A conversation with Michael Lynch

with Bob Anderson, Wesley Sharrock, Douglas Macbeth, Dušan Bjelić, and Oskar Lindwall

INTRODUCTION

On December 6th, 2023, Bob Anderson, Dušan Bjelić, Oskar Lindwall, Doug Macbeth and Wes Sharrock met in a two-hour Zoom for an interview–conversation with Michael Lynch. Topics and questions were discussed in advance among all the parties. Some addressed Lynch's professional chronology and some addressed questions about EMCA within that history. A very good transcription software spared the editors that task while entertaining us with typological puzzles. As seen below Bob Anderson's opening question is forward-looking, and Macbeth's next question begins elsewhere. These two questions bookmark the conversation. It should also be said that conversations are difficult to script. On reading the transcript there were topics we hoped for that never arrived. This led to some substantive writing by Lynch, including footnotes and references. So, the interview below is something of a hybrid of talk and text.

Bob Anderson (BA): In his note to all of us, Doug raised an important point about the need for a curriculum for people who haven't experienced the conceptual wars of the last 40 years. That's valid. However, there's also a question about the perspectives of those who have lived through these events. What do they think of what has transpired, and consequently, what measures should be implemented to ensure that, as we move forward, we don't find ourselves endlessly rehashing the issues of the past four decades? That's the essence of it. Mike, I was hoping to hear your thoughts on what, given all this time and the scars we've accumulated, you believe we should or could be doing differently to make a meaningful impact on moving the study of the social forward. Of course, if you prefer not to comment, that's completely fine. Perhaps Doug could address it in a roundup or another format. I'm not sure.

Doug Macbeth (DM): Hopefully we can, but I'd like to begin with some of those early days, and one question I'd like to ask, because I've never heard Mike say anything about it, is where your disaffections with normal social science began. What led you looking elsewhere?

Mike Lynch (ML): I can certainly deal with that. Let me back up a little bit. I was an undergraduate at Cornell. In fact, my earliest memories were near the Cornell campus when my father briefly taught there. So, the past 25 years have been sort of a coming home. I was an undergraduate at a time that was very exciting at Cornell, and at other campuses too. It was the late '60s. Cornell got turned upside down in my junior and senior years (1968–1970). Classes were suspended, and all sorts of interesting developments happened. We had courses that the students ran and graded themselves, which helped my grade point average. I had never been a very motivated student going back to primary school. And I wasn't very motivated as an undergraduate either. I started as a biology major, although my interests were more in natural history, wildlife conservation, and that kind of 'soft' biology.

I was in the College of Agriculture (now named College of Agriculture and Life Sciences). Cornell is state funded in part and privately funded for the rest of it. I was in the publicly funded part because I had a state scholarship. They had a course (a 'major' as it is called in the US) named Rural Sociology (later renamed 'Development Sociology'), and it had a lot of electives in the College of Arts and Sciences. I took a bunch of social psychology courses, which were kind of interesting to me, particularly the classic social psych experiments such as the Solomon Asch conformity line length experiment and the Muzafer Sherif autokinetic effect. It just seemed interesting that you could have a local cohort pushing somebody to at least lie about what they saw or maybe even 'see' what the confederates said they saw.

I also took a couple of philosophy courses. One was focused on analytic philosophy, which I didn't enjoy. The other was a more eclectic philosophy course. I really thrived in that one, probably the best course I had in terms of grades and interest. In my senior year, things had become topsy-turvy on campus, and I hadn't really thought about what to do next. Late in my senior year, I applied to the Sociology Master's Degree Program at SUNY Binghamton (now Binghamton University), which had one of the latest deadlines for application. And because I had done reasonably well on my math GREs (Graduate Record Exams, the standardized exams), I was assigned as a Research Assistant to a quantitative sociologist. The sociology I'd had at Cornell was largely dominated by political issues of the day. It wasn't very deep academically, but it was interesting because everybody was interested in politics.

As a research assistant at Binghamton, I worked with a sociologist who was doing surveys of high school students, trying to find out what sorts of attitudes and family backgrounds were correlated with academic success. I was assigned to code questionnaires, and to relieve the boredom I began doodling on the questionnaires where I was supposed to put a checkmark to indicate that I'd coded them. Another research assistant, who was more committed to the project, told the boss that I was drawing dirty pictures on the questionnaires (not true!).

At Binghamton, I wasn't much interested in most of the sociology courses I took, but I had a couple of courses I liked, one of which was on the sociology of knowledge, taught by a young faculty member on a limited-term contract, named Lynn Miller. I think he left academia a year or so later. He ran an interesting seminar that covered key works in the sociology of knowledge, such as Mannheim (1936) and Berger and Luckmann (1966), and also included things like encounter groups, which was a trend at the time. He had come from UC Irvine, and he knew Anita Pomerantz, and I recall that he referred to Harvey Sacks as a "genius". He knew I was disaffected at what I was doing at Binghamton, so he suggested I check into Irvine.

When I looked at the roster of courses at Irvine, they seemed really interesting. The course title that really stood out was "Zen Socialism." At the time, the School of Social Sciences was one unit with no departments; a student could take a mix of courses. Before going there, I did some reading in ethnomethodology, and then hitchhiked across the country and ended up living in my office for two weeks until the police kicked me out. I had to move in with one of the professors who offered me a room in his house, in exchange for babysitting his three-year-old son (now a quantitative sociologist).

At the start of my first year, I introduced myself to Harvey Sacks. I walked into his office, and there were a few people standing around. I turned to one of them. He was small, five feet tall or so, had long hair and was wearing a blue jeans outfit, the student uniform, and I asked him, 'Do you know when Professor Sacks will be back in his office?' He replied, 'I'm Professor Sacks.' After getting over the embarrassment, I asked if I could attend his seminars. He made it very clear to me that I could attend for one quarter, a 10-week period, and then would have to decide whether I would continue working with him and his students.

By coincidence, Harold Garfinkel was also visiting Irvine from UCLA when I arrived in 1972, and I took the seminar he was offering. He was more inviting than Sacks, and I also liked what he taught, even though I didn't understand it very well. It seemed more open-ended than Sacks' seminar. Sacks had a group of very good PhD students, and in his seminar, they were doing research on specific sequential structures: Pomerantz did compliments and responses to them, Kiku Terasaki was doing pre-announcement sequences, and Jo Ann Goldberg and Judy Davidson were working on other sequential phenomena.[1] They were the main students. And it was more like a laboratory, in fact, it was held in the Social Sciences laboratory building. And it was kind of like a laboratory science where the boss hands out pieces of the project. Having had the experience of doing quantitative sociology, it wasn't the kind of thing I wanted to do.

I had been warned that Garfinkel was 'difficult' to work with, but he was very friendly and even tolerant to an extent. I remember that I would go to the seminar, and afterward, I'd meet with some of the others in the seminar to discuss 'What the hell was he talking about?' Once, when Harold stopped by my office, I suggested to him that it would be helpful if we could have a glossary of terms he was using, since many of them were unfamiliar to us. And he said, 'Oh, that's a great idea, why don't you draw up a list of shibboleths?' (which was one of those terms). I soon discovered that he was conning me. I guess he realized how naive I was and played along. After numerous meetings of the seminar, it was either during that quarter or the next one, he disbanded the class and said we were lazy, unprepared, and not doing the projects he assigned. But he privately invited a few of us to go to his seminars at UCLA. David Weinstein, Nancy Fuller, and a few others of us would make the drive to UCLA to take his seminars for the remainder of that year and the years that followed.

I had a lot of respect for Sacks, but it was clear that he required a commitment and was uncompromising about it, which is to his credit, as he had worked out a program. It was clear that I could work with either Sacks or Garfinkel, but not both, and so I went with Harold, who was always working out his program for another first time, from week to week.

I never got to know Sacks very well, although I did take a seminar he offered in 1974 on video analysis, which he opened up to students beyond his circle of advanced students, since

1 For publications based on their dissertation research see, Pomerantz (1984), Davidson (1984), Terasaki (2004) and Goldberg (2004). Gene Lerner was a student of Sacks' who began at Irvine a year or two following my first year. We shared an office for a few years in the social science tower at UC, Irvine. See Lerner (2004) for a collection of early conversation analytic studies.

it was more exploratory and less technical. I did get to know some of his students and former students, especially Terasaki. I also got to know Pomerantz and Jefferson while I was in Graduate School and afterwards. Terasaki and I talked a lot about the difference between ethnomethodology and CA, and I used CA transcriptions and some of the conceptual issues from CA in my dissertation, which was unusual for a Garfinkel student; he didn't encourage us to go into CA in any depth.

BA: I've had an exchange with Lois Meyer.[2] She made a comment that surprised me—she could hardly ever understand what Harold was talking about, especially when he discussed what she thought she had written. I wondered whether there is something in her description which marks the distinction you encountered between Sacks and Harold. Sacks was very clear but highly disciplined in that way. You described his handing things out and having them under control. Harold was free and open, but the trade-off was that you didn't know what you were getting back.

ML: Yeah. You never knew what mood he'd be in when you talked to him or what reaction he would have to something you said or showed him. It was difficult. But what Lois is describing is a familiar theme for students of Garfinkel's. Maybe Doug could echo this. Harold would tell you about what you've said or written, but it was news to you. Yet, it was very instructive. He did this deliberately, I'm sure, to read into what you had said better than you could possibly have meant, and then give you credit for it. It's kind of a variant of how he would write a paper and give his students co-authorship when they had no idea that the paper existed. So it was a way of elevating you. But it also could be a puzzle. It was an interesting exercise. I guess you could say it was pedagogical. It's mindful of "certifying an event you did not bid for" (Garfinkel and Sacks 1970, 365–366).

I don't know if you ever encountered that, Doug.

DM: Oh yes.[3] How then, in those times, were you led to the topics and settings that became your thesis, *Art and Artifact in Laboratory Science*[4], and then join the developing literature in the 'sociology of science' from an EM perspective?

ML: My initial interest was independent of the trend towards laboratory studies. I had no idea about what was happening in science studies when I started my dissertation research. The "Strong Programme" and the Sociology of Scientific Knowledge (SSK) were getting going in Britain, but I wasn't aware of them when I started.[5] My motivation, as far as I can recall, was

2 Lois Meyer was a doctoral student in second language education at UCLA. She did course work with Garfinkel and Schegloff and then relocated to the San Francisco Bay Area in the mid 80's to pursue her thesis study of second language students. Garfinkel was a member of her dissertation committee.

3 This 'Oh' registers a recognition, not a 'change of state'.

4 A revised version of the thesis was later published (Lynch 1985a).

5 In the mid-1970s, David Bloor and Barry Barnes, members of the Science Studies Unit at the University of Edinburgh, announced what they called the "Strong Progamme" in the sociology of scientific knowledge, which aimed to investigate and explain the 'contents' of science and mathematics as social phenomena (Bloor 1976).

that I had a previous interest, as I mentioned, in social psychology, which was not what I did at Irvine, but it was still somewhat of a background interest in questions about perception. The gestalt themes would be included. One of the first things I read to try to get some sense of ethnomethodology was Mel Pollner's dissertation, which was available in mimeo, and two of his papers based on it.[6]

I was taken with Pollner's notion of reality disjunctures. I had some biology background and had played around with microscopes when I was a kid. From reading Pollner, I got the idea that the history of microscopy and possibly the contemporary teaching of microscopy would be productive of puzzles concerned with things that had not been seen before and which were not just magnified versions of already familiar things. I read some histories of the Royal Society and their correspondence with Anthony van Leeuwenhoek's, after he had developed his own simple microscopes, and the Royal Society didn't have equivalent instruments with which to confirm them. Leeuwenhoek described what he called 'animalcules' (tiny multicellular organisms and possibly bacteria), that couldn't be seen with the naked eye. He had clerics certify that they saw them with his instruments, but he did not share his lenses with the Royal Society. So, for some time there was a reality disjuncture. There was a question of whether the things Leeuwenhoek described and sketched were real.

As Pollner observed with traffic court disputes and psychiatric accounts of hallucination, you have this philosophical question about reality being worked out in a situated, practical way. I was also interested in the mediation through instruments and how instruments, not just scientific instruments, but the world that we find mediated by instruments, machines, and vehicles, would be phenomenologically interesting. Like many of Garfinkel's students, I was reading phenomenology. He didn't assign specific readings from Schutz, Merleau-Ponty, Gurwitsch, or Heidegger, but students in his seminars realized that they were part of the necessary background for understanding what he was saying.

I wrote a couple of early, not very good, papers on the history of microscopy and visited an undergraduate teaching lab in which students were given elementary lessons with light microscopes. I began to talk to Garfinkel about the project at this early stage, and talked to people at Irvine, one of whom told me that I could find variants of such problems being raised about novel phenomena, but only if I got access to "cutting-edge science." Another faculty member at Irvine (I think it was Louis Narens) connected me with a lab across the campus run by Gary Lynch (no relation to me) in the Psychobiology Department. It was more neuroscience than psychology, with anatomical and physiological studies of animal brains (mainly laboratory rats), to explore and explain the regeneration of damaged neuronal regions.[7] I was invited to visit the lab regularly, and I got along with the people there, focusing on a group of

6 For papers based on his dissertation, see Pollner (1974, 1975).

7 The region of the brain of interest in many of the lab's projects was the dentate gyrus of the hippocampus, a region linked to memory, and thus relevant for psychobiology. However, for anatomical studies, it was of interest because of its relatively stratified layering of neurons, which facilitated disciplined comparisons across layers. See Lynch (1985b).

research assistants, and graduate and undergraduate students who were working on a project with electron microscopy.

I got interested not only in perceptual issues, but also with how the lab members documented their optical and electron microscopy data with micrographs and montages of micrographs. I also became interested in the issue of how they prepared specimen materials and assembled visual displays. I delved into the preliminary work that isn't reported in finished articles. My project coincided roughly with the research by Latour and Woolgar (1979), Karen Knorr-Cetina (1981) and Sharon Traweek (1988). Particularly with Latour and Woolgar's and Knorr-Cetina's studies, there were some parallel themes because they were all investigating biology and biochemistry labs. They also had some interest and background in ethnomethodology and related subjects, and of course, Kuhn was in the air, with questions about incommensurability, but the laboratory studies dealt with these questions not in the epochal, historical way Kuhn construed them, but more immediately, on the spot, in more local temporal domains.

DM: But these several initiatives had, or still have, substantially different perspectives on their science studies from what you were doing or what you collectively were doing, than you had. Did that ever cause a problem for you?

ML: Well, professionally, it did because what I was doing wasn't as intelligible to people in history, philosophy, and social studies. I was happy to be put into the conversation with the others though there were some major differences. I remember being at a meeting in Montreal where Woolgar and Knorr-Cetina were present. Knorr-Cetina and I argued with each other for about three hours. I got typecast as an ethnomethodologist and nothing else. I was interested in laboratories as 'perspicuous sites' for elucidating specific phenomena (to use later vocabulary), and not so much in promoting a constructivist perspective. By the time I finished my dissertation in 1978 (the degree was conferred early in 1979), I was in dialogue with some of the others in the 'new' science studies. I also read Bloor, who I hadn't met yet, and briefly discussed his version of Wittgenstein and agreement as a contrast to my interest in local agreement (Lynch 1985a, 200–201, n.22).

DM: You had a remarkable debate with Bloor about his 'Strong Program' for science studies and readings of Wittgenstein (Lynch 1992). Can you summarize it for us, and how else EM took interest in these topics.

ML: This was more than a decade later, when Andy Pickering was planning a volume on current work in social studies of science. It promised to be an important book, and it turned out to be widely read and discussed. Initially, Andy asked me to write an empirical chapter to exemplify the sort of work I had been doing, but he was nice enough to allow me to write a programmatic argument. At the time, I was in Sociology at Boston University and in daily discussions with Jeff Coulter and some of the PhD students. I had a previous interest in Wittgenstein, and Garfinkel encouraged us to read the later writings, though he was much more interested in phenomenology. When I arrived at BU, mainly through Jeff's influence I became much more steeped in Wittgenstein, and also Ryle, Winch, Austin, and others. Bloor had a serious interest in Wittgenstein, and others such as Harry Collins (1985) would invoke Wittgenstein and Winch. But their versions tended to interpret themes taken from Wittgenstein, such as 'agreement in form of life', as if they were setting the groundwork for social or cultural explanations of the formation of consensus about scientific and mathematical methods and results. The understanding I was getting was quite different, which was that science and mathematics are indeed social productions, but the production and assessment of methods and results are part and parcel of the practices themselves, and that sociologists lack the tools for engaging with those practices, let alone for 'explainiing' how they work.

So, I was attuned to the 'Strong Programme' in sociology and similar developments in science studies by the time I completed my thesis, but I wasn't aware of that work until the last couple of years of my dissertation research. I recall that Garfinkel had been at the Stanford Institute of Advanced Studies in the Social Sciences. I believe Robert Merton and Harriet Zuckerman were there that year, and Zuckerman gave Harold a bibliography she had compiled of relevant studies in the sociology of science. There was one category called 'micro' studies. It consisted mainly of efforts to construct citation networks to index scientific fields. The bibliography didn't include anything from the Strong Programme or the laboratory studies that were underway but mostly unpublished at the time. So, it looked to me that there wasn't much in the sociology of science of interest.

My interests were quite different from those motivated by constructivism and the arguments about constructivism. I ended up incorporating those arguments into the dissertation but made a characteristic ethnomethodological move of treating them as members' problems. My interest in artifacts was covered in Chapter Three, called 'An Archaeology of Artifacts'; a sort of reverse archaeology. You throw out the artifacts and keep the dirt for analysis. In this way, the arguments the constructivists employed in a kind of metaphysical way became practical, local phenomena.

DM: Did they in turn find use for EM or at least pieces of it? What use, if any, was found for EM excavations of ordinary action.

ML: I think there was some respect for what I was doing, and for ethnomethodology in general, though my work tended to get lumped in with the group of laboratory studies which were treated as demonstrating that (to quote from the first line of an anonymous blurb on the back cover of one of the widely-read books at the time): "The idea that nature is constructed, not discovered—that truth is made, not found—is the keynote of recent scholarship" in the field. This sort of summary didn't encourage more ethnographic studies of laboratories; instead, it set up waves of studies that continue to this day and which elaborate on the cultural, political, gendered, racialist, colonialist shaping of technoscience. I was able to carve out a position that differed from constructivism, but did not fall back on old-school scientific realism, and to their credit, Latour, Woolgar, Knorr Cetina, Collins, Pinch, Pickering, Shapin, Schaffer, and many others were fairly open to what I had to say. They also offered opportunities for participating in international meetings and contributing to special issues and edited volumes, and they offered lifelines when I needed rescue during my career.

DM: So in the mid-1970s, were you in conversation with Garfinkel about your project and he with you about his enterprise?

ML: Well, I think around 1975, I already had written a dissertation proposal. Garfinkel and I had long conversations when I would come up to UCLA. I don't remember them very clearly; there probably are tape recordings of some of them in the archive because he recorded everything. We didn't talk about the pulsar project until around 1980. However, I'm sure he told me his stories, such as the one about experimental neuroscientist James Olds chasing an assistant out of his lab for dusting off experimental equipment that had been turning out promising results (see Garfinkel 2022, 33–35). Some of these stories went back to the 1950s, maybe even earlier.[8] He had an interest in what he came to call the "discovering sciences", but he didn't talk about them much until midway through the 70s. I'm not suggesting that my study got him going on it, because he already had quite a lot of material. His visit to the Stanford Institute—I think it was 1976—may have touched off his interest in what he called discovering sciences. During his stay at the Institute, he spent time with Gerald Holton, a prominent historian of science, and as I mentioned Merton and Zuckerman were there. So, there was a lot of discussion. Of course, Kuhn was much in the air still. Even though the first edition of *Structure of Scientific Revolutions* was published in 1962, it was the second edition published in 1970 (including a Postscript) that really touched off a lot of the talk about paradigms and incommensurability. When I was at Irvine in my first couple of years, we read Kuhn's 'Structure' in so many different classes. Everybody was reading it. Garfinkel was very interested in it, as you can see in the 1980 seminars (Garfinkel 2022, 125ff.).

Steven Shapin (2023) recently wrote a review of a book of Kuhn's last writings. An anecdote he describes was about an occasion in which Kuhn ended up throwing an ashtray at a guy who dropped into his office to enthuse about paradigm shifts. Shapin describe how Kuhn was absolutely tormented by the fact that social scientists, and countless others, were picking up on his work superficially and in ways he found confused and misleading.

Of course, in many of the social sciences, the interest in Kuhn was to come up with paradigms—to establish a new paradigm in sociology or another field, or to promote the idea that sociology was a multi-paradigm science. Harold knew that Kuhn said sociologists could have none of it (Garfinkel 2022, 125), and he was willing to go with that, although he sometimes seemed to hint that ethnomethodology could be an exception. As for CA, I think some of its proponents saw, and now see, it as a normal science. That's very rare in the social sciences. I tend to think it's still aspirational.

Aside from treating Kuhn as furnishing a model for how to do social science, which Garfinkel correctly realized was miscast, the notions of incommensurability, the gestalt issues Kuhn brought up, and his references to Wittgenstein really penetrated a lot of the social

8 Arlene, his wife, was a biochemist, and she was one of his sources. A transcript of an interview he had with her about her lab work is in the Garfinkel archive in Newburyport, MA.

sciences, including science studies at Edinburgh and elsewhere—Collins and Pinch, Bloor, Barnes, and others.

BA: So, in your view, then, the interest Garfinkel took in science wasn't really a strategic pivot, so much as reaching into a new domain to do much the same sort of thing. That's important, isn't it, Mike? If only because it's been treated as a strategic pivot. And in fact, and I'm not saying you're wrong but the way in which you described it, it is more opportunistic…

ML: From what I understand from having been a student in his seminars starting in 1972, it was at around that time that Garfinkel announced the studies of work initiative. It wasn't until the mid-1980s that publications came out, including his edited volume (1986), and three books based on dissertations by former PhD students (Liberman 1985; Livingston 1986; Lynch 1985a). Of course, 'work' was not a new topic for him and others in ethnomethodology, and he also continued his focus on manifestly 'ordinary' activities of queuing, driving in traffic, following instructions, and so forth. His treatment of work at this time made an issue of the "missing what": the fact that sociologists did not describe the distinctive practices that musicians, lawyers, mathematicians, and so on performed, and instead focused on generic sociological variables, career stages, subcultural features, and so on. He contrasted David Sudnow's (1978/2001) study of improvisational jazz, which went into the embodied working of the piano keyboard, with Howard Becker's (1951) studies of the jazz musicians' attitudes toward dance band audiences, and other aspects of their subcultural world. Becker was himself an accomplished jazz musician, but he focused on the sociality and social situations in which jazz was performed and musicians hung out together, leaving the play of jazz to the musicians and the academic analysis to members of music departments. In his seminars, Garfinkel encouraged students to take up studies of occupations, professions, and avocations, and to become competent in them as a condition for their analysis. As far as I know, he didn't assign topics, but Livingston took up mathematics as his topic, and (as noted) I got involved with studying lab microscopy. The "missing what" certainly applied to work in the sociology of science and provided some impetus for our studies and polemics. Garfinkel also became more attuned to what was happening in science studies during his year at the Stanford Institute, where he also learned about the pulsar tape, and he was invited to present a plenary talk at a large meeting in Toronto in 1980.[9] So, you can say Garfinkel's interest in science was oppor-

9 The pulsar tape was an audio recording of the talk among two astronomers and a night assistant at Steward Observatory, Arizona in 1969, while they were making a series of observations that produced results used to demonstrate that a radio pulsar was also visible in the optical range of the spectrum. Garfinkel presented a plenary talk about the optically discovered pulsar at a 1980 meeting in Toronto, "The Present State of Social Studies of Science." The meeting was sponsored by the journal *Philosophy of the Social Sciences* and four professional societies: the History of Science Society, the Philosophy of Science Association, Society for the History of Technology, and the Society for Social Studies of Science. Gerald Holton gave a commentary at the meeting. A year later, the paper and commentary were published in *Philosophy of the Social Sciences* (Garfinkel, Lynch, and Livingston 1981; Holton 1981). See also Garfinkel (2022, Part 2).

tunistic—or, at least, occasioned—but it was no less ethnomethodological, and he continued with the various other projects he and other students were working on.

BA: I can see exactly that. I was wondering whether there was something 'meta' going on because, I mean, all of us have talked about the two methodologies, the two Garfinkels etc. I have a view that there's just one. And I think you probably have the same point of view. However, others don't. Others see a strategic pivot going on in the mid to late 70s that emerged as the interest in the sciences, and they basically read your work in line with that. But what you just said, it sounded like there wasn't a strategic pivot at all. It was much more opportunistic. Interesting. Of course, your own pursuit of science took a particular career, and that, for me, was fascinating—how you managed both sides: the ethnomethodological side and what the Woolgars, the Latours, and the others were doing. You were trying to have a foot in both worlds, which is perfectly understandable or a way of making a living, certainly.

ML: In fact, it was essential for making a living for me, because I had a falling out with Garfinkel shortly after we were working on the pulsar project. Before that, sociology of science was not a big field. It still isn't, and the field of science and technology studies was much smaller than today. And as far as the profession went, ethnomethodology could be the kiss of death. I applied to law school at one point because it looked hopeless to get a job. But it turned out that several of my contemporaries, especially Latour, Woolgar, Gus Brannigan, Trevor Pinch, Steven Shapin, who became established earlier than I did, were able to open up opportunities when I needed them. Friends and colleagues in ethnomethodology weren't in a position to help as much, with the exception of Jeff Coulter and George Psathas at Boston University. Particularly with Latour and Woolgar, because they were so successful in attracting attention to their writings, and with many other writings and activities that helped spur the development of the transdisciplinary field of science and technology studies (STS), it provided space for me too.

Dušan Bjelić (DB): Mike, if I may just go briefly back to the sort of origin of interest in science, how much did Trent Eglin's (1986) paper on alchemy play a role in Garfinkel's science studies?

ML: It was a very interesting paper. Eglin was a student of Garfinkel's in the 1960s. Garfinkel clearly admired the paper, distributed it in his seminars, and included it in his edited volume, *Ethnomethodological Studies of Work*. Garfinkel discusses it on one of his 1980 seminars (Garfinkel 2022, 104–107). The paper made a very interesting argument. Whether it's true or not, I don't know. What you could do with it is an interesting question. I know that you really liked that paper and used it.

DB: To me, that's where the focus on the work of scientists became an issue rather than the familiar versions of scientific practices.

ML: Yes, the argument was that alchemy was not simply a practice that, in stereotypical hindsight, was an attempt to turn lead into gold or do other impossible things. It was a laboratory practice that aimed to bring that practice under examination; the laboratory was a self-referential or self-explicating setting. And that this was the point of alchemy. It wasn't the material products of it, which, when we look at it retrospectively, look absurd. But it was instead a kind of natural philosophy of practice. So Harold would read ethnomethodology into that agenda. I don't know why you would want to develop a science on the grounds of alchemy, but why not?

DM: Mike, I want to push a little further. It seems to me that the '80s and the '90s were this bloom of studies on science and technology. It was a firestorm of competitive work and, in a fashion, confirmed the notion that in the social sciences nothing ever gets settled. So it was a very competitive matrix. And it seems to me that would be interesting water to swim in if you have a perspective on social science that's deeply critical.

ML: A thing to keep in mind in this context is that the aspects of science and technology studies (STS) that I was able to relate to pretty much got shut down in the late '80s, early '90s, when a new history of STS was written. During the 1990s, STS also became subject to heated criticism by various philosophers and social scientists, and some active and retired physicists and mathematicians, who took umbrage at social and cultural studies of science, which, in their view, reduced the validity of scientific and mathematical results to mere conventions, beliefs, or ideologies. Overall, these criticisms did not deter the continued spread of STS.

Nowadays, like most of American humanities and social sciences, it's all about race, class, and gender. The line of work involving Bloor, Collins, and others in Britain, often called SSK, the Sociology of Scientific Knowledge, gradually faded into the growing field of STS, which had more North American input and took on a more eclectic cast. Sociology's involvement in science studies gradually was bypassed in what largely has become a shift to medical anthropology and critical cultural studies. Nowadays there are very few studies of physics or laboratory work, though there still is some very good socio-historical work on mathematics. When I was the editor of the journal *Social Studies of Science* from 2002 until 2012, I could see the field was becoming larger but also without a coherent structure to it that I could fathom or even argue against.

Now, that doesn't address your question. It was a very interesting period of time in that field during the '80s. Partly because STS was growing, forming new departments or programs. And it's still considered a very strong social science development or interdisciplinary development because it's about *science*. But the constructivist arguments went by the board. You won't find too much argument about them anymore, and it is unclear to me if they are taken for granted or simply ignored.

DM: Meaning that that argument was settled?

ML: No, not settled. Nobody argues about it anymore. It got old.

THE LAW/SCIENCE NEXUS

DM: We have a question about a major strand of your work in terms of the intersection of science and law. On the completion of your thesis, which became *Art and Artifact*, you had a postdoc in Toronto, examining plea bargaining in Canadian legal settings.

ML: That was a contingent move. It was right after I had drafted my PhD thesis, In 1978–1979. I wasn't getting much from my job applications, but I got some help from Gus Brannigan, whom I had met during a visit to Toronto in 1974 and had stayed in touch with (and still do). He had written a book on the social basis of scientific discovery (Brannigan 1981). After he finished his PhD, he took a three-year position at the University of Western Ontario. At the end of the three years, when he would have been considered for a more permanent job, they told him the sociology of science was not an important enough field, and so Gus took up criminology and went on to work at the University of Calgary. In 1978, he had a research position at the Centre of Criminology in Toronto, and he encouraged me to apply for a one-year visiting position there. He was working with a group research project headed by Jim Wilkins, who had been a student of Aaron Cicourel's and now was a research professor at the Centre. The group was part of a larger project (the "longitudinal study") that involved most of the staff at the Centre of Criminology. They were following 100 criminal cases from the beginning to wherever each one ended, after arrest, pre-trial, in the courts, and so forth. This group had a bunch of audio recordings of pre-trial sessions between opposing counsel, which would include plea bargaining. But it wasn't just that. They included various informal discussions among the lawyers before trial, often resulting in a guilty plea to avoid the contingencies of a trial.

I was hired to work with those tapes and find things of interest for the overall project. I was able to do a kind of lateral move from studying laboratory 'shop talk' where the researchers are arguing about whether something visible in a micrograph was part of a brain cell or an artifact, to studying lawyers arguing about the substantive actions the defendant committed which could justify a major or minor criminal charge. So, these argumentative conversations were of interest to me, and I was able to get to work right off the bat with them. I was there just a year, and then I got another postdoc at UCLA at the beginning of 1980. As I mentioned, Harold and I worked that year on the pulsar paper while I also was doing a study of diagnostic sessions at the Neuropsychiatric Institute and teaching the sociology of mental illness.

Both of these sojourns into criminology and sociology of mental disorders turned out to be helpful when I began teaching, and also for later projects. One of the papers I wrote for a course on the sociology of mental illness was published in *Social Problems* (Lynch 1983), and helped me in the sociology job market, along with the fact that I could list Sociology of Mental Illness and Criminology in my teaching portfolio. After my series of postdocs and some adjunct teaching, I managed to get a position in Sociology in 1983 at a small liberal arts college, Whitman College, in Walla Walla, Washington. When I was interviewed, I was asked if I could teach anything besides ethnomethodology, and I was able to point to my work in criminology and sociology of mental disorder. I was on the outs with Harold then, which helped assure them that I would be a reasonable sociologist. I had a broad and heavy teaching load at Whitman, and further expanded my portfolio with, among other things, courses in sociological theory. I made good friends, and go back to Walla Walla every year or two to visit them. Some good philosophers were teaching there at the time, and I was able to sit in on a couple of courses, one of which was on Wittgenstein. However, I was happy to get an offer from Boston University after three years at Whitman, mainly through the efforts of Jeff Coulter and George Psathas. I was still on the outs with Garfinkel and when I was being considered for the B.U. job, Jeff urged me to get a letter of reference from him. I refused to break the ice with Harold, but told Jeff that Garfinkel had written a letter years before when I applied for a position there, and they were able to retrieve it and use it for the current position. There were some fine PhD students there at the time (as Dušan can testify, as he was one of them), and I collaborated with them on different projects. Two of the projects further immersed me into the law/science nexus: the study of congressional hearings on the Iran-Contra affair in 1987–1988, which resulted in a book with David Bogen (Lynch and Bogen 1996), and a study of routine molecular biology techniques used in different contexts (university labs, diagnostics, and criminal forensics) with Kathleen Jordan (Jordan and Lynch 1992). Kathleen and I managed to get a couple of National Science Foundation grants to support our further research on forensic uses of DNA. After I moved to Brunel University in the London area in 1993, and then to Cornell in 1999, I stayed with the project with the help of two other collaborators (Lynch, Cole, McNally, and Jordan 2008). The use of DNA evidence in criminal investigations provided what Garfinkel has called a "perspicuous site" in which widely used laboratory techniques are subjected to the auspices of legal use and contestation.

I also developed an interest in the interconnections between science and law as a topic for an undergraduate class I taught at Cornell for many years. What interested me about the law and science connection is that selected legal disputes make an issue about what counts as science, scientific expertise, and scientific evidence, and how to integrate it with other bases for the judging the credibility of evidence. Related to this are distinctions that are addressed in a legal context, such as the difference between science and pseudoscience (e.g., litigation about teaching "creation science" and "intelligent design" as part of the biology curriculum in US public schools), and disputes about whether a patent claim is about an invented "composition of matter" or an appropriated "product of nature". These themes were featured in an advanced undergraduate course that I taught for many years at Cornell (Science & Technology Studies 4071: Law, Science, and Public Values). For some years, now, I have had plans to write a book on the topic of law, science, and common sense, and I hope I'll live long enough to do it.

Forensic science, particularly DNA evidence, makes for a perspicuous variant of a classic distinction between "mathematical" certainty and "moral" certainty, when probabilities associated with matching evidence are integrated with "ordinary" evidence of motive, alibi, and person identification. It is not that legal decisions provide closure on such questions, except in a highly circumscribed way, but that how they are argued and resolved in a case-by-case way is distinctive and revealing about the intertwining of law, science, and common sense. It's a remarkable sociological intersection.

Incidentally, until my final year of teaching at Cornell, when I taught a graduate seminar on ethnomethodology, I never taught a course with that word in its title, though of course I folded EM into whatever I happened to be assigned to teach.

HARVEY SACKS AND CONVERSATION ANALYSIS

Wes Sharrock (WS): I'd like to hear something about your recent work on Sacks and his nachlass.

ML: My recent work on Sacks is still very much 'in progress' at the moment. I've been part of two reading groups that have been working through Sacks' lectures. I had read the *Lectures* over the years, but never from beginning to end, but I have now read all of them in the current run-through.

My interest in Sacks is not new, of course. When I was at B.U. in the late '80s and early '90s, David Bogen, Dušan, Jeff, and I often had discussions about Sacks and what CA had become by then. Jeff was Department Chair at the time, and 'ethno' attracted the most capable and lively PhD students. Jeff received his PhD at Manchester before moving to B.U. in the 1970s, and he brought with him the distinctive treatment of ethnomethodology that had been cultivated by Wes, John Lee, Rod Watson and their many colleagues and students. This involved a much more explicit and extensive infusion of themes and insights from Wittgenstein's (1958) later philosophy, and ordinary language philosophy. Sacks was a pivotal figure in this: though he did not often mention Wittgenstein, it was clear that he had developed a novel way to use recordings of conversation (and other materials as well) to investigate and exhibit the material production of natural language-use as socially organized *action*. In a retrospective on the distinctive line of ethnomethodology and CA at Boston University, George Psathas (2010) linked it to the 'Manchester School'. George was more favourably inclined toward CA and phenomenology than was Jeff, but the two of them and their students worked together respectfully and productively. Unfortunately, in the early 1990s, the university administration (which was notoriously authoritarian at the time) scuttled the operation and sent me packing in 1993. I was able to move to Brunel University in West London, thanks largely to Steve Woolgar's help, and during my six years there I frequently met with Wes, Bob, Graham Button, and others and further connected me with the 'Manchester School'.

But while I was at BU, largely through discussions with Jeff, Dušan, and David, I took an interest in exploring Sacks' conceptions of natural language-use and science, and like them I was critical of the directions CA had been taking in the decades following his death. The papers we published did not seem to stir much beyond some harsh personal comments, and when I moved to Cornell, I basically took a holiday from CA for around 10 or 12 years. I was very busy with the Science & Technology Studies Department, editing a major journal in the field, and being involved with the major professional society and a section of the ASA on science, technology, and knowledge. Starting in 2012, when my stint as editor and some of my departmental responsibilities had ended, I began going to the EMCA sessions at the ASA, and the IIEMCA[10] meetings more regularly. At the 2013 IIEMCA meeting, I joined an informal discussion about 'epistemics' in CA, and this got me involved in doing loads or reading on the 'current state of the art', and then going back to Sacks' lectures in a search for what seems to have been lost.

Reading the lectures now casts new light on them. I have always been impressed by how different the lectures are, not only from contemporary CA, but also from many of Sacks' publications. Although the overlap with the publications is obvious, the disarming simplicity and clarity of the lectures is only partly due to the fact that he was presenting them to students, many of whom had little or no preparation for understanding what he was doing. He was not doing research in the Lectures, but he makes clear to the students that his presentations are not simply for their benefit; he's also doing a kind of research in formulating his research. He presents insights from his ongoing research, and it's the spontaneous aspect of the lectures that makes them intelligible, and often delightfully so, in a way that the more formally organized writings are not. He occasionally made remarks during introductory lectures in his undergraduate classes to the effect that he was lecturing for research colleagues who were unlikely to be in the room. For example: "the audience that I think of being directed to is not here—or it's only incidentally here, if you choose to be one of them" (Sacks 1992, Vol. 2, "Introduction," April 2, Spring, 1971, 335–339, at 336).

I've been getting a picture of Sacks' project that is very different from the current idea of CA as an empirical discipline. It's not that Sacks is not empirical, but the way he's empirical is really interesting and original, and not "inductive" in any simple way. Again, it's something I'm still struggling to come to terms with. There's a remark in Schegloff's (1992b) Introduction to Volume 2 of Sacks' lectures where he's discussing a lecture in the final series included in the published volumes.[11] He says that Sacks is doing a "detailed examination of a single small excerpt from a conversation which is turned into a window through which the phenomenology (in a non-technical sense) of a person's social circumstances and experience is captured and fleshed out in a compelling fashion" (Schegloff 1992b, xlvi). I take Schegloff to mean that, in Sacks' lectures, and in the *Aspects* book that he never published,[12] Sacks positions his analysis in the midst of conversations, sometimes calling the unit of his (and the participants') analysis a 'sentence', sometimes an 'utterance'; not until later does he call it a 'turn'. The positioning of his analysis is within a projectable grammatical organization. The grammar is kind of conventional: a sentential grammar in many instances. He adopts terms from linguistics about parts of speech, categories of phrase and clause, and syntactic organization. Yet, he is alive to how the parties are attuned to, and attune each other to, when an utterance might end or not. It has to do with turn-taking, but it also has to do with how understanding is exhibited in and

10 International Institute of Ethnomethodology and Conversation Analysis.

11 The lecture Schegloff discusses is Sacks (1992, Vol. 2, Lecture 3, Spring, 1972, 542–53).

12 Sacks drafted, re-drafted, and re-titled the manuscript in approximately 1970. One of the titles was Aspects of Sequential Organization in Conversation (Sacks 1970).

through sequential organization. He's very strong in identifying *understanding* and attunement to *possibilities* as the phenomena he's investigating.

Now, that, I guess, should be elementary for all of us. But it just is so impressive reading through the lectures and realizing that, not only the stuff we complain about with 'epistemics' but also a lot of what has happened in CA just reifies the work of conversation. Rod Watson (2008) has written about such reification in the way collections of (arguably) equivalent transcribed fragments are assembled for analysis. Wes and Bob also wrote a great paper decades ago about the assembly of collections (Anderson and Sharrock 1984).

BA: There's a thought, I've forgotten all about that. Sorry. If it's a great paper, why have we still got the scars?

ML: There have been some critiques of CA from within EMCA, but not many formal responses to them, with the notable exception of the numerous critical articles and responses Schegloff has written over the years *to defend* CA in the face of various moves within, around, and against the field.[13] Garfinkel had a blast or two at "latter-day CA" (Garfinkel 2022, Appendix 1), although he also kept alive the idea that CA was the jewel in ethnomethodology's crown. A paper I wrote with David Bogen (Bogen and Lynch 1984), and also the chapter in the book on scientific practice, which I titled "molecular sociology" (Lynch 1993, Ch. 6), never got a published response, as far as I know, from anybody in CA.

Consequently, a remarkable thing about the 2016 special issue of *Discourse Studies* on 'the epistemics of Epistemics' and the papers in it that Doug, Oskar, Jonas, Jean, Gustav, Wes, Graham, and I wrote,[14] is that we *did* get a collective 'rebuttal'; in fact, we got an entire "rebuttal" issue.[15] It was a response we were not very happy with because it didn't address or counter the strongest critiques we had made, which were that when you read their treatments of transcribed materials, they often seem, at best, equivocal and not compelling. The rebuttals failed to effectively address any of the re-analyses of their transcripts that we presented, and their collections presented cases that were at odds with the very claims they were trying to explicate.

13 These contributions include his critical remarks about Zimmerman and West's early study of gender and interruptions (Schegloff 1987), his commentaries on Goffman's treatment of CA (Schegloff 1988), his exchanges with proponents of kindred lines of research (e.g., Schegloff 1991, 1992c, 1999, 2009, 2017) and, closer to home, Schegloff's 2010) more recent commentary on Stivers and Rossano (2010).

14 The special issue (Lynch and Macbeth 2016) includes contributions by Lindwall, Lymer, and Ivarsson (2016); Lynch and Wong (2016); Macbeth, Wong, and Lynch (2016); Macbeth and Wong (2016); Button and Sharrock (2016); and Steensig and Heineman (2016).

15 See the 'rebuttal issue' of *Discourse Studies* edited by Paul Drew (2018a), which included articles by Drew (2018b), Heritage (2018), Raymond (2018), and Clift and Raymond (2018), among others. The editor of the journal foreclosed any opportunity for us to publish responses to these 'rebuttals', and so we posted our responses on the Radical Ethnomethodology website: https://radicalethno.org/documents.html (Lymer et al. 2017; Lynch 2018; Macbeth 2018). Also see chapters 9 and 10 in Button et al. 2022.

But at least we got a rise out of them. It wasn't satisfying, but it was indicative of something; I'm not sure what.

DB: Mike, you know, in the context of this issue, can you contextualize the short letter from Sacks to Schegloff before he died, if it has any relevance to the issues that you just addressed?

ML: There is a very short letter dated in 1974 from Sacks to Schegloff (Sacks died from an automobile accident a year later). Schegloff quotes from it in his Introductions to Volume 1 (1992a, xlv, fn. 38) and Volume 2 (1992b, xxxix–xl) of Sacks' lectures, although he does not reproduce all of it. His remarks present it as a kind of testimony to his influence on the development of CA, and indeed Schegloff was enormously influential, especially after Sacks' death. However, there is no mistaking that the primary initiative was Sacks', and we can see it in the lectures from very early on. The letter surfaced in a heated exchange in the late 1980s when David Sudnow, Gail Jefferson, and others were pressuring Manny to get the lectures published and set up an archive.[16] The letter is hand-written, brief, and ambiguous, but it makes some intriguing characterizations of the difference between Sacks' and Schegloff's projects at that time. I read it to be an offer of reconciliation between the two of them, perhaps in connection with a tension over authorship of the turn-taking paper, and related issues,[17] but it's hard to attribute too much definiteness to it. One interesting thing about the letter is that it includes a line from Sacks that he was working as "a methodologist for eth-meth," and a comment written in the margin: "My initial contribution, & the thrust of my stuff over the years, was in finding ways to isolate structure in particulars. This problem is Harold's, and in that sense he belongs independently." I take these comments to attest to his alignments with Garfinkel, and I also read them to be tied to his notions of primitive natural science.[18]

Sacks suggested that he would be doing science insofar as science uses vernacular descriptions to instruct the replication of methods. He said further that such instructions are no less a part of the sciences than are the findings that result from following them. And he proposed to extend this idea of primitive natural science to using vernacular descriptions for developing descriptions of the grammars of doing ordinary conversation, though neither Sacks nor Schegloff ultimately settle for vernacular descriptions of conversational methods; they

16 The letter was circulated among members of a group of former colleagues and friends of Sacks in 1987 and 1988, who called themselves The Harvey Sacks Memorial Association. The purpose of the association was to urge Schegloff, who was Sacks' literary executor, to bring Sacks' lectures into print and to set up an archive.

17 Jefferson, who was a member of the Sacks Memorial Association wrote a letter to Schegloff, copied to the other members, which mentioned that Schegloff was not listed as an author of an earlier draft of the 'turn-taking paper', and in a subsequent draft was listed third. Copies of the earlier drafts are in the Sacks archive at UCLA. See Fitzgerald 2024, note 1, and Button et al. 2022, 56–58 for further discussion of the letters by Sacks to Schegloff and Jefferson to Schegloff.

18 On "primitive science," see Sacks (1992, Vol. 1, Appendix 1, "Introduction", 802–805), and Sacks (1963) on sociological description. Further elaboration on Sacks' letter is in Button, et al. (2022, 56), and on Sacks' collaboration with Garfinkel, see Lynch (2019).

give "technical" descriptions that are based on extended study of recorded interactions of vernacular usage.

As I read Sacks' letter through Garfinkel's treatment of instructed action, it is saying something to the effect that, insofar as methods instructions are vernacular accounts—although, reading them is not so easy for lay persons, or even members of the relevant field—they provide a grounding, not only for practitioners of the relevant science but also for an ethnomethodologist or conversational analyst of ordinary action. Sacks also mentions in his letter to Schegloff that the "thrust" of his work over the years "was in finding ways to isolate structure in particulars," in contrast to Schegloff, whose work, as Sacks characterized it, "was—this sounds bizarre—in forcing that to be made to work quantitatively, on masses of data," as exemplified in Schegloff's (1968) paper on openings in phone calls. Whether the letter is evidence of a possible rift or of a reconciliation, it points to a way of doing "ethnomethodological CA," as Rod Watson (2008, 234) calls it, as opposed to CA as a technical, data-driven enterprise.

One of the things we were criticized for in the rebuttals to our papers on 'epistemics' was that our re-analyses of the transcribed instances in their publications relied on vernacular intuitions rather than technically grounded generalizations. However, it is not an either-or matter: Sacks (and also Schegloff and Jefferson) made clear that a vernacular understanding of singular sequences is both a condition for and an evident product of conversation, prior to any technical characterization, and the *Lectures are filled with astute understandings of vernacular understandings.*[19] Consequently, it is unclear how participants in the 'rebuttal' issue would be able to dismiss our arguments by pulling 'technical' rank. The materials they characterized were produced and understood *in situ* by the parties *in and as* vernacular conversations. As such, they should be intelligible to other competent speakers of the natural language, regardless of whether they are apprised of what a technical literature deems 'up to date' (and we certainly had read and re-read much of the recent literature on 'epistemics'). The larger point we argued was that the epistemic analytic framework was divorced from the local sequential production of conversation, imposing instead an omnipresent 'grounding' that clashed with a vernacular, 'untutored' sensibility of what the parties in the exemplary instances appeared to be doing. We begin with those untutored sensibilities; we don't necessarily end with them, but if your 'educated analysis' takes you astray from them, so much the worse for "what's called your education" (Sacks 1992, Volume 1, 83).

FORMAL AND CONSTRUCTIVE ANALYSIS

DM: By my lights, the distinctions you've been drawing have everything to do with the play of formal analysis in the contemporary CA literature. The phrase 'formal analysis' was a founding phrase for ethnomethodology, leveraging its distance from received social science (Garfinkel 1967, 2002). In your continuing critique of epistemics, you use two metaphors to get at

19 For further discussion on the point of how vernacular and technical treatments interrelate in CA, see Button, Lynch, and Sharrock (2022, Chapter 5).

the difference between Sacks' first work and contemporary CA. One was the epistemic engine (Heritage 2012). The other, through Sacks, was "the inference-making machine".[20] I don't know if it's useful here, in our brief time remaining—and we still want to get to Bob's interest in bringing Garfinkel's *nachlass* to press. But is it helpful to speak to how it is that those two images, the epistemic engine and the inference-making machine, notwithstanding the reliance of each on moving parts, can be read on behalf of very different understandings of the landscape of ordinary practical action.

ML: Sacks used machine metaphors in various ways throughout his work, not always in the same way. He seemed to have an attraction to the notion of a machine—a "technology of conversation," but it wasn't necessarily hard-wired. On one occasion he said it would consist of "rules, techniques, procedures, methods, maxims" which he would "use somewhat interchangeably" when speaking of that technology (1992, Vol. 2, 339). This suggests that the formal structures are not hard and fast rules, but procedures members use to accomplish conversations which can be formulated in various ways.

As I understand it, the inference-making machine is a different sort of thing than the turn-taking systematics of Sacks, Schegloff and Jefferson (1974). The latter consists of a set of conditional rules for turn transition in conversation and is a central and recurrent organizational phenomenon in Sacks' work and in CA in the years after he died. The inference-making machine is a metaphor he introduced in the first series of transcribed lectures in the 1964–1965 academic year (Sacks 1992, Vol. 1, Lecture 14, 113–125). He mentions "inferences" in a number of contexts in other lectures, but not as the product of a "machine". In the lecture (which, according to an editor's footnote on page 113 of Volume One, combined parts of several lectures from the 1964–1965 series) the "machine" is a way of speaking of conventional associations between membership categories and action predicates. In this case, he presents a piece of transcript from a call to a psychological services agency, in which the caller presents an account of a domestic incident. The recipient is some sort of psychological service professional with no prior acquaintance with the caller. He hears the story in which the caller recounts a dispute with his wife about their child, during which the wife's sister calls the police, and the police arrive. Sacks emphasizes that the service professional knows nothing about these parties or the incident other than what the caller has told him, but nonetheless accuses the caller of leaving something out of the story, which is that he "smacked" his wife.[21] The "machine" that Sacks invokes is a locally assembled narrative structure in which a piece has been left out. The machine metaphor in this case points to the 'automatic' way, for members of the relevant natural language community, that a sequence of actions and generic characters (husband, wife, child, wife's sister, police) form a recognizable account in which a key action can be heard

20 For a discussion of Sacks' "inference-making machine" see Lynch (2020).

21 The fragment of transcript Sacks presents (1992, Vol. 1, 113) begins shortly after the start of the call. Sacks characterizes the recipient (A) as a staff-member of a social service agency, and B as the caller.

1. A: Yeah, then what happened?
2. B: Okay, in the meantime she [wife of B] says, "Don't ask the child nothing." Well, she stepped between me and the child, and I got up to walk out the door. When she stepped between me and the child, I

as notably absent. The machine metaphor also provides for the way the recipient of the call assembles this complex with its missing part without any hesitancy: the 'inference' is simply made, without marking it as a guess or interpretation. The basis for it is commonplace.

The "epistemic engine", on the other hand, is an analytical construct assembled from lines of literature in CA and sociolinguistics. Like Sacks' inference-making machine, this engine also seems to be a one-off metaphor that Heritage uses in this article and nowhere else that I know of. It has to do with the communication of differential knowledge and information, mainly in two-party conversations in which there is an 'imbalance' of relevant information. The epistemic engine is postulated as a driver of conversational sequencing alternative to adjacency pars, through which an imbalance of information is subjected to a hydraulic flow of 'knowledge' or 'information' between the two parties to achieve equilibrium (Heritage 2012, 48). Both the inference-making machine and the epistemic engine are formal abstractions, but the key to the former is that it is presented as a recognizable narrative order wherein any member might see that something's missing in the account and the other is withholding. The latter is a construct that is assembled from the literature and administered to one case after another.[22]

One of the complications for drawing this distinction is that Sacks was explicit that he was doing formal analysis, but this was a matter of providing for what members do as formal analysis of each other's contributions to ongoing action, joint action.[23]

When Garfinkel talks about formal analysis, he sometimes makes it seem so pervasive that it's reasonable to ask: How could you not do it? How could you do anything that would avoid

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went to move her out of the way. And then about that time her sister had called the police. I don't know how she . . . what she . . .

3. A: Didn't you smack her one?
4. B: No.
5. A: You're not telling me the story, Mr B.
6. B: Well, you see when you say smack you mean hit.
7. A: Yeah, you shoved her. Is that it?
8. B: Yeah, I shoved her.

22 Heritage (2012, 50) asserts the workings of the epistemic engine are "present in plain sight as an object of massive orientation by interactants at all times" and yet are "seen but unnoticed." See Lynch (2018, 2020), and Lynch and Wong (2016) for more elaboration on how recognizing this hidden-in-plain-sight engine requires subscription to a theoretically postulated array of underlying codes, gradients, and metaphorical operations.

23 Sacks provides for formal analysis as follows: "With regard to maybe the most obvious topic in the study of communication, i.e., do people understand each other? How do they understand each other? What do they understand? we can examine this material for, not just an exhibiting of that understanding takes place, but a way in which the understanding that gets done and shown, involves more or less formal operations" (Sacks 1992, Vol. 2, 500). In an earlier lecture (Lecture 1, Spring 1966), in which Sacks addresses a fragment from a child's story ("The baby cried, the mommy picks it up"), he ends the lecture with the following remark, which sheds further light on what he could mean by an "inference-making machine":

> The whole possibility of, at least 19th-century, early 20th century literature, turns on the fact that descriptions are recognizably correct—interesting, exciting, but recognizable apart from having to look at

formal analysis? What would you be left to do? Certainly not "informal analysis"! There are times when Garfinkel insists that he should not be read to be criticizing formal analysis, but he also says that ethnomethodology's "methods are more methods of avoiding formal analysis than methods of research" (Garfinkel 2002, 171), and he recommends "indifference" toward formal analysis.

As for the difference between the two technical metaphors—"epistemic engine" and "inference making machine"—I'd prefer to speak of "constructive analysis" rather than "formal analysis". Although Garfinkel often seems to use them interchangeably, I get a more definite sense from how he describes 'constructive' analysis.[24] In the 'Formal Structures' paper, Garfinkel and Sacks (1970, 360) include a long list of quantitative and qualitative procedures used in sociology that exemplify constructive analysis. The list includes coding schemes, experimental procedures, mathematical models, survey questionnaires, interviews, and 'administered' metaphors and definitions. Such research procedures are ways of generating formatted data that are comparable and analyzable. Garfinkel and Sacks challenge the presumption in constructive analysis that actions are not coherently analyzable until they are coded, indexed, or otherwise generated in a way that screens out uncontrolled sources of variation.

In one of his transcribed lectures, Sacks doesn't mention "constructive analysis" in so many words, but seems to alluding to it when he says he aims not to do a "conventional" social science.[25] However, he also speaks of the "machinery" he intends to use to characterize how a specific phenomenon "gets done" in the specific conversational sequence he exhibits in a transcript.

A CA transcript done with Jefferson's system, which uses typographical symbols and spaces to denote pauses, overlaps, stress, and intonation, can also be construed as a construct. But it is important to keep in mind that it is not (or should not be) reductive in the way that a coding scheme reduces an array of actions to a limited set of categories that are set up in advance. When Garfinkel says that formal analysis is the subject matter of ethnomethodology, he's often referring to the constructive-analytic procedures of a social science, and I would argue they can be found in the natural sciences as well. And, as I noted, Sacks aimed not to use the sorts of constructs that he associated with "conventional" sociology, as he was primarily

> whatever it is that is being characterized—if anything is being characterized—and that by playing with the sets of properties and their relationships, one can in fact construct cogent remarks about something. The fact that it is that kind of formal operation is what would lend credence to the fear that, of course, computers could build novels. (1992, Vol. 1, 242)

24 As Garfinkel and Sacks (1970, 359) present it, constructive analysis is done in "a search for rigor" by means of "the ingenious practice … whereby [indexical] expressions are first transformed into ideal expressions. Structures are then analyzed as the properties of the ideals, and the results are assigned to actual expressions as their properties, though with disclaimers of 'appropriate scientific modesty'."

25 Sacks (1992, Vol. 1, 315) observes that in conventional social science "you can have machinery which is a 'valid hypothetical construct,' and it can analyze something for you. So you say, for example, 'I have a bunch of constructed phenomena that analyze some real thing. I am not saying that people do that thing via my constructs; only that the apparatus I have will predict'" outcomes with some measurable reliability. He then adds, "Now that's not what I am intending to do."

interested in the *use* of formal structures *in* conversation. This would include formal linguistic structures, such as typical sentence forms and phrase organizations that enable recipients to project possible trajectories of an utterance or story. Similarly, the "inference-making machine" is a locally used formal narrative organization. Obviously, Sacks had to observe practices and to describe them systematically. Similarly, with Garfinkel, his pairing of instructions with the work of following them is a simple case of a formalism he uses to encourage investigations of "the work" of acting in accord with a rule, plan, or instruction. When examining the use of coding schemes to produce data, he made a gestalt switch from an instrumental interest in the results of coding as the starting point for analysis, to an observation and analysis of how his research assistants were *doing* coding—how they were making equivalence judgments when assigning texts to categories. There are endless topics to explore with that agenda.[26] How that would relate to CA as it currently stands, I think, is something that would have to be worked out in detail.

LOOKING AHEAD

WS: John Lee reminded me a few days back, that in 1973, when I was in Canada, Sacks visited Manchester and sat in on a local data analysis group. After it was over, he said to John, 'You guys want to find something of your own to do. This stuff is played out.' This was a thing recognized by Sacks, by Jefferson, Schegloff, and so on. That CA, as they did it, had come to an end, more or less. It had no outstanding problematics. It had been turned into routine work you could farm out to graduate students. So, of course, the stuff that's been done in its name has no interesting problems to address. It makes no addition to the system. It does no deepening of the analytical mechanisms. Nothing is going on except the application of a well-organized routine procedure for segmenting talk into structured sections. CA, as it goes on now, isn't CA in the original sense. It's applied CA or it's applying the theories. It's not researching the phenomenon in search of new and distinctive insights, which is why they have to manufacture these pseudo-questions like, 'Is there a gradient in forms of acknowledgment/apology?' Well, you don't need to do much research to satisfy yourself. Yes, there is a gradient, and nothing beyond that is really said. The gradient doesn't matter. It has no measurable values attached to it, really.[27]

ML: I heard a slightly different version, not of that particular meeting but after links had been formed between the California CA groups and those in Manchester and elsewhere in the UK in the 1970s. The line from some of the Californians was that CA abroad hadn't yet caught up to the locals (i.e., Sacks, Schegloff, and Jefferson). What I understood from this account was not that there was nothing new left to be done in CA, but that relative newcomers to the field were still catching up to what had already been done. There is a further issue that Sacks

26 On coding, see Garfinkel (1967, 19ff.). On instructed actions, see Garfinkel (2002: Ch. 6); also see Lynch and Lindwall (2024).

27 See Button, Lynch, and Sharrock (2022, Chapter 10) for a critical discussion of a series of recent CA analyses of apologies in conversation.

may have been suggesting, which was that rather than follow after what had been done at UCLA, UC, Irvine, or wherever, try to find a different way to do it, which in fact was done in Manchester with the more explicit and extensive involvement with Wittgenstein's later philosophy.

It's interesting that 30 years ago Randall Collins (1994) wrote something about how conversation analysis was a rare example in sociology of a progressive technical discipline. This was because it appeared to cumulatively build on prior studies, a feature that Kuhn attributed to "normal science", and which he expressed doubts about it ever happening in a social science like sociology. However, CA doesn't seem that way now, though I suppose some would say that there has been a lot of progress, with new developments happening all the time, and plenty of excitement. But what I found really surprising, starting around 2013 when I began reading publications on epistemics, after several of us began our discussions on Skype, and later Zoom, every week or two, was that CA seemed to have taken a turn. The most striking problem with epistemics wasn't a theoretical matter, although there were conceptual problems. It was with their specific characterizations of data, which were presented to illustrate the models, typologies, gradients, and metaphoric engines. When you read the fragments (and, when possible, read longer sections of transcripts and listened to the recordings from which they were drawn), it was almost always easy to imagine other ways to understand what the parties were doing.[28] So, even though they were using the method of presenting the material for other readers to confirm or criticize,[29] again and again when we examined the fragments of transcript in their publications, their commentaries seemed off the mark. This is a different order of criticism than simply saying they're not doing anything new; it's that what they're doing has abandoned what came before.

WS: That's right. I mean, a lot of it. The discipline that was involved in constructing the whole line of argument from membership categories through to the 'simplest systematics' has been lost because that was all done with a close inspection of the materials in an organised way. And now that's part of the collection stuff. People just throw a whole bunch of things together because there's a syntactic similarity between them. It's essentially the equivalence case problem recreated.

ML: I recall a presentation I made during my post-doctoral year in Toronto at the Centre of Criminology. The head of the Centre was a theologian, with very little connection to the social sciences. During an internal conference at the Centre, I discussed pre-trial sessions in criminal courts, using some transcripts of arguments between the opposing attorneys. During the question session afterwards, the theologian remarked with apparent appreciation, 'That's very technical.' It seemed like he didn't fully understand the content but appreciated the se-

28 This is especially clear in the re-analyses presented by Lindwall, Lymer, and Ivarsson (2016).

29 See Sacks (1992, Vol. 1, 622) on his reasons for taking up the study of tape-recorded conversations: "It wasn't from any large interest in language, or from some theoretical formulation of what should be studied, but simply by virtue of that; I could get my hands on it, and I could study it again and again. And also, consequentially, others could look at what I had studied, and make of it what they could, if they wanted to be able to disagree with me."

riousness of the work due to its level of detail. To borrow a phrase from Sacks (1992, Vol. 1, 488), the exercise with the transcripts "shielded from examination" the very natural language materials I was exhibiting, which perhaps worked to my benefit at the time, but was ultimately unsatisfying.

DB: As is the move from the phrase conversational analysis to conversation analysis, is that on topic of the schism?

ML: I don't think it was central to any schism, though prior to the 1980s, conversation*al* analysis was the identifying phrase. There is a difference between 'conversational' and 'conversation' analysis, though both can be interpreted to signal that a constitutive analysis is performed by the parties *within* a conversation as well as by professional investigators *of* conversation. So, it's not a stark distinction. The shift appears to have become established at about the time of publication of Atkinson and Heritage's (1984) influential reader. At the time, I think Gail Jefferson was in favour of it, though Manny was not,[30] and Garfinkel continued to use 'conversational analysis' after it was no longer in fashion. While the difference may not carry significant weight, in the view of some of us, 'conversational analysis' put more emphasis on analyses endogenous to the production of conversation than did 'conversation analysis.'

While you still encounter references in the CA literature to how the analysis is conducted by the parties, the method of treating a collection as coherent, simply because the constituent instances have been grouped together on the basis of a common word or order of words, tends to shift the locus of formal analysis from the parties to the talk to the professional analyst.

BA: This can lead to the question about your recent interest in the unknown contributions in Harold's archive,[31] and how they might make a difference in the way we think about such things. I'd like to hear a little bit more about that.

ML: I have thought about that question but haven't yet fathomed what remains to be unearthed. My tentative answer would be that, after spending quite a few hours in the Garfinkel archive[32] and yet only scratching the surface of part of it (mainly, the phase of his long career in the 1980s and 1990s), I don't get the impression that there are going to be any major surprises. I think his best work is in print and has been available for quite some time. The main thing I see worth bringing out are the lectures and seminars. There are many years of them in recordings, and Garfinkel arranged to have a large number of them transcribed. I haven't read or listened to the vast majority of them. I have not dug into, as Anne Rawls has, Garfinkel's early

30 Schegloff (1988: 89, 93) indicates his dis-preference for 'conversation analysis' as a name for the practice.

31 Several of Garfinkel's early writings that have been published in recent years were indeed 'unknown' previously. They include, for example, Garfinkel (2008) and (2019). In other cases, such as the first part of Garfinkel (2022) had been circulated by Garfinkel himself in the late 1980s.

32 There are two Garfinkel archives. One is in the Special Collections in the UCLA Library, and the other, a much larger collection of Garfinkel's writings, recordings, notes, letters, files books and equipment is in Newburyport, MA.

writings from the 1940s and 1950s (with the exception of the Gulfport Field Study [Garfinkel 2019]).[33] I'm not as interested in the early work, but I think the lectures could sustain interest.

The lectures that I have edited or co-edited are just a small portion of the tapes and transcripts of his lectures.[34] There's a large notebook of lectures on instructed actions that Garfinkel put together from transcribed seminars that ran for much of a year. "Instructed actions" is a theme that he uses very broadly, and it's very important for him theoretically as an alternative to the idea of a society that's describable in terms of a hierarchy of rules, norms, folkways, and related abstract formulations that sociological theorists use to encompass social order. Instructed actions are practices that bring to life what otherwise are formulated as rule-like or normative. It is part of his picture of social order that can be viewed as a figure-ground reversal of the notion of rule-governed actions. Actions show a grammatical production history that is prior to whatever else is made of them.

BA: Right. But as you say, if the best work is already done, we've got a problem of how to position work which was clearly either not finished or early work or for whatever other reasons he didn't put the effort into publishing it or finding some way of getting it out.

ML: Harold's principal strength wasn't in conducting studies, although he did conduct interesting ones; and, needless to say, bringing his work into publication wasn't his strength either. Especially in his later decades when I knew him, he would introduce ideas with the promise that more substantial detail would follow, but the detailed elaboration never seemed to materialize. Examples include occasion maps or instructed actions. There's a considerable amount of what you might call programmatic content. While it is intriguing and suggestive, it lacks further elaboration. When we consider what Harvey Sacks did, he wasn't simply working out Garfinkel's ideas. On the contrary, he developed them in surprising and original ways that remain to be pursued and developed.

In Garfinkel's body of work, both published and unpublished, there are numerous suggestions that he doesn't himself follow up on. He proposes and promises to develop them, and even suggests that he (often together with one or more students or former students) had developed them, but as far as I know many of these ideas never get very far beyond proposals and preliminary studies. This leaves plenty of work for everyone else to do. Importantly, it's not just about working out or applying existing ideas. The challenge is to recognize that, in order to pursue his leads, you need to develop them in detail and in an original way. He insists on detail, and rightly so, but leaves much to the imagination. What the work should look like doesn't emerge, Athena-like, fully formed from his brain. I should emphasize, though, that

33 Garfinkel wrote Part II of the Gulfport Field Study during World War II, when he was serving in the Army Air Force. It was a description of a quickly assembled effort to train service recruits to repair transport aircraft. The report was written for the Army Air Force and had none of the appearance of an academic report, but it had some interesting sections, particularly on "mock-ups" of aircraft parts used for practical training of the recruits, and on the practical organization of the field training program.

34 An abridged series of seminars from 1980 is in Garfinkel (2022, Part 2). Also see transcribed and edited lectures by Garfinkel (2021, 2024).

his unfinished, and in some cases barely started, projects were gifts to his students as much as a burden upon them.

David Sudnow once made an offhand remark in a seminar that Garfinkel wasn't a very good ethnomethodologist. However, if you truly understand what Garfinkel accomplished, it's evident that he opened up topics that were sometimes uncanny, strange, or seemingly so. But they continue to provide rich grounds for exploration, as do Sacks' lectures and writings. This ties back to what I mentioned earlier about why I found it more intriguing to work with Garfinkel—because he kept us puzzled, leaving it up to us to decipher what he was talking about. Even when he asserted, "That's not it!" after you took a chance to formulate what he might have meant by something he said or wrote, it still left an opening for imagining what 'it' might be.

BA: Yeah, there was that lovely quote at the beginning of the book on information: "Having just come out of a jungle, I can't promise that in leading you in to show you what I've found that I won't lose the way for all of us" (Garfinkel 2008, 101). And somehow or other, that needs to be said because otherwise, this stuff's going to be taken as is. It would end up actually devaluing Garfinkel, because people will think it's fully cooked and fully ready, whereas they've got to put it together and make it work to do what it is that he's alluding to. Just for the sake of the younger generation needing a curriculum, this is, if you like, a warning I'm giving or some kind of packaging that needs to be provided, both in what we've been talking about and also perhaps in what is coming out now from Garfinkel's archive. We need to look at it in a particular way and be prepared to do the work ourselves. I think that we need and are doing the same with Sacks and his lectures, his archives.

We really have this schism now, it seems to me. The enormous volume of sequential analytic work is now just detached from any ethnomethodological foundations. So, I think both enterprises, returning to Garfinkel and returning to Sacks, can sort of revivify those conceptual alignments. Part of the problem too is that CA has found a home in linguistics, or at least at the edge of linguistics, and is suffering pretty badly in sociology, actually. So, there are different interests. Sacks always kept front and center the problem of order, the problem of social order, and how social production was intrinsic to not only conversation but also the meaning of sentences, that kind of thing. And Manny was very attuned to that in his own way too.

But I think there's something more I want to say, or something I want to push on, and it has to do with the questions we face and the questions they faced, the disciplinary questions that we face and the disciplinary questions that they faced. If you think about, shall we say, the period from 1960 to 1980, those 20 years when ethno was basically in startup mode, and the idea was that you needed to get on with things, show that it works, demonstrate that there are real findings to be made, real ways of moving forward. Whereas now, we find ourselves in a situation where sociology is on the rack, with CA being marginal there, and ethno is flying out in all sorts of directions. So, if we want to have a core sociology that does the sorts of things we've been talking about, then we have to think differently from the way they thought. Or perhaps it's about recovering the way they thought, seeing again how they perceived their tasks and projects, in order to respecify what the sociological grounds of everyday life could be, with no loss of order, structure, or regularity in the respecifications. It's that kind of thinking to which I was pointing when I said we need to look at what they're offering us and then position it for ourselves.

ML: Well, I think that may be part of the animating impulse behind the notion of hybrid studies that Garfinkel promoted, even though he was firmly ensconced in a major sociology department. It was a grand vision that ethnomethodology would give rise to hybrids of ethnomethodology with law, education, some of the sciences and mathematics, medicine, and who knows what else. The hybrids would always be somehow coherent as ethnomethodology, which of course is a tall order. In any case, it didn't happen,[35] and probably won't, but it was an interesting idea that presumably would not just empty out sociology; you'd find ways to re-do some sort of sociology. I'm reminded of the vision that was said to be the rationale for the founding of a non-departmental School of Social Sciences at Irvine in the late 1960s. A notable organization theorist, James March, was the first Dean of Social Sciences, and an architect of its program during the initial years, including when Sacks worked there. The idea at Irvine was not to have disciplines; instead, it was to be an environment encouraging clusters of faculty and graduate students to work on innovative problems. Sacks was one of the members of the faculty who really thrived on it, even though by the time he died, the School had already started moving towards forming departments along disciplinary lines. I recall a faculty meeting (graduate students could attend) where reorganization was discussed, and Sacks, who didn't go to faculty meetings often, attended and spoke against a proposal to departmentalize, saying something to the effect, "I understood that we weren't going to go in this bureaucratic direction." This was followed by a long silence.

REFERENCES

Anderson, R. J., and Wes Sharrock. 1984. 'Analytic Work: Aspects of the Organization of Conversational Data.' *Journal for the Theory of Social Behaviour* 14 (1): 103–24.

Atkinson, J. Maxwell, and John Heritage, eds. 1984. *Structures of Social Action: Studies in Conversation Analysis*. Cambridge: Cambridge University Press.

Becker, Howard S. 1951. 'The Professional Dance Musician and His Audience.' *American Journal of Sociology* 57 (2): 136–44.

Berger, Peter, and Thomas Luckmann. 1966. *The Social Construction of Reality*. New York, NY: Doubleday.

Bloor, David. 1976. *Knowledge and Social Imagery*. London: Routledge and Kegan Paul.

Brannigan, Augustine. 1981. *The Social Basis of Scientific Discoveries*. Cambridge: Cambridge University Press.

Button, Graham, Michael Lynch, and Wes Sharrock. 2022. *Ethnomethodology, Conversation Analysis and Constructive Analysis: On Formal Structures of Practical Action*. Abington, UK, and New York, NY: Routledge.

Button, Graham, and Wes Sharrock. 2016. 'In Support of Conversation Analysis' Radical Agenda.' *Discourse Studies* 18 (5): 610–20.

35 When I say that "it didn't happen," I don't intend to ignore the many studies of legal, educational, medical, and other work environments that, in one way or another, deploy analytical procedures or take on a 'perspective' associated with ethnomethodology and/or CA. I am referring instead to Garfinkel's ambitious proposal that "hybrid" studies would be recognized as original contributions to the practice of law, mathematics, education, and so on. See Button et al. (2022, Chs. 3 and 7) for discussion of studies of work and their mixed success.

Clift, Rebecca, and Chase Wesley Raymond. 2018. 'Actions in Practice: On Details in Collections.' *Discourse Studies* 20 (1): 90–119.
Collins, Harry M. 1985. *Changing Order: Replication and Induction in Scientific Practice*. London and Beverly Hills, CA: Sage.
Collins, Randall. 1994. 'Why the Social Sciences Won't become High-consensus, Rapid-Discovery Science.' *Sociological Forum* 9: 155–77.
Davidson, Judy A. (1984) 'Subsequent Versions of Invitations, Offers, Requests, and Proposals Dealing with Potential or Actual Rejection.' In S*tructures of Social Action: Studies in Conversation Analysis*, edited by J. Maxwell Atkinson and John Heritage, 102–28. Cambridge: Cambridge University Press.
Drew, Paul, ed. 2018a. 'Epistemics—The Rebuttal Special Issue.' *Discourse Studies* 20 (1).
———. 2018b. 'Epistemics in Social Interaction.' *Discourse Studies* 20 (1): 163–87.
Eglin, Trent. 1986. 'Introduction to a Hermeneutics of the Occult: Alchemy. In *Ethnomethodological Studies of Work*, edited by Harold Garfinkel, 123–59. London: Routledge and Kegan Paul.
Fitzgerald, Richard. 2024. Drafting A Simplest Systematics for the Organization of Turn-Taking for Conversation. *Human Studies* 47: 613–33
Garfinkel, Harold. 1967. *Studies in Ethnomethodology*. Englewood Cliffs, NJ: Prentice Hall.
———, ed. 1986. *Ethnomethodological Studies of Work*. London: Routledge & Kegan Paul.
———. 2002. *Ethnomethodology's Program: Working out Durkheim's Aphorism*. Edited by Anne Rawls. Lanham, MD: Rowman & Littlefield.
———. 2008. T*oward a Sociological Theory of Information*. Edited by Anne Rawls. London: Paradigm.
———. 2019. *The History of Gulfport Field 1942, Part II*. Edited by Tristan Thielmann. Media of Cooperation, University of Siegen, Germany.
———. 2021. 'Ethnomethodological Misreading of Aron Gurwitsch on the Phenomenal Field.' Edited by Clemens Eisenmann, and Michael Lynch. *Human Studies* 44 (1): 19–42.
———. 2022. *Studies of Work in the Sciences*. Edited by Michael Lynch. Abington, UK: Routledge.
———. 2024. 'Praxeological Validity of Instructed Action.' In *Instructed and Instructive Actions: The Situated Production and Subversion of Social Order*, edited by Michael Lynch and Oskar Lindwall, 21–36. Abington, UK: Routledge.
Garfinkel, Harold, and Harvey Sacks. 1970. 'On Formal Structures of Practical Actions.' In *Theoretical Sociology: Perspectives and Development*, edited by J. C. McKinney and E. A. Tiryakian, 337–66. New York, NY: Appleton-Century-Crofts.
Garfinkel, Harold, Michael Lynch, and Eric Livingston. 1981. 'The Work of a Discovering Science Construed with Materials from the Optically Discovered Pulsar.' *Philosophy of the Social Sciences* 11 (2): 131–58.
Goldberg, Jo Ann. 2004. 'The Amplitude Shift Mechanism in Conversational Closing Sequences.' In *Conversation Analysis: Studies from the First Generation*, edited by Gene H. Lerner, 257–97. Amsterdam: John Benjamins.
Heritage, John. 2012. 'The Epistemic Engine: Sequence Organization and Territories of Knowledge.' *Research on Language and Social Interaction* 45 (1): 30–52.
———. 2018. 'The Ubiquity of Epistemics: A Rebuttal to the 'Epistemics of Epistemics' Group.' *Discourse Studies* 20 (1): 14–56.
Holton, Gerald. 1981. 'Comments on Professor Harold Garfinkel's Paper.' *Philosophy of the Social Sciences* 11 (2): 159–61.
Jordan, Kathleen, and Michael Lynch. 1992. 'The Sociology of a Genetic Engineering Technique: Ritual and Rationality in the Performance of the Plasmid Prep.' In *The Right Tools for the Job: At Work in 20th Century Life Sciences*, edited by A. Clarke and J. Fujimura, 77–114. Princeton, NJ: Princeton University Press.
Knorr Cetina, Karin. 1981. *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*. Oxford: Pergamon.
Kuhn, Thomas S. 1970. *The Structure of Scientific Revolutions*, 2nd Edition. Chicago, IL: University of Chicago Press.
Latour, Bruno, and Steve Woolgar. 1979. *Laboratory Life: The Social Construction of Scientific Facts*. London: Sage.
Lerner, Gene, ed.. 2004. *Conversation Analysis: Studies from the First Generation*. Amsterdam: John Benjamins.
Liberman, Kenneth. 1985. *Understanding Interaction in Central Australia: An Ethnomethodological Study of Australian Aboriginal People*. London: Routledge and Kegan Paul.
Lindwall, Oskar, Gustav Lymer, and Jonas Ivarsson. 2016. 'Epistemic Status and the Recognizability of Social Actions.' *Discourse Studies* 18 (5): 500–25.

Livingston, Eric. 1986. *The Ethnomethodological Foundations of Mathematics*. London: Routledge and Kegan Paul.

Lymer, Gustav, Oskar Lindwall, and Jonas Ivarsson. 2017. 'Epistemic Status, Sequentiality, and Ambiguity: Notes on Heritage's Rebuttal.' Available online at: http://radicalethno.org/documents/lymeretal.pdf

Lynch, Michael. 1983. 'Accommodation Practices: Vernacular Treatments of Madness.' *Social Problems* 31 (2): 152–64.

———. 1985a. *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory*. London: Routledge and Kegan Paul.

———. 1985b. 'Discipline and the Material Form of Images: An Analysis of Scientific Visibility.' *Social Studies of Science* 15 (1): 37–66.

———. 1992. 'Extending Wittgenstein: The Pivotal Move from Epistemology to the Sociology of Science.' In *Science as Practice and Culture*, edited by Andrew Pickering, 215–65. Chicago: University of Chicago Press.

———. 1993. *Scientific Practice and Ordinary Action: Ethnomethodology and Social Studies of Science*. New York, NY: Cambridge University Press.

———. 2018. 'Notes on a Display of Epistemic Authority: A Rejoinder to John Heritage's Rebuttal to 'The epistemics of Epistemics'.' Available online at: https://radicalethno.org/documents/lynchrejoinder.pdf

———. 2019. 'Garfinkel, Sacks and Formal Structures: Collaborative origins, Divergences and the Vexed Unity of Ethnomethodology and Conversation Analysis.' *Human Studies* 42 (2): 183–98.

———. 2020. 'The Inference-Making Machine Meets the Epistemic Engine.' Ethnographic Studies 19: 143–60.

Lynch, Michael, and David Bogen. 1994. 'Harvey Sacks' Primitive Natural Science.' *Theory, Culture and Society* 11 (4): 65–104.

———. 1996. *The Spectacle of History: Speech, Text, and Memory at the Iran-Contra Hearings*. Durham, NC: Duke University Press.

Lynch, Michael, Simon Cole, Ruth McNally, and Kathleen Jordan. 2008. *Truth Machine: The Contentious History of DNA Fingerprinting*. Chicago: University of Chicago Press.

Lynch, Michael, and Oskar Lindwall, eds. 2024. *Instructed and Instructive Actions: The Situated Production and Subversion of Social Order*. Abington, UK: Routledge.

Lynch, Michael, and Douglas Macbeth, eds. 2016. The Epistemics of Epistemics. Special issue of *Discourse Studies* 18 (5): 493–620.

Lynch, Michael, and Jean Wong. 2016. 'Reverting to a Hidden Interactional Order: Epistemics, Informationism, and Conversation Analysis.' *Discourse Studies* 18 (5): 526–49.

Macbeth, Douglas 2018. 'Authority, Subordination and the Re–writing of 'Epistemics': A Reply to a Rebuttal.' Available online at: http://radicalethno.org/documents/macbeth.pdf

Macbeth, Douglas and Jean Wong. 2016. 'The Story of "Oh", Part 2: Animating Transcript.' *Discourse Studies* 18 (5): 574–96.

Macbeth, Douglas, Jean Wong, and Michael Lynch. 2016. 'The Story of "Oh", Part 1: Indexing Structure, Animating Transcript.' *Discourse Studies* 18 (5): 550–73.

Mannheim, Karl. 1936. *Ideology and Utopia*. New York: Harvest Books.

Pollner, Melvin. 1974. 'Mundane Reasoning.' *Philosophy of the Social Sciences* 4: 35–54.

Pollner, Melvin. 1975. 'The Very Coinage of Your Brain: The Anatomy of Reality Disjunctures.' *Philosophy of the Social Sciences* 5: 411–30.

Pomerantz, Anita. 1984. 'Agreeing and Disagreeing with Assessments: Some Features of Preferred/Dispreferred Turn Shapes.' In *Structures of Social Action: Studies in Conversation Analysis*, edited by J. Maxwell Atkinson, and John Heritage, 57–101. Cambridge: Cambridge University Press:

Psathas, George. 2010. 'Ethnomethodology and Conversation Analysis at Boston University: A Brief History.' In *The Social History of Language and Social Interaction Research: People, Places, Ideas*, edited by Wendy Leeds-Hurwitz, 179–213. Cresskill, NJ, Hampton Press.

Raymond, Geoffrey. 2018. 'Which Epistemics? Whose Conversation Analysis?' *Discourse Studies* 20 (1): 57–89.

Sacks, Harvey. 1970. *Aspects of the Sequential Organization of Conversation*. Unpublished Manuscript, School of Social Sciences, University of California, Irvine.

———. 1992. *Lectures on Conversation*, Vols. 1 & 2. Edited by Gail Jefferson. Oxford: Basil Blackwell.

Sacks, Harvey, Emanuel A. Schegloff, and Gail Jefferson. (1974). *A simplest systematics for the organization of turn-taking in conversation*. Language 50 (4): 696–735.

Schegloff, Emanuel A. 1968. 'Sequencing in Conversational Openings.' *American Anthropologist* 70: 1075–95.

———. 1987. 'Between Micro and Macro: Contexts and Other Connections.' In *The Micro-Macro Link*, edited by J. Alexander, B. Giesen, R. Münch, and N. Smelser, 207–34. Berkeley, CA: University of California Press.
———. 1988. 'Goffman and the Analysis of Conversation,' in *Erving Goffman: Exploring the Interaction Order*, edited by Paul Drew, and Anthony Wootton, 89–135. Oxford: Polity Press.
———. 1991. 'Conversation Analysis and Socially Shared Cognition.' In *Perspectives on Socially Shared Cognition*, edited by Lauren B. Resnick, John M. Levine, and Stephanie D. Teasley, 150–71. Washington, D.C.: American Psychological Association.
———. 1992a. 'Introduction.' In *Harvey Sacks, Lectures on Conversation, Vol. 1*, edited by Gail Jefferson, ix–lxii. Oxford: Blackwell.
———. 1992b. 'Introduction.' In *Harvey Sacks, Lectures on Conversation, Vol. 2*, edited by Gail Jefferson, ix–lii. Oxford: Blackwell.
———. 1992c. 'To Searle on Conversation: A Note in Return.' In *(On) Searle on Conversation*, edited by John Searle, Herman Parret and Jef Verschueren, 113–128. Amsterdam/Philadelphia: John Benjamins.
———. 1999. "Schegloff's Texts' as 'Billig's Data': A Critical Reply.' *Discourse and Society* 10: 558–72.
———. 2009. 'One Perspective on Conversation Analysis: Comparative Perspectives.' In *Conversation Analysis: Comparative Perspectives*, edited by Jack Sidnell, 358–406. Cambridge: Cambridge University Press.
———. 2010. Commentary on Stivers & Rossano, 'Mobilizing Response.' *Research on Language and Social Interaction* 43 (1): 38–48.
———. 2017. 'Reply to Levinson: On the 'Corrosiveness' of Conversation Analysis.' In *Enabling Human Conduct: Studies of Talk-in-Interaction in Honor of Emanuel A. Schegloff*, edited by Jeffery Raymond, Gene Lerner, and John Heritage, 351–353. Amsterdam: John Benjamins.
Shapin, Steven. 2023. 'Paradigms Gone Wild.' Review of The Last Writings of Thomas Kuhn: Incommensurability in Science. Chicago: University of Chicago Press, 2002. *London Review of Book*s 45(7): March 30, 2023. https://www.lrb.co.uk/the-paper/v45/n07/steven-shapin/paradigms-gone-wild
Steensig, Jacob, and Trine Heinemann. 2016. 'Throwing the Baby Out With the Bath Water? Commentary on the Criticism of the 'Epistemic Program.' *Discourse Studies* 18: 597–609.
Stivers, Tanya, and Federico Rossano. 2010. 'Mobilizing response.' *Research on Language and Social Interaction* 43 (1): 3–31.
Sudnow, David. 1978/2001. *Ways of the Hand*. Cambridge, MA: Harvard University Press; Revised & Rewritten, MIT Press, 2001.
Terasaki, Alene Kiku. 2004. 'Pre-Announcement Sequences in Conversation.' In *Conversation Analysis: Studies from the First Generation*, edited by Gene H. Lerner, 171–223. Amsterdam: John Benjamins.
Traweek, Sharon. 1988. *Beamtimes and Lifetimes: The World of High Energy Physics*. Cambridge, MA: Harvard University Press.
Watson, Rod. 2008. 'Comparative Sociology, Laic and Analytic: Some Critical Remarks on Comparison in Conversation Analysis.' *Cahiers de praxématique* 50: 197–238.
Wittgenstein, Ludwig. 1958. *Philosophical Investigations*, 2nd Edition. Oxford: Blackwell.