FQS FORUM: QUALITATIVE SOCIAL RESEARCH SOZIALFORSCHUNG

Volume 20, No. 2, Art. 13
May 2019

"The *Studies* are Probably the Best Thing That Garfinkel Ever Wrote"

Michael Lynch in Conversation With Dominik Gerst, Hannes Krämer & René Salomon

Key words: ethnomethodology; conversation analysis; history of American sociology; science and technology studies; epistemics; practice theory

Abstract: Michael LYNCH is widely known as one of the key figures of ethnomethodology. In this interview, he takes the discussion of GARFINKEL's "Studies in Ethnomethodology" (1967) as an opportunity to take the reader back to California in the 1970s as he shares his personal story of how he became acquainted with Harold GARFINKEL and ethnomethodology as a radical approach on the rise. LYNCH provides an account of ethnomethodology as a distinctive way of researching, writing, talking; which stands in high contrast to conventional social sciences and, which not only has been marginalized by the sociological mainstream at the time it came up, but may be seen as endangered nowadays. As he says in the interview, the tense relationship between ethnomethodology and conversation analysis as a robust field of inquiry can be traced back to this question as well. He reflects upon GARFINKELs central intellectual resources—namely phenomenology and the philosophy of WITTGENSTEIN—and shows how his own work embraces the relationships of ethnomethodology with science and technology studies and actor-network theory. Giving insights into how his work is driven by a confrontation of social theory and philosophy with empirical concreteness, LYNCH discusses concepts such as *practice* and *knowledge* which may be seen as in-between-phenomena within this confrontation. Finally, he suggests to continuously reread GARFINKEL's "Studies in Ethnomethodology" as the book provides a rich resource of ideas which especially become productive in light of own research.

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Biographical Note

Michael LYNCH, born 1948, is emeritus professor in the Department of Science and Technology Studies at Cornell University in Ithaca, NY. He grew up in upstate New York, and received his undergraduate degree from Cornell in 1970. It was during that time that he became interested in sociology, and he went on to enroll in a graduate program in sociology at the State University of New York campus in Binghamton. While working as a research assistant in survey analysis he became disillusioned with disciplinary sociology and moved to the University of California, Irvine (UCI), which offered a PhD in social science with no formal disciplinary requirements. It was there that he began studying ethnomethodology, and the flexible PhD program at UCI allowed him to take courses at the University of California, Los Angeles (UCLA), and to work with Harold GARFINKEL and Melvin POLLNER. He completed a dissertation in 1978-1979 based on an ethnographic study of day-to-day research practices in a neuroscience laboratory. His dissertation used on-site observations and audio recordings to study sequences of material and analytical practices, through which researchers collaboratively produced displays of data designed to investigate and demonstrate neural plasticity. The dissertation was titled "Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory" and was later published under that title by Routledge and Kegan Paul (LYNCH, 1985). While completing his dissertation, LYNCH began a visiting postdoctoral fellowship at the Centre of Criminology, University of Toronto, where he worked with a research group that was studying *plea bargaining* in a local criminal court (LYNCH, 1982), and also worked with Augustine BRANNIGAN on a paper on credibility (BRANNIGAN & LYNCH, 1987). LYNCH adapted his treatment of laboratory shop talk about possible neurological entities and processes to describe negotiations among prosecutors and defense counsel about alleged criminal events. He moved back to California at the end of 1979 and where he began a two-year postdoctoral fellowship at UCLA in which he taught part-time in the Sociology Department and investigated case conferences at the UCLA hospital, where clinicians from different psychiatric and neurological specialties examined and discussed patients with complex neuropsychiatric conditions (LYNCH, 1983, 1984). During that time, he also collaborated with Harold GARFINKEL and Eric LIVINGSTON on a study of a tape recording of astronomers in the midst of investigating a possible optical pulsar—a possible discovery that, by the end of the tape, the researchers were ready to announce as an actual discovery (GARFINKEL, LYNCH & LIVINGSTON, 1981). [1]

Positions in sociology were scarce at the time, and ethnomethodology and sociology of sciences were relatively low-priority specialties in the field. To make matters worse for LYNCH, he had a falling-out with GARFINKEL that lasted for several years. After completing the UCLA fellowship and teaching as an adjunct

at UCLA and UCI, LYNCH began to consider alternatives to academia. However, he received (and accepted) an offer from Whitman College, a small liberal arts college in Walla Walla, Washington, and taught there for three years in the mid-1980s. During those years he married Nancy RICHARDS, who also received her PhD from UCI, for an anthropological study of the legal coca economy in the highlands of Peru. In 1987, he took up a position in sociology at Boston University (BU), which had a vibrant program in ethnomethodology, with George PSATHAS and Jeff COULTER on the faculty and a talented and energetic group of PhD students including Dusan BJELIC, David BOGEN, Timothy COSTELLOE, Eileen CRIST and Kathleen JORDAN. While at BU, LYNCH (1993) drafted his second book "Scientific Practice and Ordinary Action." Although he was forced to leave BU in 1993, due to decisions by the notoriously authoritarian administration that ruled over the university at the time, he and some of his former students continued to collaborate on a series of papers for years afterwards. LYNCH collaborated with BJELIC on an ethnomethodological demonstration of GOETHE's arguments against NEWTON's theory of colors (BJELIC & LYNCH, 1992). LYNCH and BOGEN published a series of critical articles that focused on developments in social constructivism and conversation analysis (BOGEN & LYNCH, 1993; LYNCH & BOGEN, 1994). They also published a book that examined televised testimony at the 1987 US Congressional Iran-contra hearings (LYNCH & BOGEN, 1996). LYNCH and JORDAN also collaborated on a series of papers and, together with Simon COLE and Ruth McNALLY, eventually co-authored a study of "DNA Fingerprinting" in Criminal Justice (LYNCH, COLE, McNALLY & JORDAN, 2008). [2]

LYNCH moved to England in 1993, and worked for six years in the Department of Human Sciences at Brunel University in West London. There, he benefited from having a strong group of colleagues in science and technology studies (STS)—including Steve WOOLGAR, Alan IRWIN, and Christine HINE—and he collaborated with a post-doctoral fellow, Ruth McNALLY, on a project that examined the development of a national criminal DNA database in the UK (LYNCH & McNALLY, 2003). In 1999 he moved back to the US to take up a position in the Department of Science & Technology Studies, where he worked with Simon COLE on the final phase of the project on forensics that resulted in the publication of "Truth Machine" (LYNCH et al., 2008). During his time at Cornell he actively participated in service to the growing STS field. He participated in STS panels with the National Science Foundation; was chair of the Science, Knowledge & Technology Section of the American Sociological Association; and president of the Society for Social Studies of Science (4S). He also edited one of the main journals in the field, *Social Studies of Science*, from 2002 until 2012. In 2016, he received the J.D. Bernal Prize from the 4S for career contributions to the STS field. [3]

In the past several years, Lynch has been working with Douglas MACBETH, Oskar LINDWALL, Jonas IVARSSON, Jean WONG, Gustav LYMER, and Wendy SHERMAN-HECKLER on a critical examination of recent developments in Conversation Analysis—particularly the analytical framework that goes by the name of *epistemics*. A series of papers on the topic were presented in the journal

Discourse Studies (LYNCH & MACBETH, 2016), and further arguments are available on the Radical Ethnomethodology website. LYNCH retired in 2018, but continues to be active in STS and ethnomethodology. [4]

About the Interview

The following interview was undertaken via Skype in June 2017 as part of the project "Harold Garfinkel and the Studies in Ethnomethodology. An Interview Issue," edited by Dominik GERST, Hannes KRÄMER and RENÉ SALOMON[1]. Michael LYNCH was in his private workroom in New York while Dominik GERST and Hannes KRÄMER were in their office at European-University Viadrina in Frankfurt (Oder). René SALOMON was patched-in from his office at University of Würzburg. The connection via Skype was constantly interrupted, and also an annoying and constant feedback tone occurred whose origin could not be detected, whereas all participants collaboratively managed to find a rather unusual solution: not only was video transmission switched off during most of the time, but also those parties who would not speak had to mute their audio transmission as well. Further questions were raised and answered via e-mail soon afterwards. The present version was authorized by Michael LYNCH in April 2019. [5]

1. "I Made the Decision to Work With GARFINKEL"

Hannes KRÄMER: Hello Mike, let us start directly with the first question: How did you get in contact with the "Studies in Ethnomethodology" (in the following also *Studies*) and GARFINKEL's ideas? When and why did you read the book for the first time?

Michael LYNCH: I was a master's degree student in the State University of New York at Binghamton (later named Binghamton University) in the early 1970s and I was not a very well-motivated sociology student. I was assigned to a survey analyst as a research assistant and I found it not very satisfying for a lot of reasons. But I had a sociology of knowledge course from a junior professor named Lynn MILLER[2], who left the field of sociology soon afterwards. I've lost touch with him completely, I have no idea what he does now. But he had been at UCI and he described to me some of the work that was being done by Harvey SACKS[3] and some of his students. And he recommended that I transfer to Irvine and work with SACKS and whoever else was there. I did that and got accepted the next year, that was fall of 1972. And it turned out by coincidence that Harold GARFINKEL was teaching at Irvine for a visiting appointment while he was on leave for part of that year from UCLA. And he was occasionally meeting with

1 See the Introduction by Dominik GERST, Hannes KRÄMER and René SALOMON (2019) in this issue.

2 Lynn MILLER was an assistant professor in sociology at State University of New York, Binghamton (now Binghamton University) around 1971. He left academia in the 1970s.

3 Harvey SACKS (1935-1975), founder of conversation analysis and prominent figure of early ethnomethodology, had a huge impact on sociology, linguistics and discursive psychology. Lecturer in Los Angeles and professor in Irvine (1964-1975). Major areas of his work include sequential analysis, membership categorization, and social science methodology (see SACKS, 1992).

SACKS during his visits. SACKS had his own group of students and he invited me to sit in the research seminar with his students. But he said that after a ten-week quarter—the academic year was divided into ten-week units—I had to choose whether I was going to commit to that group or do something else. But I had also been taking GARFINKEL's seminars at the same time and found them more to my interest. Consequently, I made the decision to work with GARFINKEL. At the time, even though GARFINKEL and SACKS had a good relationship, it was clear I had to work with one or the other. Doing conversation analysis (CA) was at that point already quite a technical field and SACKS wasn't interested in having students who didn't master the technique, you know, the turn-taking-systems and different types of adjacency pairs (SACKS, 1992). I did stay interested in CA, but I mainly worked with GARFINKEL after that point. [6]

Hannes KRÄMER: That was the time when you read *Studies*?

Michael LYNCH: It was before I arrived at Irvine that I started reading *Studies*. And with no prior acquaintance with it, it was very difficult. But when I arrived at Irvine I also met Melvin POLLNER[4] and I found that his dissertation, which was in mimeo-form then and later published as the book "Mundane Reason" (POLLNER, 1987), that it was much more comprehensible for me at that point. I found it very interesting, particularly the notion of *reality disjunctures*[5]*.* And that gave me insight into controversies in the sciences about ontological matters. But I did stick with GARFINKEL. He left Irvine to go back to UCLA by the end of that year and he invited me and a few others from Irvine to attend the seminars at UCLA, which was about 75km from Irvine. And a few friends of mine and I would drive back and forth from Irvine to GARFINKEL's seminars. So that's how I got acquainted with him. And it was through being acquainted with him that I took a more sustained effort to read *Studies.* And GARFINKEL also encouraged us to read Martin HEIDEGGER[6], Edmund HUSSERL[7], Maurice MERLEAU-PONTY[8],

4 Melvin POLLNER (1940-2007), sociologist. Since 1968 he worked at UCLA, where he was also professor of sociology until his death. He was a colleague of GARFINKEL's and one of the early participants in ethnomethodology. His research interests included everyday reasoning, mental illness and the interactional accomplishment of reality (see POLLNER, 1987).

5 With the concept of *reality disjunctures*, POLLNER tries to grasp the phenomenon that the world can be seen differently by different observers through which produces they negotiate competing versions of reality. In his book, he deals with the ways this puzzle is resolved or dissolved (see POLLNER, 1987, Chapter 4).

6 Martin HEIDEGGER (1889-1976), one of the most influential philosophers in the 20th century, was professor in Marburg, Germany (1923) and Freiburg, Germany (1928) where he taught until 1967. He had to take some breaks in teaching because of his controversial and dubious role in Nazi-Germany during World War II. "Being and Time" [*Sein und Zeit*] (1962 [1927]) is one of his most important books that, as a study of being, is related to existentialism. He also published works about modern technology and criticized traditional metaphysics.

7 Edmund HUSSERL (1859-1938), one of the most influential philosophers in the 20th century and founder of phenomenology, was professor in Göttingen, Germany (1901-1916) and in Freiburg until his retirement in 1928. Important topics in his work include intersubjectivity, psychology, intentionality, logic and the phenomenological reduction as a contribution to transcendental idealism (see HUSSERL, 1973 [1913]).

8 Maurice MERLEAU-PONTY (1908-1961), prominent philosopher in the phenomenological tradition, was professor of philosophy at the Collège de France in Paris since 1952. His work was important for further developments of phenomenology; in his conception especially the body plays an important role (see MERLEAU-PONTY, 1962 [1945]).

Alfred SCHÜTZ[9]—you know, the basic phenomenological and existentialist readings. And I did quite a lot of reading and that allowed me to make more sense of what *Studies* was about, because much of the terminology and the way of thinking draws from phenomenology and develops phenomenological themes. That gave me more appreciation for the book. And of course, I was taking GARFINKEL's seminars and so I was steeped in that. And it was important that at Irvine I didn't have constant contact with GARFINKEL, since it is a different university. I had many long conversations with some friends who were also pursuing PhDs in ethnomethodology (EM) and CA, particularly David WEINSTEIN[10], and Alene Kiku TERASAKI.[11] These conversations and friendships really helped establish my interest in doing EM. [7]

2. Intellectual Environment

Hannes KRÄMER: Could you maybe say a bit more about the intellectual environment there? You said it wasn't only you reading or discussing EM, there were other people as well. Was there a direct contact? Was there a reading group?

Michael LYNCH: No, we didn't have a reading group *per se*, but there was a group of graduate students who became friends and who would regularly talk about EM in the context of the research we were doing and the puzzles we had about EM, and our efforts to write in a way that was compatible with what we were reading and being exposed to in seminars. And so, we never systematically worked through *Studies* as a text, or discussed it in that way, but we were interested in that text, and also with what GARFINKEL was doing in the seminars and tutorials we had with him. And we did a lot of background readings, as I said, in phenomenology, mainly, and also WITTGENSTEIN[12]. So, it was through informal groups. [8]

9 Alfred SCHÜTZ (1899-1959), Austrian/US-American sociologist, philosopher, banker and jurist, banker and jurist, was the founding father of phenomenological sociology. Major areas of work include the life-world, everyday life, social theory and methodology of social inquiries (see SCHÜTZ & LUCKMANN, 1973).

10 David WEINSTEIN was a PhD student in the School of Social Sciences at UCI in the 1970s. He worked with GARFINKEL, and drafted a dissertation on interstate truck drivers, focusing on the textual and praxiological organization of the log books that truck drivers are required to keep. Due to difficulties with GARFINKEL, WEINSTEIN did not complete the PhD, and he left academia and began a career as a wholesale broker for organic produce. He lives in Los Angeles with Nancy FULLER, who completed a PhD at Irvine in social sciences and co-authored a well-known paper with Brigitte JORDAN (see JORDAN & FULLER, 1975).

11 Alene Kiku TERASAKI was a PhD student in the School of Social Sciences at UCI in the 1970s. She was working on a dissertation on pre-announcement sequences in conversation, which was supervised by Harvey SACKS and was never completed. After leaving UCI, TERASAKI began work in the media industry. In recent years, she has been an associate faculty member in communication arts at Saddleback College, and also has worked with Home Box Office and Paramount Television as a network/studio executive (see TERASAKI, 2004 [1976] for a paper, based on her research).

12 Ludwig WITTGENSTEIN (1889-1951), philosopher, is one of the most influential philosophers in the 20th century. His ordinary language philosophy forms the bases for the linguistic turn (see WITTGENSTEIN, 2001 [1953]).

There were also other faculty members in the School of Social Sciences Irvine at the time in addition to SACKS. There was David SUDNOW[13] for a short time, until he quit his job and taught music and various other things. There also was Craig MacANDREW[14], who was co-chair of my PhD committee. I don't know if you've heard of him but he co-authored some work with GARFINKEL (MacANDREW & GARFINKEL, 1962). In the 1960s, MacANDREW was a co-author of a book called "Drunken Comportment" (MacANDREW & EDGERTON, 1969), which was about the issue of drunkenness and its relationship with behavior and the legal system. He was especially helpful for me, in carefully going over my writing in a way that suggested how it could be more intelligible outside of EM. And he had a lot of reflections about GARFINKEL, although he had not been working with him for a long time. There was another fellow who later left academia and became a lawyer, Henry BECK[15]. He also was on my committee and had general interest in EM. [9]

Dominik GERST: You described UCI as a very different university—what was so special about it? How did this contribute to the development of your work and discussions?

Michael LYNCH: At UCI, the School of Social Sciences, which had been formed about five years or seven years before I began my PhD studies, included all the social sciences, from economics to anthropology. And there were no formal departments in the School at the time. So, it was a very good environment for SACKS to do what he wanted to do with his students and for others of us, too. Very little structure: we could explore what was interesting to us. The basic requirement was to write a dissertation that was acceptable to the student's faculty committee. And at the time I found that to be very helpful. Other students found it to be too unstructured. But for me the flexibility helped, especially since I was working with GARFINKEL and POLLNER at UCLA. That allowed for flexibility to put together a program. [10]

13 David SUDNOW (1938-2007), American sociologist and first generation ethnomethodologist. Famous for his thorough research on the interactional care of dying, learning music, video gaming (see SUDNOW, 1967).

14 Craig MacANDREW (died 2015), social psychologist, received his PhD from the University of Chicago and was a professor in social science at UCI. He is known for his study about the influence of alcohol on social behavior (see MacANDREW & EDGERTON, 1969).

15 Henry BECK was an assistant professor in the School of Social Sciences, UCI, in the 1970s, who had eclectic interests in psychology, neuroscience, and ethnomethodology. He left Irvine in the late 1970s, and after receiving a law degree he practiced law in New York City (see BECK, 1972).

3. "It's Hard to Separate the Influence of the Book from the Influence of the Person"

Dominik GERST: Could you elaborate a little more on what specific influence GARFINKEL's *Studies* had on your sociological imagination or sociological questioning at that time?

Michael LYNCH: Yes, although it's hard to separate the influence of the book from the influence of the person. Because I was working with GARFINKEL at the time I was reading and re-reading the book I was trying to find in that book how to do EM; particularly in the first two chapters, "What is Ethnomethodology?" and "Studies of the Routine Grounds of Everyday Activities," where he lays out the program of EM. But he was also doing more recent work. At the time I was studying with him, he had initiated a program called "Studies of Work," later called "Studies of Work and the Professions and Sciences." And it was in connection with that program that, at Irvine, I was trying to work out methodologically and intellectually how to conduct my own study of a laboratory. And *Studies* provided basic themes such as *indexicality* and *reflexivity*. Reflexivity was of particular interest also to POLLNER. And those themes were developed in distinctive ways through CA and through POLLNER's (1987, 1991) work. [11]

One of the things that is clear about GARFINKEL, and I guess you can find it in his book, is that it inspires work—and yet it does not give you detailed guidance. And I think it's partly deliberate that he doesn't give step by step recipes for how to do what he viewed as sociology. A lot of the discussions I would have with colleagues and with GARFINKEL himself had to do with trying to work out how to do a study. In the particular research I was doing, I was going to a laboratory talking to people, observing them, recording some of their conversations, trying to gain some confidence in how they were assembling data into displays, and trying to understand their practices by repeatedly going through recordings of their conversations with each other. And it was in recording the way they were trying to work out the details of their work, that *Studies* was one resource among many; but the most immediate issue was that I was meeting with GARFINKEL and he was criticizing my work. And so, he had much more vivid and immediate contact with the work I was doing than through my reading of his texts; although, that certainly was very important. [12]

4. "GARFINKEL and I Did Not Speak to Each Other for Several Years"

René SALOMON: Your dissertation was finally published in a series with other ethnomethodological studies. Did GARFINKEL take interest in the question where and when studies are published? And was he open to suggestions and comments on his own work? Did he for example give you his texts to revise them?

Michael LYNCH: When I was writing my dissertation, GARFINKEL offered to publish it in a series with Routledge & Kegan Paul; a series that eventually included dissertations by Ken LIBERMAN[16] and Eric LIVINGSTON[17], and the edited collection "Ethnomethodological Studies of Work" (GARFINKEL, 1986). He had agreed with the Routledge editors to publish many more volumes in the series, but he never delivered his own writings to them and the arrangement ended after those three books and my book were published. My dissertation was completed in 1978/79, and was submitted to Routledge soon after that, but publication was delayed until 1985, mainly because GARFINKEL held it back until the other three books were ready. They were published in 1986. I was not happy about that, and GARFINKEL and I did not speak to each other for several years during that interval. [13]

He never gave me manuscripts of his texts to edit. He would distribute unpublished writings of his, and would discuss them, but I never attempted to revise them. With the two papers we co-authored, "The Work of a Discovering Science Construed with Materials from the Optically Discovered Pulsar" (GARFINKEL et al., 1981) and "Temporal Order in Laboratory Work" (LYNCH, LIVINGSTON & GARFINKEL, 1983), we did exchange notes and writings. GARFINKEL drafted the main argument of the 1981 Pulsar paper, and I transcribed the tape, interviewed one of the original participants, and conducted research on the history of pulsar discoveries. I also contributed some of the conceptual matters that ended up in the final paper. Eric LIVINGSTON didn't usually participate in sessions with GARFINKEL and me, but he had separate discussions with GARFINKEL. I drafted most of the 1983 paper and GARFINKEL and LIVINGSTON gave me comments on it, and they supplied some passages that I incorporated. GARFINKEL was open to some revisions of passages he wrote for both papers, but even with simple changes in phrasing or wording he would tend to revert back to his distinctive ways of saying things. [14]

René SALOMON: It seems remarkable that GARFINKEL decided not to publish a book about his approach earlier—for example to make EM more available for a larger audience. Do you know the reason why he decided to publish the book at the time he did?

16 Kenneth LIBERMAN (*1948), sociologist. Professor at the University of Oregon (1999-2009) and emeritus professor at the same university (2010-2016). His research interests include ethnomethodology, contemporary ethnic groups, intercultural communication and Tibetan philosophical practices as well as practices of reasoning in non-Western societies (see LIBERMAN, 2013).

17 Eric LIVINGSTON, sociologist. Student of GARFINKEL and senior lecturer at the school of psychology, University of New England, Australia. His research interests include central research interests lie in the study of mundane expertise, the technologies of everyday social life, and the work practices and practical reasoning of the discovering sciences (see LIVINGSTON, 2007).

Michael LYNCH: He never discussed it with me directly but I have been told, though I don't recall by whom it was, that David SUDNOW urged GARFINKEL to write it and helped him with the writing of the introductory chapters. It's possible that GARFINKEL needed the book for a promotion to full professor, but I don't really know. It is clear that several of the chapters were based on studies that he conducted years before 1967. During the time I knew him, starting in the early 1970s, GARFINKEL often proposed to publish books and series of books, but he had great difficulty with getting his writing completed. [15]

5. Writing and Talking Ethnomethodologically

Dominik GERST: Coming back to what you said about how to do a study, it reminds me of what people have described as some kind of analytic mentality that is very unique in EM (SACKS, 1992; SCHENKEIN, 1978). Could you describe what this analytic mentality consists of, what it basically is? Or in other words: what does it mean to *do* EM?

Michael LYNCH: As I'm sure you know, that's a very difficult question. I guess the most superficial way to think of that question as a beginning student was to consider how to emulate the way ethnomethodologists talked. I think for people who aren't experienced with EM, especially with GARFINKEL, both the writing and the way he talked was very confusing. And at the same time, for at least for some of us, it was very fascinating to try to develop a way of talking that was at the same time reflective of how the talking was being done and the implications of the talking. And of course, that means that the sentences sometimes get very long, very convoluted, in the sense that they're kind of circling back on themselves. It's very easy to get lost. In his seminars during the first years I couldn't take notes very easily. I'd get lost in midsentence and so my notes would break off. He sometimes would allow us to record, but not very often. [16]

So, it's something of a mentality, you can think of it as a distinctive way of talking that many people find repellent and unintelligible. Whereas for those of us who, for whatever reason, were fascinated by the depth of it; we would try to write and talk in a way that was recognizably *ethnomethodological*. I don't know if I'd call it a mentality, I don't think it can be described that way in a very coherent way, because there are lots of different ways people do EM—different schools of it if you like. But certainly, working with GARFINKEL in seminars, it was almost like being taught a language and being corrected when you didn't quite get it. [17]

Dominik GERST: So, EM is more like ...

Michael LYNCH: It's hard to describe what that is from the outside. Probably a cynic would say that it's brainwashing and people did indeed say that it's like a cult learning some specialized language (COSER, 1975). The question was: how do you learn to do that? Even if you don't figure that it's the only or best way to present what you do. And certainly, for the last 30, 35 years I tried not to write exclusively in that idiom. Partly because of my work as an editor I moved away from that language. There is a number of things you could say that is distinctive about—if you want to call it a mentality—the attitude of EM. I think it's important

to see that it begins with a distance from the social sciences. And not just quantitative methods, but many of the ambitions to be scientific and to have a kind of privileged view of the social world. Instead you try to both recover the sensibility of actions as they occur, and yet not just to treat them as *just so*; to treat them in a way that's reflective about how the actions are being done. And that's a hard thing to do. [18]

6. Fascination and Rejection

Dominik GERST: You mentioned the distance to social sciences or, to back then, contemporary social sciences: How would you describe the reception of the book back then, especially in the sociological mainstream?

Michael LYNCH: I wasn't doing EM at the time *Studies* was published. And it was only afterwards that I've read some of the book reviews by people like James COLEMAN[18] (1968), where the reaction of many mainstream sociologists was: 1. this is unintelligible, and 2. there is nothing new here. It's an interesting combination. They didn't understand it, but they didn't see it to be new. So that was pretty much the dismissal. But what I was aware of at the time was that there was a continuing combination of fascination with what GARFINKEL was up to and SACKS was up to. And also, rejection of it by many sociologists and anthropologists—but mainly it was sociologists at that time. And Lewis COSER's[19] famous presidential address "Two Methods in Search of a Substance" (COSER, 1975) which criticized both quantitative survey-analytical sociology and EM was delivered in 1975. I was there at the meeting. So, there were denunciations, but there were also a lot of younger people in my generation at the time who were quite interested in EM, including some of the very ambitious, bright students who were fascinated with lots of different topics. But they found it challenging, maybe because it was new but also because it was challenging to understand, whereas much of sociology was not very challenging. I mean you could learn the latest quantitative techniques and that was challenging in a sense, but the interpretation of the intellectual aspect of sociology didn't seem that interesting. [19]

You know, it's politically important to oppose certain structures of society, and sociology helped in that respect to some extent. But intellectually EM seemed to offer at least an initial challenge that sociology didn't seem to take up. And yet it wasn't trying to do a science—at least not in the way other social sciences tried to do it. It was grounded in phenomenology, trying to understand the world in which we live from a different vantage point, and to treat objectivity as constituted by, rather than foundational for, some version of the sciences. For people coming through the education system in the United States who studied sociology that was

18 James Samuel COLEMAN (1926-1995), sociologist, 83rd president of the American Sociological Association. Professor at Johns Hopkins University, Baltimore (1959-1973) and University of Chicago (1973-1995). Major contributions in the fields of social theory, mathematical sociology, social inequality (see COLEMAN, 1990).

19 Lewis Alfred COSER (1913-2003), sociologist, 66th president of the American Sociological Association. Professor at Brandeis University, Massachusetts (1951-1968), where he also founded the sociological department. Subsequently he was professor at the State University of New York, Stoney Brook. His research focused on structural functionalism, conflict theory and political sociology (see COSER, 1956).

quite different and—for some of them—quite intriguing. And not everybody understood it, or tried to understand it, but those who did found it impressive. [20]

7. WITTGENSTEIN and Phenomenology

Hannes KRÄMER: Do you identify central resources of GARFINKEL's thinking? You mentioned phenomenology, for instance. Which references within his work are of interest for you or for your own work? Could you elaborate a little bit on this?

Michael LYNCH: GARFINKEL didn't talk about them explicitly very often at the time I was working with him, but the basic themes of *indexicality*, *reflexivity*, *accountability*[20] were for me important to go back to and to reflect on; not only for what they mean, but also for how to address them in studies of detailed activity. And GARFINKEL at the time, like SACKS, insisted that you do studies of material phenomena produced by people in action. Not necessarily through self-reflective methods like phenomenology or philosophical reflection, but through studying other people and their language and their interactions in coherent sequences and to bring these basic themes to bear on them. [21]

To some extent, at the time I was a grad student but mostly later, I took an interest in WITTGENSTEIN's later philosophy. And not just the basic themes of *language games* and so forth, but the sensibility that WITTGENSTEIN conveys of treating the surface of action as an adequate ground for specifying and re-specifying the basic themes of philosophy in a way that kind of dissolves them (LYNCH, 1992). So that you don't come back to grand ideas but you come back to how people do reference or not. How do they provide for the coherence of what they are doing? What does it mean to speak of an intention, when it's not psychological, inside the head? I see that compatible with EM. But it's mainly through later contact with people like Jeff COULTER[21] and Wes SHARROCK[22] that I became more deeply interested in WITTGENSTEIN. [22]

20 *Indexicality*, *reflexivity*, and *accountability* are the central concepts ethnomethodology is built upon (GARFINKEL, 1967). According to GARFINKEL (p.vii) "[e]thnomethodological studies analyze everyday activities as members' methods for making those same activities visibly-rational-and-reportable-for-all-practical-purposes, i.e., 'accountable,' as organizations of commonplace everyday activities." Thus, accountability refers to the fact that social activities are *orderly*, that this orderliness is *observable* and that this observable orderliness is *oriented*, that means interactionally displayed and thus it can be acted upon. Furthermore, it is *rational* for those who are competent members and *describable* by use of ordinary language (LYNCH, 1993, pp.14f.) *Reflexivity* deals with the fact that accounts of social activities are reflexive in the sense that they are demonstrably orderly phenomena embracing the context of action in a non-mentalistic and non-causal way. Finally, *indexicality* refers to the fundamental occasionality of language use. Borrowing linguistic ideas about deictic or *indexical expressions* such as *here* or *then* which are bound to situational context (BAR-HILLEL, 1954), ethnomethodology states that all language use is indexical which sets the task to describe these indexical constraints by avoiding remedying indexicality by claiming *objective* sense.

21 Jeff COULTER, sociologist, professor emeritus of sociology and associate faculty of philosophy at Boston University, senior fellow of the Humanities Foundation at Boston University, Massachusetts, honorary senior research fellow at the University of Lancaster, England. Areas of work include ethnomethodology of practical actions, cognition and emotions (see COULTER & SHARROCK, 2007).

22 Wes SHARROCK (*1943), sociologist, professor emeritus at the University of Manchester, England and leading figure of the *Manchester School* of ethnomethodology. He is concerned with ethnomethodology, the social studies of science, language philosophy, technology and design (see SHARROCK & ANDERSON, 1986).

GARFINKEL himself had very little to say about WITTGENSTEIN during the time I had studied with him. I think he had certainly read the "Philosophical Investigations" (WITTGENSTEIN, 2001 [1953]) and beyond that, but he tended to have rather limited regard for WITTGENSTEIN, and much more for the major phenomenologists. Not just Alfred SCHÜTZ, who he draws heavily from in his early work. He maintained an interest in Aron GURWITSCH[23], whom he had personal contact with and who was kind of a tutor for him. But he basically turned to HUSSERL, HEIDEGGER, and MERLEAU-PONTY as major resources. And in my research on visualization in the sciences (LYNCH & WOOLGAR, 1988) I found it interesting to go to those sources and then find a way to understand, for example, MERLEAU-PONTY but in a different way than the way he wrote about perception. Visualization has to do with making exhibits in a laboratory or some other context, rather than thinking of perception as something that an individual subject does. And so, there is a transformation involved. [23]

8. Misreading Texts

Hannes KRÄMER: So, on the one hand you did your studies of material phenomena, and on the other hand you were engaged in theoretical endeavors. But how did you bring them both together? I find it quite interesting that you mentioned you tried to learn a certain language. Could you talk a bit more about the way you were dealing with this within the seminars and within the work context with GARFINKEL?

Michael LYNCH: That's an interesting issue, a difficult one in that GARFINKEL would warn his students away from what he would call "literary understanding" or "literary treatments" of the basic texts. Perhaps you've come across some of his writings where he speaks about "misreading" the phenomenologists (GARFINKEL, 2002, p.177). And what we would not spend much time doing in seminars was reading or interpreting texts. GARFINKEL would usually mention a theme, sometimes without mentioning that he got it originally from GURWITSCH or MERLEAU-PONTY. The idea in the seminars was to take these things, like for instance GARFINKEL would demonstrate a notion of *topical contexture* (GURWITSCH, 1964 [1957]; LYNCH, 1991). But it involved a very simple demonstration. He devised student exercises with inverting lenses—a prism mounted in a helmet you put on your head to see the scene upside down. And that's something that MERLEAU-PONTY talks about in "Phenomenology of Perception" (1962 [1945]) at great length, but GARFINKEL would derive a different lesson from the exercise. It was not always clear to me what the point was but he was treating it as a way of disrupting the fluency of everyday actions, such as writing on the blackboard. MERLEAU-PONTY was fascinated with the *gestalt* psychology issue of how the inverted field would be adapted to after a few days with the inverting lens, so that the field of view no longer seemed inverted. But what was of interest to GARFINKEL was the way wearing the inverting lens disrupted the fluency of embodied action in a field. This is what he would talk

23 Aron GURWITSCH (1901-1973), Lithuanian/US-American philosopher. Professor of philosophy at the New School for Social Research in New York (1959 till his death). Major areas of his work include Gestalt psychology, phenomenology and especially the topic of consciousness (see GURWITSCH, 1964 [1957]).

about in the seminars, and we wouldn't be extensively explicating the texts of MERLEAU-PONTY. However, reading what MERLEAU-PONTY says about the inverting lens would give me some understanding of where this theme was coming from, but GARFINKEL would not bother to give you that kind of background. And in fact, there were occasions when I would refer to MERLEAU-PONTY in discussions with GARFINKEL, but he would say that's not the understanding he was aiming for. [24]

So, it was kind of tricky. He'd pick up a point from, say, HEIDEGGER—for example, the equipmentality of the world (HEIDEGGER, 1962 [1927])—and incorporate it into ethnomethodological studies, but he wouldn't be concerned with what HEIDEGGER says about it. The important point for GARFINKEL is how the theme related to the production of actions *in situ*—in the course of witnessable activities. And the seminars never really went into much scholarly depth about the writings we used, including GARFINKEL's own writings. He instead would often use a passage from a text that seemed to touch off associations with research that he and others were doing, and then he would explicate it in the context of that research. That's what he meant by *misreading* sources. But we would not just talk about that literature like we do in many seminars I've been part of and that I've taught, where we have some readings and we talk about the readings. We didn't do that in GARFINKEL's seminars. Nor did SACKS for that matter. I mean, SACKS would tell his students that there is a background of writings that relate to what he's talking about. For example, gestures: He referred to the relatively few linguists who dealt with gestures, and he recommended that we read them, but we never went over them in a seminar. Instead, he would talk about a recorded conversation and try to unpack it. But for students both of SACKS and of GARFINKEL it was useful to know what their literary background was, while at the same time not turning EM into a scholastic, scholarly thing. [25]

9. A Tense Relationship: Ethnomethodology and Conversation Analysis

Dominik GERST: Speaking of SACKS: How would you describe the relationship of EM and language? And as the relation of EM and CA, I would say, is seen to be tense nowadays, how was it back then?

Michael LYNCH: It was tense back then, too, in a different sort of way. When I started as a student in 1972, CA—actually, it was more common to call it conversational analysis—wasn't quite so reified as it is now. But it had already developed analyses of adjacency pairs, sequential organization, turn-taking organization, and so on, of tape-recorded talk—initially audio, but at the time I started we began using video. And CA had started to diverge from GARFINKEL's EM, which was much more open-ended in terms of the kinds of actions and modes of demonstration used. I don't think it's correct to say that GARFINKEL did ethnography and SACKS did studies of language on the basis of tape-recorded talk. GARFINKEL made use of video and audio too. And I certainly did so in my own studies. It's just that we did not aim to map out what people were doing on linguistic structures or even sequential structures of talk. Instead the interest was: What is the activity in which the talk is a part, rather than what is the talk doing as an activity that seems to incorporate that it's doctors talking to patients or lawyers interrogating a witness in a courtroom trial. Instead the issue was to try to get hold of what it was as lawyer's work or what it was as doctor's work and patient's interaction, but what was involved wasn't just talking. And that became increasingly a difference between EM and CA. [26]

But at the time there were a number of different small groups of people in EM working at very different things. And they were all at odds with each other in the 1970s. Like, Aaron CICOUREL was very much at odds with GARFINKEL and he had students doing what he called "Cognitive Sociology"[24] (CICOUREL, 1974). SACKS was doing CA with JEFFERSON and SCHEGLOFF; POLLNER had his own interest in reflexivity and ethnography. At UC Santa Barbara, you had people doing more applied sorts of EM to try to integrate with sociology. And there were differences with all of them. Manchester had already started using a WITTGENSTEINian version of EM with a different interest in conceptual issues in language beyond the mechanical sides of turn-taking. So, it was quite a diversity of different, and sometimes hostile to each other, approaches. But of course, there was also some respect. Certainly, there was respect between GARFINKEL, SCHEGLOFF, JEFFERSON and SACKS. SACKS died in 1975, so he was gone before I finished my PhD, but there was a community of people at Irvine and UCLA who talked to each other. [27]

Nowadays I've been working with Douglas MACBETH[25], who was a student of GARFINKEL's when I was there, and some others in Sweden and the US—Oskar

24 See the interview with Jürgen STREECK in this issue (STREECK, KRÄMER & SALOMON, 2019).

25 Douglas MACBETH, sociologist. Faculty emeritus at the Ohio State University and member of the steering committee of the International Institute of Ethnomethodology and Conversation Analysis. Major areas of his work include workplace studies, esp. classroom interactions, epistemics, instructions and authority, qualitative research (see MACBETH, 1990).

LINDWALL[26], Jonas IVARSSON[27], Jean WONG[28], Gustav LYMER[29] and Wendy SHERMAN-HECKLER[30]—to come to terms with some of the work that is being done in CA today, particularly by John HERITAGE[31] but not just by him, which strikes us as very out of line with the CA that we remember. I think CA has very much embraced the identity as a progressive *normal* science and they've been moving closer to psycholinguistics, sociolinguistics etc. So, we've been trying to pull it in a different direction; trying to, again, keep in the foreground the analysis of action from the point of view of the actor. A very simple issue, but it's become very contentious. [28]

10. "Where Is Ethnomethodology?"

Hannes KRÄMER: How would you describe the situation today? Are there still a lot of different groups doing different kinds of EM? And is EM nowadays more of a mainstream approach and people aren't, in a way, at odds like they had been in the 1970s as you just described?

Michael LYNCH: In the 1970s it was common for people to argue. It wasn't always very friendly arguing and people were hurt, damaged and so forth by it. But particularly in CA the attitude against any kind of show of disunity seems to be stronger than it was in EM. In EM, there was a much more open-ended and argumentative environment both within the seminars and between different groups, not just in ethnomethodology but with other social sciences. I think that currently EM isn't done in very many places. I taught it for the first time as a titled course at Cornell in 2017. I had a good group of students. But they're not doing ethnomethodology in the same way that we did at UCLA or UCI years ago. They're incorporating it into studies in information science or in science and technology studies and it becomes a kind of piece of what they do. And I think that's about the best I can get here—and they're good students, respectful of all of that. There are faculty members at Cornell, and in the Department of Science & Technology Studies who are interested in ethnomethodology, again in the

26 Oskar LINDWALL, associate professor at the department of education, communication and learning at the University of Gothenburg in Sweden and deputy head of the department. His research interests encompass learning and instruction in higher education, ethnomethodology and conversation analysis, and video research (see LINDWALL & EKSTRÖM, 2012)

27 Jonas IVARSSON, professor of education at the University of Gothenburg in Sweden (since 2013). General research interests concern the role of representational technologies, specialized knowledge and competence, science education, computer gaming, and architectural education (see IVARSSON, 2010).

28 Jean WONG, linguist. Associate professor at the College of New Jersey. Her research interests include conversation analysis, second language development, multilingual interactions, telephone conversations (see WONG & WARING, 2010).

29 Gustav LYMER, lecturer at the department of education, University of Stockholm, Sweden. His research interests encompass ethnomethodology and conversation analysis, instruction and education, especially the practice of critique in architectural education (see LYMER, 2013)

30 Wendy SHERMAN HECKLER, professor of education at the Otterbein University in Ohio (since 2007). Her research interests include science learning, science and technology studies, and education (see SHERMAN, 2004).

31 John HERITAGE (*1946), sociologist. Distinguished professor of sociology at UCLA. One of the key figures of conversation analysis. Areas of his work include institutional talk, turn taking, and epistemic authority. His book "Garfinkel and Ethnomethodology" (1984) introduced EM to a wider public.

context of broader interests in science, technology, and information science. They include Trevor PINCH[32], Christine LEUENBERGER[33], and Malte ZIEWITZ[34]. [29]

But aside from CA, which is an ongoing concern—but still even there are not that many folks who are actually doing it in collective groups; UCLA has a group, obviously, with John HERITAGE, Steven CLAYMAN[35], and Tanya STIVERS,[36] and until recently Charles (Chuck) and Marjorie GOODWIN[37]. And, of course, Chuck died in 2018, and is very much missed by all of us. But there is still a group of students, there is a community there. Loughborough, England has quite a collection of folks, Paul DREW[38] as well as others. And then there are some of the people in Germany like Christian MEYER[39], and a group at the University of Siegen headed by Erhard SCHÜTTPELZ[40] and Tristan THIELMANN,[41] who are working with Anne RAWLS[42] on projects related to the GARFINKEL Archive. There are a few scholars working in Paris, such as Baudouin DUPRET[43], but

32 Trevor PINCH, sociologist. Goldwin Smith Professor of Science & Technology Studies at Cornell University. Major areas of his work include the sociology of technology, sound studies as well as markets and economy. He is widely known for his history of the solar neutrino problem (PINCH, 1986). His ethnomethodological interests are most obvious in a study of street sellers (see CLARK & PINCH, 1995).

33 Christine LEUENBERGER, sociologist. Senior lecturer in the Department of Science and Technology Studies at Cornell University. Her research includes sociology of knowledge and science, studies of the political and rhetorical significance of walls and maps (see LEUENBERGER & SCHNELL, 2010).

34 Malte ZIEWITZ is assistant professor in the Department of Science and Technology Studies at Cornell University. Mayor areas of work include STS, sociology of (e)valuation, governance and accountability relations and digital media. He incorporates insights from ethnomethodology into his work on the uses of algorithms in governance and everyday life (see ZIEWITZ, 2017).

35 Steven E. CLAYMAN, sociologist. Professor of sociology at UCLA (since 2002). His research interests are mass communication, broadcast talk, interaction in institutional settings and the integration of conversation analysis with quantitative methods (see HERITAGE & CLAYMAN, 2010).

36 Tanya STIVERS, sociologist. Professor at UCLA and vice chair of sociology at the same university (since 2014). Her research interests include conversation analysis, person reference in interaction, medical settings, race/ethnicity and cultural difference (see STIVERS, 2007).

37 See the interview with Charles GOODWIN in this issue (GOODWIN & SALOMON, 2019).

38 Paul DREW, conversation analyst. Professor at Loughborough University, England, and director of the Advanced Methods Institute at Loughborough. Major areas of his work include ordinary social interaction, interactions in institutional and workplace settings, medical communication (see DREW & COUPER-KUHLEN, 2014).

39 See the interview with Jörg BERGMANN and Christian MEYER in this issue (BERGMANN, MEYER, SALOMON & KRÄMER, 2019).

40 Erhard SCHÜTTPELZ, media scientist. Professor in the School of Media and Information at the University of Siegen, Germany (since 2005) and speaker of the Sonderforschungsbereich [*Collaborative Research Centre*] 1187 "Media of Cooperation" (since 2013). Mayor areas of work are media, social and literary theory (see SCHÜTTPELZ & THIELMANN, 2013).

41 Tristan THIELMANN, media scientist. Professor of science, technology and media studies at the University of Siegen, Germany. His current research includes projects on GARFINKEL's conception of information and its relevance for media studies today (see THIELMANN, 2012).

42 Anne Warfield RAWLS, sociologist. Professor of sociology at Bentley College in Waltham, MA, and research professor in socioinformatics at the University of Siegen, Germany. She has written extensively on GARFINKEL and ethnomethodology, and is currently directing the assembly of a large archive of GARFINKEL's writings, lectures, and recorded conversations with other prominent sociologists (see RAWLS, 2002).

43 Baudouin DUPRET, sociologist, anthropologist. Directeur de recherche at the French National Centre for Scientific Research (CNRS), guest lecturer at the University of Strasbourg, France,

others have retired or moved on to other subjects. In Manchester, England there is not much left. Wes SHARROCK is retired, he had a large group of students. So, I think EM is rather endangered. It still has a presence, and is familiar in some circles, but there aren't too many people who are picking it up as an exclusive or relatively exclusive preoccupation. Except for the ways CA is being integrated into linguistic pragmatics and sociolinguistics, there is not much left of it. And I don't see that version of CA to be emblematic of EM. So, I think it's a good question, where is EM? [30]

Hannes KRÄMER: So, where is it?

Mike LYNCH: I think it's in trouble in some ways and has been all along. There isn't much going on that's coherent. We have a couple of professional associations, and we're going to be meeting in a few weeks in Ohio[44]. But it's a relatively small group. I work in a department of STS and have been active in the Society for Social Studies of Science for many years. That's been growing, that's very viable. There is some respect for EM in that group, but it's much larger and much more vital. And, even then, it's small compared to, say, sociology or feminist studies or certain strands of psychology. So, it's small and fragile. And, you know, I think it's good to have arguments and differences because I think that enlivens the field; especially one that really is open to lots of different judgments about what it is and what it should be. To reify it into some sort of single line of work, I think, is disastrous. [31]

11. Criticisms

René SALOMON: What do you think is the biggest misunderstanding about EM and was there any critic of EM that you thought was right and that changed your own way of looking at EM? And what would be your own main criticism at EM?

Michael LYNCH: There have been many criticisms of EM over the years, and from many different angles. As I mentioned before, there also have been many versions of EM to criticize, as well as many criticisms of some versions of EM or CA by others who hold to other versions. So, it is difficult to sort all this out. I partially agree with POLLNER's criticism in "Left of Ethnomethodology" (POLLNER, 1991) that EM and CA had settled into rather uninteresting and self-satisfied trajectories, though I don't think that POLLNER's version of radical reflexivity offers a way out. LATOUR's (1996) criticisms of EM as focusing only on immediate face-to-face interaction portrays the field too narrowly, but I agree with his point that writing and other communicative technologies are far from incidental to the action. I have made a number of criticisms of CA and of particular developments in the field such as institutional talk and epistemics (LYNCH & BOGEN, 1994; LYNCH & MACBETH, 2016). As for GARFINKEL's EM, in the more recent publications such as "Ethnomethodology's Program" (GARFINKEL,

and at the Louvain University, Belgium. Former director of the Centre Jacques-Berque in Rabat, Morocco (2005-2010). His research interests include sociology of law, cultural norms, ethnomethodology, practices, media and the Middle East (see DUPRET, 2011).

44 Conference "IIEMCA 2017: A Half Century of Studies," July 10-13, 2017, Westerville, Ohio (see https://iiemca2017.wordpress.com [accessed: March 2, 2019]).

2002), he sometimes writes in such a totalistic fashion about the *worldwide social science movement* that I am left wondering what space is left for an alternative to it. And, often GARFINKEL fails to get beyond programmatic pronouncements about EM, while leaving his readers to imagine what it could possibly look like in practice. That is both a criticism and a challenge. [32]

12. Linking Ethnomethodology and Science and Technology Studies

Hannes KRÄMER: Because you previously mentioned it: What is the link between the *Studies* and STS? Or the link you would draw to your approaches within STS? Could you say a little bit more about that?

Michael LYNCH: Well, I think the initial link was with the generation in Social Studies of Science, who were largely British, who took issue with what they called the MERTONian program—basically structural functionalism.[45] And at the time, of course, EM was one of the lines of criticism against structural functionalism. And one of the very basic themes from that criticism was that the sociologists would talk about the structure of society, but they did not get down to detail. That was adopted in Science Studies in a specific way that linked up with some philosophical criticisms coming from both scientists late in their careers like Peter MEDAWAR[46] (1964) and professional historians and philosophers such as Thomas KUHN[47] (1962); where they criticized the picture of science that had been portrayed through Philosophy of Science and reflections of prominent scientists who said: What we need to look at is what scientists *do*. This is the sort of concern that motivated Michael POLANYI[48] (1966) to develop his notion of *tacit knowledge*. And that inspired a generation of people to do ethnographies of various kinds, certainly Harry COLLINS[49] (1985), Trevor PINCH (1986), Bruno

45 Robert King MERTON (1910-2003), sociologist. 47th president of the American Sociological Association in 1957. In 1943 he became professor at the Tulane University, Louisiana, and later he taught at the Columbia University, New York, where he was Giddings Professor of Sociology (1961) and later (full) special service professor (1974-1979). He is best known for his preference of middle-range theories, the "Merton thesis" which is about the link between protestant pietism and science development, and he contributed to the field of structural-functionalism, role theory as well as anomie/deviance (see MERTON, 1973).

46 Peter Brian MEDAWAR (1915-1987), biologist and founder of transplant immunology. Nobel Prize winner (1960) and famous for his popular scientific writings (see MEDAWAR, 1964).

47 Thomas Samuel KUHN (1922-1996), physicist, historian, famous for his contributions to the philosophy of science. Professor at the Princeton University, New Jersey (1964-1979). Subsequently he moved to the Massachusetts Institute of Technology (MIT) where he held the Laurance S. Rockefeller Professorship of Philosophy (until his retirement in 1991). He coined the term *paradigm shift*. Major work areas include the development of scientific knowledge (see KUHN, 1962).

48 Michael POLANYI (1891-1976), chemist, philosopher, sociologist. Director of the Fritz Haber Institute for Physical Chemistry and Electrochemistry in Germany (1923 -1933). Subsequently he moved to Manchester, England, as a chemist, and later held the chair in social science at the University of Manchester as a philosopher (1948-1958). As a chemist he was important for his analysis of the formability of crystals. As a philosopher he published texts about freedom, community, positivism and reductionism and the practice of science which had a huge impact in the philosophy of science (see POLANYI, 1966).

49 Harry M. COLLINS (*1943), sociologist. Professor of social sciences at Cardiff University in Wales and fellow of the British Academy (since 2012). Major areas of his work include science and technology studies, esp. the sociology of gravitational wave physics (see COLLINS, 2017).

LATOUR[50] (LATOUR & WOOLGAR, 1979), Karin KNORR-CETINA[51] (1981) and me (LYNCH, 1985), where the effort was to examine science in its practice. [33]

And ethnomethodology was one of the resources, not the only resource but an important one, for examining how the practices of science were put together, not just in the laboratory, but in field studies—wherever science is at issue. And so that wave of laboratory studies in the late 1970s and the 1980s was in part inspired by EM. Others came out of symbolic interaction or anthropology. But that certainly was one concrete link. And it was related to criticisms coming out of social theory, coming out of philosophy of science, that the theories of science, and theories of social science, were very abstract and never tied to what people did in their day's work. Laboratory studies or studies of scientific practice are still done in STS, although they are no longer considered cutting-edge by any means. And there is a lot of discussion about politics, reforming science; questions about race, gender and so forth, which are more in line with established sociological, anthropological and other interests. But EM was important. [34]

Hannes KRÄMER: What are the mutual topics of STS and EM and who else was and is involved?

Michael LYNCH: A particular interest of mine, and also of LATOUR, was visualization as a way of working the materials into analyzable data, quantifying and that kind of practice, which involved both literary formats and embodied action and talk. Therefore, EM was an important resource. And then finally, this came out independently, of course, by Lucy SUCHMAN (1987)[52] who was the first who treated EM as resource to do ethnographic work in context of high-tech-workplaces. And so, the whole workplace studies movement, largely sponsored by private sector-organizations like Xerox and the field of computer supported cooperative work (CSCW), was importantly involved in EM. Graham BUTTON[53], who is a good friend of mine, was very important in the Palo Alto Research Center in Europe. He worked initially in Cambridge and then Grenoble, France. Bob ANDERSON[54] also was very involved in the Cambridge Center. [35]

50 Bruno LATOUR (*1947), sociologist and philosopher, now emeritus professor associated with the médialab and the program in political arts (SPEAP) of Sciences Po Paris, France. One of the founding fathers of actor-network-theory (ANT) where he votes for a symmetric consideration of human and non-human entities in accomplishing the social. His sociology and social theory rests on his examination of microsociological approaches like ethnomethodology and French socialphilosophy. His major work areas encompass science and technology studies, materiality and agency, the nature/culture divide (see LATOUR, 2013).

51 See the interview with Karin KNORR-CETINA in this issue (KNORR-CETINA, KRÄMER & SALOMON, 2019).

52 See the interview with Lucy SUCHMAN in this issue (SUCHMAN, GERST & KRÄMER, 2019).

53 Graham BUTTON, sociologist. Lecturer at the University of Plymouth, England (1975-1992), principal scientist and area manager for the Studies of Technology, Organisation and Work group at the EuroPARC and Laboratory Director of Xerox's European research center in Grenoble, France (in 2002). Executive dean, later pro vice chancellor of the Faculty of Arts, Computing, Engineering and Sciences at Sheffield Hallam University, England (since 2005). His research interests are conversation analysis, interaction at work, action analysis, organizations and technology (see BUTTON, 2012).

54 Robert J. (Bob) ANDERSON, ethnomethodologist. Lecturer at Manchester Metropolitan University, England (1947-1989) and researcher at EuroPARC, director of the institution (1991-1999) and pro vice chancellor for research at Sheffield Hallam University, England (till

That kind of work is still going on. There are debates in CSCW about doing EM properly. There is a book that came out a couple of years ago by Graham BUTTON, Andy CRABTREE[55] and others (BUTTON, CRABTREE, ROUNCEFIELD & TOLMIE, 2015), that's about ethnography and presents those debates. So, I think there are three lines of work: One is the laboratory ethnographies; a second one investigates the idea of how data are produced from phenomena—how things become data and then how the data get digested through mediation; and then the third line is made up of the CSCW studies. [36]

13. Ethnomethodology's Take on Knowledge and Practice

Dominik GERST: You mentioned the concept of *tacit knowledge* before. Could you maybe elaborate a little bit more on the notion of knowledge within or regarding EM? Could we say that EM is some kind of sociology of knowledge?

Michael LYNCH: Yes, that's an interesting question. I've been puzzling over this recently (LYNCH & MACBETH, 2016) and will continue to do so. You know, GARFINKEL did not often talk about *knowledge* as a specific concept. Obviously, there was the documentary method of interpretation—it's a sociology of knowledge theme. But he did different things with it. I think that one of the problems with the concept of knowledge is it's too abstract from everyday practices, including professional practices in research and other such activities as well as everyday sorts of things. It needs to be differentiated pretty quickly before you can do much with it at that level. And yet it's the usual term for glossing all sorts of matters. [37]

I think that, at least as I understand it, EM is very much not a cognitive approach —especially in the mentalistic sense. Of course, cognition and knowledge are not necessarily the same term, but EM doesn't treat knowledge as an undifferentiated abstract phenomenon. Instead it picks it up in terms of modes of expression, modes of activity, actions that imply and express experience and so forth. I guess my arguments with John HERITAGE's notion of *epistemics* is in part an objection to the abstractness of the way in which the epistemic program, or whatever it is, is expressed and theorized (HERITAGE, 2012; LYNCH & WONG, 2016). I don't think I am alone in being dissatisfied with treating knowledge as a unitary phenomenon. But is ethnomethodology doing sociology of knowledge? Yes, it's doing sociology of knowledge, but very differently than it had been done before in connection with not just the pluralization of *knowledges*, but also in a way that is integrated with various actions and expressions that can be found, analyzed and examined in detail; rather than treating knowledge as a subject matter. [38]

retirement in 2010). Now CEO of the University Campus Suffolk, England. Prominent figure of the *Manchester School* of ethnomethodology. Major areas of work include ethnomethodology, philosophy of the social sciences, technology and design, management studies (see SHARROCK & ANDERSON, 1986).

55 Andy CRABTREE, ethnomethodologist. Professor at the school of computer science at the University of Nottingham, England. His research interests are ethnomethodology, ethnography, systems development and the internet of things (see CRABTREE, 2003).

Dominik GERST: That is very interesting, thank you. Could you tell us now a little bit more about the idea of practice and practice theory regarding EM? One could say that practice theory is on the rise, but when it comes to the relationship of practice, knowledge, materiality, and maybe language—it's very diffuse, but quite *en vogue*.

Michael LYNCH: I've been involved in some meetings over the years about that topic. There was the book that KNORR-CETINA and co-authors published, it is about practices and the turn to practice (SCHATZKI, KNORR-CETINA & VON SAVIGNY, 2000). That turn to practice was quite a while ago. More recently, there was a group in Nancy, France that had a series of meetings on questions of tacit knowledge and practice. Harry COLLINS, who is very much part of that group had quite a few arguments (SOLER, ZWART & CATINAUD, 2013). And I guess the ethnomethodological position—if there is one on it—is that, like I just said with knowledge, the word *practice* is a bit too general and undifferentiated for our purposes. It's not like we're doing microsociology and *practice* is too macro. It's that EM sees conceptual fields to be differentiated. When we speak of a field of practice, we beg the question: What does this field consist of? It's an abstract way of talking—when you go into the details it falls apart. [39]

Certainly, in EM there is a commitment to examining what people *do*, their actions in the course of doing things. And you could say that such actions are matters of practice; in the sense that they are not just randomly done, they are done on the basis of prior training and collectively established ways to navigate in the world. But rather than a theory of practice, I think EM treats practices of all sorts as phenomena. And then the task is to describe how practices are performed in a mundane sense, which can quickly get very complicated. But it's not a theory of how practice can solve theoretical problems, like with Pierre BOURDIEU (1977 [1972])[56]. It's not a theory of practice of this sort. Of course, BOURDIEU has produced empirical studies, but with practice as a kind of overarching term. And I think that, in EM, practice is one term among many, and we try to come to terms with actions in temporal sequence and particularly with other people and materials involved. It is not just something that is known to an individual, but something that unfolds in workplaces and everyday life and all sorts of settings. [40]

I'm not sure what exactly practice theory includes, but maybe where it relates to theory is that when you view basic terms of theoretical discussion through a description of a practice, you get a different sense of topics like *perception*. What is it to see an object when you are playing a game? This is a WITTGENSTEINian way of thinking about it. How does one see, or how does one navigate in an environment when you're driving a car? Using a GPS? You'd say these are practical understandings of environments that are coherent, intelligible, communicable, but they are tied to a specific activity in which perception, action,

56 Pierre BOURDIEU (1930-2002), one of the most influential sociologists of the 20th century. Professor at the Collège de France in Paris (since 1981) and director of studies at the École des Hautes Études en Sciences Sociales. His work covered topics like power and cultural reproduction, he coined terms such as *habitus*, *social field*, *symbolic violence* and redefined and differentiated concepts like *capital* and *class* in the framework of his "Theory of Practice" (see BOURDIEU, 1977 [1972]).

expression, all these things are done. And they don't come back to a unified theory of practice, but instead they start with practices of a conventional sort that establish what relevant *environments* could be, what *perception* could be, what *seeing* something successfully could be. So, in EM studies there is not really a *theory* of practice, but instead there is an insistence in starting out in the course of research to see what actions and what the world can involve and resolve for us. [41]

14. "I Get a Different Understanding of What GARFINKEL Is Saying Whenever I Do Reread that Book"

Hannes KRÄMER: In Germany, it seems that EM gets new attention through practice theory or through people who are concerned with praxeological questions, either coming from BOURDIEU or through LATOUR and some STS-studies as well. We can witness a rereading of ethnomethodological ideas, sometimes they are more explicit than other times. And so, this brings the question to us: Could you say that *Studies* is still a book which is up to date? Or which one should read nowadays? Or would you consider, like you said before, the best time for EM is over—and thus for the book as well?

Michael LYNCH: I still read it because it's a very deep and challenging book. Also, I reread it again and again and I'm actually going to give a talk about the GARFINKEL and SACKS paper that was written just about the time that "Studies in Ethnomethodology" was published (LYNCH, 2017), and that's the paper "Formal Structures of Practical Actions" (GARFINKEL & SACKS, 1970). What makes it worth rereading *Studies*? Well, first of all, it's hard to read. I get a different understanding of what GARFINKEL is saying whenever I do reread that book. And it's often insightful, and I get insights from it that I didn't have before, or that I've forgotten. And because it's a very deep book and probably still the best thing that GARFINKEL ever wrote—and I've seen a lot of his unpublished writings. I think this is really where he brought it to a head, that there is a lot to be fathomed in it. [42]

And obviously, for somebody who's theorizing about practice, there is material there. Or if you're doing studies, there are challenging issues to be engaged with. Obviously, it's not the last word; it wasn't for him. He kept on expressing some dissatisfaction with *Studies* and much of what he does in the book is a struggle to come to terms with some difficult issues. You know, the chapters in *Studies* were written and rewritten over a long period of time. And they are not all consistent with one another. I think the earlier chapters—"What is Ethnomethodology?," "Studies of the Routine Grounds of Everyday Activities" and "Common Sense Knowledge of Social Structures: The Documentary Method of Interpretation In Lay and Professional Fact Finding"—were the most recently written and the most coherently written in terms of what EM entails. So, I generally focus on those chapters, although I think chapter six "'Good' Organizational Reasons for 'Bad'

Clinical Records," the one he wrote with Egon BITTNER[57], was always a source of some insight. [43]

All in all, yes, I encourage rereading it. And not just because that's when EM was at its most provocative, popular or whatever, but because it's useful now. I also think that Harvey SACKS' (1992) transcribed lectures are a continuing resource. I read them frequently, and I get insights from them that I don't get from present-day CA. And it's not just to nostalgically revisit these ideas from people who are regarded as geniuses, whatever that might mean. But you get insights, that if you are doing work that is related to it in any kind of way, or if you're theorizing in a way that is related to it, you can get insights that either you've forgotten or that are hard to come to terms with. And so yes, I recommend it. But I recommend it for today, not just to remember what EM was fifty years ago. And I'd say it's not like it's up to date in every respect and I think that GARFINKEL would probably change a lot of things in it if he could, but it's a very insightful or insight-generating book and we should read it again, and again, and again alongside SACKS' lectures. [44]

15. On How to Read the *Studies*

Hannes KRÄMER: So, do you have any recommendations on reading *Studies* for people who are reading the book for the first time?

Michael LYNCH: Well, I failed when I first started reading it, because it doesn't make sense unless you've got some preparation for it. And I found reading phenomenology gave me some preparation that I didn't have before. But I know GARFINKEL recommended, and I think it's worth taking into account, that if you read what he's doing not as a sociology text in which to understand the world abstractly, but read it in light of research that you're doing, you can get insights. Even something as simple as: in the first chapters, he talks about turning his attention to his students who were coding interview responses and coding transcripts by means of what he calls *ad-hoc practices*; this is a theme that he didn't continue to pursue much after *Studies* was published. But the idea that the coders were transforming something into analyzable data categories which happens to be intelligible for a social science project is something that you can relate to in lots of different data-productive enterprises. The case can give you insights, aside from the *ad-hoc practices* gloss, which is an initial way to get a handle on it. It's very helpful. [45]

The way I read some text like *Studies*, partly because I'm a very slow reader but also because they provoke extended thought, as though in between the sentences, is that I read short passages and find in those passages some insight that I can use that I wouldn't necessarily get when reading from cover to cover. You read parts of a text, even starting in the middle of a chapter, to see what

57 Egon BITTNER (1921-2011), sociologist, close collaborator of GARFINKEL. Chair of the sociology department at Brandeis University in Massachusetts and Harry Coplan professor of social sciences. Commissioner on Accreditation for Law Enforcement Agencies (CALEA) (1981-1988). His research focused on criminal justice and especially on the role of the police (see BITTNER 1970).

sense you can make out of it and then work out that sense. It doesn't lead to a coherent understanding or explication of a text, but it enables the text to be a productive and in some ways an endless resource for doing research or thinking about researching daily life. [46]

16. "It Could Be a Manifesto for Initiating Local Revolutionary Action"

Dominik GERST: I think we have two more questions for you which we ask everyone we're doing interviews with. And the first one would be: Are *Studies* a political book? Is there a political reading possible and does it maybe change you or influence you on being a political person?

Michael LYNCH: It's a political book in reference to established sociology and I think you can generalize to other social sciences, that it's an attempt to initiate a different way of understanding social order, that is: taking actions and intelligibility from the so-called ground-up rather than treating structures as autonomous or semi-autonomous causal elements. And you could say that that's a political view not just of sociology but of action and the social world as a whole. And so, it could be a manifesto for initiating local revolutionary action. It certainly doesn't call us to revolution, except in the academic confines of sociology at the time. [47]

I've been writing some blogs lately about *post-truth* and the regime that has been visited upon us by our electoral system. Certainly, EM doesn't have a theory of *truth*, *fake news* and *junk science* and that kind of thing. But it also provides an alternative to a kind of social constructivist view that I think has run into some trouble politically as well as academically. And so, I think that EM's difference from realism and constructivism provides an interesting point of view for reflecting on the kind of confusing situation that we're living through in this country. That's a very vague way of putting it, but I think that there is a potential there and it's something that I'm preoccupied with right now. [48]

Another thing is that there are many people in STS who are very political. And they entertain the hope that their academic study would provide them—and others—with a basis for political action. My own view is that, for the most part, we don't get our politics from our academic studies. We express our political views and perhaps they inform our academic studies, but there is a lot more to life than academic perspectives. And I think it's a mistake to think that we owe our overall view of our political life-situation to academic study, and I think it would be a pity if we did. So, I think there is a lot of room left over for politics. [49]

Another thing on the politics issues is one thing I find fascinating: I was co-editor of a book with Baudouin DUPRET and Tim BERARD[58], "Law at Work" (DUPRET, LYNCH & BERARD, 2015). And Lena JAYYUSI[59] had an interesting paper about the use of video, you know, cell phone video and sometimes news footage video

58 Tim BERARD, sociologist. Associate professor of sociology, Kent State University, Ohio. Two times council member of the American Sociological Association for the Section Ethnomethodology and Conversation Analysis. His works covers topics like ethnomethodology and social theory, institutional racism, authoritarianism and crime, Institutions, groups and identity (see BERARD, 2006).

to counteract governmental and official reports that claim to exonerate, say, an aggressive act by a police officer on a suspect, or a nation for civilian deaths from a bombing (JAYYUSI, 2015). She is talking about the Gaza wars, and Israeli efforts to disavow responsibility for civilian casualties. I've been interested in police videos of police shootings of usually unarmed or barely armed African Americans. And I don't know whether they all have to do anything with this, but it relates to ongoing research by Thomas SCHEFFER,[60] Patrick WATSON[61], Michael MAIR[62] and others, who have been doing studies of video footage of war situations (KOLANOSKI, LIVIO & SCHEFFER, 2015; MAIR et al., 2013; WATSON, 2018). And that kind of material allows not only for conversation analytic engagement with details of conduct, but, as in the JAYYUSI-case, the elucidation of an *ordinary* sensibility to reports of on-the-ground damage from a war-like action, which is not an *expert* academic or governmental sensibility, to what can be seen on a videotape. I think it's a very difficult thing, but there is a possibility of explicating a *common sense* understanding of social scenes, as documented by these videos, which could be an important source of leverage in reported conflicts. But again, it's a very difficult sort of thing. Video is not fact: video is, you know, video. And it is easily discounted as we've seen in visual investigations. [50]

17. Experiments

Hannes KRÄMER: One last question we're asking everybody: Is there any part or any quotation within the book that is your favorite?

Michael LYNCH: One thing I haven't talked about that I think is worth mentioning is: GARFINKEL had a great sense of humor. And obviously the most well-known parts of *Studies* are his exercises, his so-called experiments, which he called "*aids to a sluggish imagination"* (GARFINKEL, 1967, p.38). And they are delightful to read and they end up in textbooks. It's the one part of the book that just about any educated reader can relate to or find interesting. In some ways, they're like practical jokes. They're funny, but they're also critical in some places. My favorite ones change from time to time, but there are a few of those, like the *counselor experiments*, and the one that's a more obscure one, where GARFINKEL is talking to somebody, and in the middle of the conversation

59 Lena JAYYUSI, professor of communication and media sciences at the Zayed University in Dubai. Former chair of the department of communication studies at the Cedar Crest College in Pennsylvania. Her research interests include ethnomethodology, visual studies, cultural studies, discourse studies, media, oral history and national identity (see JAYYUSI, 1984).

60 Thomas SCHEFFER, sociologist. Professor for sociology and interpretative social research at the Goethe-University in Frankfurt/M., Germany (since 2012). His research interests are ethnography, discourse analysis, the state (from a microsociological perspective) and police practices (see SCHEFFER, 2010).

61 Patrick G. WATSON, sociologist. Assistant professor for criminology at the Wilfried Laurier University in Waterloo, Canada. His research interests encompass science and technology studies, symbolic interactionism, ethnomethodology, criminological theory and state violence (see MAIR, ELSEY, WATSON & SMITH, 2013).

62 Michael MAIR, sociologist. Senior lecturer at the University of Liverpool, England in the department of sociology, social policy and criminology. His research interests include ethnomethodology and conversation analysis, politics, war, philosophy and methodology of research (see ELSEY, MAIR & KOLANOSKI, 2018).

casually mentions that he has been recording the conversation. He observes that the *subjects* treated it as a breach of a prior agreement, but there was no such agreement (GARFINKEL 1967, p.75). [51]

At the time, this is the 1950s, the idea of tape-recording somebody was not considered a possibility—GARFINKEL mentions that the hidden recorder was a wire recorder—an early recording device. So, it gives you an interesting, paradoxical sense of what the background knowledge could have been, when the expectancy or norm that is violated would not have been considered relevant. And I think that the sensibility which runs through the early chapters, where GARFINKEL is doing his exercises, is delightful and also provocative, and also provocative of some really deep thought. Like many people, I think those are the places to start. Where you go with those examples I think is quite open. I think GARFINKEL changed his views over time of what those exercises—or *experiments*—were doing, what they were showing. Some of them go back to his dissertation (GARFINKEL, 1952). So, pick your favorite ones of those, they're quite different and they don't always make the same point. [52]

Hannes KRÄMER & René SALOMON: Alright, thank you very much for your time and thoughts!

Michael LYNCH: It's been a pleasure. As I mentioned I have to give a talk at a meeting in a couple of weeks about the 50th anniversary of GARFINKEL's book and the conversation with you was really helpful.

Dominik GERST: That's great to hear. Thank you very much once again! [53]

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Authors

Michael LYNCH, born 1948 in New York, professor emeritus at the Department of Science and Technology Studies at Cornell University, Ithaca, NY. After studying sociology at the State University of New York he gained a PhD from UCI for an ethnographic study of day-to-day research practices in a neuroscience laboratory in 1978-1979. After a visiting postdoctoral fellowship at the Centre of Criminology, University of Toronto, he received a postdoctoral fellowship at UCLA. Thereafter he taught at Whitman College in Walla Walla, Washington. In 1987, he took up a position in sociology at Boston University, Massachusetts, and since 1993 he worked in the Department of Human Sciences at Brunel University in West London. In 1999 he took up a position in the Department of Science & Technology Studies at Cornell University, New York. His research interests include ethnomethodology and conversation analysis, science and technology studies, social theory and philosophy of the social sciences.

Contact:

Prof. em. Michael Lynch, PhD

Cornell University
Department of Science and Technology Studies
303 Morrill Hall, Ithaca, New York 14853, USA

Tel.: +1 (0)607 / 266 7063

E-mail: mel27@cornell.edu
URL: http://sts.cornell.edu/michael-e-lynch

Dominik GERST, born 1986 in Kassel, studied sociology and German philology at the Georg-August University in Göttingen, Germany. From 2014-2016 he received a doctoral scholarship in the post-graduate program "Perceiving and Negotiating Borders in Talk." Until September 2018 he was research associate in the research group "Border & Boundary Studies" at the Viadrina Center B/ORDERS IN MOTION. Since October 2018 he is research associate at the Institute for Communication Studies at the University Duisburg-Essen, Germany. In his dissertation project at the faculty of cultural and social studies at the Viadrina he is working on border knowledge in the German-Polish field of security. His research interests are border & boundary studies; ethnomethodology and conversation analysis, esp. membership categorization analysis; sociology of knowledge; qualitative methodology.

Contact:

Dominik Gerst, M.A.

University Duisburg-Essen
Institute for Communication Studies
Universitätsstraße 12, 45141 Essen, Germany

Tel.: +49 (0)201 / 183 3440
Fax: +49 (0)201 / 183 3129

E-mail: dominik.gerst@uni-due.de
URL: https://www.uni-due.de/kowi/instikom/dgerst.php

Hannes KRÄMER, born 1980 in Weimar, studied communication studies and social sciences at the universities of Duisburg-Essen, Germany, Maynooth, Ireland, and Bern, Switzerland. He was research associate at the excellence cluster 16 at the University Konstanz, Germany. He received his PhD in 2013 for his work on creative work at the faculty of cultural and social studies at the European-University Viadrina. From 2014-2016 he led the research project "Temporal Boundaries of the Presence" and afterwards he led the research group "Border & Boundary Studies" and was scientific coordinator at the Viadrina Center B/ORDERS IN MOTION. Since 2018 he is professor of communication in institutions and organizations at the University Duisburg-Essen. His research interests are studies of work and organization; cultural sociology; practice theory and micro-sociology; border & boundary studies; sociology of time; mobility studies; ethnography.

Contact:

Prof. Dr. Hannes Krämer

University Duisburg-Essen
Institute for Communication Studies
Universitätsstraße 12, 45141 Essen, Germany

Tel.: +49 (0)201 / 183 3540
Fax: +49.(0)201 / 183 3129

E-mail: hannes.kraemer@uni-due.de
URL: https://www.uni-due.de/kowi/instikom/hkraemer.php

René SALOMON, born 1976, member of faculty and researcher at the chair for general sociology at the Julius-Maximilians-University Würzburg. Research interests: practice and systems theories; qualitative methodology; sociology of knowledge.

Contact:

René Salomon, M.A.

University of Würzburg
Institute of Political Science and Sociology
Wittelsbacherplatz 1, 97074 Würzburg, Germany

Tel.: +49 (0)931 / 31 80083
Fax: +49 (0)931 / 31 800830

E-mail: rene.salomon@uni-wuerzburg.de
URL: https://www.politikwissenschaft.uni-wuerzburg.de/en/lehrbereiche/allgemeinesoziologie/academic-staff/rene-salomon/

Citation

Lynch, Michael; Gerst, Dominik; Krämer, Hannes & Salomon, René (2019). "The *Studies* are Probably the Best Thing That Garfinkel Ever Wrote." Michael Lynch in Conversation With Dominik Gerst, Hannes Krämer & René Salomon [53 paragraphs]. *Forum Qualitative Sozialforschung / Forum: Qualitative Social Research*, *20*(2), Art. 13, http://dx.doi.org/10.17169/fqs-20.2.3251.