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Reflections on Conversation Analysis and Nonnative Speaker Talk: An Interview with Emanuel A. Schegloff[1]

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Emanuel A. Schegloff is, along with Gail Jefferson and the late Harvey Sacks, one of the founders of conversation analysis, a mode of inquiry and research methodology. While he is most widely known for the foundational articles on turn taking (Sacks, Schegloff, & Jefferson, 1974) and repair (Schegloff, Jefferson, & Sacks, 1977) published in the journal *Language*, and the first published paper in conversation analysis (1968), Dr. Schegloff has published over 70 articles on talk and interaction, and continues to publish his ongoing research widely, including recent articles in the *American Journal of Sociology, Social Research, Language in Society, Discourse and Society, Discourse Processes, Research on Language and Social Interaction, Language and Speech, Discourse Studies, Aphasiology,* and *Applied Linguistics.* He co-edited the 1996 volume *Interaction and Grammar* with Elinor Ochs and Sandra Thompson and is currently writing a synthesis of work in conversation analysis that might also serve as a textbook for students. Dr. Schegloff is Professor of Sociology at the University of California, Los Angeles, where he has taught a course sequence on "Conversational Structures" on a regular basis for over twenty-five years. Through his teaching, data sessions he has organized, and his personal mentoring, Dr. Schegloff has been instrumental in training many of the practitioners of conversation analysis working today, including researchers working on a growing variety of languages (such as German, Finnish, Swedish, Japanese, Mandarin, and Korean).

Wong: *In its history of roughly thirty-five years, conversation analysis (henceforth CA) has moved from a focus on ordinary conversation to the inclusion of talk in institutional settings. But the CA tradition has been predominantly monolingual, focusing on native speaker talk-in-interaction. What are your thoughts, Manny, on why CA has not included the talk of nonnative speakers in its research program?*

Schegloff: Let me say something about the preface to the question. What you say is true and not true at the same time. When you look to see what sort of data Harvey Sacks and I first worked on, it was institutional data. I worked on

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telephone calls to the police. Harvey worked on telephone calls to a suicide prevention center and then group therapy sessions. The issue isn't what kind of data anybody has worked on, but how one worked on it. When I began looking at phone calls to the police, I was starting out with a particular kind of problem, and I kept getting driven back earlier and earlier in those calls, until I ended up with "Police desk, can I help you?" To deal with that I had to deal with the beginning of telephone conversations, and, subsequently, the beginning of conversations. So the fact that you have a certain kind of material does not dictate the terms of the analysis. It can, but it doesn't have to. It's not that CA started out with ordinary conversation and then eventually developed an interest in institutional talk, but that at some point it became more pressing or more inviting to focus on those aspects of the talk that couldn't be adequately addressed without taking into account the constraints of the institutional context in which the talk was being conducted. So that's just to the preface.

Now I want to return to your question concerning why we always dealt with, as you put it, monolingual rather than nonnative speaker talk. I want to treat the question as odd because it ascribes to workers in the past a relevancy which was not theirs, but is that of the askers of the question—and not just you two, but the literature that you've called to my attention as well.[2] Nobody was concerned particularly to avoid nonnative speakers, even though it was a constraint in my pedagogy. Those of you who went through my course know that in the very first material that I asked students to collect to work on, I insisted on conversation involving adults and discouraged dealing with data involving children, nonnative speakers, and institutional settings, that is, when students are getting their initial exposure to CA. But in the larger research arena, there was no particular interest in avoiding anything of the sort. The question comes up from the point of view of people who work specifically on nonnative speakers, for example, those who work specifically in applied linguistics on problems of teaching and the acquisition of a second or foreign language, who, looking for succor, help, and support from the conversation analysis research literature, find it isn't there. So it may look like CA workers have overlooked or ignored nonnative talk, but ignoring it means you know it's there and you turn your head away. It just wasn't a relevant category distinction to begin with. I was never confronted particularly with such materials until you, Jean, brought along yours (Wong, 1994). Then I said okay, let's go to work on those. And now that this sort of material does come along more frequently, one question is: How does one work on it? From my point of view, anything that goes on in talk-in-interaction is fair game for us, and we need to ask: What does it require of us as analysts to figure out how to deal with it adequately? That's an open question. There may be some people who feel it will require big innovations, but until someone shows that, I'm skeptical.

Olsher: *But aren't there some fundamental issues with nonnative talk which may not be present or relevant when doing CA using native speaker talk? For example,*

there is the problem of competence in the language of the interaction. How might this special problem affect the work that a CA analyst can do when examining nonnative talk?

Schegloff: I'm not sure it affects it at all. Let me draw an analogy from another area. Many years ago when I first taught CA at Columbia University, I had a small undergraduate seminar in which we worked on data from a radio talk show, the Brad Crandall material. Now when we were working with that material, one sort of proposal that students would sometimes put forward as we were examining a segment is that "they're doing X because it's on the radio." People often said that when they couldn't figure out something more penetrating to say about the data. It was problematic because you could say that for just about anything. So I instituted a kind of constraint on proposing that line, and that was that one could only invoke that account when one could show in the data some reason for invoking it, that is, offer some affirmative analysis which turned on (and illuminated) something specific in the data. It wasn't just that one knew as a kind of contextual background that this was from a radio broadcast; one needed to show *in the data* evidence of an orientation *by the participants* to a broadcast audience. So I want to put the issue of not being a competent speaker in the same category. Once you've got someone who is a nonnative speaker of the language, you can argue that there are places where [their] competence isn't what a native speaker's competence would be, and invoke that as your analysis almost anyplace. I would like to apply the same constraint here. So if someone wants to argue that something is unique, idiosyncratic, and category-specific to some particular interaction (or episode or exchange in an interaction) by virtue of the fact that someone's linguistic competence is problematic, then they accept a burden of further argument which involves specifying what that competency problem is and laying out how, in particular, that is what is at issue in the particular segment of the data being examined. The point is that I don't see that there is anything, in principle, that has to be different from other work in CA.

Olsher: *That's a very interesting answer. I'd like to take a specific example in native speaker interaction. An inter-turn silence or gap between turns doesn't necessarily mean anything in particular, but, in context, inferences can be drawn, and that might be seen in a different light when nonnative speakers are involved.*

Schegloff: Absolutely. I think that is the right sort of example to offer back, and I think it's one of the major places where one could suspect there might be a modification made by the co-participants. It does not require any change in the whole analytic strategy of CA because it's exactly the sort of thing we have in mind by recipient design. I think it's precisely things like delays—especially ones like those you allude to, ones that otherwise might be understood as prefiguring disagreement or misalignment or things of that sort, which can perfectly well be written off

by either a native speaker talking to a nonnative speaker, or a nonnative speaker talking to another nonnative speaker, as reflecting greater "processing problems," or the like. I think that's entirely plausible. But it's one thing to have it as a vernacular intuition, and another to show in detail *how* it works, *where* it works, *that* it works, and so on.

The global question that we're talking about here is: Is there some major transformation in the practices of analysis that has to be introduced by virtue of presuppositions otherwise made by conversation analysts that aren't the case for this domain of participation, so that how you go about doing analysis has to be altogether different? I remain skeptical about that because I think notions like recipient design are a very formal kind of vehicle. It's open to whoever the recipient might be, whatever the categories or terms of recipient design might be, and whatever the aspects of the talk on which they're brought to bear might be.

So what would be involved here? Well, one thing would be to show that for the recipient, who you want to argue isn't interpreting the gap in the way it might otherwise be interpreted or analyzed, is oriented, among other relevancies, to the categorical membership of the speaker as a nonnative speaker. Now you have to be able to *show* that somehow, and that might involve showing how nonnative speakers are "doing being" nonnative speakers, thereby making relevant their categorical membership. The point is that they're both oriented to the fact that one of them isn't a native speaker. Once you've shown that, you want to examine the particular places that seem to invoke it. And maybe you want to have a kind of global relevance for certain sorts of events, for example, what we talk about as "overlong transition spaces," or even turn-internal pauses. For example, Gene Lerner (1999) talks about how "delicates" are often displayed by a little hesitation before the utterance is produced. If the speaker is a nonnative speaker and you can show the orientation to the fact that he or she is a nonnative speaker, it could be proposed that the common interpretive resource is neutralized, so that when someone who knows they're dealing with a nonnative speaker hears a silence, they figure it has to do with finding the right word or taking a moment longer to fully grasp a preceding turn, which is a different sort of an issue for a nonnative speaker. So silence could become, for example, a key resource for grounding a claim for the relevance of the categorical identity of the speaker as a "nonnative speaker."

Wong: *One of the hallmarks of CA research is its ability to get at members' practices in talk and social structure. In this regard, the analyst and the participants in the data examined are, more often than not, from the same group, culture, or language. Isn't there, then, a problem for a CA analyst when confronted with nonnative speaker data because the researcher and the participants in the data analyzed may not share a set of cultural practices or interactional resources*?

Schegloff: A couple of things are to be said here. First, I thoroughly embrace the premise of the question. I basically think that people ought to work on materials in

a language and culture that they're native members of, so that all the native intuitions, whatever those are, are mobilized. How it happens that people can get trained in a language that's not their own is quite opaque to me, almost a miracle. How it is that people come to UCLA knowing the English that they do from Japan, Taiwan, Korea, Finland, Sweden, Germany, etc., and somehow, using American English materials, learn how to do CA analysis, and then take it back and do it on Swedish, Finnish, or Korean materials, I don't understand. But it happens, and it seems that's the ideal way. Ideally, people work on their own language and culture. Okay, but we can't always have the ideal, or have it in all respects. I just think we ought to have it where we can have it and as much as we can have it.

But what about where we can't have it? I don't think it's as hopeless as the premises of the question suggest. One basis for this is my own experience in working with people who are working with materials to which I have no direct access at all. Several instances come to mind, but I'll just mention one of them here. It involved a student from Korea with whom I was working on Korean material. Now I don't know any Korean, and I know virtually nothing about Korean culture, kinship and so on. We were working over some fragment of conversation, and I heard myself asking the following question, and I had no idea where it came from because I don't know anything about the culture of this society. I asked the student: Is it the case that there is kind of a position in Korean society where you have an older brother who isn't actually a blood relative, but plays the role of older brother? He said "Yeah." Well it came out of the data! It turned out that I didn't have to know that about Korean society in order to recognize it in the data. The way the talk was going just suggested that there would be such a person, or, as sociology and anthropology refer to it, such a role in the structure of the family. But I needed the student's knowledge to confirm the viability of the observation.

So it seems to me that, if we understand how interaction works, it's not that you've got to know in advance about the culture and the language in order to be able to get at the interaction, although in many instances you have to. The point is that you're not without resources. The implication of the question is that analysts can't do anything. Well, the techniques we have access to allow you often to make a pretty good guess about what's going on, and tell you where else to look if you don't have a member of the culture standing by as co-analyst confirming.

Wong: *Firth (1996) refers to two features of lingua franca talk in the form of two maxims, one being 'let it pass' and another being 'make the other's abnormal talk appear normal.' These rules or maxims are hard to get at using a CA framework because of the constraint that if interactants do not orient to a problem and let it pass, then the analyst doesn't deal with the problem and lets it pass as well. But certainly these practices of conversation appear to be somewhat different in native-nonnative talk than they are in native-native talk. We do seem to hold native speakers to a different standard than we do nonnative speakers when it comes to producing correct or acceptable speech. How can CA deal with these practices of*

native-nonnative conversation, namely, 'let it pass' and 'make it normal,' since they seem to run counter to the notion of procedural relevance[3] (Schegloff, 1991) yet they seem to make sense on an intuitive level?

Schegloff: I disagree with the premise. First of all, Firth (1996) attributes the notion of 'let it pass' to Cicourel by way of Schütz or Garfinkel. Garfinkel may have had it from Schütz as well. But they weren't consulting nonnative conversation, social life, or phenomenology in proposing that as a practice of mundane discourse; they were consulting ordinary discourse. So I think that, though Firth may want to put a particular twist on the notion of 'let it pass' for native-nonnative or nonnative–nonnative discourse, it may be misguided to say that it is distinctive to such discourse. And there are some fairly obvious places, I think, where this comes out. You'll find, for example, in some story-tellings, and in other types of turn which are projected to be extended or "multi-unit," that one of the things that interpolated 'uh huh's do is to pass an opportunity to initiate repair (Schegloff, 1982). Now sometimes when people pass an opportunity to initiate repair, it's because they don't have any problem with the preceding talk, and therefore there's nothing to initiate repair on, so nothing has been foregone in passing the opportunity. But there are other instances in which there *is* a problem, but what people do is let it pass—for now. It's not 'let it pass;' it's 'let it pass *for now*,' in the hope, expectation, or in allowing for the possibility, that things said subsequently will clarify the problem and avoid the need to initiate repair, and if they don't, then you can ask later on when it's next relevant. So that's a version of 'let it pass,' and it informs lots of ordinary talk among native speakers. Now it obviously may have a very special application where someone is not talking well or clearly.

There's another place where we see it clearly in native-native talk. If you remember early in the self-correction paper (Schegloff, Jefferson, & Sacks, 1977), we made the point about replacing the notion of 'error' with 'trouble-source' and the notion of 'correction' with 'repair' and 'repair initiation.' One of the reasons was that there are some things that are clearly 'errors' which do not have repair initiated on them. And those are all instances of 'let it pass.' You may remember the data fragment in which an "Avon lady" says (Schegloff et al., 1977, p. 363), "And for ninety-nine cents uh especially in, Rapture, and the Au Coeur which is the newest fragrances, uh that is a very good value," to which the customer responds with "Uh huh," thereby "letting pass" a non-agreement between singular verb and plural subject. You just don't find very many grammatical corrections with native-native talk. Even when there are grammatical 'errors,' recipients let them pass.

Still, the question was "What do you do about it?" If the constraint is that you have to show relevance to the parties in order to have it as a real thing, and the parties "let it pass" and thereby apparently do *not* display its relevance, what do you do about it? Nothing I've said has addressed the problem; I've just been suggesting that it is not distinctive to non-native talk, even if it may arise there in

aggravated form.

Actually, I think several issues are involved here. The first question is: What sort of result is wanted? Do you want to end up with compelling, warrantable analyses of particular, singular episodes of interaction? Or is the goal to end up with accounts of *practices* of conversation, of *recurrent phenomena* in conversation, across diverse episodes, participants, settings, etc.? If what you want to end up with is essentially a clinical result, in the sense of a definitive account of something "anomalous" in a single case, I think there can always be some principled doubt with the analysis of a particular stretch of interaction. I used to refer to this (before the disappearance of the Soviet Union) as the KGB problem: "What if these people mean X by Y?" (e.g., "Hello" means "The submarines leave at midnight"). Well, you can't ever show it's not the case that the parties to some interaction are using the words or constructions in some distinctive way or private language; in the long run, you always have to be agnostic about any analysis as to whether that was for those people at that moment exactly how they understood what went on in that exchange. That's a real problem for people who have an applied discipline and who need to have a compelling individual diagnosis for each case.

But if the goal of inquiry is the establishment and elucidation of recurrent phenomena and the practices by which they are produced, then work will be grounded in collections of single instances, and collections provide a different sort of resource for addressing the problem of relevance. A *collection* of extracts can display the way persons (or persons in a certain sequential or interactional context, or persons doing some activity) do some action. Suppose that you find a particular episode in which someone has done it differently and the interlocutor(s) "let(s) it pass." But how can you say that they "let it pass" ? The question is: How can you *analytically*, *defensibly* claim that there was an "it," if that claim cannot be grounded in the displayed orientation of the recipient(s)?

One way is to lift the level of analysis from definitive explication of a single fragment to the fragment set in the context of certain practices in interaction. So if you can show the way that that type of action is ordinarily done, and you have some empirically grounded account of it, and a speaker has departed from that practice in some interactional episode with no observable consequence in the conduct of interlocutors, then you can proceed in the following way: You can return to your data base and see if you can find other such instances (in which case you have expanded your account of the practice); or you find other instances of that anomalous form of the practice in which there are consequences in the uptake by recipients; and maybe it's for a person from a particular source community, as in the data you've examined, Jean (Wong, 1994, 2000a), in which the participants are all native speakers of Mandarin, now speaking English, and when they do a certain thing, there's a practiced way of doing it, and this is a departure from it. Then that's the way you try to establish that there was some sort of problematic thing that was done (see Schegloff, 2000; Wong 2000a).

So the issue is: How do you show that something was problematic if you don't show the participant in some particular interaction orienting to its problematicity? And one way to do it is to have a collection of data in which you have a notion of a category or class of actions someone is doing, or practices or reference forms or whatever, and this instance departs from that, and that's your grounds for saying it's problematic. It's a departure from the practice used to do this action, and it's not picked up by others. (For more on this methodology, see Schegloff, 1996a.)

Olsher: *So one piece of advice we could get out of this is that if we're taking a conversation analytic approach, our analysis of the practices of any speakers should never rest solely on our intuition or on any prescriptive notion of what language should be. Our analysis should always be grounded in a body of data examined with the same analytic method.*

Schegloff: That's right. I mean surely you start with an intuition. Then the first question you ask yourself is: What is it in the data that made me think that it was doing such and such? Now the key phrase in that question is what *in the data,* because you can think of all sorts of elements in your own biographical experience that underlie that "intuition," for example, that you had an uncle who always did that. But, of course, that's totally irrelevant. Then the next question is: Is there any evidence that anybody other than you understands the talk in question in that way; most importantly, is there any evidence that the *recipient* understood it in that way? And another question is: Is there any evidence that this is something that is not idiosyncratic to that speaker? So now you look for other instances of that candidate phenomenon. Of course, you need to have an analytic account of what that phenomenon is, and look to see whether, in that description of it, you find other instances which also seem to be doing the same action or which seem to constitute the same phenomenon, and whether, wherever you find it, you find recipients understanding it in that way. If you have substantial evidence to that effect, you've got something. If you find instances that don't look like that in one respect or another and find that they nonetheless can be understood as instances of the same phenomenon or practice, you can start to broaden what actually serves to implement the action or the practice that you've been examining. And then you can try to take what you've learned from such a collection of instances, bring it to bear on particular instances, and see how the account works for a particular instance.

Wong: *What domains of inquiry would you consider of potential interest if you were to use CA in an explication of conversational structures of native-nonnative talk or nonnative-nonnative talk, and why those domains as opposed to any others?*

Schegloff: I plead innocent! I don't think I can speak to that.

Olsher: *Well, one caveat is that you're not completely unfamiliar with this domain of inquiry because you have advised at least two students I know, namely Jean [Wong] and Irene [Koshik], who have worked with some such data.*

Schegloff: That's very kind of you, but I treat native-nonnative discourse much as I treat a foreign language. I know what I consider relevant for me to be knowledgeable about a domain; it would be demeaning to Jean and Irene to claim that, on the basis of the exposure I got looking over their shoulders at their material, I'm knowledgeable about that domain (see Wong, 1994, 2000a, 2000b; Koshik, 1999).

But I'll answer the question anyhow, not on the basis of having any expertise in nonnative discourse, but because I'm going to answer it without naming any domains of principled special interest, because I don't think there *are* any *a priori* things that people ought to look at. I think the most productive way to work is to take your data and try to make observations just as you would with native-native speaker talk. I don't think there's anything different. As I said at the end of the paper that's going to be published with yours, Jean, in *Applied Linguistics* (Schegloff, 2000; Wong, 2000a), nonnative talk is just a sub-area in the study of talk-in-interaction. I think you work on it in the same way. You get the materials, you transcribe them, that gets you as intimate as you'll ever get with the materials when you're attentive to them in that way. You make observations about what you are examining even if you have no idea what significance or further thinking it will set off, if any. You write them all down. You see some things start to pop up recurrently; you notice something happening over and over again.

In fact, if I were to answer the question in the terms in which it was asked, I would be discouraging discovery, because I would be encouraging people to see in the data only things that were already known. They would have to be already known for me to mention them as domains of interest. But the big payoffs are always when somebody notices something and it *doesn't* have any name. People are sometimes disinclined to work on such things because they don't have a place in the map of CA topics that they've read about. But that, of course, is the hallmark of a discovery—that it's something that was not otherwise known. So the point is, again, a methodological or procedural one. You sit down; you make observations; you try to describe them as carefully as you can. You keep on doing that, and you keep on doing that. And you keep on doing that, and then hopefully you start to find, "Oh I've seen something like that before!" And you find the prior thing(s) you were put in mind of, and yes, sometimes they do look like the same thing, and sometimes they don't. You get some embryonic notion of what that thing is, and you try to get more instances. You have no idea of where it's going to end up. You have no idea to begin with whether it's a little thing, whether it's a footnote to a bigger thing, or whether it's a big discovery. Or whether, in the end, it's nothing at all. You never know that in advance. That's why it takes guts. You have to be prepared to take a chance and to persist with it, and hope that it will turn into something that you can build a livelihood on. But to say in advance this or that is

the best thing to work on, even if I *knew* something about the domain, it would be counterproductive for me to say; how much more so when I don't know the domain.

Wong: *In native-nonnative speaker interaction, equal participation may be uncertain due to an unequal distribution of linguistic knowledge or language competence. There is even greater potential for talk to go awry, a greater risk for misunderstanding. Here I cite your recent work (Schegloff, 2000) and mine (Wong, 2000a) on delayed other-initiated repair. Does this imply that a generic component of conversation, namely, the organization of repair, might be differently constituted with respect to nonnative talk?*

Schegloff: No, I don't think that they're differently constituted. I think that the product of the papers that we're publishing together is, if anything, quite the contrary. There is still the same set of positions in which repair gets initiated. There's always the possibility that the implementing linguistic resources for doing repair work differ depending, as I once put it, on the linguistic inventory that society makes available for its participants (Schegloff, 1987). I don't think that the organization of repair is different in different languages, or at least I haven't seen any evidence that it is, even though it's true that, if you actually look at instances of repair, they will look different because various of the parts that comprise an episode of repair—such as how to do its initiation, how to do the repair proper, how to do its aftermath—those may differ depending on the linguistic inventory of the language being deployed by the speakers.

The case of the work on Quiché that I discussed briefly in that paper is instructive. As you know, in English the most common two forms used to initiate same turn self-repair are cutoffs and sound stretches. And in English cutoffs and sound stretches are not phonemic for the language. So they're available for other kinds of uses, like initiating repair. As it happens in Quiché, both *are* phonemic for the language; surprise of surprises, self-initiated repair is *not* initiated with cutoffs or sound stretches. Instead it seemed to be the case (based on limited fieldwork and data collection) that unusually long sound stretches, which are *not* phonemic in Quiché, were used for self-initiation of repair in same turn. A place for self-initiation of repair is not a place in this world other than by reference to CA's ways of parsing and characterizing the talk, and, of course, if we're right, the *participants'* parsing of one another's talk. But there *is* such a place, apparently, for the Quiché speakers of the Guatemalan Highlands, just as for the American speakers of English, even if what they put there is different (although doing the same job).

So what should we say that the organization of repair is the same or that it's different? Well, I want to say that it's the same—the structure of doing repair is the same. It involves an initiation, it involves the repair proper, it involves the way you come out of the repair back into the turn (in the case of same turn repair) and so on. And those issues and places are the same for Quiché speakers and English

speakers. The job of repair initiation is to alert the recipient that the next thing to come out of the speaker's mouth may or may not be more of what she or he has just been saying; that's the same job in both English and Quiché. But what you do with the resources of the Quiché language to accomplish that are very different from what you do in the English language, and that's because of what the Quiché language and English language have as their phonological and lexical resources.

So I don't think that the organization *of repair* is different. I mean I wouldn't stake my life on it; it could be different. But if we found someplace where it did appear to be different, that would just serve to pose the problem: How can we describe the organization of repair in a somewhat more formal and abstract way that yields all the instances we knew until that day, and the new one that someone came back from the field with, as alternative specifications? That is so even though the linguistic resources that are used to implement those structures and those practices may be different, and may be adapted to aspects of the host language, the host culture, or whatever.

Olsher: *In the study of nonnative discourse, attention often turns to language classrooms since this is not only the professional area of many who are interested in nonnative language use, but is also one of the main locations where students of foreign languages have the opportunity and need to use the language they are learning. Yet, as has been noted by Kasper (1997) and others, language classrooms offer a more limited range of interaction to nonnative speakers than they face in their target speaking contexts in the outside world. The institutional roles of teacher and student, along with their associated rights and obligations, are both constructed and reflected in the discourse. Does it seem practical or profitable to bring the tools of CA to the study of nonnative discourse in classroom contexts? What would be the potential value and problems with working on such data, including teacher-fronted classroom talk, one-to-one tutorials, small group work, role plays, problem-solving tasks, and the range of things that might occur?*

Schegloff: Once again I plead ignorance about the domain, but I do have a couple of responses. I think the problem with classroom talk isn't just that there are often pre-assigned roles of teacher and student because, as you said, a lot of what goes on there is that students are divided up into little clusters, and they are told to talk to each other. That's the problem—that they're told to talk to each other. It must be a nightmare for the students who are trying to learn English, because not only are they having trouble with the language itself, but they've been given an interactionally impossible task, which is to make talk when they have nothing to talk to one another about, or if they do have something to talk about, that's not where they want to talk about it, and not with these other people listening to them. It's a very difficult situation, so I don't think it's just a matter of the hierarchy between the teacher and the student and the instructional roles that differentiate them. I think Wagner (1998) is quite right in his article about "doing being a guinea pig." It's guinea

piggish sort of stuff in the sense that guinea pigs, when they're put in experimental mazes, although being given food, are induced to perform something that they wouldn't otherwise perform for their own good; that's the whole point of the enterprise. And that is a problem.

I also understand why professionals want to look at this setting because it's the site of their own professional practice. They want to do as well as they can, both for themselves and for their students. The setting in which they get a chance to do their job is the classroom, so they want to know more about their classroom. Just like surgeons are concerned about antiseptics, because that concerns their work place, and they know that if they didn't know about it, people would die from infections. It's reasonable to want to get as good a knowledge as is possible of the setting where you do your work so you can do it better.

Now there are two separate issues here. First, is talk in the classroom setting good talk in which to examine native-nonnative discourse or nonnative-nonnative discourse? My answer to that is no. I think that a lot of the concern in the applied linguistics literature that you called to my attention is warranted.[4] The talk that language learners are going to have to do when they're not in the hothouse of the classroom is situated in the real world where they have real things to do, and that's the talk that people ideally should be recording and studying if they want to understand what the real world problems are for those who are speaking a language that is not their native language. I understand that that's its own problematic distinction, but, leaving that aside, I think that those are the settings you need to get.

But now back to the classroom, because there are two sorts of things worth further comment. One is that it's an institutionally defined environment for a certain kind of discourse and interaction. It's the one in which the learning and the teaching, the official learning, the official pedagogy takes place. In that respect, of course, it's as worthy of study as any other institutional environment, and, given its "double payoffs," maybe a little bit more so for people who want to make their careers and lives serving this class of problems faced by nonnative speakers. I think it's subject to particular difficulties, not particular as compared to other classroom settings, but in particular as compared to other institutional settings. And that impacts differentially on different areas of inquiry that enter into CA work. One of them that is the most impacted is research on repair.

One of the hallmarks of repair is that whatever was in progress—a turn, a sequence through which some activity was being pursued, etc.—is put on hold temporarily in order to deal with some trouble or problem in speaking, hearing, or understanding the talk. When the ongoing activity is concerned with learning, explaining, understanding, problems in understanding, correcting, and the like, it can be quite difficult to keep distinct the "mainline" sequences of activity on the one hand, and problems of repair that arise in its course on the other hand. Such environments then pose very special kinds of problems in discerning where the mainline activity stops and repair issues arise. I don't think these problems are insoluble, but they underscore that problems of repair are very different kinds of problems

than people who wanted to work in that area may have had in mind to work on. Researchers may treat this distinction as a technical obstacle to their work, rather than what the work should be focused on. They may want to get over it as quickly as possible so that they can get on with their research, when that—the relationship between understanding the instructional material and understanding the talk involved in presenting and receiving it—may be at the heart of their research.

Wong: *I'd like to turn now to the notion of grammar. You have written about interaction and grammar and have looked at turn organization in this regard (Schegloff, 1996b). What thoughts come to mind with respect to issues concerning interaction and grammar when the context involves nonnative speakers or native-nonnative conversation?*

Schegloff: One product of the work on grammar and interaction (for example, that collected in Ochs, Schegloff, and Thompson, 1996) has been that grammar is a resource for accomplishing different actions and that things that one might not have thought mattered turn out to matter. For example, I tried to make this point (Schegloff, 1986) with the difference between 'Are you awake?' and 'Did I wake you?' Now someone who is still working at a basic understanding of how the grammar of some language works might stand in a very different relationship than a native or fluent speaker does to the use of the grammar, and of such alternative grammatical realizations of a "same question," as having more than grammatical import—as having *action* import in the interaction.

Firth (1996) talks about the fact that the "order at all points" premise, to use one of Sacks' formulations of it (or the other version which is that nothing in principle can be ruled out as being relevant) might serve one ill if one were looking at native-nonnative or nonnative-nonnative conversation. We've come back to that by way of this grammar question, and I think that there is a slight misunderstanding here, because it's not literally "order at *all* points." And it's not that everything hearable is a locus of order. It's that nothing in principle can be *dismissed* in advance as possibly being a locus of order. There are lots of things that are not a locus of order—that turn out not to be (or to have been) a locus of order. It's the job of the analyst to sort out which ones are and which ones are not. And it's an issue for an interlocutor to figure out which ones are and which ones are not. We always talk about the basic question for interactants, and, therefore, for the analyst, being "Why that now?"—for any 'that,' for any 'now,' and for any 'why.' The question is: Does everything have to lead to some interpretive consequence?

One of the things that the work on interaction and grammar has tried to do is to show that some things which might have been treated just as grammatical alternatives to each other are interactionally consequential, and that speakers deploy them differently and recipients often will get the point and understand them differently—in other words, that the particular grammatical realization is a 'that' in the 'why that now?' question. But not everything is, and it may well be that, when

dealing with a native speaker in conversation with a nonnative speaker, the grammar may be treated by the former as not a locus of order in the usual way among native or fluent speakers. And once one realizes that grammar is not a resource which this co-interactant competently commands, how much latitude a recipient has in blocking the usual interpretive process is also unclear. It may well be—and again it would have to be shown—that the grammatical choices are not grammatical choices in the same sense as they are for someone who's speaking with native fluency.

Wong: *Do you think that CA might benefit from expanding its research agenda to include native-nonnative talk and nonnative-nonnative talk, even if as a point of departure for understanding native speaker talk? For example, one might ask: What is "native" about our mother tongue and mother culture? What does it mean to be a native speaker as opposed to being a nonnative speaker? What does it mean to be a native speaker as opposed to being a fluent but nonnative speaker?*

Schegloff: In the terms in which you've asked the question, I really can't answer. Here again, I have a problem with the premise of the question. The problem is where the categories "nonnative speaker," "fluent speaker," and the like come from, and whether those are terms in which someone undertaking a piece of research would think of the work in the first instance. Well, one thing we could do to advance CA as a field is to do some work on native-nonnative talk. I wouldn't recommend that anybody take on a piece of work in those terms. If they have some reason to look at native-nonnative data, look at native-nonnative data. But don't look at it as "native-nonnative" unless you have to—that is, unless something in the data requires you to because those are the terms in which the participants are conducting themselves. It's no more transparently relevant that the parties be characterized as 'native' or 'nonnative' than it is that they be characterized by gender, by race, age, etc. And it's not the case that once it's been made relevant at the beginning of an interaction, it's therefore relevant for the duration. It could be relevant or it could not be relevant and, here again, the insistence on its relevance and on making those categories primary categories in thinking about the data, I think, is an imposition of the professional interests of the people who pick it to study because of its relevance to their professional commitments. I'm not putting down those professional commitments. I just want to bracket that that's the source, in the first instance, of characterizing the data in this way, and of asking the question whether that kind of work would benefit the field. I don't see any reason to think it would benefit the field any more than cross gender or same gender, cross race or same race, cross anything or same anything, right? I see no reason to privilege any of those other categories unless you can show why the materials make it relevant—that is, the parties are doing it.

Once you've got that to warrant the inquiry, then we'll find out if it is going to confer any distinctive benefits on the field. So this is not meant to be a discour-

agement; it's just meant to contribute to a kind of self-awareness about where this native-nonnative interest is coming from. There's nothing wrong with it as long as you're clear about it so you don't *insist it* into the data, so that you don't insist that the participants be as preoccupied with native/nonnative speakers as you are . . . unless *they* are. If they are, of course, you need to recognize that and trace out the consequences and the way it figures in the way they conduct the interaction. But that's because *they're* preoccupied with it or oriented to it, not because *you* are.

Nor do I think you should give up working on the data if it turns out they're not preoccupied with it or oriented to it, because then it would be a self-fulfilling prophecy. Applied linguists would only seriously examine data in which the parties were preoccupied with the fact that they were not native speakers. We would never learn about all the nonnative speakers who are *not* preoccupied with it and for whom it's a totally incidental thing about their interaction. If you're going to have a field that is concerned with nonnative speakers, one of the first things you would want to know is that in 65% of the cases or 25% or always or never, it really doesn't matter to them, or it matters but it doesn't actually affect how they do the interaction.

Olsher: *So you're really invoking the procedural relevance of the identity in the interaction. And you're also suggesting that the fact that the identity may not be procedurally relevant to the talk is something that probably should be of interest to people who are interested in describing nonnative talk.*

Schegloff: That's right. The reason why in my (1991) paper I discriminated between relevance and procedural consequentiality was that in many instances the identity will be relevant. But if you have to show how it's consequential for what they do in the talk, that's another story. The other way around is unlikely; if you can show it's consequential—that particular sorts of talk have consequences and they're not understood by the interlocutor as being related to the fact that this is a nonnative speaker of English—then the person could be in big trouble because it could be thought that they're just being ornery rather than that they just don't know any better. But I think both things are the issue and, no matter what interests you bring, you want as much as possible to cultivate the interests not because *you've* brought them, but because, having brought them, you can find them in what was already there.

Wong: *Have we left out anything with respect to CA and the analysis of native-nonnative talk on which you'd like to comment?*

Schegloff: Yes, one thing. I'm not discounting the usefulness of talking about the area in the way that we have, and there is really no alternative. But for some people who will read this, this will be the mainline activity—*talking* about it—and there's no more important alert than to warn people off of that. If that's what people get

out of reading this, it will be perhaps useful for their academic careers; it will be another "entry" that they can include in teaching a survey of "approaches." But I don't think of what we do in CA as an approach. Approaches are ways of getting on a freeway; they don't take you anyplace. The freeway takes you someplace; the approach just gets you onto it. And so all the talk about it and all the characterization of CA as compared to this or that are like waiting in the anteroom and talking about the experience you're going to have when you go into the theater or something like that. It's not until our colleagues actually engage with materials, and try to make sense of them, and understand how they're orderly, how they are organized, how they underlie and underwrite what people do with their social lives in interaction. Not until then will they have a clue of what we've been talking about for the last two hours. They'll think they have a clue, and that's what makes it more dangerous, because at least if they understood fully that they didn't have a clue, then they would be prompted to go on and to get into the thick of it, but often people think "that's it." To be able to give an account, to say what this or that scholar said, that is what it's all about. It may be what having an academic life (or one type of academic life) is all about. It's not what doing conversation analytic research is all about. I suppose that's self-evident, but too many self-evident things often escape notice. So my sermon is always, in the long run: It's doing the work that matters, not the talking about it, except if that happens to recruit people who do it. Then it was worthwhile.

NOTES

[1] The interview took place at the 85th Annual Convention of the National Communication Association held in Chicago, Illinois (November, 1999). Jean Wong was a student of Emanuel Schegloff. She completed a master's thesis, doctoral qualifying paper, and dissertation under his primary supervision at the University of California, Los Angeles. Her dissertation (1994; see bibliography) involved examination of features of the organization of repair using native/nonnative speaker English conversation. David Olsher is a doctoral student in Applied Linguistics at UCLA. His research interests include broadcast news interview talk and classroom discourse, conversation analysis, and functional linguistics. The copy editing assistance of Monica Link for this interview is gratefully acknowledged.

[2] Prior to this interview, Wong and Olsher provided Schegloff with a sampling of the literature in applied linguistics which has addressed the issue of using CA to further our understanding of concerns in second language acquisition. References provided included: Firth (1996), Firth & Wagner (1997),Wagner (1996, 1998), Seedhouse (1998), and Wong (2000a).

[3] By *procedural relevance,* we refer to Schegloff's (1991) discussion of how aspects of social structure, such as the interactional setting and participants, can be analyzably relevant to some aspect of talk and interaction. The term Schegloff uses is *procedural consequentiality,* and this choice of words is designed to emphasize a focus on what the participants observably orient to through their specific micro-interactional practices. Schegloff argues that if we want to claim that a participant's identity (e.g., as female, as a doctor, or as a nonnative speaker) is relevant to some discursive practice, then it is not enough that the characterization be true, not enough that the characterization mattered to the participants on the occasion. It must matter for the particular aspect of the interaction under consideration, Schegloff argues, and the analyst must be able to demonstrate that. "There is still the problem of *showing from the details of the talk or other conduct in the materials* that we are analyzing that those aspects of the scene [or identity] are what the *parties* are oriented to" (p. 53).

[4] See note 2.

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