

From *Aha* Moments to Ethnomethodology: A Conversation with Hugh Mehan

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On April 1, 2015, Professor Hansun Waring's doctoral seminar had the great pleasure and honor of being joined over Skype by Dr. Hugh (Bud) Mehan, Professor Emeritus at the University of California, San Diego and author of the 1979 classic *Learning Lesson: Social Organization in the Classroom*. Dr. Mehan has done extensive research on classroom organization and interaction, educational testing, tracking and untracking, construction of student identities in the classroom, and so on. He has managed to achieve the delicate and yet crucial balance that so many educators and education researchers aspire to—the balance between doing research and improving the world. He has not only shaped the academic discourse on education issues but has been directly working with the most vulnerable members of our society in overcoming and amending those issues. His work has been an inspiration to so many of us whose research passions and real-life concerns lie in education, classroom interaction, and social inequity.

The Skype meeting was proposed as a continuation of conversation inspired by Dr. Mehan's invigorating invited lecture at the 4th Annual LANSI Conference at Teachers College in October 2014. Members of Dr. Waring's Spring 2015 seminar (Nancy Boblett, Catherine Box, Sarah Creider, Donna Delprete, Rong Rong Le, Heidi Liu, Carol Lo, Saerhim Oh, Elizabeth Reddington, Gahye Song, Nadja Tadic, Junko Takahashi, and Di Yu) compiled a list of questions for Dr. Mehan in advance and asked follow-up questions to his responses during the meeting. We are very pleased to share with our journal readers a transcript of our conversation, and we hope you will find his words as inspiring and illuminating as we have.

Question: In your (1979) book, you referred to your work as “constitutive ethnography.” Did you make up that label?

Dr. Mehan: Yes, I made it up. It was a historical moment and I was struggling with the best way to characterize the work that we had started to do, which included not just ethnographic orientation, which has traditionally meant detailed participant observation, the researcher going into the field, trying to understand the practices of the natives, writing them up mostly without any kind of technical assistance other than pen and paper. A number of us, the group closest to me included Fred Erickson, Ray McDermott, Courtney Cazden, Peg Griffin, and Mike Cole, started to use videotape as a data-gathering tool. In a paper I wrote in 1978 called *Structuring School Structure*, I pointed out that the kind of work we were doing in gathering and analyzing videotaped materials provided a different perspective. We were not proposing that one method was perfect and the other was imperfect. Likewise, it wasn't that substituting videotape for paper and pencil was going to solve all methodological problems. But this practice did give us the ability to look at materials over and over again and allowed us to do more particular, more specific, more detailed analysis.

Having said that, detailed analysis also raised several questions and problems, one of which is point of view. Let's say I've placed my video camera at the corner of your classroom, and the camera is wide-angled and captures everybody, and it's very clear. Notice there's a point of view here. I see everybody in wide angle, but I don't have available any one-to-one interactions that might go on among people or any side comments or anything of that sort. So at that time there was an attempt to rescue those methodological issues, and I didn't quite know what to call it.

My friend Fred Erickson had introduced the term *microethnography* at about the same time in the work that he and Jeffrey Schultz published. I didn't like the idea of micro because in sociology there is a micro-macro distinction. *Macro* is the big picture, for example, global politics, globalization, exchange of capital, etc. And *micro* is the face-to-face interaction that occurs in circumscribed events. And at least in the world that I inhabited the micro is disparaged and not as important as the macro. I was entirely against that stratified notion. *Constitutive* or *constructive* or *constructivist* were terms that I had begun to use because I was trying to make a point, influenced by ethnomethodologists, that actions that occur in face-to-face encounters are responsible for constructing larger-scale social structures. My research has hopefully shown that face-to-face interactions among people, in classrooms, education testing situations or counseling sessions, etc., constitute aspects of a student's life. They might constitute or construct a special-education student or a handicapped student or a gifted and talented student, etc.

I found the word *constitutive* to be helpful and I preferred it over *micro* while attempting to distinguish that kind of close analysis from general ethnography. So I settled on constitutive ethnography. In retrospect, it made a point; some people used the term, but it didn't really catch on.

Question: Why didn't you characterize what you did as ethnomethodology or even CA?

Dr. Mehan: I was of the mind that the kind of work that my colleagues and I were doing was more informed by ethnography than what I understood conversation analysis to be in the 70s. My orientation to that conclusion was informed by informal seminars with Harvey Sacks and Manny Schegloff and by my advisor Aaron Cicourel. We were doing something different than conversation analysis, therefore it wouldn't be appropriate to appropriate that label. In retrospect, I could have and perhaps should have called it a research method informed by ethnomethodology, in light of the way history has been played out. But I didn't come to that conclusion at that moment.

Question: What are the philosophical schools of thought that shaped your research and theory?

Dr. Mehan: In the book that I wrote along with my good friend Houston Wood called *The Reality of Ethnomethodology*, which came out in 1975, we spent a chapter describing the philosophical origins of ethnomethodology. We located these origins primarily in European schools of thought. We claimed that ethnomethodology was an interesting amalgam of phenomenology and ordinary language philosophy, the work of Wittgenstein and J. L. Austin primarily. Austin and Wittgenstein make powerful philosophical, theoretical points that language is action, that speech is action. When people talk, they're not just referring to objects, you know like the word 'coffee cup' could stand for that plastic cup from Starbucks (referring to a cup he

sees in our seminar room on the Skype screen). Speech is not just referential; it is constitutive action.

That being said, the really interesting thing about ethnomethodology is its rabid empirical focus. If CA is anything, it is thoroughly empirical. You know how demanding the analysis of conversation is. Findings have to be located in the talk. They can't be just alluded to. Talk can't be used as just something that stands for something else. That combination of phenomenological orientations and the empirical demands of conversation analysis influenced my attempt to do work. The constitutive elements, the constructivist elements were influenced by phenomenology. The empirical demands were influenced by conversation analysis.

Houston Wood and I also pointed out that ethnomethodology embodies a constitutive approach, looking at how the social order and social facts were constructed in interaction. The *Learning Lessons* book claims that the problem of social order is not just a problem that sociologists study. The problem of social order is a practical concern that teachers face in the classroom moment to moment. High-school students, college students, graduate students—they have this problem of social order figured out. But little kids have to learn how to do it. Therefore, we proposed that the turn-taking structures and procedures that Harvey figured out were actually contributing to the maintenance and co-construction of social order in the classroom. The solution to the problem of social order in the classroom was being facilitated by conversational practices.

There were other influences on my version of ethnomethodology. One was the work of George Herbert Mead and his interpreters that in sociology came to be called symbolic interactionism. The other was the work of C. Wright Mills, which introduced the political dimension into my work. When I was in graduate school I was fortunate to be at Santa Barbara at a time when ethnomethodology was starting and taking off. Some of my professors, like Aaron Cicourel who became my advisor; and Don Zimmerman, Larry Wieder, Tom Wilson were all members of the faculty on the ethnomethodology side. On the symbolic interactionism side we had Tomatsu Shibutani, who was one of Mead's last students; Donald Cressey, who was famous for his studies of crime from an interactionist point of view; and Tom Scheff, the sociologist famous for talking about mental illness as a social construction. As a consequence of this amazing collection, the graduate student cohort that I was part of really had powerful influences. Our work blended ideas—to a certain extent from symbolic interactionism and ethnomethodology—into a constructivist orientation.

Sarah Creider: How can you stay close and true to the small picture—the specific sample of interaction under analysis—when you also have big picture questions and ideas concerning social inequality?

Dr. Mehan: I think that's one of the big challenges that any of us doing empirical work face, especially if we consider that what's happening in interaction among people is influential on any kind of outcome or product. One of the ways in which I departed from CA work is contextualization. What I learned from Manny Schegloff and Harvey Sacks was that you started with the talk, and what's available to you in the talk is what you're able to discuss. But also being strongly influenced by ethnographers, I developed the understanding that broader social context in which talk is embedded is important. In a sense it's not impossible—but it's really

difficult—to suspend contextual features and institutional or organizational features when analyzing the close work between a teacher and students in the classroom.

Let me give you an example from my own work to illustrate that point. The next big project that I did after the *Classroom Lessons* work with Courtney Cazden was the study of special education, specifically the placement of students in special education classes. In *Handicapping the Handicapped*, my graduate students and I examined the career path that started in the classroom when a teacher identified a student or students as in need of special help. Next the school psychologist would perhaps enter the classroom, talk to the teacher, and decide whether to pursue the issue or not. If the special education psychologist decided to pursue the referral, then he or she administered a battery of psychological tests. The process culminated in a large committee meeting which included the parents and other members of the school.

From the many cases we started with, we ended up with 16 that we followed from the beginning referral all the way through to the final meeting. When it came to the part of the final meeting where the committee asked the parents' permission to engage in a certain kind of special education intervention, we noticed that the proceedings did not occur according to the formal constraints of the law. The Special Education Law in place in California at that time said that once the meeting is underway, the special education committee is to inform the parents of their rights to special assistance, including if the student needs to be placed outside the district, then the district would pay for that special assistance. This implies that, after that announcement, there should be a discussion of what those options are. Well, we noticed that wasn't the way the meetings unfolded. The meetings unfolded such that the parents were told that their child was special Ed. and could be offered this option, and did they agree? Now, if you just looked at the talk as we did, you couldn't notice the disparity between what happened in the actual interaction and what the law provided. You had to know something about the law and the institution. And also it is true that, having *talked* to the committee members outside of the meetings, we heard different opinions. I remember in one case they said "We can't afford to send Johnny to Camarillo for special Ed. We can't afford it." And that worked out the way in which the committee talked to the parents. So see, quite a bit would have been lost if I analyzed the sequence of interactions in that hearing only according to CA principles. It would have shown turn-taking, all the processes of being in control, but it wouldn't have captured the variation from law. I needed to lay that institutional context in the analysis in order to understand the meaning of the events in those meetings.

In the final analysis, I think, it is a matter of going back and forth between participant observations and a close analysis of the talk that produces a project more sensitive to, as you said, "the bigger issues." My bigger issue ended up being education inequality and how kids were being mistreated in a variety of different ways. I could show that in the details of the classroom interaction or testing interaction. So it is the interplay between organizational context, immediate context, and examining the talk between the players that I found to be productive.

However, I don't think there is a single prescription for that analytic process, for example—"Let's start with the larger context and work our way down to talk," or "Let's start with the talk and move up." The researcher could start with a hypothesis and then analyze the data in light of that hypothesis—changing the hypothesis as needed and then repeating the process. I didn't frame a specific hypothesis, but if I had, it would have been: "The special education final

committee meeting is going to provide an equitable solution in its referral.” Then, when examining the data one might find a discrepancy: “But wait a minute, in this case at least, or in these five cases at least, that’s not what it looks like.” That would be starting with a theoretical assumption, gathering data, and then modifying the hypothesis to say: “Oh, it isn’t this equitable distribution of outcomes. It looks like parents are not being given the full set of alternatives.” Based on a close examination of the talk, I’d modify my theoretical assumption and then continue with my research process.

I actually started with the data—with the transcripts or videotapes. By watching and looking I notice a point in the meeting where people start talking differently than I expected. I’d start wondering what that’s about. That led me to think “What was the law about?” So the analysis can go either way. But from my point of view, it’s a matter of close analysis, immediate context, and then institutional context that’s required.

Question: What are some of the ‘aha!’ moments during your professional life? How have they shaped your research trajectory?

Dr. Mehan: I was an undergraduate psychology major because I took a class in my senior year of high school where we examined psychological questions through literature. I thought psychology sounded like a great major. You know you’ve got personality theory, and all that fascinating abnormal behavior. I took a psychology class, but unfortunately I didn’t know that the program was behavioristic and all we would do was look at rats in labs. My savior was that my college required a minor. And this is a true confession: I took sociology classes because they were offered in the afternoon, rather than the anthropology classes that were offered in the morning. Anthro was at eight in the morning and sociology started at two, so I said: “Oh, sounds like sociology is gonna be for me.” (Laughter) Purely by coincidence, serendipity, one of the professors that I interacted with, this rabid Marxist, Irving Louis Horowitz, was the literary executor of C. Wright Mills’ corpus. In his undergraduate courses in sociology, we read Mills’ unpublished work. And for my generation C. Wright Mills was an inspiration—showing that sociology could be directed not just at understanding social practice, but that it could also be aimed at trying to ameliorate massive social problems. Those classes were illuminating.

The second one occurred when I was in graduate school. I went to Santa Barbara to study symbolic interactionism. I had read Scheff’s work, but I didn’t know anything about ethnomethodology. I wandered into these courses quite by accident. I thought it was all in French and German because the language was so arcane. They were talking about a different world view, and then there was Aaron Cicourel who was trying to link animal communication theory of bees to human interaction. It was bizarre, man. And I had been in the military and thought that the war was bad, but this was really crazy. (Laughter) Fortunately there was a cohort of graduate students that were two years ahead of me and they pulled me in, and then at some point the lights went on: “Oh my golly!” I had an ‘aha’ moment. And that’s how I came to link symbolic interactionism with ethnomethodology. And I remember at one time in our seminar Aaron Cicourel was talking about status and role, two main concepts of sociology, as both being negotiated. And I remember saying: “Professor Cicourel, you can talk about a person’s *role* being negotiated, but status?! That’s a given, that’s the structure, that’s in the world.” And he said: “Boy... you’ve got a lot to learn.” And at some point I realized what was being talked

about. It's a negotiated order. What we call structures have been *structured* by action. So that was my second aha moment.

And one final one I'll mention. Most of my early work was about documenting inequality, educational inequality in all its forms—in classrooms, in testing situations, in tracking, special education, etc. Then, quite by accident, a colleague asked me to monitor a class that he was teaching because he couldn't be there, and he asked me to introduce the guest speaker that was coming to class. It turned out that the guest speaker was running a program in San Diego, which has now gone national, called AVID—an attempt to provide college-going education for underrepresented youth by offering a special seminar course to them in high school. And I thought “Wow! That's really cool!” Engaging in a program like AVID might help me not just study inequality but move me toward helping to create, construct, constitute educationally productive and equitable environments. That ‘aha’ moment led me to work on setting up schools in San Diego for underrepresented youth aimed at preparing them to go to college. Those were the ‘aha’ moments that shaped the work that I did from the very beginning until now. I'm still working on those things.

Question: For many of us budding researchers, research grants are often rewarded to people conducting quantitative or mixed methods studies. If a conversation analyst were to collaborate with a quantitative researcher, what would an ideal project on classroom discourse look like? What are some of the possibilities?

Dr. Mehan: I interpret this question as: “Is it possible to do a combined piece of work with someone who counts and someone who does close analysis like CA?” And I think the answer is yes. In some cases a mixed methods study can be accomplished by a team—a “quantoid” and a CA person or qualitative researcher look at the same activity or event. In one case a Ph.D. student of mine did both. He was a Ph.D. student in sociology who first got a law degree and then came back to sociology. While he was getting a law degree he was a law clerk in Alaska. This gave him access to court case records including transcripts. He was especially interested in how native Alaskans were treated by the courts as opposed to people from the “lower 48” because from his legal practice he thought that natives were being mistreated by the system. He looked at the transcripts, and he tabulated how many cases there were and how many got settled, what the outcome was, and how much punishment there was, and whether there were variances by ethnicity. Then he went one step further. Using the notion of constitutive activity he asked: Can we see any differences in the interaction in the courtroom between the lawyers and the defendant and the judge? And the answer was yes. The native people were treated differently, they had less time to talk, they were interrupted, all the classic stuff.

Another example: Wayne Beach combined quantitative and qualitative activity very neatly, in legal settings, looking at quantitative distribution and then how the interaction plays out and influences that distribution. There has been nice work in doctor-patient interaction. Sue Fisher, Alexandra Todd, and others have looked at the distribution of patient outcomes and how the doctor interaction with the patient can influence that outcome. What's interesting in Alexandra and Sue's case is that they assumed there would be a gender bias—that male physicians would treat female patients differently than female physicians. It turned out that the few female doctors that they had in their study were just as officious as the males.

In short, there are existing studies that combine a distributional analysis—how many times things happen—and then ask the constitutive question “How does this come about? What is the interaction that produces this array?” Answers to those questions make sociologists very happy. Now, I don’t know all of your backgrounds. That may or may not be as well received in your fields, but I think this is a really stunning way of doing work. I realized there is this idea that close analysis like CA is doing insignificant things. I mean, who cares if there are 40 interruptions in a conversation. Well, that’s a big difference if that is a doctor interrupting a female patient. So your work may be done in a way that has influence on social issues beyond the sequencing of the conversation.

Question: We understand that the discontinuity between the home discourse patterns of minority students and the discourse patterns in US schools may contribute to lower academic achievement of certain minority groups. However, this does not seem to apply to Asian students in US schools, whose home discourse patterns also differ from those in US schools yet whose academic achievement does not appear to suffer as a result. We would be interested to hear your thoughts on this.

Dr. Mehan: Your observation is exactly correct. That faulty conclusion is partially the flaw of the discontinuity theory or social difference approach to studying classroom interaction. Some really great work has been done showing that black students get differentially treated in the classroom, or their speech is not celebrated in the same way; so do native speakers both in Alaska and in Canada, and so do Latino students and other economically underrepresented minorities in the United States. These people and their language are being mistreated. The explanation offered about this phenomenon has been that there is this discontinuity between the conventional ways of talking at home and the way in which school talk is organized in the three-part sequence and all.

That was chapter one of that story. Then, it became known that this does not apply to Asian students. They too have discontinuity between home and school, but they don’t have the same educational outcomes that plague blacks and Latinos. That information requires a revision of the discontinuity theory, frankly.

Question: Why do you think it’s so difficult for teachers to change traditional interactional patterns in the classroom?

Dr. Mehan: It certainly is true that it is difficult for teachers to change, but figuring out “why” is a whole other matter. When I worked with Courtney that was an issue that we really wrestled with. A lot of people have been trying to systematically change traditional teachers. For example, Sarah Michaels at Clark University, Cathy O’Conner and Curt Dudley-Marling at Boston College, as well as Ann Roseberry and Beth Warren who are the directors of the Chèche Konnen Center at TERC. In the paper that Courtney and I wrote recently, we referenced researchers and investigators who are trying to investigate *reasoning*, not just the sequence of classroom interaction. They have very systematic and careful interactions and interventions for teachers. Their extensive work contrasts with one shot approaches in which a coach goes into the class and tells teachers: “Ask divergent questions, you’ll have lots of kids answer them.” And the teacher says: “Oh yeah. Sure,” and end of session. But one shot isn’t going to convince teachers to change.

The work that Ann, Beth, and Sarah engage in requires a long period of time. They have found that when things are going smoothly, the social order of the classroom is being maintained. Teachers are not being challenged. The kids' behavior is being acceptable and appropriate. Everything is fine. But if there's some kind of breach in the social order, when a kid acts out or no one is on task, teachers revert to the normal form. They revert to the way they were probably taught, the way they went through school, the way most of us were taught in school. When there is a distortion or a challenge, they revert to the norm, the conventional form. So changing teachers' practice, their discourse style if you will, requires even more systematic investigation of the sort that people like Ann, Beth, and Sarah are doing. The goal is to have teachers who run into those problems avoid reverting to the conventional form. I guess we can say it's a matter of habit. We had so much experience doing it the conventional way that it's now deeply embedded in the fabric of our social life.

What do you think as teacher trainers?

Catherine Box: I'm teaching a student teaching seminar right now, and we actually read *What time is it Denise* last night. And so they were very excited that I'm actually speaking with you today. I had them track over the period of a week how many times they or their co-teachers use the conventional sequences, and it was something like 90 percent. Go figure. And we talked about why this might be. One of the things we thought is exactly what you were saying—it's really the unmarked form in the classroom.

Dr. Mehan: Exactly. The unmarked form.

Catherine Box: And the other reason, which is kind of sad now, is that there is so much pressure to cover the curriculum. And the one thing that a conventional interactional pattern allows you to do is to move the lesson forward. They are so afraid of not being able to get through an X amount of books in a week because that is what they are being evaluated on. Particularly, in New York City now, where their evaluations are tied to their salary.

Dr. Mehan: What you just said indexes the larger institutional context that's influencing the moment-to-moment interactions in the classroom: federal policy, or state policy, or district policy. As you well know there is a big push for teacher ability as measured by the students' performance on a test, which is in my opinion an abomination. I'm sure many of you will agree. But the point is that the larger institutional constraints influence what a teacher does in the classroom. And I hate it when teachers say they can't answer students' questions because "We had to move on" or "I have to cover the material." Come on. So you raise a really important point: the unmarked form plus the institutional constraints.

Nancy Boblett: I also train teachers, but they teach ESL to adults. We have both international and American student teachers, many of who have never seen communicative language teaching and don't know what it should look like. They have seen the PowerPoint with grammatical explanations without the focus on meaning and use, and they go to their "default" setting while still trying to figure out how to do it.

Dr. Mehan: Yes. It takes a long time to learn how to do it. Think of teaching like dance. The ballerinas don't look at PowerPoints and go: "Oh, I can stand on tiptoes." No. That's a practice.

It's true of any sport and any art form. Teaching is an art form as well as a science. So, I think both of your comments regarding the "default condition" are right on the mark. On top of that, people haven't been trained well to do the alternative. They haven't seen the alternative in vivid form. It takes looking at videotapes and then modeling it in the classroom.

Catherine Box and Nancy Boblett: Yeah, that's what we are trying to do.

Catherine Box: One of my students asked: "Well, what else is there, [besides the default form]?" And this is their last semester before going into the field, which is why it shocked me.

Dr. Mehan: Take that as an invitation, as "part one" of a "two-part sequence." Right? (Laughter)

Question: What recommendations do you have for someone who hopes to combine academic research with active work on changing the inequities that are inherent in our educational system?

Dr. Mehan: I would say do it. But keep in mind the institutional context in which that work is done. If you are pursuing an academic career in the traditional sense of joining a faculty in a college or university, not all departments, colleges, or universities reward that kind of combination of scholarship and activism. It's important to find out if they do. If they do, that's terrific. In my experience, the departments that tend to reward that kind of activism are in schools of education, in Applied Linguistics departments, and places that have built into their repertoire, mantra, or mission the idea of that combination. But my department, the Sociology department of UCSD, did not support that kind of work when I went there, frankly. I encouraged them to do that. I would suggest finding the right milieu and being careful to make sure that the work will be respected and rewarded if you are in a traditional academic environment.

Now, if you are in a different kind of environment, for example an empirical research setting, like TERC, or AIR, or SRI, you will find they are much more prone to support the research-policy linkage because they are about policy. It's also hard to be both the activist and the researcher. Doing so invites collaborative work of some kind. If you are a researcher working with a practitioner, whether the practitioner would be a teacher, or a medical doctor, or a lawyer, you can form a team. This collaboration enables you to do activism-oriented work with others who bring different sets of skills to the table.

Question: We've been doing some readings on ethnomethodology and have been really struggling with its core concepts of accountability, reflexivity, and indexicality as they've been written up in the literature. Can you help us understand in simple terms what they mean and why they're so central to ethnomethodology?

Dr. Mehan: So you want a 'primer' on core concepts. (Laughter) We should have done this question first. That's the hardest. Why are these concepts so central to ethnomethodology? I think it goes back to Harold Garfinkel's first development of what came to be called ethnomethodology. In fact, my mentor Aaron Cicourel and Harold Garfinkel really diverged on what that work was to be.

In *Studies in Ethnomethodology*, the mantra was "We are not trying to repair sociology." What Harold had in mind was not doing a better sociology. He wasn't trying to repair sociology. He wanted a whole new field. There was going to be sociology, anthropology, and

ethnomethodology. A new field with new guidelines, new ways of gathering data, and new theoretical concepts. Aaron Cicourel, my advisor, saw his work as more trying to improve upon sociology by bringing to bear different theoretical assumptions and different ways of doing research. That is why these concepts are so central to ethnomethodology. For Garfinkel, these would be new ideas that were going to be instrumental to this whole new field.

The ordinary language philosopher Yehoshua Bar-Hillel in a very influential paper written in the 1950s called *Indexical Expressions* really laid out what was meant by indexicality. An example: Let's assume that I'm there with you in the seminar room. You are students in my class, and we meet every Monday, Wednesday, and Friday. I come into the class and write on the board "Class will not meet today." And I write that on a Wednesday. Let's say that class is supposed to start at 5:30 and you walk in at 5:15. You say: "Oh, Bud is not having his class today. I hope he is not sick." Now, what happens if someone walks in on Thursday, and the board hasn't been erased? It still says on the board: "Class will not meet today." Well, whose class is it? I wrote it for the class, let's call it Linguistics 270. But Physics 100 meets in that room, and Anthro 10 meets in that room. Does that expression apply to those classes? No. How do you know the difference? That has to do with the indexical properties of speech. The understanding of that expression is dependent upon a set of indexical features: in this example, the time of the day, who wrote it, who it is written for, and how long it can last. Maybe you can say: "Well I recognize Bud's handwriting, so it must be for our class." Well, does that also mean on Friday or next Wednesday? No. How long does the instruction last? All the features of the context inform the meaning that we gather from that utterance, in this case the utterance on the board.

Bar-Hillel's paper caused incredible controversy in the field, because he said that basically all utterances are indexical, meaning that we require an understanding of the context in which [they were] uttered in order to understand their meaning. Phenomenologists, at least those who dealt with language, also talk about the relationship between speech and the context in which it's heard. Merleau-Ponty, the French phenomenologist, talked about the "hermeneutic circle" as a way to understand speech. "Hermeneutic circle," which later was reformulated as the "hermeneutic cycle," (to indicate a more open view of meaning than the circle metaphor which can indicate a closed loop of meaning) was the idea that in order to understand an utterance, you have to understand the context in which it was uttered; and in order to understand the context, you have to understand the utterances that are part of it. There is a circle, a relationship between a particular utterance and a larger setting in which it was uttered, and the institutional arrangements around it.

Returning to the sentence I gave you at the beginning of this commentary: "Class will not meet today," that is offered in an educational context. Everybody who is part of a university knows what a class is. In fact, imagine it was written on the board in a courthouse: "What do you mean class will not meet today? What's that about?" If you take hermeneutics and Bar-Hillel's notion of indexicality, the idea is that the meaning of a particular utterance is understood in its context or for being part of a context, and vice versa.

That leads to an understanding of reflexivity. Let me use as an example the relationship between interaction that occurs in a face-to-face environment, or the sort that we are engaged in now, and the structure that is being created in and through our interactions. We are constituting a seminar. The actions that we are engaged in have a sequential structure, an order, and a ritual to them.

There are topics that are understood as appropriate or inappropriate; and if inappropriate topics were introduced, people might laugh about them or the instructor might get mad. Right? So there is a whole set of rituals and meanings that are associated with this particular type of face-to-face interaction. Well, we have also constituted the meaning of a seminar on April Fools' Day, and it has not been a joke, right? It's been real. So, reflexivity can be used as a concept that speaks to the relationship between the interaction and its constitutive actions, the structure, or a convention, or some kind of outcome. There is a reflexive relation between the two, and one cannot exist without the other. From an ethnomethodological point of view, you cannot have a structure without structuring activities. If you have structured activities without a consequent structure, you have nothing.

Have you read any Melvin Pollner's work? Mel, who died way too prematurely, has this wonderful book called *Mundane Reason*. It's a brilliant book in and of itself, and he also does a really good job of dealing with reflexivity in the book. I highly recommend it. It has a wonderful picture, at least on my copy of the book, showing the world being held up by an elephant. And the elephant is on the back of another elephant, and the elephant is on the back of yet another elephant. Because in certain Indian philosophies, there is the question of how the world is held up, and the answer is: "It's on the back of an elephant." But wait a minute, "Who's holding up the elephant?" Answer: "Another elephant."

OK, last one, accountability. Accounts are a crucial term within ethnomethodology insofar as they are seen as a practice that people employ to make sense of the world. Accounts are ways in which people make sense of the world—by providing explanations of it or telling stories about it. Ethnomethodologists call this sense making practice "making accounts." Imagine a couple. One of the partners steps into the room and the other says: "Where have you been?" She responds: "I went to the library to study." That's an account, an explanation, a reflection upon her actions that presumably is intended to make sense of that action. Doing so makes the action accountable.

Ethnomethodology makes very strong claims that veracity, or truth, is not isomorphic with the situation because people inevitably are interpreting their actions and the actions of others. The notion of truth is questioned by social accountability. The social world is up for interpretation. In the ethnomethodological study of language, the meaning of an utterance is not contained in the utterance itself. It is to be found in the way in which others in the scene make it accountable. If I say: "Oh it's cold in here," and my partner rises up, goes to the closet, and brings back a sweater, that sentence can then be heard as a request. But if my partner says in that moment: "I don't think it's cold," the sentence is not being heard as a request; it's being heard as a comment on the weather. In that way, meaning is established in the interaction, all of which is part of the accounting process. How's that? Does that help? I see that at least a couple of heads are nodding. (Laughter)

Those are really tough concepts. And the question always becomes: where is the utility in employing them when doing your own investigations. Right? OK, let me stop there and see if you have any comments or questions.

Dr. Waring: I have a reflexivity-related question. The example you gave is really, really helpful. In our reading of ethnomethodology, we keep encountering the "documentary method of interpretation," we thought this was how they explain reflexivity?

Dr. Mehan: I should have said at the beginning, what I am giving you are my interpretations of these concepts, and I cannot guarantee that my interpretations are universally accepted within the ethnomethodological community but they were helpful to me. (Laughter)

The documentary method of interpretation is a concept borrowed from Karl Mannheim, who makes the important point, and Garfinkel picks up on this very powerfully, that what people do in everyday life, is to use documents either in written or audio or video form as artifacts that create a record of speech. Ordinary people rely on such documents or artifacts to interpret what's happening in everyday life. That is, we use a documentary method to make sense. What Garfinkel did, as Schutz did before him, was to extend that idea to the social and natural sciences. Scientists, too, are engaged in similar processes. Scientists, too, use artifacts to come to an interpretation of the materials in front of us. Once again, the concept *interpretation* is central to our understanding. Interpretation challenges veridicality which is an underlying assumption of a positivist orientation to the social sciences. If science is presumed to be discovering facts, it is possible to assume you have veridicality. But if social science is a matter of interpretation, you don't. I think it was Nietzsche who said: "There are no facts. Only interpretations." In discussing the documentary method of interpretation, I think Garfinkel pushed the idea of interpretation as a central tenet of the way in which everyday life is lived and the way in which professionals conduct practice. Doctors engage in interpretive activities, teachers engage in interpretive activities, lawyers, jurors, everyone in the professional and everyday world. Investigating that act of interpretation became part of the research agenda.

George Lakoff, a cognitive scientist, has a couple of wonderful examples of how interpretation and documentation play out. The title of one of his books is *Don't Think of an Elephant*. Of course, as soon as you say: "Don't think of an elephant," what do you do? You think of an elephant. He uses that example to support the claim he makes, first in the realm of cognitive science and second in the realm of politics. He asserts it isn't the facts that matter, it is the context or "frames" in which the facts are placed that matter. In ethnomethodological terms, Lakoff is indexing the interpretive process. The frame in which facts are placed matter because of the interpretive process that we as human beings are engaged in.

Lakoff first made these observations in cognitive science, but he has also been very active in the political realm. He has developed, like Chomsky, a kind of dual personality. "Chomsky 1" has written fundamentally important linguistic theory; and there is "Chomsky 2," who writes really powerful, vivid political commentaries. George Lakoff is similar to Chomsky in that respect. He does basic cognitive science, and then puts on his activist cloak and makes admonitions to politicians to understand the way in which thought applies and develops. He has a progressive orientation to politics. He takes out after Republicans or the way in which they frame political events, getting people to understand the world in terms of a set of frames or documentary methods that are presented by politicians. His basic claim, in cognitive science and in politics, is it's not the facts that matter, it's the frames. Does that help in terms of documentary method?

Dr. Waring and students: Yes. Very much.

Students: Can we do this every week? (Laughter)

Dr. Mehan: I've enjoyed my time in the fall at the LANSI conference, and I've enjoyed the very thoughtful email and in-person exchanges with this group. If you find this valuable, we can arrange to do it again.

Students: Wonderful. Thank you!

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