

## CHAPTER 8

# Response\*

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The interview presented in Chapter 2 of this volume has given me more than ample opportunity to express myself (to “sound off,” in the American idiom) on a range of matters related to my work and that of my closest colleagues, and to take up issues raised by some of the reactions to that work in neighboring disciplines. It therefore behooves me to exercise discretion and self-control when presented with yet a further opportunity to sound off. In what follows, then, I try to suppress my garrulous side in favor of my more terse one.

My good fortune in having colleagues like John Heritage and Chuck Goodwin is exemplified not only in the quality of the original research work that they do, but also in the generosity displayed in their contributions to this volume. The synoptic overview of the disciplinary nexus and intellectual ancestry of the work in conversation analysis provided by John Heritage is tellingly complemented by Chuck Goodwin. His is an intersecting account of much of the same terrain, as encountered from the trenches of a then-graduate student’s astute and insistent efforts to find sensible and effective resources for coming to terms with the naturally-occurring events of people’s lives in the world, at the level of detail to which we had long been alerted by work in the arts and letters, and which was no longer to be denied to observant workers in the social and human sciences. It has been my good fortune to find readers and colleagues who were attracted by the same things that intrigued me, who were able to find something valuable in what I have done, and who, by making something of it and going beyond it, have enhanced whatever value it may turn out in the longer term to have had. If conversation analytic work ends up having made the contribution I think it can make, it will because of the convergent and cumulative work of a community of inquiry; here, as elsewhere, the central note is the shaping and realization of an enterprise as a collaborative and interactional outcome. John and Chuck have been important figures in that enterprise, and I am grateful to them for the generosity of their comments.

The other contributors to this volume I have never met, and I thank them for their willingness to invest time and effort in it. The stances they take up are quite different from one another, of course, and reflect the very different scholarly, professional, scientific and academic contexts and commitments characterizing their work. In keeping with my above-taken pledge, my responses to their several contributions will be brief, but, I hope, to the point.

There are two elements in Ruth Lesser's contribution which I would like to address. One is the richly informative account of some of the ways in which work in conversation analysis has been taken over by language pathologists, aphasiologists and other clinicians, which has enriched their capacity to do their work — both research and therapeutic — and has been subjected to modifications in coming to address the contingencies of work on so-called "disordered language." The work which Lesser reviews goes well beyond that with which I was familiar, and it is both exciting and gratifying to know of the appeal of CA to workers in this historically quite separate area, and of its anticipated efficacy for improving the lives of those who suffer from the limitations of language impairments. To my mind, this is a vindication of our hope that coming to understand the underlying organizations of practice which inform the organization of any talk-in-interaction should contribute to our understanding of *any* setting in which talk-in-interaction occurs. Even though this may initially serve to introduce "problems" for researchers and practitioners in this field — as Lesser terms the areas which serve to organize her review of the literature — it will serve (one hopes) to bring the field into more intimate contact with the naturally occurring home of the conduct and troubles which supply the field's mandate.

Lesser's discussion vindicates as well my sense that workers using CA work and methods ought, as a matter of course, to try to stay in touch with both work in their special area — aphasia, stuttering, etc. — and with ongoing research and the data of "ordinary conversation." She remarks, for example, on a finding which registers "an aphasic tendency to use words which have already been used by another speaker" (this volume: 145). In work which I have been doing and teaching for some time, but which remains unpublished, such a tendency has turned up as a recurrent feature of ordinary talk-in-interaction by persons with no known neurological compromise. It may make quite a difference for both students of "normal" talk-in-interaction and of talk which is impacted by untoward neurological events to know that this practice is common to both, and is not itself (for example) a "symptom" of some neurological disease process. Perhaps some day it will be part of the expected profes-

sional/scientific activities of language pathologists and aphasiologists to take a continuing active research interest in ordinary talk-in-interaction as part of their growing leverage on the characteristics of “disordered” talk and how it can be addressed.

I will be briefer in my second remark on Professor Lesser’s contribution. The point of departure in the interview for her discussion of CA’s bearing on work in language pathology and aphasiology is my interest in the “describing the behavior” part of the recent surge of interest in the “neurobiology of behavior.” I take her point about the difficulty, at the current stage of work and of technology, of bringing brain-imaging methods to bear on the sorts of things conversation analysts describe. Still, one important aspect of CA work is its commitment to getting at the practices and resources of ordinary conversation in the terms in which they are oriented to by participants. That is, that they are natural categories being discovered, and not analytic ones being imposed. If that is the case, then these categories of resources and practices must inform behavior or conduct in its very course of production, on the one hand, and uptake, on the other. And this is just the sort of thing which should “interface” with a neurological substrate, if there is in fact an interface to be specified. Attending such a possibility may help to encourage the development of a technology appropriate to the implied inquiry, even if it does not now exist. If, as Lesser reports at the very outset of her contribution, functional magnetic resonance imaging can capture such categories as “lexical-phonological” and “semantic,” or “practiced/automatic” behaviors and “novel” ones, then why not “turns” and their possible completion, or “first-pair parts” and “second-pair parts” — categories of conversational events which demonstrably have extremely robust recurrent consequences for the generation of behavior in talk-in-interaction?

Rick Iedema is coming from an altogether different direction. He seems to wish to treat my work as a foil for his own commitments and preoccupations. He has collated extracts from the interview and various past papers of mine to construct a supposed representation of my “position” on some matters on which he has a different position. I say a “supposed representation,” because he has selected his quotations carefully and refracted them through a prism of his own making to project a position on to my work which is designed more for its vulnerability to his critique than for its adequacy as a representation of the object of the critique. How or why else would he come to the astonishing (to me, at least) assertion that “Schegloff’s inquiry [...] is ultimately a critique of understandings grounded in everyday commonsense: a *démasqué* of false con-

sciousness which normally permeates the common and everyday” (Iedema,, this Volume 67)?!! To rectify only the misreadings or misinterpretations of the interview in the first several pages of his contribution would exceed the limits of sensible investment in this exchange. Readers so inclined can see for themselves by (re-)reading the interview in juxtaposition with Iedema’s account of it.

Pirkko Raudaskoski’s contribution, like Ruth Lesser’s, considers the possibility that CA may have some implications for domains of event different in a greater or lesser degree from that which gave it rise and which supplies its core subject matter. I share her hopes that this may be the case. However, her enterprise is rather further removed from ordinary conversation than the disordered talk implicated in Lesser’s field, and is, I fear, rather more problematic. As it happens, the material which Raudaskoski has examined — humans engaged with a computer — does not really involve “conversation” or “interaction” in the usual sense in CA at all. Rather, it involves dealing with a “text” whose “sender” is not present. Raudaskoski means to investigate “how asynchronously produced language is interpreted;” she is concerned with misunderstanding, and undertakes to bring the resources developed in CA under the rubric of “repair” to bear on this problem. The entire undertaking, however, rests on several assumptions which seem to me problematic for the sort of work I take to be at the heart of the interests which I bring to this volume. Raudaskoski writes:

[...] the whole encounter between a text and a reader can be analyzed as social interaction unfolding in time and space which, in addition to the local history of the encounter, also forms an integral context of understanding or misunderstanding. (Raudaskoski, this volume: 114–115)

And following up on this premise, she ascribes to the computer the taking of turns, the initiation of repair, etc.; for example:

Extract (1’) begins with the repair initiator that appeared at the lower part of the screen after a wrong action by B. Now there is a clear connection between the computer’s *turn* in line 102, and what B says in lines 109 (“move the mouse pointer”) and 111 (“click I”) [...]. (Raudaskoski, this volume: 118).

And so on.

Proceeding in this fashion may make sense in the domain in which Raudaskoski works. To me it is deeply problematic.

The encounter between a text and a reader, or between a computer and a user, can *not* properly be analyzed as social interaction, etc., in the same sense

as ordinary interaction between humans can. To assert that is to reveal either a) a fundamental misunderstanding of text-reader/computer-user processes, ordinary human interaction, or both, or b) a willingness to settle for analogic analysis prematurely, that is, before exhausting the effort to describe the object of interest in its own terms. Whenever my students say about some fragment of data, while working with me, “this is like an X,” I always counsel against it. To say it is “*like* an X” is to concede that it is *not* an X — else one would have said, “this *is* an X.” And it is to give up the search for what it *is*, and settle for what it is *like*. From what I know about ordinary human interaction, human/computer “interaction” is not seriously “like it.” At least, that has not been shown, to my knowledge. And considerable mischief and errors can be incurred by proceeding on that basis.

And the text/computer can *not* be seen as taking turns, at least not in the sense in which conversation analysts properly speak of taking turns (and I must assume that that is what is here intended, given the auspices of this volume, and this article being a contribution to it). Again, analogical analysis is premature. In settling for the computer acting like it was taking a turn by writing, “[...] can be seen as taking turns [...],” or by putting ‘turn’ in scare-marks, we are discouraged, even stopped, from specifying exactly what the computer may actually be understood to be “doing” in the kind of interactional phenomenology which Raudaskoski appears to be pursuing. There is now a substantial literature on what is involved in taking a turn in conversation and in timing such a taking, or delaying it, etc., or declining to take it; how that is contingent on a running analysis (i.e., synchronously at a fine level of granularity) by the prospective turn-taker of the turn in progress before — and issuing in — that “taking of a turn”; and so on and so forth — none of which a computer demonstrably does (and cannot do if the whole process is asynchronous).

And since the organization of repair, which is also invoked here, is organized in substantial measure by reference to turn-taking organization and the relationship between one turn and that which follows or precedes it (cf. Schegloff *et al.* 1977; Schegloff 1992, *inter alia*), the relevance of the CA literature on repair in this context is at most analogical. And to so deploy it is to risk misleading readers who lack their own independent access to that literature about what its claims and findings are, and on what they rest.

I must enter one more general demurrer here: it is a central feature of those actions we term repair-initiations that they put the ongoing course of action — whether turn or sequence — “on hold” in order to address some trouble in speaking, hearing or understanding the talk, with resumption of that activity

following solution of the trouble or abandonment of the effort to deal with it. When the main activity in progress is instructional, special care is needed to distinguish actions which promote or advance that mainline activity and are parts of it from those which put it into temporary abeyance in order to initiate dealing with trouble in speaking, hearing or understanding the talk. Difficulties in making this discrimination appear relevant to Raudaskoski's project, whatever else may be said about the invocation of "turns" and "repair" to the understanding of its very different "universe of discourse."

I hope I can be forgiven for not entering into any greater detail into this analysis. I understand that when authors put a piece of work into the public domain, they largely cede control over what will be made of it. It may well be that Raudaskoski's work makes an important or useful contribution to the field in which she works. If so, I hope its effects continue to be of benefit. But they do not serve well a proper grasp of CA, at least as I understand it, even if aspects of CA work have served as a model or inspiration.

Pär Segerdahl's contribution reminded me of a striking utterance in a fragment of conversation presented in the turn-taking paper (Sacks *et al.* 1974: 715). A young married couple are in the car on the way to her parents' home for Sunday brunch, and she says to him (presented here in regularized orthography), "That's a really nice sweater; that's my favorite sweater on you; it's the only one that looks right on you." Although introduced in that paper for a different reason, I am always struck on encountering this utterance by the rapidity with which what started out as a compliment ends up as a critique. Segerdahl starts me off in the company of Husserl, but ends me up as yet another naive believer in the omnipotence of science — no, of *his own* science; he starts me as a noble rebel against the deadening constraints of academic disciplines, and ends me up as a reproducer of that same prison — indeed a *worse* one. What can I say? None of these assessments is warranted, in my view.

I cannot here go through all the misunderstandings which seem to me to underlie Segerdahl's discussion, from his belief that I believe that people do things because of rules like a terminating rule which *makes* them do such things, to his belief that we impute features of our research stance to the world under study rather than using the latter to constrain and shape the former, to the relevance of people's reactions if we presented them with an account of ordinary behavior framed in technical terms, etc.. For a while, I thought that a reading of some of my work written after 1981 would make a difference (as their respective initial notes make clear, the "1984" paper which Segerdahl cites was written in 1972; the "1986" one which he cites is a reprint of a 1968 paper;

he has apparently read nothing more recent), but it became clear to me that nothing would make a difference; that it is a principled philosophical stance that is being exercised here, and is being brought to bear on this work.

Naive as it may be, I have not given up on the accessibility of the world to empirical inquiry; on our capacity to frame accounts in ways that are more or less correct, in ways that capture better and worse the ways that some things are organized; and, most striking of all, on the special and distinctive leverage for such inquiry that ordinary human interaction, and especially talk-in-interaction, and conversation in particular, affords us because of its very interactivity, because of the ways in which subsequent utterances and other forms of action give us access to the endogenous understanding of what is going on, on which the participants build the ensuing trajectory of the interaction — in other words, which give us access to the terms of human practice in interaction itself. Pursuing inquiry along these lines, we have been led to find things that appear real, that are fresh once found, that seem to give us access of a sort we did not have to how it is with humans. Philosophy has often told empirical inquiry what it could not do, only to have to revise its view once that was done — successfully. We should try once again.

## Note

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## References

- Iedema, Rick. This volume. "Putting Schegloff's principles and practices in context".
- Sacks, Harvey, Schegloff, Emanuel A. and Jefferson, Gail. 1974. "A simplest systematics for the organization of turn-taking for conversation". *Language* 50 (4): 696–735.
- Schegloff, Emanuel A. 1968. "Sequencing in conversational openings". *American Anthropologist* 70 (6): 1075–1095.
- Schegloff, Emanuel A. 1984. "On some questions and ambiguities in conversation". In *Structures of Social Action: Studies in Conversation Analysis* [Studies in Emotion and Social Interaction], J. M. Atkinson and J. Heritage (eds.), 28–52. Cambridge: Cambridge University Press.
- Schegloff, Emanuel A. 1990. "On the organization of sequences as a source of 'coherence' in talk-in-interaction." In *Conversational Organization and its Development* [Advances in Discourse Processes, Vol. XXXVIII], B. Dorval (ed.), 51–77. Norwood, NJ: Ablex.

- Schegloff, Emanuel A. 1992. "Repair after next turn: the last structurally provided place for the defence of intersubjectivity in conversation." *American Journal of Sociology* 95 (5): 1295–1345.
- Schegloff, Emanuel A., Jefferson, Gail and Sacks, Harvey. 1977. "The preference for self-correction in the organization of repair in conversation". *Language* 53 (2): 361–382.