of several characters central to the plot:

HG, 7:23–8:08

```
   Hyla:a→   =[h E]n the gir]l:: .hh is jist learning t'trust
             people yihknow en starting tih date'n she doesn't
             think she's pretty, .hh en  [she- ]
  Nancy:                                 [ How] old is she,
   Hyla:     tShe:'s I guess she's like about fifte[en er some]thing,=
  Nancy:                                           [Uh h u :h, ]
   Hyla:     =.hh En she's fixed up, (0.4) en she meets this gu:y,
             .hh a:n' yihknow en he's (·) rilly gorgeous'n
             eez rilly nice en evrythi[:ng bud li]ke=
  Nancy:                              [Uh h u :h,]
   Hyla:     =hh He's ah .hh Hollywood (0.3) s:sta:r's son yihknow who
             wz a mista:[ke en they [put im in'n [Academy,]
  Nancy:               [O o this    [s o u n d s [so goo:: ]::[d?
   Hyla:                                                      [school,
             .hh Buh wai:t.='n then, .hhm (0.2) .tch en the:(w)- the
         b→  mother's ˙hh sister is a real bigot.
             (·)
  Nancy:     [i – Y a : h ,]
   Hyla:c→   [Yihknow en sh]e hates anyone who isn' a Cathlic=. .hhh
         d→  a:nd this boy is Jewish.  .hh an' tshe- this girl's fixed
             up onna da- a bline da:te.An' the(g)- en turns out t'be
             this gu:y.=
  Nancy:     =[Uh hu[:h,
   Hyla:     =[.hhhh[ An' they goes oh I hear yer of the Jewish faith
             yihknow so 'ere's a whole thing i[n tha[t, .hhhhhh]=
  Nancy:                                      [O h :[ w o :w  ]=
```

The target here is at "d" – the replacement of the trouble-source "she" by the repair "this girl." The nature of the trouble is clear enough: at "a" a female character is introduced – "the girl," and is subsequently pro-termed with "she;" at "b" another female character is introduced – "the mother's sister," and is subsequently pro-termed (at "c") with "she"; and so the trouble-source is the "she" at "d," which is taken by its speaker to be possibly equivocal in its referential target – an equivocality resolved by the repair "this girl."

In asking how the target of inquiry figures interactionally, we are asking about the relevance *for the parties* of the placement of the initiation of the repair "in the final sound of the pronoun *she*" – both in its own right and as compared to Extract 2: "I wish I'd had **m- a** camera earlier today because" and the placement of the initiation of *its* repair "just after the bilabial nasal closure (which could be heard to begin the word *my,* replaced by *a*)."[12]

One other observation for now: The questions in the preceding paragraphs are not, in the first instance, *researchers'* questions. They are meant to be among the issues *parties to the conversation* are addressed to in producing and parsing talk-in-a-turn, in real time, in real interactional episodes, with real-life consequences; and we researchers do our job by examining as best we can how the parties to the interaction deploy these resources and understand their deployment by others. They are the ones whose conduct defines the identity and the integrity of the phenomena; where are *they* in this investigation?

This brings us to the second section of the chapter – on data collection, the data collected, and the form of data on which the analysis was actually based.

Comparability across data

There are several problems concerning the data:

First, the authors write, "The instances were taken from recordings of naturally occurring speech. The recordings were not made specifically for this project; rather they had been collected by the individual researchers at different times and in different places, for a variety of purposes." The term "naturally occurring speech" provides us with no idea whether the data were drawn from ordinary conversation, religious ritual, market bargaining, courtship,

educational/classroom setting, legal proceedings, etc. (not at all unlikely given the further characterization "collected […] for a variety of purposes"). These can have very different turn-taking organizations, and that can be profoundly consequential for repair.

Second, the authors write:

In the present study, we further limit our focus to simple repairs, both simple recycling and simple replacement. By "simple" we mean that no other repair operation is involved in the repair. Thus simple recycles are just repetitions of words, with no additions, deletions or replacements; simple replacements are instances in which a morpheme, word or phrase is replaced, without recycling, addition or deletion.

Unhappily, this leaves a great deal unspecified.

When the authors write, "Thus simple recycles are just repetitions of words, with no additions, deletions or replacements," I find myself wondering: Have they allowed for prosodic repair, in which the same words are repeated but with different prosody? How about resaying the same words, but fixing one whose pronunciation was a bit off? I am not making this up; these are decisions I have had to make on English, a language of which I am a native speaker.[13]

And when the authors write, "simple replacements are instances in which a morpheme, word or phrase is replaced, without recycling, addition or deletion," I find myself wondering about the sort of replacements Jefferson (1974) wrote about (and which I suspect characterize a substantial proportion of at least the English language replacements in the Fox *et al.* corpus). In them, the "trouble-source item" – for example, the word to be replaced – is cut off before completion but after enough is "out" to recognize what it was going to be, and is then replaced by another word which is brought to full completion. Has not something then been added? And, as in the instances taken up in Jefferson's paper, is not something else being done in addition to simple replacement when, in a court of law, the partially articulated disrespectful term for police ("cop") is stopped midcourse to be replaced by its respectful alternative "officer" (1974: 193): "When thuh ku- officer came up …"?

Third, whatever the reasons for it, the absence of all the other forms of same-turn repair – insertion, deletion, search, reformatting, etc. – is seriously problematic. One way of motivating such

a study in indigenous terms – i.e. for how it concerns practices that may matter to the participants, is to see whether and how the type of repair operation being initiated can be indicated by the form of the repair initiation and its placement. If you leave out most of the repair operations, and do not recognize that repetition/recycling can be the locus for prosodic repair, then you compromise the possibility of finding something that matters … to the participants … dealing with it on a case-by-case basis, not with significance tests.[14]

Fourth, this last concern raises another: Did the investigators work from the recorded data or from transcriptions of it? Were they in a position to tell whether a repetition included re-doing of the prosody?

Problems in analysis and findings

I limit myself here to tracking one proposed finding as the chapter progresses from initial statement to formulation in the "Conclusions" section. We begin in "Repair Type and Site of Initiation." Based on earlier work by Jasperson (1998), the authors test the hypothesis

that initiation before the word is recognizably complete is associated with repairs that change an element in the preceding talk (for example by replacing it), while initiation when the word is recognizably complete is associated with repairs that initiate repair on upcoming talk, for example by simply delaying next item due (by recycling current word).

(Fox *et al.*, p. 74)

The ensuing table and chi-square tests, they remark, "support this claim strongly."

Here, we should note, is a proposal that is embodied in a virtual parenthetical aside, namely: The authors assert that recycling is not repair on the item(s) repeated, but is repair on the upcoming talk, by delaying it ("repairs that initiate repair on upcoming talk, for example by simple delaying next item due [*by recycling current word*]," emphasis added). There are several reasons to be concerned about this claim.

First, while it is literally true that recycling current talk does delay the talk that follows, so do various other practices that occur in the environment of repair, notably such usages as "uh(m)," "y'know"

and silence (to cite only the most common ones), either separately or together. What then is done by recycling *distinctively?*

Second, there is a literature on recycling which shows recycling to be used to do other jobs even if, as is claimed by the authors for their data, there is no other change besides repeated saying. Schegloff (1987c) describes its use to get talk that was possibly compromised by implication in overlapping talk by another resaid "in the clear." And Goodwin (1980) shows recycling as among the resources used by speakers to attract the gaze of nongazing recipients. These recyclings are not doing repair on the next thing said; they are dealing with the possibly compromised hearing of the thing said in interactionally problematic moments (overlap and inattentiveness).

Since some recycling is demonstrably oriented to dealing with trouble concerning the repeated talk, what evidence is there that recycling is done just to delay next items? How much of this is related to the fact that the authors (and therefore the readers) are looking at statistics, and not at actual segments of data?

Third, what is made of this arguably problematic account of recycling as a repair practice, initially introduced in a parenthetical remark? The next section of their chapter ends like this (emphases in italics and boldface supplied):

Before examining the patterns in our other languages, it is worth exploring possible explanations for this association in English. One obvious explanation is the function of each repair type. Recycling in English tends to be used to repeat function words, *which we hypothesize is done in order to delay the production of the next content word due* (80 percent of all simple recycling in our English data repeat function words). **Because their purpose in most cases is exactly to delay production of next item due**, there is no hurry in initiating the repair. Replacement, on the other hand, tends to be used to replace content words (61 percent of simple replacements in English replace content words), and may occur in cases where an inappropriate word or pronunciation has been produced. In such cases there may be pressure to initiate repair as early as possible, to catch the potentially inappropriate production (cf. Jasperson, 1998).

(Fox *et al.*, p. 75)

This explanation appears to rest on what might be universal motivations. But do other languages actually manifest the same patterns as English? The section "Summary of Cross-Linguistic Comparison" examines how the other languages pattern.

What had been a parenthetical aside before has become a hypothesis (italicized above), and, from one sentence to the next, the *italicized* hypothesis becomes the **bolded** assertion, and the attributable explanation of other described effects (although qualified by an "in most cases"). And, by the next paragraph, it is made possibly universal! By the "Discussion" it serves as an explanation (unqualified by "in most cases"): "Indonesian is like English in having function words precede content words, so function words can be used in recycles as they are in English, to delay next item due" (Fox *et al.,* pp. 90–91).

And by the "Summary and Conclusions" section, it is a fact (emphasis supplied):

We suggested that there may be a tendency for monosyllabic words to be function words, and that function words tend to be recycled to delay next content word due; in such cases – in languages for which function words precede content words – speakers may want a beat (or two) of delay, *since one of the main purposes of recycling in these instances is to provide a temporal delay.*   (Fox *et al.,* pp. 100–101)

The upshot

Two observations serve (to my mind) to point to the sources of these troubles. First, it is striking that, apart from four fragments of conversation presented as illustrative exemplars, all the "evidence" in this chapter consists of tables and chi-squares. Second, and not unrelated to the first: In the diction of the later sections of the chapter, the authors are no longer describing what *parties to interaction* do; the main actors have become *the languages,* as in, "Indonesian should pattern with Finnish and Bikol, because […];" "But Indonesian patterns with English rather than with Finnish and Bikol in […];" "English is another exception to the findings […]" and so forth. And the supporting actors in this inquiry are syllablicity (uni-, bi-, and multi-), function words and content words, words that are recognizably complete and words before being recognizably complete … nowhere are there speakers and recipients, practices, actions, turns and turn-constructional units, repair segments, etc., – things that are interaction-relevant.

The conclusion to which this examination leads me is that the Fox *et al.* chapter is not a work of CA but a work in one of the varieties

of linguistics that find its relationship to the social or interactional to be important to address. Understanding the Fox *et al.* contribution to be a work in linguistics does not, *ipso facto,* render the discussion of it in the present chapter irrelevant. Linguists working in this area or related areas will have to sort out for themselves the bearing of these critical observations on an assessment of its results. One focus of such a review should be on the core methods employed in the inquiry, and this brings me back to an observation early in this section.

However briefly, some attention must be given to two features that were earlier noted to be distinctive to the Fox *et al.* and the Rossano *et al.* chapters: quantitative/statistical analysis as compared to the analysis of single cases and aggregates of them; and "coding" on the one hand as compared to "analysis" on the other.

First, on quantitative and statistical analysis: I have no objection to quantification per se, when, for example, it is employed to show that the results of qualitative analysis of single cases hold across aggregates of cases. It has been important in my own work, and in the work of students and colleagues. It was, for example, a key part of the argument in the first published paper in CA (Schegloff 1968), which turned on the adequacy of a candidate formulation of a finding for 499 out 500 instances, but which could not be reconciled with the 500th case, leading to a more general formulation which provided for the two different surface outcomes as alternative specifications of an underlying set of practices. More recently, Bolden (2006: 664) builds confidence in her finding that "so"-prefaced sequence initiations "are almost always other-attentive (i.e., they concern the addressee), whereas '"oh"'-prefaced sequences are self-attentive (i.e., they raise speaker-centered issues)" by displaying this table:

Table 12.1 *Distribution of "so"- and "oh"-prefaced sequence initiators*

	"So" Preface	"Oh" Preface
Other-attentive action trajectory	88	1
Self-attentive action trajectory	4	65

And she subsequently shows the interactional consequences that follow the "cases" in the "mis-match" cells – such as the ironically exaggerated expression of interest by the recipient of a self-attentive sequence initiation started with a "so," thereby providing evidence that the interactional participants are oriented to just this orderliness; the empirical analysis has explicated *their* practice, not imposed a researcher's order.

The point, then, is not simply that the CA "qualitative" analysis gets us the candidate phenomenon, which is then "shown" by quantitative analysis to be the case or not. The quantitative analysis serves to provide reassurance that the candidate phenomenon is/was not an isolated, idiosyncratic usage of some local setting (a particular speaker or category of interactants), but has a prima facie robustness. Specifying the phenomenon, showing its variants, showing that the participants are oriented to it, etc. – all return to case-by-case analysis; and when that analysis seems to arrive at another plateau of stable findings, quantitative work may be called upon again to attest to its non-idiosyncraticity. At least for the sort of data CA tries to come to terms with, one does not go to work on a corpus of data to conduct quantitative or statistical analysis and arrive at findings; rather, one works up the data case by case, instance by instance, the results of these analyses serving to compose a corpus which may then be subjected to quantitative analysis to underwrite its robustness.[15] And if that effort encounters trouble, then the trouble is dealt with by going back to the individual cases to reanalyze the data.

By contrast, when quantitative analysis is taken to be the main road to findings, as in the Fox *et al.* chapter, when the researchers get a puzzling or striking quantitative result, they form hypotheses and "test" them by further quantitative analysis, so the locus of the findings and the research is shifted from the empirical reality of the individual case to the analytical domain of the aggregate. The robustness and relevance of the findings is established not by returning to the individual cases to see whether or where they work or do not, and how they go awry, but by resorting to tests of statistical significance – chi-square tests in the Fox *et al.* chapter.

Whatever it is that chi-square tests actually test and attest, they presumably underwrite the robustness of the findings in the larger

universe from which the actually analyzed data were drawn and of which the data at hand provide a reliable – or at least calculable – sample. As I have argued elsewhere (Schegloff 1993), I am at a loss to know what that universe is, and which of its parameters a sample should approximate to be deemed adequate (an issue aggravated here by the previously noted diversity of the collectors and projects shaping the data set). When CA papers use the loose language of "overwhelmingly," "rarely," etc., they speak to the issue of whether some proposed finding is plausibly to be considered a stylistic quirk of one or several speakers or categories of speaker. That seems to me a viable undertaking and one that readers can assess to decide whether to take the proposed finding seriously, seek to replicate it in the data to which they have access, etc. It remains to be seen what relationship there is, if any, between these goals of what might be termed "loose" or "rough" quantification and the scientific results claimed for conventional statistics, with paradoxically uncertain credibility at best.

But where do the data come from that supply the grist for quantitative and statistical mills? The "data" are not the actual things said or done by parties to the interaction; those are the "*raw* data." The data for quantitative and statistical analysis are the product of "coding" the raw data, and that is the other matter needing to be addressed here – what is the relationship between coding and analysis?

Second, on coding and analysis: Although an early footnote in the Fox *et al.* chapter reports on who "analyzed" each language data corpus, it seems clear that the primary product of "analysis" was a coding scheme, and it was the outcome of processing the data via this coding schema on which the chapter is based. Indeed, the authors write, "Each instance of repair was coded for a variety of features. The coding scheme was developed for the larger project, and it included information relating to type of repair and site of initiation" (Fox *et al.*, p. 62), which suggests that the coding scheme was not designed with this work in mind, but the outcome of its application to the data corpora has been exploited for the announced topic.

But coding is not analysis. Coding produces the distribution of instances of "values" of pre-selected variables as observed in the raw data; it does not determine (or modify) the variables, how they were formulated, and on behalf of what project they

presented themselves as interesting. Above all, it is an instrument for the processing of aggregates of data; it makes the data serve a pre-scribed discipline of inquiry rather than making the inquiry responsive to the observable features of the cases examined one by one, that are then collected into the aggregate.

Analysis – at least in *conversation* analysis – treats each case in its particulars, indeed, is responsible for the determination of what will constitute a "case" or an "instance" of a putative phenomenon in the first place. Each candidate instance of a putative phenomenon has to survive such an examination and can in the course of the examination transform the researcher's understanding of what the phenomenon is, rather than simply being included in, or excluded from, class-membership status. Here is one account of how this works.

Having spelled out how a strip of interaction or some practice of acting seems to have worked, the analyst tries to ground the effort to go beyond a single case by analyzing additional single cases. These may reveal the same way of working, or may allow or require a different account whose development allows (and drives) the reformulation of the analysis in more general terms – terms which subsume the initial analysis and the subsequent one(s) as alternative specifications. And this procedure is reiterated until a formulation of the target of inquiry is arrived at that is adequate to the vast majority of further examined instances. Then, instances that are not adequately and straightforwardly addressed by this formulation are subjected to single case analysis which (if one has gotten it pretty much right) shows that the ways in which that exemplar differs from the arrived at account can be understood by reference to special, describable features of the local context on that occasion, or as doing a specially inflected version of the phenomenon being described, which is itself describable.

Of course, at any of the steps of this course of analysis, the process may fail – a next candidate exemplar is unanalyzable, or is analyzable in a fashion that does *not* allow a more general formulation that yields alternative specifications. Then one does not "have a phenomenon," though one may have several candidates, none of which is what one started with. But if one *does* have a phenomenon, then one can go about collecting instances in greater numbers – perhaps exemplifying the several alternative specifications that the analysis

has engendered so as to get some clues to what "conditions" their respective occurrence, clues which will themselves lead back to single case analysis so as to ensure that the result *is true not only of the aggregate,* but *of the single cases that compose it.* After all, parties to interaction do not act in the aggregate; they do not behave so as to produce profiles of aggregates of action types; they deal with a/ the moment in interaction, what has led up to it, what they mean to do in or with it, how they mean to advance its course.

In the "best case" scenario, one comes upon ideal evidence: data internal evidence that parties meant by some bit of conduct to be doing what the analysis of it claims, and evidence that recipients understood that to be what a prior speaker was doing – each of those displayed in the ensuing conduct in the interaction. Not all phenomena are conducive to such evidence, but it is the gold standard if one can get it.

All of this is most likely to happen and to yield results if the analyst has collected and, most importantly, transcribed the data herself or himself. Most of the authors in this volume collected and transcribed the data themselves, thereby achieving (and suffering!) the most intimate exposure and familiarity with data an investigator can have.[16]

In my experience, the practice and process of coding do not work like this. They are for the most part routine operations: The coders are trained to use designated features of the data to recognize membership in one or another coding category; their job is not to register new observations or decide that some category should be split into two. The adequacy of the job done will be measured by a statistic that testifies to the success or failure of the coders to arrive at identical assignment of cases to classes.

I have tried to do coding in my own research. One might have thought that I, having figured out what things seemed to possibly matter for the way some object of inquiry worked, could create a coding scheme whose users could then use it on multiple instances to determine whether and which of those things was present or absent and did or did not affect the object of inquiry, and relieve me from doing so. But when I sit down to do a bit of that work myself to see if it is ready for others to take over, I find that the elements of the coding scheme represent and evoke a memory in me of what made them relevant, and, looking at the raw data, I

find myself in a position to grasp the import of the presence or absence of some "coding" outcome, in a position to register other features of the datum – perhaps previously unnoticed – that do the same job as what has been coded as "missing" or explain that missingness, or I can register that, even if present, it is not doing in *this* instance the thing it is presumed in the coding scheme to be doing.[17] And I know again that I have to do it myself. And I always do it myself. What might a discipline look like if the coding was ordinarily done by experienced investigators? Ones who know the language as a native language? And ones who have become, or at least are becoming, astute analysts of talk-in-interaction? What would then be the relationship between detailed analysis of single cases and quantitative representations of "the data?"

Or hasn't that actually been the general practice in CA? How then should we assess the introduction of quantitative/statistical analysis – with its requirements for coding – into conversation analytic work? The chapter by Rossano, Brown, and Levinson offers a case in point.[18]

Rossano et al., "Gaze, Questioning, and Culture"

The title of this chapter is an accurate guide to what will figure centrally in it. The chapter concerns the deployment of gaze by speakers and recipients in two-person interactions, focused specifically on the questions in question–answer sequences, examining this target in three geographically, culturally, and linguistically different (and remote) social contexts. It is based on extensive prior work by one of the authors (Rossano) doing detailed qualitative CA analysis on Italian data that has yielded empirically grounded evidence for accounts of gaze deployment that are counter to the preceding literature.[19] That work has provided the basis of the hypotheses to be addressed by statistical methods in the chapter being introduced. One reason for turning to these methods is "to understand whether gaze as an interactional practice has universal properties across cultures or not. We cannot make easy judgments about sameness and difference in conversational practices unless we can be sure that the examples we analyze are reasonably representative of interactions in the culture in question" (Rossano *et al.,* p. 188).

As in the Fox *et al.* chapter, this work exploits data from a larger project ("concerned with the crosslinguistic comparison of question-answer pairs") to look at "gaze behaviour in a sample of 300 question-answer pairs from each of the three cultures, as used by roughly ten distinct dyads (or c. twenty individuals) from each culture engaged in naturally occurring conversation in informal settings" (Rossano *et al.*, pp. 188–189). Unlike the Fox *et al.* chapter, this chapter contains brief but detailed accounts of the circumstances in which the data were collected, well beyond the characterization "spoken interaction."

The outcome projected in the chapter's first section is that … this restricted sample is quite enough to show that earlier analyses of the occurrence of gaze in interaction are not general across functions and cultures. The results are also suggestive of more positive general hypotheses, which would need to be followed up by qualitative analysis of a wider range of actions and sequence types. Given the work earlier referred to (Rossano, 2009), it is clear that we cannot expect a full understanding of gaze in interaction without such a wider analysis – question-answer pairs for example constitute a very different sequential environment if compared with storytellings. But we hope that these preliminary results will already serve as a useful orientation for this future work.

The quantitative analysis is, then, depicted from the outset as supportive of further qualitative work, rather than as being the payoff in its own right, except for calling into question the suggestion of universal applicability in some of the prior literature, which it succeeds in doing.

So what's good here is:

1. the undertaking is grounded in preceding detailed CA work;
2. that preceding work has already showed differences from the prior literature; and
3. there is reason to believe that the differences between the preceding CA analysis and the received literature have to do with what analytic *features of conversation* gaze is organized by reference to, rather than it being *language* or *culture,* but that remains to be shown.

What is less clear is:

1. that twenty individuals from each culture, and 300 instances of one utterance type (questions, and three specific types of

question at that), are representative of a culture, so that statistical analysis is relevant and probative, or, indeed what the universe is for which the data bases on which they rely are representative in a fashion that will support the statistical procedures that they employ; and

2. if not, whether the objectives of this study would not be as achievable by close analysis of a half dozen actual segments from each of the cultural settings instead of being largely composed of quantitative analyses of data that the readers for the most part do not get to see.

The second section is a thorough and cogent critical review of the relevant literature which is rather more substantial and reliable than the literature on the topic of the Fox *et al.* paper. This is important because the finding to be reported stand in sharp contrast to the findings of the previous literature.

The third section is key. Although introduced as providing a "few examples that motivate the development of the coding scheme and show a different organization of gaze in interaction" (Rossano *et al.*, p. 193), it does far more than the subservient role that the first clause conveys. Far from facilitating the statistical analysis and the coding scheme that provides grist for its mill, here are the raw data being examined, and a detailed analysis of such data – including two episodes from the Italian data and one each from the Tzeltal and Yélî Dnye data. Here we get to see some actual instances of the things the statistics will be statistics about. And this access makes it possible for us, the readers, to evaluate critically the aptness of the terms of analysis in which the statistics will be reported – a possibility to be exploited a few paragraphs hence.

Aside from the substance of the analysis and the sheer value of its presence, is the testimony it provides for the immanence here of *analysis,* not coding. The transcripts contain not only the conventional (for CA) three-lined-representation of the talk – in the native tongue, a morpheme-by-morpheme gloss and a translation into idiomatic English; they have funny little ovals and arrows (explained in Appendix B, pp. 242–244). These little drawings depict gaze direction and change in gaze direction, located relative to the moment-by-moment progress of the talk. They evidence the kind of detailed analysis that leaves its author inescapably informed about what

is going on in the data. The robustness of the analysis can have
no better guarantor than these little pictures; given the choice
between these transcripts-with-pictures and the statistical analysis
that depends in large measure on the work they embody, I would
pick the former any day. And, at the very end of the chapter, its
payoff will surface again, when some statistical outliers that might
easily be dismissed as "noise" by those relying on the statistics get
satisfying accounts by virtue of the detailed analysis that had been
done in the first place.

   The fourth section describes data and method. It is explicit,
detailed, and argued … and therefore arguable with. For example,
here is how the authors present the selection of their sample:

This extensive database of interaction in the three cultures was sampled
in a systematic way. Within each cultural sample, we selected roughly ten
dyads, and searched for question–response sequences until we had 300
such sequences for each cultural sample. By "question" we understood
any utterance that functioned as an information soliciting action, regard-
less of whether it was in interrogative form or otherwise marked mor-
phosyntactically, lexically or prosodically. For example, many "yes"/"no"
questions in Yélî Dnye are delivered in declarative format with falling
intonation – they are recognizable as questions just because they appear
to be statements about facts that are privileged information of the recipi-
ent's, of the kind "You have a stomach ache" (cf. Labov and Fanshel's
(1977) "B-event statements").   (Rossano *et al*., p. 205)

The explicitness of this account allows a reader to think along with
the researchers, for example, along lines such as the following.
Aren't these "statements about facts that are privileged information
of the recipient's, of the kind 'You have a stomach ache'" more aptly
understood as "noticings" than as "questions"? As such, they are
inspectable by their recipient for what has prompted or motivated
their articulation at some "this moment" – complaining, sympa-
thizing, etc. So, does the category "questions" serve well here? As
compared, for example, to "first pair parts that make (or *can* make)
a spoken response relevant next"? Whether this point is well taken
or not, it is the very possibility of putting it forward, of check-
ing it out vis-à-vis actual data, that makes this way of proceeding
so fruitful. Indeed, it is so appealing that I would have welcomed
the display of one or two exemplars of each of the sequence types
they draw on ("requests for new information, requests for repair

[e.g., for repetition, or clarification], requests for confirmation of previously established information, or presumed facts],” Rossano *et al.*) in each of the languages, so the reader could have a more informed understanding of the sort of interactional events being referred to in the ensuing text. If the only obstacle to doing this was space, this reader would have preferred relocating some of the statistical discussion to an appendix, the payoff of which would not be limited to this “data and method” section but would be even more welcome in the Results section that follows.

The Results section is largely statistical and takes on the appearance of most of the Fox *et al.* chapter: statistical findings followed with hypothetical understandings of the statistics. For example:

Another general fact emerging from all three cultures is that questions initiating repair are especially likely to involve speaker gaze. One possibility is that gaze here reinforces that the Q-speaker, though speaking, is committing to a more attentive engagement as a listener in the conversation. The speaker of the repair initiation is asking the recipient to repair his or her prior talk because of some problem, and the speaker of the repair question can ask to delay the progressivity of the talk by projecting a full recipiency once the repair is produced.   (Rossano *et al.*, p. 216)

It would be most welcome if these speculations (“Perhaps gaze here reinforces …”) could be grounded in, or superceded by, actual analyses of such sequences. One might suspect, for example, that rather than a solicitous “more attentive engagement as a listener,” other-initiation of repair conveys disaffiliation, nonalignment, or some other problematic stance toward the question-recipient and Q-recipient’s just prior utterance; it might therefore be more a kind of interactionally “loaded” moment in the interaction than is suggested by the text as it now stands. And this speculation – both the authors’ and this reader/writer’s – has implications for the larger scope of the research undertaking being reported in this chapter.

Recall that the point of this research was characterized at the outset as “to understand whether gaze as an interactional practice has universal properties across cultures or not” (Rossano *et al.*, p. 188). That may be the right way to approach this area of work, but let me suggest an alternative.

What if we began by asking how we are to understand the job(s) that gaze is doing in the data site most intensively studied here: the

Italian data. Then we can ask whether gaze is doing the same job(s) in Yélî Dnye, where gaze behavior looks pretty much the same. And then we can ask whether gaze behavior does the same jobs in Tzeltal, where it looks in some respect different. And then we can ask what else is being done in Tzeltal (other than gaze) that does the jobs done in Italian by gaze? That is, the issue ultimately is not gaze (as the given "primitive"); the issue is "What does the organization of interaction require that gaze does in Italian and that other forms of conduct may do in other cultures, and what are the differential consequences, if any, of getting this job – or these jobs – done by gaze in Italy and otherwise elsewhere?" Such questions can only be answered – or modified – by engagement with the details of what is going on in single episodes of interaction and in aggregates of such analyzed episodes; and if statistics are needed to get there, or to get the funding to get there, or to get the attention and respect of those who have not yet figured out where the real payoffs are to be found, so be it, but let us always keep in mind what is the cart and what is the horse.

Where does this leave us?

The Rossano *et al.* chapter goes a long way – but (in my view) not all the way – toward what can currently be achieved in CA comparative analysis in which the terms of comparison are linguistic or cultural.

On the whole, it does what I earlier suggested CA comparative analysis should begin by doing: It states clearly and explicitly what the target phenomenon or practice is; it tries to replicate it in a corpus of data different from the one(s) drawn upon in prior inquiry, and in doing so arrives at findings that differ from those previously reported; it undertakes to explore the target practice in two more environments that differ both from those explored in the inherited literature and in the environment of the new undertaking; it establishes a reasonable basis for comparability – holding constant the interactional activity (specified forms of questioning), targeting practices of gazing across variations in cultural and linguistic formations.

On the other hand, in doing so it seems to hold one culture's conduct answerable to the conduct of others. Specifically, the

convergent practices of gaze in the Italian and Yélî Dnye data render the Tzeltal practices "different" and suggest a search for what will explain or "normalize" this Tzeltal practice – just the sort of undertaking I have tried to call into question in an earlier section of this chapter. How are we to reconcile all the attractive features of the Rossano *et al.* chapter with the reservations raised by our commitment to understanding the practices of interaction that constitute the world in which those actors live? We surely will not arrive at an answer to that question here. But it will as surely involve treating each language/culture gestalt in its own terms to begin with. Paradoxically perhaps this picture has to be enlarged to a still greater canvas – one in which it is not gaze (or repair, initiation, etc.) that is the focus, with gaze (or other target) practices in one cultural setting being juxtaposed to gaze (or the same other target) practices in another, but rather contingencies of interactional organization that transcend cultural boundaries and help us to mediate the variations between linguistic, cultural and social formations (Schegloff 2006). Might it not be these contingencies of interactional organization that will underwrite CA comparative studies in a fashion that will not elevate or demean one culture vis-à-vis another but will take the measure of each by reference to the organization of interaction for the human species?

Notes

1 Although this is (to the best of my knowledge) the first volume that overtly announces itself to be addressed to conversation-analytically informed comparative research, it has two less overt predecessors. (There are, of course, also papers devoted to comparative treatment of CA findings (e.g., Fox *et al.* 1996; Hacohen and Schegloff 2006; Lerner and Takagi 1999; and Moerman 1977, inter alia.) One is the volume edited by K. K. Luke and T. S. Pavlidou (2002) on *Telephone Calls;* the other is the volume edited by N. J. Enfield and T. Stivers (2007) on *Person Reference in Interaction*. As the topic of the first is virtually a creation of CA, it turns out to be conversation analysis in comparative inquiry even though not all of the contributors are trained in CA or perhaps even think of themselves as conversation analysts. The second of these volumes addresses a topic surely *not* invented by CA, but is focused for the most part on a particular stance on person reference that is the product of conversation-analytic research (Sacks

and Schegloff 1979). Some of its contributors are conversation analysts, some are "friends of CA" or "fellow travelers," some are specifically *not* conversation analysts who, while respecting CA work, have profound and persistent objections to it, largely centered in linguistic anthropology. I address myself to some issues raised in that volume in part 2 of this chapter. The present volume, however, not only makes conversation analysis its title; the majority of its contributors were trained in CA, offer their work as conversation-analytic by intent, design and realization, though in some cases identifying themselves as not exactly conversation analysts, but kindred folk – "interactional linguists" most often. I was represented in the first volume in much the same capacity as in the present one (2002a: 249–81), and for the most part try to avoid repeating myself, though the early pages of my contribution to that volume have a bearing on some of the pieces in this volume as well. Finally, I have benefited from input from a number of colleagues – Nick Enfield, John Heritage, Celia Kitzinger, Gene Lerner, and Jack Sidnell, none of whom bear any responsibility (nor will they be specifically credited) for what I have made of their suggestions.

2 Terms such as "children" and "adults" are in scare quotes as an alert that these are categories from the "age" and "stage-of-life" categorization devices whose relevance has not been grounded and would have to be grounded in data to warrant serious reception as CA work. Cf. Sacks 1972a, 1972b, 1992 passim; Schegloff 1991, 1997a, 2007a.

3 In addition to the temporality of individuals' lives, there is the passage of social, cultural, and historical time. The sort of data which has proven indispensable for CA work limits the possibility of comparative analysis at present, but not for long. At the very least, there is the material collected by conversation analysts since the early 1960s – now almost a half century ago. There may well be retrievable recorded data of ordinary talk-in-interaction – at least audio data – going back some twenty years before that which would support modestly detailed analysis (cf., e.g., Clayman and Heritage 2002). And there is also the more limited possibility of using much older records to ground claims about the historical robustness of practices initially found for contemporary data (as in my effort to show that an account of summons/answer sequences was not limited in its relevance to contemporary telephone conversation by citing a biblical report of an exchange between Abraham and his God: "And He said, Abraham; and he said, Behold, I am here." (Genesis 22:1); Schegloff 1968: 1075).

4 I should make clear that, in what follows, I am addressing "the-Kitzinger-that-Enfield-invokes" as part of my addressing Enfield's argument. Kitzinger herself has taken these issues on in a variety of papers engaged

with a variety of data (cf. Kitzinger 2000, 2005a, 2005b, in preparation; Land and Kitzinger 2005), papers embodying a developing trajectory of argument which it is beyond the scope of this chapter to address.

5 Actually Jeannette does later correct Estelle's reference to "a colored lady" by saying, "No, no, that was one of the employees."

6 Both Enfield and Kitzinger invoke Sacks' programmatic call for accounts of "doing being ordinary" as the central thrust of what they are seeking (Enfield's chapter begins with a citation from one of Sacks' lectures on that theme). It is surely a theme worthy of pursuit, but how is it to be pursued? One way of pursuing it is to take as one's point of departure data in which participants are overtly engaged in "doing being ordinary," or in which they overtly juxtapose the non-ordinary with the ordinary.

One example is the work Sacks had undertaken and Jefferson has brought to fruition on the initial "normalization" which people report themselves to have imposed on what turned out to be anything but ordinary (Jefferson 2004: 131–167).

A second example is the previously mentioned account by Sacks of alternative ways of "seeing" or grasping the scene with the police, an account which may well have turned on the identity of one of the people being seen and referred to by Estelle as "a colored lady" and later as "a negro woman" (with the implication of her involvement in "the trouble") and having that rejected by the woman who works at the store, "No, no, that was one of the employees," as a way of absolving her of such suspicion.

A third tack turns on a different sort of realization of the "ordinary" and the "present case"; for example, Enfield finds it useful to underscore the compelling character of Kitzinger's account of the premises of heterosexuality by juxtaposing it to data examined in a separate paper by Land and Kitzinger (2005) in which a lesbian client corrects an insurance salesman's subsequent reference to her "spouse" as her "husband."

And a fourth, and for now final, way of grounding such proposed findings is exemplified in a paper by Lerner and Kitzinger (2007), in which they examine occasions in which the usually "unmarked" and "unnoticed" practices of self-reference are given explicit attention through self-repair operations on self-reference (replacing "I" with "we," or "we" by "I").

Invocations of such data exemplars serve to ground the account of the otherwise ordinary by exemplifying its consequentiality in actual occasions of interaction. Otherwise, there is virtually no end to the categories whose members can find themselves demeaned, excluded, or otherwise not taken into account in ordinary talk-in-interaction (a theme pursued in Goffman's *Stigma* 1963: 137–138).

7 These accounts are taken up in somewhat greater detail in Schegloff
   1996b: 168–181 and Schegloff 1997b: 501–502.

8 A notable example has been past and contemporary work on TCUs in
   Japanese (Hayashi 2003, 2004a, 2004b; Iwasaki 2007, forthcoming;
   Tanaka, 1999, 2000, 2004; Thompson and Couper-Kuhlen 2005)
   and Korean (Kim 1999b, 2007; Park, in preparation).

9 Actually, the citation to Schegloff 1979 hardly qualifies as an excep-
   tion to the observation that "very little has been written on the ques-
   tion of where within a word speakers tend to initiate repair." When
   this citation is later taken up at the start of their third section, the
   authors write: "The first proposal we will explore concerning site of
   initiation appears in Schegloff (1979). There, Schegloff writes, "Just
   post-initiation and just pre-completion of various unit types seem to
   be specially common loci of repair initiation. Thus, just after the start
   of a turn-constructional unit (e.g., a sentence) or just before its com-
   pletion; *after the first sound of a word or just before its last sound*"
   (1979: 275 [emphasis added by Fox *et al*.]). This is just one point in
   a long paper covering many topics, and Schegloff does not elaborate
   on it, nor give any examples. Nor is it clear from the paper exactly
   what is meant by "after the first sound of a word or just before its last
   sound"; after the first sound could mean after the first sound is recog-
   nizable, or after the first sound is recognizably complete; just before its
   last sound could likewise mean before the last sound is articulated or
   before the last sound is complete. However, Schegloff (personal com-
   munication) has defined the relevant domain for just post-beginning
   as starting after the first sound is recognizable, thus has begun to be
   articulated, and continues until the first sound is complete, while the
   relevant domain for just pre-completion begins just before the final
   sound is articulated, in the penultimate sound, and continues until just
   before the final sound is complete. He has further suggested that site of
   initiation of repair may depend on the type of repair involved."

   As the authors note, I offer no elaboration and no data, and this
   should have served as an alert (given the densely empirical character
   of virtually everything I have published) that this was an impression
   gathered informally and mentioned in passing, not a claimed finding
   on which further work could be based. And the "personal communi-
   cation" that is cited took place almost thirty years after the cited text
   was published, after initial resistance on my part, and hardly amounts
   to a "definition." In any case, it is no exception to the absence of a
   literature on the matter in question.

10 In fact, what is technically a repair segment can be even shorter than
   the initiation and the repair: as remarked earlier, sometimes there is

no repair initiator at all, just a recognizable repair; and sometimes there is a repair initiator and no *repair* at all, but the production of a projectably next component of the turn-constructional unit in progress – a "cancellation," if you will, after initiation and before realization.

11 Examination of the 1996 paper was prompted by the absence from an earlier draft of the Fox *et al.* chapter of data presenting actual talk in which same-turn repair is initiated. The extracts included in the final draft that appears in this volume do not exemplify the problems discussed in the text. As well, the discussion in the following two sections, pages 383–387, was addressed to that earlier draft.

12 Here is another, quite different, example of the sort of thing I have in mind. In Schegloff 1987b, the analysis focuses on a single turn, one which happens to include a self-initiated same turn repair. The turn is:

```
01 Curt:   [He- he's about the only regular <he's about the
02         only good regular out there'z, Keegan still go out?
```

Here, as it happens, the repair is *not* initiated by a cut-off but by a rushed restart of the turn after "regular" on the first line. But the import comes from

(a) where it *is* initiated;

(b) how far back it goes in framing the trouble-source and its repair;

(c) how best to characterize where it is initiated, and

(d) whether all this is pretty much the default form of the same-turn repair or even of same-turn *insertion* repair, or whether it is a departure, and, if so, what the import of the departure is, if any. At the time the paper was conceived and then written, I did not know the default form that such insertion-repairs have. I now know that this is an unusual and exceptional instance of its type on counts a, b, and c, and this bears on what is getting done interactionally. To be sure, Fox *et al.* limit their inquiry to replacements, but are not the issues the same?

13 An example: Debbie and Shelley are two young attorneys who were to have gone with a group of others to a football weekend in another city, except that Shelley has backed out at the last moment. Debbie has called Shelley and accused her of "blowing off her girlfriends just because this guy's not going." Shelley has denied the accusation and has been explaining why she is not going and its connection to her boyfriend's not going. Here is the end of her account:

```
01 S:   → So:I mean it's not becuz he's- he's- I mean it's
02      → not becuz he':s not going, it's becuz (0.5) his
03        money's not¿ (0.5) funding me.
```

04 D: okay¿

Note here, at lines 1 and 2, Shelley's extended effort to distribute the stress in her turn so as to set up the contrast on which her account for not going depends. I submit that though the lexical composition does not change across the several tries at getting this said, the recycling is not "simple"; it is not there so as to delay the talk that will follow, as the authors go on to claim.

Another example is presented in Sidnell (2006) as in the service of what Stivers (2005) terms a "modified repeat":

01 F: [the thing ( )
02 S: =we need to deal with this an' get on with [it.
03 F: [(yih)
04 F: this is an important issue to a lot of people.
05 S:→   ah-ah I agree. it's a-it's-it is an important issue.
06 S: but there are many other important issues that we
07 S: must address as well.

14   That this *does* matter to the authors is indicated by their citation to the work of Jasperson (1998) in the just-following text.

15   In the last 10–15 years, there has been substantial growth in the application of quantitative and statistical resources in CA work – largely in CA work on data from so-called institutional contexts such as politics, media and medicine: on politics and media, cf. Heritage and Greatbatch, 1986; Clayman, Heritage, Elliott, and McDonald, 2007 inter alia; on medicine, cf. Heritage, Boyd and Kleinman, 2001; Stivers, Mangione-Smith, Elliott and McDonald, and Heritage, 2003; Robinson and Heritage 2005; Stivers and Majid, 2007; and Heritage, Robinson, Elliott, Beckett, and Wilkes, 2007 inter alia. My thanks to John Heritage (pc) for orienting me to some of this literature:

"I think the key thing about the statistically oriented studies is that almost, if not all, of them use stat[istics] to build a relationship between already identified practices of talk and exogenous putative causes (e.g., attitudes, race or gender, the state of the economy) of talk, or of its consequences (prescribing decisions etc.). Stat[istics] are not used to build analyses of the internal relationships among practices of talking. In the older language, CA provides adequacy at the level of meaning, the stat[istics] provide causal adequacy for claims that tie practices of talk as causal to, or caused by, exogenous social processes. The papers you mention may represent departures from this pattern, or not." Heritage's characterization of this form of application of statistics in CA work does not apply to the Fox, Wouk *et al.* chapter.

16 I either know, or confidently suppose, this to be the case for Bolden, Egbert, Hayashi, Heineman, Lindström, Rossano, Sidnell, Sorjonen, and Wu, and perhaps others whom I know less well, and it shows in their respective chapters.

17 I use the present tense here because, while working on this chapter and waiting for the other chapters to be made available in final draft, I was also working on my own research – in the present case, getting a better grasp on the use of "y'know" in American English in preparation for writing the next volume in the Primer series (Schegloff 2007b) which will most likely be on the organization of repair, in which some uses of "y'know" figure. I have assembled a collection of some 220 instances and have myself been doing what the text of the preceding pages has been describing. I mean to be contrasting the rote practices by often otherwise uninvolved RAs in coding data into prespecified categories whose import (i.e. what they are taken to be indicators of) they may know nothing about, on the one hand, with fully committed and involved researchers examining such data with the coding categories at hand who can see in the juxtaposition of the coding categories and the details of the data more than just a "yes" or "no" – but also what is involved in the presence or absence of the thing being coded for, leading to a revised understanding of the phenomenon.

18 In proceeding this way, I stand by the judgement of the Fox, Wouk *et al.* chapter as more Interactional-Linguistic than Conversation-Analytic. At the time of writing, I have not seen the final version of the Rossano, Brown and Levinson chapter.

19 Given the extended discussion of the relevance of categories and their deployment in CA work in "Comparative Analysis: One CA Perspective" above, I am reluctant to linger on it further here. Yet the centrality of "comparative culture" analysis in the Rossano *et al.* chapter prompts inclusion of the following footnote from one of the earliest CA papers, apropos the characterization of the data examined there, in response to an editorial suggestion: "that all the conversations are in 'American English' is no warrant for so characterizing them. For there are many other characterizations which are equally 'true,' e.g., that they are 'adult,' 'spoken' (not yelled or whispered), etc. That the materials are all 'American English' does not entail that they are *relevantly* 'American English,' or relevantly in any larger or smaller domain that might be invoked to characterize them. All such characterizations must be warranted, and except for the account we offer in the final section of the paper, we cannot warrant them now. Ethnic, national, or language identifications differ from many

others only in their *prima facie* plausibility, especially to those in the tradition of anthropological linguistics" (Schegloff and Sacks 1973, 291–292, note 4, emphasis in original). For "American English," substitute Italian, Tzeltal, or Yélî Dnye. But note that this issue is being raised where the target of inquiry is not specifically language, but was not raised for the Fox *et al.* chapter or any of the others, in which deployment of the language was the object of inquiry. In this respect, I agree with the position taken up in Lerner and Takagi (1999): "In examining language use it seems safe to assume that the language spoken has a continuing relevance for turn construction, since the choices and possibilities for a speaker are always found within the linguistic resources available. However, it is another matter to demonstrate just how these linguistic resources are consequential for the organization of talk-in-interaction. This is one place where a comparison – or co-investigation – of distinct languages can be advantageous." Should the gaze practices being explored by Rossano *et al.* turn out to be linked to features of the language, this reservation would not be in point. But in the chapter published here, the text varies between referring to "cultures" (as in "Another general fact emerging from all three cultures" (Rossano *et al.*, p. 216) and referring to "language X speakers" (Rossano *et al.*, passim) or both, as in these consecutive sentences, "We turn now to consider possible cross-cultural differences in gaze behavior. Figure 7.3 compares the gaze behavior of Q-speakers and recipients across the three languages" (p. 208). It is unclear how deployment of gaze would be part-and-parcel of language, but plausible conjectures about it can readily be