Chapter 2

On conversation analysis

An interview with Emanuel A. Schegloff*

Světla Čmejrková and Carlo L. Prevignano

Carlo L. Prevignano: I'd like to start by asking you to tell us something about your present research programs as you conceive of them.

Emanuel A. Schegloff: Well I have three major sorts of research and writing undertakings to which I am committed. One is a book that I am working on; a second consists of a few substantial research projects which have been under development for varying amounts of time; the third is composed of a large collection of research 'seeds' or 'buds', and I'll have to come back to that to explain what I mean.

One of the things I'm trying to work on now, at the urging of a number of colleagues, is a kind of synthetic manuscript that could provide something of an overview of CA work, at least as I understand it, and maybe could be used as a text for teaching purposes as well. It is largely based on the course sequence which I have been teaching for quite a few years at UCLA. This is no small undertaking, though I found that, until I got into it, I had no serious idea of what it would require. During a sabbatical leave a few years ago, I developed an overall plan for the work, and started writing text. I ended up with what I thought of as one chapter of this book. It came to over 250 pages on "sequence organization", so you can imagine the scope of the book that is in the offing; it almost seems as if each of the chapters might be a short book or something like that in its own right.

So one of the projects is to produce a work that will be synoptic or synthetic, in the sense that it brings together the product of studies over the last twenty to thirty years in what, from my point of view, are the major topical areas that we work in. So there will be probably a chapter on turn-taking, and one on turn organization; one on action formation, and one on sequence organization; one on repair, one on word selection, and one on overall structural organization of conversation.[1] But there will have to be other chapters — and other kinds of chapters — as well, for example relating the work in conversation analysis to the half dozen conventionally-bounded disciplines that surround us — so, its relationship to sociology, to linguistics, to anthropology, to communications, to psychology, to philosophy, or to some elements of these disciplines. There will have to be discussions of methodological commitments, theoretical background and the contributing streams of prior work on which CA has drawn. I'll try to have a chapter on doing a piece of work, including transcription, making observations, making collections, and so on. And there should probably be a chapter whose title — this probably could be a whole book in its own right — whose title is the frame sentence, "The trouble with conversation analysis is …", and of course there are many ways of finishing that sentence and many replies to each of them. So, as you can see, this is a considerable undertaking, and I have to do it in a way that will make it accessible to students, while at the same time providing a level of sophistication for already working professionals and scientists. So that's one ongoing project, and I don't know how long it will take to finish it.

OK, fine. Another ongoing project is this. Several years ago, I had a grant from the National Science Foundation to study what we call "other-initiated repairs". These are repairs initiated by the hearer of some utterance, who has had, or at least claims to have had, some problem in hearing or understanding it. The project had a number of analytic goals: one set of goals focussed on the variety of forms which other-initiated repair can take, what consequences these have and how we are best to understand the circumstances of their selection. A second set of analytic issues concerned the use of other-initiated repair sequences as a kind of prototype case of sequence organization. But the project was designed to speak to other themes as well, even if less centrally.

One of the "troubles" which gets mentioned in the frame, "The trouble with CA is …", is the absence of quantification in CA work, and the claimed disinclination among conversation analysts to deal with large amounts of data. Now, as with many such "troubles with CA," there are *prima facie* counters to the complaint. My own first published work in CA (Schegloff 1968) dealt with some 500 instances of what I was dealing with, and subsequent work (both my own and that of many colleagues; for example, I think of Gail Jefferson[2] and John Heritage,[3] among others) has also often dealt with substantial collections of instances. I've also now written a paper on quantification and the study of interaction (Schegloff 1993). But the "other-initiated repair" study was also designed to work with a very large number of instances, and ended up with over thirteen hundred of them. Each one of them, of course, requires careful analysis as an episode in its own right, so that project has taken a very long time to develop, even after the formal end of the grant some years ago. So work continues on that project as an enduring preoccupation (though there have been publications from it, for example, Schegloff 1997a).

By the way, that project was designed to address another of the "troubles with CA . . .," and that is the complaint that conversation analysis is thoroughly anglophone, or exclusively English in orientation. So when, several years ago, I realised that I was working with seven or eight graduate students who were natives of seven or eight different societies and cultures, native speakers of seven or eight different languages — German, Finnish, Swedish, Hebrew, Korean, Japanese, etc., — all of whom were quite far along in their training, we launched a project on other-initiated repair across languages and cultures which was very exciting but, unfortunately, was unable to attract the research support necessary to underwrite a seriously sustainable research undertaking. (Still, there have been results of that project as well, for example, Egbert 1996, 1997b; Kim 1993.)

I might mention that this business about CA being exclusively anglophone is something of a historical accident. CA work has been done on materials from cultures and in languages quite different from American English — as different as Finnish (Sorjonen 1996), German (Egbert 1997a and *op. cit.*), Japanese (Hayashi 1999; Hayashi and Mori 1998; Hayashi, Mori and Takagi 2002; Lerner and Takagi 1999; Tanaka 1999), Korean (Kim 1999 and op. cit.; Park 1998), Mandarin (Wu 1997), Swedish (Lindström 1994), Thai (Moerman 1977, 1988), and others (to cite only language/culture complexes, and writers, with all but one of whom I have been associated, only work published in reasonably accessible places, and only a single reference to each, else there would be a great many more citations, languages, etc.). So the work is not differentially suited to English, nor are there languages that we know about that resist analysis along conversation-analytic lines. My own belief is that the best way to have this work done in other languages is to have native speakers of those other languages and native members of those cultures learn how to do the analysis and then go to work on materials in the culture and in the language that they have a native control over. That is what has been done in the languages I just mentioned. So it's just a question of getting people from other cultures to come and learn how to do the work and then go to do it (my apologies to my anthropological colleagues; in our area, it seems to me, we need to explore increasingly the virtues of developing, if I may paraphrase Virginia Woolf, "anthropologies of our own").

So that's one of the larger-scale, continuing projects. I should add that the work on other-initiated repair is part of an ongoing series of studies which have appeared over the years about repair in various "positions." This started with the overview presented in the paper with Jefferson and Sacks on "The Preference for Self-Correction" (1977), which sketched an organization of repair in various positions around the "trouble-source" turn or "repairable." There are papers then about "same turn" repair (such as Jefferson 1974, and Schegloff 1979) and what I call "third turn repair" (Schegloff 1997b) and "third and fourth position repair" (Schegloff 1992c; some "special cases" are discussed in Schegloff 1991a). The work on other-initiated repair is, in effect, about "second position" repair. So gradually we get more and more detailed studies and get them in more languages and cultures, and so I want to participate in that, filling in the picture, so to speak. That's part of this second ongoing work commitment.

Another project that comes to mind, of quite a different sort, is maintaining a lively and hopefully convincing dialogue with a number of disciplines and subdisciplines which ostensibly work in the same area, or partially intersect the sort of work which my colleagues and I do. I think it's useful to try to discriminate what we do respectively, not in a pejorative way, but in a way that makes clear where the differences of opinion and commitment are, where it looks like either one or the other is going to be most productive, where they can both be working. So I've been writing some things in the last several years in particular directed at a field that I think is more widespread and has more vitality in the United States than in Europe (though exceptions in Europe immediately spring to mind!). It's called "communications" or "speech communications" in the United States and people in that field have taken a lot of interest in conversation analysis in the last ten years or so. So I've been trying to work to build bridges to that field and join forces with people in that field who came across CA work and found it fruitful for their own interests. A lot of the work in the field of Communications emerged from information theory in the fifties and some of it from social psychology, and so there's still some clarifying to be done about the difference between "communication" as an idea and "interaction" as an idea, and the difference between more traditional social psychological work on language and interaction and conversation analytic work. So that's another project. Of course this interaction with other, more established disciplines continues with linguistics, anthropology, sociology, and so forth.

Another research preoccupation for the last several years surfaced in the talk I gave at the conference which is the venue for the present interview, and concerns the analysis of interaction with "neurologically-compromised" participants (Schegloff 1999a, 2002; Heeschen and Schegloff 1999, 2002). Even though my venture into dealing with people with neurological "problems" was actually quite accidental, there's at least one important message I have wanted to get out, especially to people who are in the neurosciences. It mainly concerns the area that's called now "the neurobiology of behavior." The main point, just to say it briefly and informally, is this: it's clear that it is the neuroscientists who have to describe the brain and what about the brain underlies whatever behavior they're trying to explain. The question is, who is going to describe the behavior? Right now most of the behavior being dealt with is of a relatively simple sort: small muscular movements, sensory experience, and the like, and these things can for now be described in pretty much commonsense or vernacular terms. But even now more complicated behavior — for example, involving "rational" calculations of comparative value and risk (Damasio 1994) — is being brought under examination, and this will surely continue and expand. As it does so, a descriptive apparatus of appropriate sophistication and relevance will increasingly be needed, especially (but not exclusively) for conduct in interaction, and commonsense terminology will not do. So, even though it will be quite some time before we and they get to that point, it will be useful for neuroscientists to understand early on what's "on the other side of the river." If you are going to build a bridge with the brain on one side and with ordinary human behavior on the other side, it's a good idea to know roughly how you're going to be describing human behavior so that you can build your *neuro*-discipline with an eye to that. That's really most of what I want to get out of this neurologically-oriented work I've been involved in. Of course, if I can help alleviate some of the misunderstanding of the folks who are beset by these problems, that would be most welcome, but theoretically the point is to open a dialogue with neuroscientists so we can see how the meshing of their concerns and ours might occur some day. (In the meantime, really outstanding work in this area is being done by my colleague Chuck Goodwin (1995, for example), much of which has yet to appear.)

There are other substantial projects in much earlier stages of gestation — for example, one I call (after the title of Schutz's 1964 essay) "Making Music Together," for which I videotaped a string quartet's series of rehearsals preparing a concert and then the concert itself. The initial motivating idea was to examine several distinct orders of interactivity which supply the infrastructure for making music together: the embodied interactive conduct of the playing itself, the interaction at rehearsal through which the playing is developed, and the interaction written into the score by the composer. At this point, I must say, I have no clear idea of what advances to conversation analysis are to be found here, but I have high hopes for my own enhanced appreciation of music and its realization, and perhaps that of others as well. In any case, the data are still being transcribed and I have no idea when I will be able to work on the project seriously.

I should say, however, that most of my research "growth points" are not in such large-scale projects, nor do most of them have that sort of "on-an-agenda-of-work" status. The way my research work is organized is much more under the control of the data which I encounter — in literature which I read in journals or which people send me, in the work of my students and colleagues, in the regular data sessions which we hold at UCLA or at conference venues, etc.. The way this works, briefly, is this: some observation made about some data prompts me to open a folder — formerly on paper, now on the computer — about the observation and the phenomenon it seems to exemplify, the practice which it appears to instantiate, etc.. As I encounter other candidate instances in other data which I happen to encounter, I add them to the folder. These folders grow by gradual accretion, then, and (in the first instance) not by any systematic search. At irregular intervals, I have a look at some of these folders, and seeing what has accumulated there may prompt a spurt of writing about what seems to be going on, and that may prompt a systematic search for all the additional instances that I can find in some set of data corpora. And sometimes this may lead to writing up a paper, sometimes a little one to satisfy an invitation to do a paper which must fit into a twenty-minute slot at a convention panel, sometimes a more major oral presentation, sometimes a written product which far exceeds what can be done in even a plenary address (as for example with Schegloff 1996a, which followed just the trajectory described here, as is recounted in that paper). And sometimes it is the invitation to participate in a panel with a twenty-minute paper that sets off a search through my directory of "collections" to find something suitable in content, potential length and interest for me and for the audience — which may, after an investment of time and work, turn out to have been a misjudgement.

This is not best understood by reference to the phrase in the question to which I'm responding, which asked about "your present research programs," except insofar as one might say I have one research program — developing our understanding of how it is with humans in talk- and other-conduct-in-interaction, and how that relates to other disciplines whose activities intersect this domain. Within that research program, there are lots of "seeds" and "buds," growing at different rates, at different stages of development, some of which will come to full flower while others do not (because they are/were wrong, because I lack the wit, because we do not yet know the things one must know first before we can understand them, etc.). There is, of course, no way of conveying what is included in the array of collections, but the book, if/when I get it done, will convey something of the domain within which they fall or which they are meant to expand.

Oh, there is one other very major undertaking; maybe the most major one, certainly the one with the biggest claim on my time. We have quite a vibrant community of inquiry, however we define it. I mean, whether you think of it in the most narrowly-circumscribed terms, as conversation analysts, or in terms which include each of the larger concentric circles that you can build up around that: linguistic anthropologists, students of dialogue, however you want to define it. But at the moment I'm thinking about the more narrowly-defined group of conversation analysts. It's quite vibrant and it has been growing in spite of a largely unfavorable academic environment, I think. Somehow we've survived and thrived. And it seems to me the most critical project for me right now, and for other colleagues who've reached relatively senior positions in the universities, is to help train a new and expanded generation of students who can then train students of their own. We need not only to produce work, but to reproduce workers. And that's happening. But organizing and providing good training and helping people find secure positions is very time-consuming. But it's at least as important as the writing and the research itself, because there's a natural and necessary end to that for each of us. But the way communities and disciplines develop depends entirely on the capacity to transcend an individual scholar's life.

C.L.P.: How did you come to the enterprise called conversation analysis?

E.A.S.: Well, I didn't really, because there was no such thing as "conversation analysis" to come to — at least not in the sense of what has developed over the last thirty-five years or so.

C.L.P.: I mean, how did conversation analysis come into being?

Světla Čmejrková: Was it the use of data from tape recordings that initiated your interest, or did your general idea of turn-taking come at the beginning?

E.A.S.: There's no question that without tape-recording it would not have thrived. It's just improbable that it would have thrived as it did, and taken on the character that it has. On the other hand, I'm not a technological determinist. Tape-recording had already existed for at least fifty years. In fact, social scientists had used tape recorders, including (perhaps even especially) students of interaction. For example, there was a famous social psychologist at Harvard, where I had my undergraduate education, named Robert Freed Bales, who in the 1940's and 1950's studied small groups in the experimental, social psychological tradition. When he started to do his work, he had graduate students coding the behavior of these small groups as it happened in real time, but it became obvious at some point in his work that this was really not adequate. So Bales then began tape-recording these experimental sessions and the research assistants would code the behavior from the tapes into the analytic categories of the research project … and then they erased the tapes and re-used them. For Bales and a great many other social psychologists (and other students of conduct-in-interaction), "the data" were the coded categories, the statistical frequency distributions in them, and the variables they represented, not the actual talk and conduct. So, the fact of actually having the tape recorder as an available technology didn't determine anything. But it was almost certainly the case that without it we could never have had a field; and we can talk a little bit more in a moment about why that's so.

So, how did I come to be doing this (kind of) work? Well, the question is, how much tape do you have. I've actually written a little bit about some of the story in my "Introduction" to Volume I of Sacks' *Lectures on Conversation* (1992: xii–xxx) and in an introduction to a posthumous publication of an early paper of Sacks (Schegloff 1999b). Institutionally, the two most important converging intellectual backgrounds come from Goffman and Garfinkel, and this sort of background is discussed in the "Introduction" to *Interaction and Grammar* (Schegloff, Ochs and Thompson, 1996: 11–16). But if you're asking the question biographically, I'll tell it biographically rather than institutionally.

You said: "how did *you* come to it?". I came to it in a way that is plausible and orderly only in retrospect. In real time, of course, it felt quite disjunctive. As an undergraduate at Harvard I had been interested in the sociology of knowledge, in *Wissensoziologie*, and pursued that interest under the guidance first of Talcott Parsons and then of Barrington Moore, Jr.. After I wrote an Honors thesis in that area in 1957–58, it occurred to me — I'm sure I didn't think it as clearly at the time as I can say it now, but I sensed in an inarticulate way — that the things that were most studied by the "sociology of knowledge" included everything except knowledge. That suggested that there would be a separation between what was called sociology of knowledge and the sociology of science, and that to succeed as a sociologist of science you had better know some science and some mathematics; and I didn't. So this was a problem, and when I got to Berkeley for graduate school, this converged with something that was just beginning as an intellectual development in the United States; perhaps in Europe as well. That was the development of what came to be called about ten to fifteen years later the sociology of culture and/or cultural sociology. During my first years of graduate school, I worked with (among others) Leo Lowenthal, a German *emigré* who had been one of the original members of the so-called "Frankfurt school" of critical theorists, who was among the pioneers of the sociology of literature, and also, with Reinhardt Bendix, deeply immersed in continental social theory of a somewhat different sort. I ended up writing a Masters thesis (1960) in the sociology of literary criticism (a bit of it is described in Schegloff 1997c).

What was important about the thesis for the present story was its leading me to understand in a different way than I had previously how context could have a "bearing" on the form and substance of social life. In particular, in coming to understand the rise to predominance of a formalist style of literary criticism that (in the then canonical understanding of the social bases of ideas) ought to have been receding in influence at just that time, I was led to focus not on the overall political/economic structure as the relevant "social context," but on the much more immediate circumstances and practical exigencies of literary people — their increasing concentration in colleges and universities awash in the post-war democratization that brought to their classrooms students with little background in the sophisticated reading of great literature. The key was to be found in a more narrowly-drawn, more proximate, sense of context.

So that's more or less where I came from, academically speaking. I was trained as a classical sociologist; when I took my Ph.D. exams, I was examined in social theory, in the sociology of culture/knowledge, in social stratification (or class analysis), and in studies of deviance.

But in my third year of graduate school, I encountered this other graduate student named Sacks, who had come to Berkeley two years after I did (having spent several years in law school and its aftermath). We were "auditing" the same course (in the U. S. that means attending the lectures but not enrolled for credit), and he would ask what seemed to me very unusual questions. One day we ran into each other on campus, we went to have coffee together, and we had (we both agreed subsequently) an amazing conversation; to each of us it was amazing, even though in different ways. It was from him … he had encountered Garfinkel … I won't give you Sacks' story; I've written some about that elsewhere (1992b, I: xii-xvii; 1999b). Briefly, after he had finished Yale Law School, he went to Cambridge (in the U. S.) for a while to try to figure out how the law worked, and he tried first to do it at Harvard with Talcott Parsons, but gave it up after a year. But, as it happened, the year that he was in Cambridge, Garfinkel was on sabbatical leave there. So Sacks encountered Garfinkel, found his thinking serious in a way in which a lot of sociology was not serious, formed a relationship with him and became familiar with his writing. And so Sacks had in manuscript form a lot of Garfinkel's work, and I got that from Sacks — an important new contribution to my own thinking. But of course in talking about the issues which were preoccupying him, Sacks had his own quite distinctive views, which in some ways overlapped with Garfinkel's and in other ways were expressed quite differently; altogether, an eye-opening encounter for me. What it was for Sacks would have been for him to tell, but that is no longer possible.

Anyway, we became very good friends, talked together a lot, and worked together as much as we could until he was killed in 1975. But when we first met, he was coming from quite a different academic commitment. He had finished law school, and he came to Berkeley interested in industrial relations and collective bargaining. But through a friend of Garfinkel's, he had been alerted to Goffman's work, and so we went to Goffman's classes together. This is how the Goffman and the Garfinkel connections got made.

Goffman ended up being my dissertation supervisor (and Sacks' too; cf. Schegloff 1992b: xxiii-xxiv and note 18). With me he was a very nice combination of tolerance and discipline, in the sense that he didn't supervise the substance of the dissertation in any serious sense, or at least did not require much change in what I had written. In part, this was because he was surprised by what I was doing. Because he knew the work I had done in my first years at Berkeley, he thought of me as a theorist, as a critical theorist, as a *Luftmensch* of sorts, and the notion that I would actually be analyzing data was, so he once told me, completely a shock to him. I had taken a job in Ohio in order to get access to the data I hoped to work on. I would come back to Berkeley, show him what I had written, he would go off and read it while I waited in his study, we would discuss it, and he would pretty much leave it alone. There was one exception. He said to me at one point that it was a responsibility of writing a dissertation to survey the literature of the field in which the dissertation was being written. But, he said, there was no field in the area in which I was writing; there was no literature to survey. But that did not mean that I didn't have to survey the literature. Rather, he said, I had to survey the literature of all the fields that were contiguous to what I was working on — and he specified some nine fields for me to survey the literature of. That was another six months of my life — to review the literatures of all these areas.

In the end this turned out to be very valuable. At the time, of course, I resented it deeply. But, the exposure to all this literature added to my prior training a resource that was invaluable to building an academic career. In my first several years at Berkeley, I had done almost every kind of sociology there was: I was a survey researcher for a year, I did historical research, political sociology, etc. etc.. It turned out to be very important, because the way I earned my way and found a place in the universities in which I taught as a junior faculty member was not because people necessarily appreciated or understood what I did — the work on conversation was pretty much an enigma to sociologists in those days (and to many sociologists these days as well). I was able to make my way in the University because I could talk sociology or philosophy or psychology or anthropology with my colleagues on their terms, to their satisfaction and so they were willing to tolerate this crazy thing that I said I was doing. And I think Goffman's insistence that I know all these other literatures contributed to my ability to earn my way in ways distinct from my own work.

This contribution aside, I learned from Goffman of the very possibility of studying interaction *per se*, and of the possibility of description as a serious disciplined undertaking. If the M. A. thesis had helped me focus on a narrower sense of social context than the earlier macro-sociological orientation to which I had been exposed from the perspectives of both the right/centrist sociology of Parsons and the left-oriented sociology of Moore and of many at Berkeley, Goffman brought into view a much more proximate sense of social context … by several orders of magnitude.

There's one other piece to this puzzle (and we haven't yet gotten to the conversation analysis!). And that is that, when Sacks came to Berkeley, he came in the first instance to work with a sociologist named Philip Selznick, who had been a student of bureaucracy and organizations in the 1940's and 1950's, and had gotten interested in law, and had just founded an Institute called The Center for the Study of Law and Society. Selznick arranged to bring a number of graduate students into the Center, essentially as junior fellows. For the 1962–63 academic year, Sacks was one of them, I was one of them and there was a third — one of a triumvirate of graduate students who used to hang out together — named David Sudnow. And so we were all at the Center that year, all working together and at this point we were all to varying degrees, as we say in America, 'into' ethnomethodology, pretty much of a Garfinkelian sort (I have written a bit about that year in Schegloff 1999b). There was still no conversation analysis in the sense that that term later came to have. I think in many ways Sudnow was into the Garfinkelian version of ethnomethodology the most, Sacks had a distinctive stance in that area, and I was halfway in and halfway out, and I think they recognized that.

I started a dissertation in Berkeley that was concerned with a question in the sociology of law, at least I was treating it as that. The question was how a/the society decides whether its members are responsible for their own conduct or not. What I undertook to study was the plea of "insanity" as a defense to criminal charges. In the United States (as an inheritor of British common law), if someone has been accused of a felony, one thing they can do is claim to be "not guilty by reason of insanity" — because they were insane at the time of the felonious act, they are/were not responsible for their own conduct. My plan was first to study how this is dealt with legally, and then to examine how this was dealt with psychiatrically. At that time, in California, if a defendant pleaded "not guilty by reason of insanity," two psychiatrists were appointed by the court, they interviewed the defendant in the jail and from the exchange of talk between them, they offered an opinion about whether this person was insane or not, and therefore responsible or not. My plan was to tape-record the interview, obtain the psychiatrist's informal notes and formal report, as well as any testimony that might be subsequently offered in court, and then track the series of transformations which began with some talk in the initial interview and ended with a finding concerning "responsibility." It became obvious very quickly that to do both the legal side and the psychiatric side was impossible. Since my father was a psychiatrist, and since that was where the talk was, I quickly decided to work on the psychiatric side. But there were so few cases of insanity pleas in the local courts that I simply could not do the project in the Berkeley area. So at the end of that academic year, I moved to Los Angeles because it had a vastly larger court system and I expected there to be many cases of people pleading insanity. Now, as it happens, because there was a vastly larger court system, there was an administrator who ran the court system, and he was suspicious about any sociologist poking around in "his" system, and he eventually blocked my access to the data, so after a year and a half, I had no dissertation. But in the meantime, Sacks had also moved to Los Angeles to be (with Garfinkel) a Fellow of the Center for the Study of Suicide. So we were both living in Los Angeles, and it was during that year that work of the sort now recognized as conversation analysis got started.

As it happened, the Suicide Prevention Center received telephone calls from people who were suicidal or who were *with* suicidal people in search of help, and the Center tape-recorded those calls and had someone transcribe them — stenographically and badly, as it turned out, but they were transcribed and that somehow made them accessible to examination in a different way. Sacks got hold of some of those tapes and it was a windfall. For years, Sacks had had the habit of attending to conversations going on around him — in cafés, at bus stops, in supermarkets, and so forth — and often jotting down bits and fragments of what he heard in a little notebook he always had with him. But the taped and transcribed calls did not have to be overheard, did not have to be jotted down on a single hearing. The material made available that way supplied the raw material for the start of this work.

I count the start to have been in an exchange which I described in the introduction to Sacks' *Lectures*. We were at the UCLA campus one day and he proposed to try out a conjecture he had about some data from one of the suicide calls. This was a particular call to the suicide center in which someone "didn't hear" what the answerer at the Suicide Prevention Center had said and by the time the "repair" was accomplished (we weren't calling it repair at the time, of course; it was just an observation), somehow the caller had managed to avoid identifying himself. Sacks connected that observation with discussions that the personnel of the Suicide Prevention Center were preoccupied with because they needed to get the names of the callers to the Center (to document their service function for their sources of financial support), and they too often couldn't get them. It seemed that if they couldn't get the caller's name at the beginning of the call, they couldn't get it at all; and the easiest way of getting the caller's name was that the answerer — the psychological volunteers who answered the phone — would give their name and they would often get the caller's name back in return. But when the answerer on the phone said, "hello, this is so-and-so. Can I help you?". And the other person said, "I'm sorry, I didn't get your name," "this is so-and-so," "oh," and they didn't give their name in exchange, there was trouble. So it was at that point that Harvey said, "Do you think that could be systematic?".

C.L.P.: Would you say that it was that discovery that initiated conversation analysis?

E.A.S.: Ok. So, it's hard to say at what moment conversation analysis "started," but if I had to pick a point, that's the point I would pick. Harvey started from then to work intensely on the suicide calls, and then on other data he managed to tape-record — in particular a number of group therapy sessions with adolescents, conducted by a psychologist also affiliated with the Suicide Prevention Center (though the group therapy sessions themselves were unrelated to the Suicide Center's activities).

For my part, I learned a few months later that my access to the data for the dissertation I had been working on for 18 months was blocked. I had no dissertation, I had a wife, I had no income, I had to find something else to do. It seemed to be a disaster, but it turned out to be a fortunate accident. I found out about a research center in Ohio which had telephone calls to the police. I asked if I could get them. They said, "we don't give the data to people who don't work for us. However we have a job as a research associate." They were paying $9,000 a year. This was three times as much money as I had ever earned in my life, so we went to Ohio, and there I got the data from which I wrote my dissertation. The cost of that "fortunate accident" was, however, that Harvey and I were no longer together and for the next seven years we could only work together sporadically, during holidays and for brief spells in the summer. Finally in 1972, when I got a job at UCLA, I went back to the West coast and we had just about three years of working together more sustainedly, and then he was killed.

About that first episode of CA, and the work that followed it, I must say that we had no idea, no sense of what lay ahead. I can only speak for myself. I had no idea what all this was going to amount to. I doubt that Harvey did and we had very different kinds of minds. It turned out that they were peculiarly complementary; we thought the same in some ways and very differently in others. So maybe Harvey had an idea of what might develop from the outset; I don't think so. Later on, of course, it became clear — at least to us — that something substantial might well be involved. There's a place in his diaries where he writes about us as two little boys. There we are wandering around really having no idea the depths that this would go to, the extensiveness of it. We'll never know what discipline it would have turned into had he still been alive.

Anyway, I've gone into a lot of detail here.[4] The upshot is that, intellectually, I came to conversation analysis via these way stations. It started at Harvard with an interest in the sociology of knowledge and a classical sociological canvas of largely macrosociological shape. Several things happened to that. First, recognizing the imminent divergence and separation of the sociologies of science and culture, I took the path of sociology of culture. Second, I found myself dealing with the puzzle of literary criticism in the U. S. in the period 1930–60, and ended up with a "solution" at a different level of social context than the macrosociological one with which I had started — more proximate, more practically engaged in the thinker's life, more "real," even if not entirely disengaged from larger social contexts. Third, Garfinkel gave me resources that consolidated my critiques of the several sociologies I had tried — Parsonian, survey, political, etc.. Looking as I was for honest, defensible, engaging work, Garfinkel made it impossible for me to continue doing the received sociology. Fourth, Goffman made interaction a viable topic of inquiry, in a fashion different from the social psychology I had previously been exposed to. Finally, the interest of Harvey's mind, and our "clicking" together, provided the context for exploring what might be doable instead.

S.Č.: Earlier, you mentioned the description of behavior, especially in connection with the use of video recordings. I think it was the background to the paper you gave at the IADA congress here in Prague. So, do you have any idea of how this direction could continue in the future? You also mentioned that you had short strips of behavior and that now it is possible to study larger complexes of behavior. Could you tell us something more about that?

E.A.S.: I'm not sure I've understood the question properly, but let me answer the one I think you're asking and if that's not the one you're asking, you'll correct me. There are two ways of extending out from a little bit of material. One is to have many instances of such bits (by "such bits" I mean bits which have the feature(s) being examined), and the other is to have larger bits.

S.Č.: Larger bits, yes.

E.A.S.: And I think there's an interest in both of these ways of extending the basis for analysis. But before talking a bit about each of these, let me just say as a matter of general principle that it seems to me that directions of development in this work are driven by two forces, and they are of quite unequal and asymmetrical weight, in my judgement.

The most important consideration, theoretically speaking, is (and ought to be) that whatever seems to animate, to preoccupy, to shape the interaction *for the participants in the interaction* mandates how we do our work, and what work we have to do. One of the reasons there has been a focus for many years now on relatively small bits of conduct is because we can show that the participants are oriented to constructing the talk and other conduct in detail, and that makes that level of detail — with those facets of detail — matter *for the participants*, and that is the warrant for our focussing on them. It is not just that it appears "clever" or "insightful," or that most persons — including most professional students of human conduct — are not aware of seeing these details (though they must be doing so if they are making their way through life in the company of others), and we can elicit an "ahah!" experience in them by describing in detail what goes on and how it gets done. It's that we think that this level of detail in such small chunks of interaction demonstrably matters to the participants. Not that they could tell us that if we asked them; it is not a matter of self-conscious awareness, of what Giddens termed "discursive consciousness," but that they in fact appear to construct — and "take care" to construct — their conduct in these ways, and to understand the conduct of others by reference to them. So, the primary consideration that theoretically justifies this aspect of our work — this level of focus — is the demonstrable orientation and conduct of the participants in the interaction which we study, that is, it is grounded in, and warranted by, the data as we understand it. To the degree that we can progressively become aware of, and show the orientation by the participants to, larger stretches of the talk as organizational units for the participants in constructing and interpreting talk-in-interaction, we can find methodological resources for capturing those and studying them as well. That's a direction in which the work will develop, and has already developed to some degree (see for example, Jefferson 1988; Jefferson and Lee 1981; Schegloff 1980, 1990, 1992a, 1995b). So, that's one of the things that drives the direction of work, and that can drive it from small to larger bits of data (but also from small bits to smaller bits).

The other thing that shapes the focus and development of research is interaction with our academic colleagues. Now, that is a much more problematic matter, because often our academic colleagues are motivated by considerations other than the demonstrable relevance to the participants. In particular, they are most often motivated by the traditional or contemporary preoccupations of their discipline, by its current theoretical commitments or controversies, by the methodological paradigms currently in favor or seeking to be, by the apparent political tenor or implications of various directions of work, and the like. These often have as much or more to do with the *situation of inquiry for the investigators* than with the *situation of interaction for the participants*. Now arguably inquiry can never be free of the contexts in which it is framed and pursued, and it would be naive and pointless to pretend otherwise. But the impossibility of de-contexted inquiry is no excuse for analytical libertinism — for abandoning the effort to make the terms and practices of research as responsible as possible to the demonstrable features of the data, at the very least to avoid as much as possible making the terms of inquiry incompatible with the internal features of what is being studied, and not superseding them (for example, theorizing as if every action in interaction was an independent "atomic particle," rather than conditioned by its position in a stream of interaction). And nowhere is this more in point than with sentient actors who bring their own orientations and their own understanding of what is transpiring to the arena of action, understandings and orientations which are the critical formative input on which is based the construction *by them* of the next bit of the data which is being studied. I would like my own work to be motivated virtually exclusively by what is demonstrably relevant to the participants in the way they construct and understand the conduct which they build together. Obviously most people working in the social and human sciences are not as exclusively driven by those preoccupations. In interacting with them and the analytic terms of their own work, as well as their critiques of, and recommendations for, *our* work, we have had to talk about other things that are not demonstrably relevant to the interactants whose lives we study. And some of the interest in conversation analysis expanding the range of the units which it addresses is grounded in such considerations, in efforts to make CA commensurate with other undertakings in the social and human sciences on grounds other than its relevance to the materials being studied.

So, what I try to do, to the degree that I can and I'm sure that this does not win us any friends, is I try to defer as long as I can answering academic colleagues who insist that we speak to these issues. So many people (this is actually something I welcome the chance to talk about) complain a lot about conversation analysis — maybe not all conversation analysts, but certainly they complain about me — that I don't cite lots of other work, for example, that seems ostensibly to be in the same area. This is something I feel really bad about in some cases; the texts in question are in fact the product of engagement with repeatably examinable, naturally-occurring materials examined with differing interests in mind and arriving at different results; and too often I just can't read all of it, and/or have failed to do so. But in a great many instances, even though work in other fields and styles of inquiry seems to be about the same subject matter, it is not about the same subject matter. It's about common-sense knowledge of, or supposition about, what goes on in the empirical mundane world (as often in some variants of linguistics and philosophy), or accounts that are based on other methodologies, which, however respectable their histories are, seem to me no longer the state-of-the-art in the study of naturally-occurring human interaction. These days, only such work as is grounded in tape (video tape where the parties are visually accessible to one another) or other repeatably (and intersubjectively) examinable media can be subjected to serious comparative and competitive analysis. So, even though people seem to have very robust concepts and analytical tools, if they are grounded in very different kinds of materials (as for example in ethnographic observation based on one exposure in real time, yielding remembered, necessarily selective, field notes which supply the basis for subsequent thinking and writing about the episode in question), from my point of view they are ordinarily not about the things that I study. Nonetheless, there is a pressure to speak to those literatures and those preoccupations, and that regularly includes a pressure to examine different — and larger — units of interaction than have been central in the past.

Where the two converge, where our academic colleagues want us to deal with longer stretches of talk, for example, and that converges with a demonstrable orientation by the participants in the interaction to such larger trajectories, it is of course an inviting thing to do and some will take up that invitation. But I think it's also important to recognise that we do not start to work on longer stretches of talk because we have pretty much exhausted the shorter ones. Frequently people get this impression. Students especially talk as if all the work on turn-taking has been done and there's nothing left for them to do. It's a terrible misconception! Just because there's a lot of literature, it doesn't mean it's all correct. It doesn't mean that everything has been "covered." In part this misperception is an artifact of people learning what the problems are from the little literature that there is. Until they become more competent and autonomous investigators, they're not in a position to see for themselves that there's a wide open empirical domain, only little islands of which have actually been explored. This comes only from looking at data with an open mind both to the relevance and adequacy (when merited) of past work and to the relevance of that domain (e.g., turn-taking, repair, etc.) for other observable features of the data along lines not previously registered in the literature.

So, I think almost certainly there will be people who try to deal with longer stretches of talk and with more instances of stretches of various sizes but that's not because other areas have been used up. It's just because we've increasingly developed the analytic tools to do that and it seems in fact relevant to the participants in the data being examined. For example, in this long chapter on sequence organization that I have drafted for the book I am writing (and also in Schegloff 1990), I try to show just this — that there are exceptionally long stretches of talk that can be shown to be constructed on the armature of a single underlying unit of sequence construction; that is, there can be very long sequences indeed — some of them running four, five, six pages of transcript and longer. That means segments as long as twelve minutes or longer, in which all of the talk is really built on a single adjacency pair with multiple expansions, and one doesn't really understand the coherence of that stretch of interaction without seeing that it's based on a teeny little thing. So "teeny little" and "great big" are not really necessarily alternatives to each other. Often the way of understanding "great big" is to understand "teeny little."

I think I'll skip talking about extending analysis from a single little extract by examining many little extracts; I have written about that in various places (*inter alia*, Schegloff 1996a: 174–81; 1997a: 501–2 *et passim*), and I think you were mainly interested in the possibility of expanding the size of the targets of inquiry. Did I speak to your question?

S.Č.: Yes, yes, I think that you did.

C.L.P.: Some people comment with concern about the formalism of some of the work in conversation analysis, especially in view of the reaction against much formalism in other approaches to language. This is a common reaction, for example, to the paper on turn-taking in 1974. Can you say something about formalism and alternatives to it in this area of work?

E.A.S.: One of the most puzzling reactions to the turn-taking paper for me is the claim that it is *merely* formalistic, concerned *only* with forms and rules and structure, and not with action or "meaning." As puzzling is the extension of this characterization to other conversation analytic work, for example work on sequence organization or repair, and in some instances to conversation analytic work generally. Leaving aside the implicit theoretical and analytic antinomies which underlie the expressed concerns which might themselves merit discussion, let me instead respond by considering briefly why it was in point to have a "systematics" for turn-taking at all, how it related to other work at the time, and how that juxtaposition may have partially prompted the reaction to the work as "formalism."

So why was it in point to have a systematics for turn-taking? Here is one view, briefly put.

From early on in conversation-analytic work, a great many analyses of discrete bits of talk-in-interaction seemed to prompt, and then be shaped by, observations about the construction of utterances in turns. These were analyses otherwise largely directed to what some utterance was doing or how some activity was constructed, and yet they required reference to turn-oriented practices. Sacks' *Lectures* (1992) are full of such discussions, ones which involve only truncated observations about turn-taking organization — just enough to return to the preoccupation on whose behalf they were undertaken. I offer just one case in point out of many.

Much of Sacks' treatment of story-telling in conversation and its sequential organization (aside from 1992, *passim*, cf. Sacks 1974) is launched from two observations. First, that units like clauses and sentences can constitute possibly complete turns, on whose completion transition to a next speaker may become relevant; and, second, that virtually in the nature of the case, stories take more than one such unit to tell. This pair of observations leads to the recognition and formulation of the problem for prospective tellers of getting to tell the whole story — namely, that at the first possible completion of a turn unit, or any subsequent one, a recipient may start talking along lines which frustrate a continuation of the telling. They lead as well to one solution to that problem for prospective tellers — the story-preface and the sequence which it initiates (e.g., "A funny thing happened on the way to the forum"), and the place of that sequence in the larger organization of story-telling.

The focus here was *story-telling* in conversation, but it required an incursion into turn-taking organization to explicate important parts of its structuring. There are many such discussions in the L*ectures*, including ones addressed to even more narrowly circumscribed "actions." So also in Jefferson's work around the same period. Those familiar with the so-called "precision-placement" paper (Jefferson 1973) may recall how multi-faceted were the ways in which what someone was doing was contingent on where in the developing structure of a turn some bit of talk was placed. And this theme figured in my *own* early work as well — on sequence structure, on overlapping talk, on conversational openings, and the like.

All these analytic exercises had, however, a scent of the *ad hoc* about them. They articulated only those observations about turn-taking which were prompted by, and were needed for, the exigencies of the "other" analytic project in progress, whatever it happened to be. They were, in that sense, opportunistic. They pointed to a larger domain of organization, and were parasitic on it, but always turned as quickly as possible to the project for which they were borrowing. But if that more extensive turn-taking organization was there, and if so often the elucidation of other particular practices, devices, phenomena, activities, etc. relied on facets of that turn-taking organization, it was virtually mandatory that our understanding of it be not limited to those aspects we were directed to by what were, strictly speaking, exogenous interests. At some point, turn-taking *had* to be examined as a domain in its own right, so as to make explicit the fund, the resource, on which we were so often drawing.

Of course, that meant that there would be (in that undertaking) no quick return to a more limited action/activity/device or practice as the topical preoccupation and analytic payoff. And it is that juxtaposition — between the terms on which turn-taking had *previously* figured in conversation-analytic work, and the way in which it figured in *this*, *systematic*, undertaking — which I think engendered in many readers of the turn-taking paper a sense of desiccated formalism, of "the clacking of 'turns' over their 'possible completion points'," as Michael Moerman (1988: xi) so graphically and disapprovingly put it several years ago. It appeared as if the situated substantive analysis of discrete actions and discrete episodes of interaction and their interactional import had been severed from the explication of the formal organization of turn-taking itself.

However understandable as a narrative line, I think this is a deeply flawed understanding of the place of formal and systematic analysis in the larger enterprise of studies of talk-in-interaction — whether the formal analysis is of turn-taking, of sequence organization, of repair, or of any other organizational domain of practices of talk-in-interaction. *In my view, such formal resources are like a reservoir of tools, materials and know-how from which particular academic analytic undertakings can draw in inquiry, because practising interactants draw on them in concertedly constructing what transpires in interaction.* That is why disciplined control of these analytic resources should be part of any competent analyst's tool-kit — not necessarily particular *terminologies*, only the actual phenomena and practices which such work has in the past brought to attention. Only now they have been explored and described more systematically as an ordered set of practices — a domain of organization with determinate internal shape.

I can't, however, give the most effective response to your question within the context of an interview. That would be to exemplify the claim I have just made about the role of formal work by examining several bits of data and their explication to show the role which the resources provided by formal analysis of the sort exemplified by work on turn-taking or sequence organization can play in examining stretches of talk-in-interaction, including the action import of their components. For that I will have to refer interested readers to various papers (among my own, readers may find particularly suitable: Schegloff 1987, 1995a, 1996a, 1996b, 1997a, 1997c) which I hope embody the opposite message, which is this. It is ill-considered to fault a focus of formal inquiry (like turn-taking or sequence structure or repair or vernacular poetics) simply for not taking "meaning" or "action" as its officially central pre-occupation; for it may be by reference to just such formal features of the talk that action, and what is vernacularly termed "meaning," are constituted and grasped in the first instance. The upshot is that analytic resources which were developed as part of *formally*-oriented inquiry into what can be called "generic" organizations for talk-in-interaction serve as tools in explicating the *action and interactional* import of particular episodes of interactional conduct. But here I can give only a promissory note. The payoffs are to be found in the papers — by various workers in the field — which bring these resources to bear on other data with results which people must be finding worthwhile, else there wouldn't be the interest in this field which has prompted this very interview.

C.L.P.: I'd like to insist a little more on historical, autobiographical matters. Not so much about Sacks; rather, would you like to say more about two other figures you mentioned, Garfinkel and Goffman? I think as a younger researcher, you tried to find your own answers, to put some distance between yourself and them.

E.A.S.: OK. Let me talk a bit about Goffman. I'll try to avoid repeating some things I've written about Goffman and my (and CA's) relationship to him elsewhere (Schegloff, 1988).

Goffman was a shock to me. As I remarked earlier, I had been educated as an undergraduate and trained in graduate school up to that point as a classical sociologist (though I'm reminded that as an undergraduate I took a course with Roger Brown on the "psychology of language." Now why I did that I really don't know. So, there were apparently some concerns way back, perhaps as an offshoot of my interest in "knowledge"). I came to Goffman at Sacks' suggestion, and I reacted the way most conventionally-trained American sociologists would. A great many graduate students regularly reacted the same way, although the reservations were only occasionally articulated in class. When they were articulated, the other students would suck in their breath and wait expectantly for a nasty response, which Goffman was quite capable of delivering. I remember only a few of these episodes, in one of which I raised an issue which would ironically later come to be directed to *me*.

One of the things that many American sociologists would ordinarily think about Goffman in those days (this was about 1960–61) was that it wasn't "explanatory," but "merely descriptive." And I remember putting this to him in class during the only lecture course I took from/with him. He was a wonderful teacher, though not necessarily in the conventional way. He taught his own work (in his graduate courses, that is), and the term that I took the course with him he was writing the book that later appeared as *Stigma* (1963) and the course was ostensibly about "deviance." What he did was this. In the first three or four weeks of the course, he gave us a very compressed introduction to studies of deviance in sociology, casting the broadest net imaginable and by no means constrained by contemporary understandings of what might be relevant, but delivered in a familiar academic format. I don't think I ever took more extensive and detailed notes in my life than in those first weeks. But then, when he started to talk about his stuff on stigma, we got the very characteristic Goffmanian mode of delivery, often a simple listing of a series of "issues" posed by observations he had made or prompted by an excerpt from some book or magazine or diary etc.:"…and then there's the issue of XYZ, as when someone does ABC." And, being exposed for the first time to that kind of work, I remember at some point saying to him, "how is this different from journalism?" A gasp went out across the room because, of course, this was one major concern — that it was "merely descriptive" when there was no obvious technical terminology, and so on. It was a concern both of the students inclined in a descriptivist direction but still without a way of formulating a rationale for such work in the face of conventional critiques, and of students with conventional commitments who were reluctant to voice their reservations in an open arena.

But such anecdotes aside, much of what I wrote about the role Goffman played in sociology in my paper on Goffman (*op. cit.*) is surely true for his effect on me. He opened my eyes to a domain of inquiry that I just had no idea existed, even though I had been exposed to lots of social psychology as an undergraduate. I had not had much exposure as a graduate student because I was interested in "big issues," *Wissensoziologie*, and so on and so forth. But the notion that there was a world here (that is, in these little scenes of interaction), that it was accessible to inquiry (I didn't have the same concerns for precision and rigor at that point, or, rather, I understood them differently), this was revelatory. It was not an "ahah!" experience; he only had to say it and I saw it. It took a while to cultivate an understanding of adequate breadth and depth and articulate it with my previous training and education, and there were lots of impediments; but its impact on me was that it just opened a whole possible domain of work that I had not understood the existence of as a field of inquiry before.

I don't know that there's a whole lot more that I can say. Goffman supported me in various ways. There are a lot of bad stories, nasty stories, about Goffman, but I must say that he never conducted himself in a bad way with me. There were some eccentricities, but we always got on very well and, if anything, I was overly paranoid about him. I mean, he really did once give me cause for that, after my degree, when I was already in a very good job, though still a junior faculty member. His paper "Replies and Responses" (1976) was a kind of massive attack on CA, and we were after all still "kids," but in some ways it just showed his respect (in fact, a great deal of his writing after *Frame Analysis* (1974) — and even *in* that book — was addressed in some fashion to CA work, concerns, etc.). I regret that I didn't respond to it while he was still alive. In any case, he was a really smart man and an extraordinarily careful and perspicuous observer of the social world, however refracted through his own prism. There's no question: he had a distinctive vision but I think that was important. I don't think a person with a conventional vision could have come to do what he did.

In the end, Goffman provided a point of departure for the direction our work took, and our work seemed increasingly in tension with his. Much of that was a function of generations and of technology. I am told that in much of his teaching and occasional lecturing after he moved from Berkeley to Pennsylvania, he conceded that working from tape had become the *state-of-the-art* way of working, though he never committed himself to that view in print. He tried to work with such materials in some of his writing (for example, his paper "Radio Talk" in Goffman, 1981: 197–327), and I have been told by former students at Pennsylvania that he taught seminars based on videotape there, but the fundamental anchoring of his work was in extensive observations of the world in single exposures in real time and in his collections of fragments from written material, ranging from ethnography to confessionals, from fiction to memoirs, from training manuals to case reports. Our work started from the domain he had shown to be there, but was built on different foundations.

With Garfinkel the story was different. I think he would probably be most unhappy at the form his initial influence on me took, because he often specifically denies intending any critical stance toward conventional sociology. It is for him a form of practical theorizing, to be studied and understood together with other embodiments of practical theorizing, not to be criticized as a competing way of working. (Sometimes I think this was only ironic; sometimes I think it was meant seriously when formulated, though at other moments one could have heard Garfinkel speaking of conventional sociology and other social sciences in an unmistakably derisive idiom.) But a good part of Garfinkel's initial impact on me was what I took to be — however naively or mistakenly — its critical import.

I had migrated from one kind of sociology to another trying to find, as I thought of it at the time, "honest work." What I mean by that is that I would encounter some kind of sociology — some substantive sub-field or some methodological stance — and work at it for a while. There would then be colleagues who would ask challenging questions about it, or I would read a critical literature that found trouble with that way of working, and those critiques would seem to me compelling, and I found that I just couldn't do that (kind of) work any more. It wasn't that I was determined to do the perfect inquiry; I just couldn't do the work if I already felt that I knew it — the genre — was wrong, I couldn't practise doing it, I just couldn't — whatever I thought the particular problem with that genre was. The first major impact that Garfinkel's work had on me was of a critical sort, even though he forever denies that the point of ethnomethodological studies is some sort of ironic critique of sociology. Nonetheless, however wrongheaded it was, it allowed me to consolidate all the separate critiques I had of all the separate kinds of sociology I had tried to learn how to do. Whether correctly or incorrectly, all of a sudden I could see, for example, in the relationship between "indexical and objective indicators," or in the studies of "good organisational reasons for bad organisational records," or in the coding study, all those themes of Garfinkel's work allowed me to subsume under a single overarching "critique" what had previously been a whole series of separately specialized critiques. And that was a tremendous burden lifted off my shoulders because I didn't have to carry all of that critical baggage around with me. I could see a way of consolidating and having some sort of homogeneous grasp of the field (and, indeed, much more than the field), and there were all sorts of other sociologies I didn't have to "subject myself to," only then to learn what was wrong with them. So it allowed me to see almost in prospect that other areas were going to be just like things I already knew.

What wasn't clear to me, and never became clear to me, was what to put in its place within Garfinkel's way of working. My own mind does not work well in the phenomenological idiom. To this day, every time I have taken responsibility for teaching some of Garfinkel's work, I have had to read *Studies in Ethnomethodology* (1967) again, and each time it has been "news" to me all over again. I would stop at various places after having read something and think, "Gee that's clever," and then I would vaguely remember every other time I had read that essay, and when I got to that point I would say "oh, that's clever," and I had said it again this time. That's just not the natural idiom of my own mind, and though Garfinkel was clearly something quite distinct from a phenomenologist, it's clear also that his work and his world view are very much cast in that idiom. And in many ways his undertaking was deployed as fundamentally a "critical discipline." For me, it fairly quickly became not satisfying; I just couldn't find the affirmative program there. In any case, whether the effect Garfinkel's work had on me was intended on Garfinkel's part or not, whether based in misunderstanding or misinterpretation on my part or not, for me his writing worked to consolidate a critical stance toward a great deal of conventional sociology, and to alert me to some issues which have remained continuing analytic constraints — ones which I got nowhere more forcefully than from Garfinkel's writing (though not necessarily in the form in which he expressed it or in ways he would any longer accept). For example, I don't think of it as "commonsense knowledge," but as "vernacular knowledge;" the relationship between vernacular knowledge and technical inquiry is something that certainly was not invented as a topic by Garfinkel, but Garfinkel introduced it into contemporary sociology in a form which, at least at the time that it intersected my life, was much more compelling than other ways in which I had encountered it before. I had gotten it obviously from Parsons with whom I had had a series of private reading courses at Harvard years earlier. It didn't make any big impression on me with Parsons.

So, I learned a lot from Garfinkel, and spent a lot of time explaining and defending his work in various sociological venues. But I think Garfinkel sensed from early on — I've never actually asked him this — that I was not as much taken with ethnomethodology as Sacks and Sudnow were. Garfinkel held a number of conferences on ethnomethodology in Los Angeles at the time I was still in Berkeley. I wasn't invited to the first couple of them. When I finally did go to one, the experience was exhilarating. Among Garfinkel's many distinctive characteristics was (and is) an amazing capacity to listen in a perspicuous way to what others say, and to hear in what they say something they never dreamed of saying themselves, and to appreciate it and applaud it. He did that for me, and it was an extremely heady experience. My presentation was given pride of place. It was a Saturday evening in his living room, a group of some forty people crowded around, the tape recorder on, and I had this paper which I'd published in the *Berkeley Journal of Sociology* (1963). It was on psychiatric theorising as part of that "insanity" project that I had been doing. With Garfinkel leading it, the reception was, of course, intoxicating. Nonetheless, I felt I wasn't entirely *of* that group, but its effects on me were there, and Harold and I have always had a relationship alternating — and combining — tension and support. He is, after all, primarily responsible for my being at UCLA. I've made contributions of my own, I think and I hope, to his side of the ledger.

Three people — Goffman, Garfinkel, and Sacks — made a critical difference to my scholarly development. And I think in each instance there has been some mutuality of effect. The fact of the matter is that Goffman's last work in *Forms of Talk* was largely a dialogue with conversation analysis, and CA has I think been of consequence for Garfinkel. And Harvey and I, of course, went in an entirely different direction once we encountered each other, though most of what Harvey got from me went to the grave with him because it wasn't written.

C.L.P.: Conversation analysis is concerned with the discovery and analysis of verbal procedures in human interaction. There are conversational procedures as object of study, but also procedures of discovery and analysis of conversational procedures. What are your ideas about the cognitivist interpretation of human cognition and interaction as corresponding to some kind of procedure? And what do you think about the idea of social cognition?

E.A.S.: Well, it seems to me that there are two questions in search of responses here, if I understand the question properly. One concerns the relationship between conversation analysis and more cognitively-oriented undertakings, perhaps even cognitive science. The other concerns the relationship between the practices of ordinary conduct in mundane settings of social life and the practices of our inquiry into those practices. Both questions present "tall orders," so I'll try to be brief at the risk of being unsatisfying.

Although conversation analysis was once taken to be part of the nascent larger development called "cognitive science," there are contrasting presuppositions which underlie them and render such a "merger" problematic. I can only sketch a few.

In general, a cognitivist stance begins with the broad cultural presuppositions of the so-called Judeo-Christian stream of European culture. That cultural tradition (and cognitivist and other "psychologically-oriented" disciplines emerging from it) takes the single, "minded," embodied individual person as the basic, enduring, integrally-organized reality to be studied. The setting such a "person" is virtually always in, the complement of other persons in that setting, etc. are taken to be contingent, transient, ephemeral contextual properties. Settings are treated, in effect, as composed of an aggregate of such "individual person realities," perhaps adding something (something "social," which is thus treated as external and subsequent to the constitutive reality of the individuals) to the *given* features, capacities, resources, predilections, etc. of those individual persons, rather than shaping or even engendering those features, capacities, resources, and predilections, and therefore, in a sense, as *constituting* the effective actors/participants in those settings. So when a little group or conversational cluster breaks up — like the one composing the present interview occasion — each of the embodied named individuals who composed it will be taken to continue to exist, even if not accessible to perception, but the group that has (as we say) "dissolved" is taken not to continue to exist. The episodic setting, the little interaction system, as Goffman might have called it, is taken *not* to have perduring reality.

But, as Goffman (1967: 3) conveyed in his telling contrast between "men and their moments" on the one hand and "moments and their men" on the other, there is an alternative way of conceiving matters. We can understand "the situation" as the reality, and the individuals who happen to compose the situation on any particular occasion as what is transient. A scholar of classical Greece named John Jones some years ago (1962) wrote a book called *On Aristotle and Greek Tragedy*, in which he argued that it is mistaken, or simply a subsequent cultural imposition, to treat the Oedipus myth as involving a tragic hero. That grows out of a tacit ontology in the Judeo-Christian stream of western culture that it is the single, "minded" and embodied individual that is the locus of social reality — here realized in the notion that the person named Oedipus is the locus of the play's action and import, and its "tragic hero." The alternative view is that there are certain sorts of recurrent situation that are the locus of tragedy (as well as of other "narratives," as the current parlance would have it), and the point of putting Oedipus into one such situation is to make the point that if a king, who is the son of a king, could be battered by the world by being caught up in this situation, how much more so is it the case for "lesser" individuals. But it is the situation which is the relevant reality, the effective source of Oedipus' — and any person's — story and fate. The individuals who are caught up in it at any given moment are what is transient.

Well, when you juxtapose these two ways of seeing what's fundamental and what's transient and relatively epiphenomenal, and especially when you see that the second view is clearly the minority view in western culture and in the contemporary academic scene, it becomes increasingly important for those who have found a way to study matters, human and social, in the second way to insist on studying them that way. Fundamentally, cognitive science is a thoroughly psychological enterprise, and saying that it's a thoroughly psychological enterprise is to say that it falls in step with, rather than resisting or giving us any leverage on, the otherwise inbuilt cultural presuppositions that a great many of us share as members of western culture.

I should add one further point, though I can't go into it in detail. What I have suggested above about the focus on the single individual gets carried further in cognitively- and psychologically-oriented inquiry by a focus on the single sentence, the single act or action, etc. as the target of study and the fundamental locus of reality. We see this not only in contemporary linguistics, but in enterprises like speech act theory. The very conception of action having its origins in the acting individual's "intention" treats the single action as the unit to be analyzed, and the single individual as the proper locus of its analysis. Thought about in the abstract, this may sound unexceptional to academicians trained in a scientific culture grounded in the dominant strand of western culture. But if you look not at *imagined* actions but at *actual* ones, it becomes not only unviable, but almost peculiar. And here again the availability of tape-recorded, repeatably inspectable material, is deeply consequential. If one is committed to understanding actual actions (by which I mean ones which actually occurred in real time), it is virtually impossible to detach them from their context for isolated analysis with a straight face. And once called to attention, it is difficult to understand their source as being in an "intention" rather than in the immediately preceding course of action to which the act being examined is a response and to which it is built to address itself.

So an approach to work that starts from the individual as the real — whether the individual person, or action, or utterance, or sentence — which treats that individual entity as designed for integrity as a free-standing object with its context as an extrinsic environment, can hardly avoid being characterized by atomism, atemporality, ahistoricism, and asociality. And the study of interaction and of humans in it would do well to avoid such a path.

Such a view is not incompatible in principle with an interest in studying cognitive matters, but it places cognitive issues, processes, etc. within the framework of a world which is social and interactional from the outset, within which cognition is to be understood not necessarily by reference to the individual cut off from a world around, but by reference to an individual engendered and constituted by the world around in the first instance. A "cognitivism" or "cognitive science" along such lines, and responsible to details of naturally-occurring interaction in ordinary-for-the-participants settings, would be of considerable potential interest.

S.Č.: I think that not all branches of philosophy of mind assume the "single individual" as their basis and ground. There are also disciplines of mind that are based on the assumption that the pair of persons is primary, not one individual, but two people interacting or trying to understand each other.

E.A.S.: Give me a name or two.

S.Č.: Don't know. Habermas, for example.

E.A.S.: Ahh, I think not. Habermas, it seems to me, made a fatal misstep very early on (e.g., Habermas 1970) when he incorporated what is essentially Searlean (or Austinian-Searlean) speech act theory (in the key respects that matter here the differences are of little moment).[5] That's one of the problems, because I think it is very difficult to recover a socially or historically sensitive view of action once you've started that way.

There are other related problems with the tack which Habermas takes which make it an unpromising alternative, at least for work which means to be "empirically capable." It's critical to the larger program of Habermas' studies to have a pre-analytic conception of rational discourse as the model, the critical leverage, with which to critique the "distortions" introduced into actual communicative action by malformations of social structure. But this presupposes that we have or can develop in full measure what Giddens calls "discursive consciousness" for our conduct in interaction, if we are to have a pre-analytic, pre-empirical grasp of its rational character and possibilities. In a recent paper (Schegloff 1996a) I describe what I think is a "new" social action, that is, an action that I did not know existed (and, as far as I can make out, that other people didn't know existed either). It bodes ill for the possibility of a pre-empirical, pre-analytic pragmatics; it seems to entail that you have to have an empirical grounding to understand what this form of human communicative action is all about, and if that's so, you can't have it pre-empirically, pre-analytically and use that as the critical leverage for a vision of rational communication.

S.Č.: In European philosophy, perhaps Buber, for example, or Levinas in French philosophy.

E.A.S.: Yes, I read Buber a long time ago, but I haven't for many years, and for that reason perhaps, I never approached him seriously as a figure in the investigative enterprise we are discussing, but rather as a figure in theological discourse. That's an interesting suggestion. I will go back and look at Buber again. Levinas, I'm afraid I don't know and cannot comment on. But I'm happy to hear that there are others working along such less individualistic and atomistic lines. Recall though that it was not philosophy that I had my reservations about, it was psychology, and those forms of philosophy which adopt an empirical-sounding diction without having done the sort of work which would warrant it. But if there is hope to be had on any front, I'm perfectly happy to have it.

The other question lurking in this portion of our discussion may have been more in order under a more cognitivist understanding of conversation analysis; it would then have been the reflexive question: "If you are studying processes or practices of knowing, then what do you have to say about your own practices of knowing?" But there may be something of interest to be said here nonetheless, and that is how the practices of understanding and describing conduct in academic/professional inquiry relate to the indigenous practices of understanding and describing conduct in ordinary interaction itself. Discussion in this area could get very complicated indeed, so let me limit it in the following way.

Sacks pointed out (Sacks 1972b, 1992 (I): 236–266) about ordinary or vernacular or common sense description that it is recognizable as such without inspection of the circumstances or objects being described. In that discussion, Sacks sketched the importance of this feature and the economies which it affords the conduct of ordinary affairs. There are domains, however, in which the practices of describing and taking up descriptions are different — in which descriptions are in the first instance to be juxtaposed with what is ostensibly or purportedly being described, and description grounded in these practices operates differently — it does not afford the same economies, but delivers outcomes potentially different in kind from those of ordinary description.

What may appear a merely stubborn insistence in conversation analysis in grounding all work in the details of actual data, ideally with the recorded version present, but with at least the transcript if that is not possible, has a grounding not only in our past experience with the productivity of proceeding in this way, but in the commitment to a different enterprise than the practices and forms of description that characterize mundane description. Papers take the form they do to maximize the opportunity for readers to immediately juxtapose every bit of description with the data of which it claims to be a description. One basis for reservations about other forms of inquiry which appear to intersect on the same subject matter but use different research methods is grounded here. To take but one example: in ethnographic work, the investigator gets to observe occurrences once in real time. Even the best ethnographer or ethnographic team will register only "the take" possible under this constraint. Under ideal circumstances, field notes are made as soon as possible, but are grounded in the ethnographer's memory of the events that were seen. The text of the ethnographic report draws together those notes and memories into an (ideally) coherent account of the object of description. The upshot is that the reader must essentially take up the description with no access at all to the object of description itself, but at best with access to an account reconstructed from notes grounded in memories of the sorts of observation that can be made with one exposure in real time (and under the constraints of the operative "social and cultural organization of seeing" in that context). The practices of description and description-uptake underlying these two approaches to a "same subject matter" are sufficiently different to call into question whether this is really the same enterprise and the same subject matter taken up by two different approaches or methods, or whether two quite different sorts of undertaking are involved.

This may not be quite what you had in mind by asking about the bearing of our research targets on our own research practices (if you were indeed asking about that), but it is for me a compelling outcome of reflection about just that issue.

C.L.P. : Before coming to your research on pragmatic deficits, I have just one further question: what about the "interaction order" according to Schegloff?

E.A.S.: Oh, Goffman's article? What about Goffman's article?

C.L.P.: How do you situate yourself in relation to the idea of the "interaction order" and to that article, now, fifteen years after its publication?

E.A.S.: I haven't read it for a long time. There is a lot of vitality to the idea of the interaction order. To the degree that Goffman is one of the main feeder streams to the sort of work that we do now, it's in part his calling attention to the existence of that domain of organization that is responsible. But if I have to imagine what reservation(s) I might have, if I sat down and read it right now, it would be whether one would still or would want to retain the severity of autonomy and disjunction which I recall when first reading that paper. And the reservation would extend in two directions.

Given Goffman's own earlier writing, it was not surprising that he would have taken this stance in much the way that he did in "The Interaction Order." I don't know if you're familiar with a paper of his that was published in 1961. It was in a little book published by the Bobbs Merrill Advanced Studies in Sociology; the book was called *Encounters* and the first paper in it was called "Fun in Games." In that paper he coined the term, "the membrane around the interaction" (or something like that), and one of the things he was concerned with there we would now speak of, in contemporary parlance, as the difference between discourse identities and other identities. One of his points was that the membrane that surrounds an interaction — that marks how and where it is bounded off from the surrounding setting and world — can serve to filter out a lot of the things that are in some sense "objectively" true about the individuals who compose the interaction. Many — perhaps most — of the identities, memberships, debilities and strengths, achievements and stigmata which are in fact "the case" about a person do not permeate the membrane; they do not necessarily affect the conduct that goes on within it; they do not survive the test of relevance. (Of course, he is overtly concerned with "fun in games", but the "game" he is ultimately interested in is all of interaction, and much of the text is about that. That's a common strategy for Goffman; he begins ostensibly talking about a very particular and limited phenomenon, but by the time he is done, it is everyone's contingent reality. Just think about "face," "demeanor," "stigma," etc..)

One of the points I think is most important — whether it is recognizable already in Goffman's take on "the interactional membrane" and what gets through it or not — is that this is not for analysts to decide. This issue has figured centrally in subsequent conversation-analytic thinking about various identities or categories of membership of persons in society (as for example in Sacks 1972a, 1972b; and see also Schegloff 1991b, 1997c). As with everything else, it is the participants who embody in their conduct which features of their co-participants they are oriented to as relevant and which not (though, to be sure, efforts at concealment and camouflage can be at work as well); that is a *contingent* matter. And features of co-participants which are "macrostructural" in the terms of social science theorizing are subject to that contingency as well. This seems to me, at the very least, the appropriate default position from which analysis must begin. Someone could undertake to show that some identities — for example, gender identity, to cite one which is often urged in this regard — are "omni-relevant," and are never fully filtered into irrelevance by the "interactional membrane" (though what a fully satisfactory demonstration would look like is not entirely clear). This then could be one possible reservation about Goffman's account of the interaction order — that one cannot theoretically legislate out of existence the prerogatives of participants in interaction to treat as relevant features of their co-participants ones which are macrosociological in character, which would compromise the "separation of orders" which many take Goffman to have asserted. But it is quite possible that if I re-read Goffman's paper, I might well find that this problem does not actually arise. In any case, my only objection to the conventionally claimed interfaces of the so-called micro-social with macrosociology is the insistence on the inescapable and often exclusive relevance of, to use the terms that are most powerful in contemporary American sociology, the intersection of race, class, and gender. My objection is only to people's insisting that the only exclusive, centrally important thing is whether someone is a woman or a man, this or that ethnicity, and this or that social class. But that the co-participants can treat those on any given occasion, or some moment in it, as relevant (and potentially consequential) seems to me to be beyond question. As with everything else, it seems to me we have to put our analysis at the disposal of what the participants are actually doing. Now, that may be compatible with the autonomy of the interaction order or it may not be. So that would be one possible reservation. And that is, so to speak, a reservation about the filtering down of the more macrostructural into the more "microstructural."

The other reservation is going in the other direction, conventionally speaking. Here, I suppose, it's more a diffuse scepticism than it is a determinate reservation. This is I suppose a leftover for me of the Garfinkelian "critique." As a member of the society, I share in the vernacular culture, and mine is American and sociological and upper middle-class and Jewish and all the other sorts of things that frame one's "social location" in vernacular terms. As a member of society, I perfectly well understand about social classes and all the rest of a moderately sophisticated citizenship; but the fact that I understand and see the world — or *can* see the world — in those terms *as a member of a society* is not the same as qualifying all those ways of seeing it technically, let alone subscribing to it and underwriting it as part of one's technical apparatus for understanding the world.

In fact, it's just the opposite. The more they recommend themselves to my vernacular understanding, the more suspect they ought to be for me as part of my technical apparatus. The common or vernacular culture is, after all, a sort of "propaganda arm" of the society, serving to undergird the cultural component of the more or less smooth functioning of the society itself, not to advance or enhance a rigorous *understanding* of society.

And so there's a question here, because what Goffman in effect does is, by implication, to ratify all of macrosociology as not "his business," but he appears to stipulate that there surely are all these economic structures and political structures and bureaucratic structures and so on and so forth. I don't know whether he did that as a vernacular member of the society or as a technical sociologist. He did it in his Presidential address to the American Sociological Association. He surely was aware of the fact that he was at risk of being understood to be saying it as a sociologist. And it seems to me that there are enough reasons to be uneasy about that. It's not that there aren't ample things that you could point to within the domain of rigorous inquiry to make them serious things to entertain, but entertaining something seriously and seeing how actually to work it up as a robust part of one's technical understanding of the organisation of social life are two very different things. And for me, as I say, the fact is that what we know as part of our vernacular knowledge is part of having the *society work properly*. And that's very different from how to have *a discipline of the society work properly*. Wherever those things look like they're bumping into each other, I think — especially because we can be treated to be experts about the matter — we have to be specially careful. Again, if I re-read Goffman's "Interaction Order," I might find he was way ahead of me on this and that he's anticipated all of these concerns; but that's certainly not the message that has filtered down to us in the field about the interaction order.

C. L. P.: Can you say something now on your work on pragmatic disorders, and give some suggestions for analysts in that domain?

E. A. S.: Well, first I have to say that I obviously have no competence to help people therapeutically; we are talking here only about helping people who would like to do research work in this area along conversation-analytic lines to situate themselves better for doing that with some success.

I think that the key thing — and there are historical grounds for saying this — the key thing is for people to get themselves properly trained in analyzing materials of talk and interaction of *whatever* sort. In the past, when people have used some particular, predefined, practical interest to inform or constrain general training and the general course of research, there have been problematic outcomes. I'm going to just repeat, if I may, some things I have written about this (Schegloff 1991b: 66–7) because they may be of interest in this connection.

For a very long time, you could not get a societal "license" to study ordinary interaction closely — either in the educational or in the research sense of "study." These were things that presumably we all knew because that's what the meaning of "commonsense" was; why would you waste your time, why should the university waste its money, in supporting a degree of this sort, or research of this sort. So the only way such inquiry on "ordinary, everyday behaviour" was done was typically under one of two conditions. Either the participants in the interaction to be studied were formulated as "defective" in some way — and so you could study people who had had strokes or who were mentally retarded or were schizophrenic and so on and so forth; or the activity to be studied was so strategically positioned in social life that the activity could be made more "profitable" in some sense to the society if we understood how it worked better — so bargaining, negotiation, conflict resolution, "salesmanship" and the like could be examined closely. You couldn't study ordinary interaction just in order to study ordinary interaction.

A consequence of that situation of inquiry historically has been that people would study a particular corner of the world under the warrant of one of these "licenses," and, whatever the auspices were under which the inquiry was conducted, the results were taken to be specific to that object of inquiry. So when people studied schizophrenics, that was an acceptable thing to study because of the promise of therapeutic payoffs. When the focus turned to thought and language (I am thinking here of work in the first half or so of this century), much of what was found out was taken to be characteristic of "schizophrenic thought and language." In the last thirty-five years or so, when some of us were able to make a little space for studying ordinary talk in interaction, mostly without substantial research support but with our own funds and on our own time, it would often enough turn out that things that had been figured to be specially characteristic of "schizophrenics" or "retarded people" or other such "special" categories of person were actually quite common in ordinary conversation. That is not to say there is no difference between schizophrenics and ordinary people. It is to say that, if you have not studied ordinary people (and been *trained* properly to study ordinary talk in interaction), you have no way of figuring out what is specific to schizophrenics and what is the case about conversation *per se*, except that, like so much else that is "common," it generally falls below the threshold of ordinary observability.

So, somebody who's interested in doing research on pragmatic deficits and neurologically-impaired people has to begin, it seems to me, with understanding how ordinary interaction is organized among people without respect to neurological status. What I am saying seems to me obvious enough a point: anybody who wants to practise something in particular in a domain (playing Bach toccatas, doing cardiac surgery, training retrievers, writing sonnets, etc.) needs first to become adept at practise in that domain in general (playing the piano, doing surgery, training dogs, writing poetry). And if you want to study some particular thing, you need to know how to do research in that domain, for example, how to analyze talk and interaction and body behavior and so on and so forth among humans. So, the first thing is to learn how to do good work and bring it to bear on *any* data.

If somebody was contemplating working in this area of the pragmatics of neurologically-impacted people, and was in search of additional components of distinctively relevant training, it might be interesting to have some people who were both competent and sensitive analysts of talk and interaction and who knew something about neurology as well. Surely there are some things that can get done when all the relevant input and knowledge are controlled by the same person and not "distributed" amongst several individuals; there are things you can conceive of if various facts that have never been brought together are in the same mind. But that's a very big order for someone who's undertaking to be trained.

In most (if not all) respects, however, concentrating on pragmatic deficits or neurologically-specialized data is just one instance among many of focussing one's work on some sub-domain of data, whether defined by technical criteria or by commonsense categories of the society at large. So the next suggestion would be that, for the health, vitality, quality of work, and continuing growth both of individual researchers and of the field as a whole, students in this area (and I use "students" here in its most general sense, and not referring to a stage of life and career) need to continuously play back and forth between the specialized domain that they study, whatever it is, and the ordinary run of human interaction. Many workers in the field, both senior and junior, have cultivated specialized domains of work which articulate well with the current organization of universities — with traditional disciplines and recognized subfields within them (for example, institutional sectors such as medicine, law, education, communications within sociology, or subfields such as pragmatics, discourse and the like within linguistics) — and other organizational centers of research in order to maximise their own individual chances in the employment markets as well as the institutionalization of this type of research in the society. But from my point of view, it is absolutely critical — and I think my colleagues at UCLA John Heritage and Steve Clayman agree with this — that students should first start getting trained on ordinary conversation, not on particular institutions. But even more important is that, even if they have developed a specialized knowledge in some particular institutional sector, they nonetheless keep working on two fronts — both in that specialist institutional sector and on ordinary conversation. There are several reasons it seems to me for this.

The first is that no institutional domain is totally segregated from general social life. Everybody knows that when they go to see the doctor or mechanic or salesperson, the talk slips in and out of the institutional framework. Not all the talk *in* a work setting is *of* that work setting, and this contingent character of conduct is not specific to work settings. If you lock yourself entirely into the institutional domain being studied and assume that once the episode has started as a professional interaction it will be a professional interaction, you simply are not in a position to understand what's really going on. Not only do you become un-alert about the non-professional aspects and potentials of the talk; you disattend how the very professional character of what is professional is produced *as* professional.

A second reason is that, because the society is prepared to support the work in particular professional domains defined by its vernacular culture, there is a serious danger that we would begin no longer to get general analytic tools being developed because we would less and less be having general interactional practices being studied. We would keep learning more and more about doctors and patients, teachers and students, cooperative work groups, news interviews and so on, but we would not be developing our understanding of the generic practices of talk-in-interaction and the tools for analyzing them. All of that could become really stagnant. We could have the pool of analytic resources that we developed in the first fifteen or twenty years which could just stop growing. But it has to grow, and the same people have to help it grow as are making the separate more specialized substantive areas grow.

So, what I would say about and to people who are interested in neurologically-compromised participants in interaction isn't unique to them. Here, as in any place else where there's a knowledge-based skill, whether it's the practise of medicine or of music, you have to have your craft. You have to know how to do the basic work. In this area:

– you need to know how to collect data and have recurrent experience collecting it yourself, because you often enough have to be on the scene where it was collected to know what that scene was like;
– you need to be transcribing it yourself, all the time, and not just hiring others to do it, because then you don't know what the data sound like. You don't give yourself the best possible opportunity to hear something entirely new. It's you, after all, who will over time come to have ten or fifteen or twenty years of experience, who are now in a position, if only you listened to the raw data, to hear things you could never have heard before you had that experience. Your assistant almost certainly doesn't yet have that experience.
– You have to make the basic observations and ground them firmly in the observable details of the material. That's the basic craft. If you don't do that, however fancy the written papers look, they'll be based on water and eventually somebody will come along and actually look at the material, and the ungrounded, clever writing will all collapse.

So, people have to have their craft under control and they have to keep it updated. Then it's a question of intuition, skill, learning, and luck. There's always luck, right? You're lucky you get the right data, you happen to be sharp on the day that something comes up, so you see something which on a day when you were dull you would never have seen. But that, of course, we all live with, no matter what we do. So as for the "luck" part, there's nothing to be done. The other parts you can do something about.

C.L.P.: I don't know if you would like to add something concerning future directions for research, I mean some suggestions for younger researchers …

E.A.S.: I suppose only this: this kind of work is right for some people and it's not right for other people, and I have given up trying to figure out in advance which is which. I wish I could, because it would save them and me a lot of time and pain if I were to spot it early enough. There are some people who decide wrongly that it's not for them, and they decide that because they've always been 'A' students and I don't give 'A's just because people are attentive or loyal. If they do good work, they get 'A's and if they're not doing good work *yet*, they get 'B's, and some students figure that the latter grades are telling them they're not wanted here. That isn't the message; the message is that they haven't "got it" yet — not that they are incapable of getting it. So, I would urge students who feel themselves drawn to this kind of work, who feel that it gives them a kind of insight and access to interaction and culture, or think that it might, to stick with it for a while. In my experience, it requires of most students — and most colleagues, who have come to terms with it after a previous professional training — a tremendous wrench, a tremendous transformation in the way they see the world. I forget about that from time to time. The students remind me of this almost painful reorientation, and other students have to know this; that this seems often to be quite different from simply taking on a new academic subject and absorbing it like one has absorbed the previous ones. So I think the thing to say to students is, first of all it's a long, hard road. If they are not prepared for this, if they must get big payoffs early on, this may not be the way to go. They have to take stock and decide whether they can stay the long course. Some people find the exposure transformative once they get into it (I hope this will not be taken in the wrong way). I can't tell you the number of students who've told me later on how this work has changed their lives, that they see the world in a wholly different way, that they found themselves with a commitment to working that's just of a different order — that's grounded in the world in a different way — than their previous academic commitment had been, and these were all obviously successful people to begin with. They survived to the point of graduate studies with financial support; they haven't ever been "bad" students.

It's easy for me to forget that, I suppose, I'm now part of the establishment. That's how we come to be doing this interview. Thirty-five years ago this was a brand new venture, and I find it's still exciting, and I still find that students who come into it feel that way. It's not easy to keep that spirit alive in a world that treats you as the older generation and the establishment. But it seems to me that it's this spirit — a sense that this work is providing a different kind of access to what it is to be human — that somehow still inhabits the work as an enterprise. It is not passed on from person to person. It is passed on by the nature of the enterprise to those who come to participate in it, to be stewards for it.

I think I'd better shut up. Thank you.

S.Č. and C.L.P.: Thank you.

Notes

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1. Actually, I recently had occasion to draft about half of the chapter on word selection in preparing a paper for a volume on anaphora which focuses on person reference (Schegloff 1996b), and this is to be taken up in the chapter on word selection.

2. For example, Jefferson 1989 and 1993, among many others.

3. For example, Heritage 1992; Heritage and Greatbatch 1986; Heritage and Roth 1995, among others.

4. And have nonetheless left a lot out, for example my parallel education in Jewish studies, through my college years; my first degree is a Bachelor of Jewish Education — B. J.Ed., 1957 — the year before my Harvard B. A.. When I cast my lot with the study of interaction, the path I did *not* take was the study of the expected demise of the Yiddish language and the culture revolving around it, a project I was going to call (borrowing from the British literary

critic, Christopher Caudwell (1938), who meant something quite different by it) "Studies in a Dying Culture." The sustained engagement with topics in which language figures seems quite likely related to my growing up with three of them as a child (English, Hebrew and Yiddish) and three more in school (French, German and Latin), not to mention music.

5. I discuss this form of speech act theory in greater detail in Schegloff 1992b: xxiv-xxvii and 1992d. The bearing on Habermas is briefly addressed in 1992c: 1139–1141. Other problems with Habermas' stance towards communication and its place in social life and inquiry into it are taken up in Schegloff 1996a: 209–212.

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