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"What the World is Made Up of": The Chicago School's Alternates and Laterals in Ethnomethodology and Conversation Analysis

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Abstract
This article explores the intricate relationship between the development of ethnomethodology and conversation analysis (EM/CA) and the ethnographic traditions of the Chicago School (CS). By examining the historical and methodological intersections, the study highlights the complex and nuanced resonances between these influential sociological approaches with a focus on the ways CS writing featured in the development of EM/CA work. Drawing on archival materials, published resources, and conversations with scholars in EM/CA, the article explores the mutual influences and divergences. It discusses the foundational ideas of Harold Garfinkel and Harvey Sacks and their involvements with the CS tradition. Special attention is given to the relevance of fieldwork and the role of detail in both approaches. Our discussion examines how EM/CA emerged as a distinct and rigorous approach to studying the social, emphasizing the organized, situated and embodied practices of everyday life, and how this development intersected with CS. The article also addresses the methodological challenges and contributions of both traditions, offering a comparative account that enriches the understanding of the enduring questions of observational studies and fieldwork in the social sciences. It ends on a central commonality, the important reminder that both approaches provide for the crucial importance of fieldwork and getting out there to see what is actually going on.

Keywords Conversation analysis · Ethnography · Ethnomethodology · Fieldwork · Harold Garfinkel · Harvey Sacks

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Introduction

In this article, we explore the development of ethnomethodology and conversation analysis (EM/CA) in relation to the Chicago School (CS) ethnographies and program of field studies. Although there might be an assumed shared interest between these approaches, the relationship between the symbolic interactionist and pragmatist inspired fieldwork of the CS and the phenomenological and natural language philosophy inspired EM/CA studies of Harold Garfinkel, Harvey Sacks, and others is complex and delicate. Indeed, CS ethnographies – in their diverse forms (see Becker, 1999) – might be said to have provided both an important and rich resource of inspiration, as well as a contrastive backdrop for the development of EM/CA in the 1960s and beyond. At the same time, there are strong divergences and differences that we do not intend to gloss over.[1] We do not, however, aim to offer an appraisal of either approach but, instead, offer to shed a little light on the relationship between two key developments in the landscape of post-war American sociology. We believe that doing so offers important methodological insights into the doing of observational studies and descriptions of "what the world is made up of" (Sacks, 1992, Vol. I., p. 27).

'Close Enough' Colleagues

A recently published 10-page "Program of ethnomethodology" (Garfinkel, 2019a), written as early as 1961, ends with a list of persons Garfinkel considered to be fellow travelers in the incipient program of EM at the time; Erving Goffman, Anselm Strauss, and Eleanor Bernert Sheldon feature. Strauss attended some of the earliest EM conferences in 1962 and 1963, where Garfinkel and Sacks discussed their work with the early collaborators such as Edward Rose and Egon Bittner (see Carlin & Watson, 2019; Meehan, 2018, 2025; Mlynář, 2023). Furthermore, Howard S. Becker, along with Bernert Sheldon, actively participated in the famous Purdue Symposium in 1967 (Hill & Crittenden, 1968).[2] The importance of the Tremont Institute in San Francisco (Atkinson, 1997; Lynch, 1991) emerges as something of a rendezvous point in this context, a 'home from home' for Chicagoan visitors to California, founded by Elihu M. Gerson and organized around Strauss' studies of medical work. Current research also shows that the relationship between Garfinkel and Goffman (who saw himself to be a "Hughesian urban ethnographer": Verhoeven, 1993, p. 318) has indeed been much closer than previously known (Carlin, 2022; Rawls, 2022b). After first meeting in 1953, they were reading each other's unpublished texts and meeting to discuss them (Meyer, 2023, pp. 31–32), and in the early 1960s even working together on a book called *On Passing*, combining Garfinkel's

[1] See also: Denzin, 1969; Zimmerman & Wieder, 1970.

[2] Ethnomethodologists have also engaged with specific aspects of the later Chicago interactionism, such as the "labelling theory" (Pollner, 1978; see also Berard, 2003). This variant of CS, however, remains mostly outside of our scope here.

"Agnes" study with Goffman's writings on stigma (Rawls, 2023).[3] This rich history of mutual influences and connections between EM/CA and CS, both personal and substantive, has, however, been almost entirely overlooked. Its crucial details remain unknown and underexplored.

In what follows, we investigate this relationship beyond assumed influences and connections, charting something of the landscape in which EM/CA, as an alternate, incommensurable, and asymmetrical sociology (Garfinkel & Wieder, 1992) developed, and diverged from, the pragmatist and symbolic interactionist program of the CS fieldworkers. Although CS and EM/CA share some important intellectual origins, their divergence cannot be attributed only to distinct ways of developing them but seems to be based on deeper epistemological differences addressed throughout the paper.

Combining published resources with archival materials from the Harold Garfinkel Archive in Newburyport (MA) and the Harvey Sacks Papers collection at UCLA, we examine how these two key figures positioned their work regarding the Chicago tradition of ethnography. We also incorporate conversations with several of Garfinkel's and Sacks' students and early followers by "exploring socially shared knowledge" (Marková et al., 2007). We spoke with Jörg Bergmann, Christian Heath, Michael E. Lynch, Albert J. Meehan, and Wes Sharrock, while Andrew Carlin and Doug Macbeth responded in writing.[4] Given our own background and that of our conversation partners, we primarily consider how EM/CA may have developed through critical or appreciative orientations to CS – but we do not systematically attend to the other direction. To be clear, however, this article is not an effort to construct a relationship that suggests a direct influence of CS upon the development of EM/CA, or vice versa. Indeed, as all researchers know, non-responses to requests for an interview can also be illuminating, and we note that the response of some scholars was, simply, that there was not much to say about the relationship at all. We use these combined sources to develop a comparative account from the perspective of those working in EM/CA, discussing how this work can be seen as a development and radicalization of some of CS's foundational ideas and principles, particularly a deeply 'humanized sociology' bearing a deep respect for the things that people do in living a life together.[5]

[3] Garfinkel's contribution was eventually published as chapter 5 in *Studies* (1967) and Goffman's part as *Stigma* (1963). At the time, they were both meeting with other American sociologists to explore substantive disciplinary questions – as an example, we can mention the discussion that is available at the Garfinkel Archive in Newburyport (MA): "In a remarkable audio recording of a session with Parsons, Garfinkel, Goffman and Sacks that took place at the Suicide Prevention Center in Los Angeles during the spring of 1964, Parsons demonstrates a surprisingly nuanced understanding of what Sacks and Garfinkel were saying about situated meaning, at times even explaining their position to Goffman, when he was not following the argument" (Rawls & Turowetz, 2019, p. 11).

[4] We would like to thank our conversation partners for their time and willingness to share their recollections and ideas with us. We have invited several other scholars, but they were either not available or did not feel that they could contribute much on the subject matter of our study.

[5] In the phrase "humanized sociology", we are taking inspiration from Carlin's response to our questions, where he wrote about the "humanised anthropology" of Chicago School in contrast to "many anthropological ethnographies [that], despite the discipline's name, … are devoid of humanity, … just uninhabited, people-less".

As should be expected, we have encountered different perspectives on the relationship of CS and EM/CA, among and beyond our conversation partners. We shared and listened to many anecdotes and fond memories of first encounters with key texts and figures in the field. For instance, Wes Sharrock[6] recalled how Goffman's work was an important starting point and source of inspiration:

> I got a pass to the University Library at Manchester, where they happened to have all of Goffman's books then published in stock. So, I got to read *Asylums* and "Fun in Games" and stuff, and was just generally very, very taken with it. So, Goffman made a really big impact and just shifted me right round from where I'd been.

Jay Meehan[7] also recalled how his first encounter with *Asylums*, in a social work class at King's College (a Catholic college in Pennsylvania) resonated with his life experiences. Growing up in a juvenile detention home, not as an inmate but as a member of the family that managed it, Meehan possessed a practical knowledge of the routines and risks of such a setting: "And so I basically read Goffman and said, oh my God! This is my life. I grew up in a total institution." As such,

> … I could understand how his analysis of the daily rounds, the inmates and the keepers, all of that sort of stuff and its connection to self really resonated with me because I was an outsider.

We also heard how Strauss gave significant early impulses and was an important connection to California, and inspired studies that focus on actual social practice and work in detail:

> He took us by the hand and led us into the field, you could say... It was already much more concrete than everything else in sociology. They all, so to speak, put on gloves and then gloves over the gloves and only then started touching reality. So, taking off your gloves and get your hands dirty and actually deal with and analyze data as brutally raw as they come, that was definitely the new thing. (Jörg Bergmann[8])

[6] Wes Sharrock has been professor of sociology at the University of Manchester from 1965 until his retirement in 2017. His contributions span a variety of fields, including EM, CA, Computer-Supported Collaborative Work, Human–Computer Interaction, ordinary language philosophy, and philosophy of mind. Among his works are his early chapter "On Owning Knowledge" (Sharrock, 1974), many excellent introductions to sociology and ethnomethodology, like *The Ethnomethodologists* (1986) with Bob Anderson, and his recent joint publication with Graham Button and Michael Lynch, *Ethnomethodology, Conversation Analysis and Constructive Analysis: On Formal Structures of Practical Action* (2022).

[7] Albert Jay Meehan is emeritus professor of sociology at Oakland University in Rochester (MI). Publishing in a variety of journals in sociology, law, criminology, and psychiatry (e.g., Meehan, 1995; Meehan, 2018; Meehan & Ponder, 2002), he has extensively explored police and policing practice, including studies of record keeping, policing the mentally ill, gang statistics and policing gangs, racial profiling, the use of information technology by patrol officers, as well as engagements with the work of Egon Bittner. More recently he has collaborated with colleagues to study deadly force encounters using body-worn and in-car camera data and how video evidence is used in the trials of officer-involved shootings (e.g., Nave, Meehan & Dennis, 2024).

[8] Jörg Bergmann is emeritus professor of sociology at Bielefeld University. He produced early interpretations, translations, and applications of EM and CA in German language, studies of communicative genres, and studies of trouble and repair in interaction. Among his contributions are works such as his 1974 diploma thesis on *Harold Garfinkel's Contribution to the Foundation of Ethnomethodology* (own translation), and his 1987 study on *Discreet Indiscretions: The Social Organization of Gossip* (English translation published in 1993).

Moreover, Hughes' empirical attitude shaped some ethnomethodologists' early understandings of sociology, also through Becker's advice that Christian Heath[9] received in response to his uncertainty about which approach to choose when he was first starting his fieldwork in a medical center: "Don't worry about that; just go out and tell it how it is."

We learned about the importance of CS scholars' help in securing funding during postgraduate and early career stages. For example, Meehan underscored the crucial support that he, along with other EM/CA colleagues (see Psathas, 2008), received at Boston University from Paule Verdet, a student of Hughes who obtained her PhD in 1957 at the University of Chicago:

> In fact, actually she was the head of the graduate program and she made sure I got funding all throughout my… when I didn't work for the Center for Criminal Justice, when I needed graduate funding, she always threw funding my way because she was a big supporter.

Such reflections indicated how colleagues associated with CS sociology and EM/CA formed important departmental alliances in response to external circumstances (such as the predominantly quantitative and functionalist focus of American sociology at the time; cf. Rawls, 2018b): it can be seen as a "necessary relationship because both were considered marginal to the main quantitative thrust of American sociology," as Mike Lynch[10] put it in conversation with us. A similar point was made by Ken Liberman[11] who responded to our invitation briefly in writing, saying: "As important as they are, [CS] ideas and methods had little influence on EM; however, it contributed considerable institutional support for EM by supporting graduate students to study EM and occasionally voting for EM hires. Without them, we might not have got our foot in the door (which is where our foot is still stuck today)." Such accounts of supportive encounters and friendly relationships suggest some degree of collegial solidarity. Substantive relationships and possible influences remain to be explored, and that is precisely the aim of this text.

[9] Christian Heath is emeritus professor of Work and Organisation at King's College London. His contributions develop the idea of workplace ethnography and video studies in a broad range of areas, including medical interaction and practice, the London Underground, Computer-Supported Collaborative Work, Human–Computer Interaction, newsrooms, museums, and auctions. Among his contributions are *Technology in Action* (2000) with Paul Luff, *Video in Qualitative Research* (2010) with Jon Hindmarsh and Paul Luff, and *The Dynamics of Auction: Social Interaction and the Sale of Fine Art and Antiques* (2014).

[10] Michael Lynch is emeritus professor in science and technology studies at Cornell University. After his early work with Harold Garfinkel and Eric Livingston (1981) on "The work of a discovering science construed with materials from the optically discovered pulsar", his contributions have covered a wide range of areas, including studies of laboratory work in his 1985 *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk*, his 1993 *Scientific Practice and Ordinary Activity: Ethnomethodology and Social Studies of Science*, the exploration of public hearings and interrogation in the 1996 *The Spectacle of History: Speech, Text, and Memory at the Iran-Contra Hearings* with David Bogen, and his more recent contributions such as the 2022 *Ethnomethodology, Conversation Analysis and Constructive Analysis: On Formal Structures of Practical Action* with Graham Button and Wes Sharrock.

[11] Ken Liberman is emeritus professor of sociology and Asian studies at the University of Oregon. His contributions span from his 1985 *Understanding Interaction in Central Australia: An Ethnomethodological Study of Australian Aboriginal People*, over his 2004 *Dialectical Practice in Tibetan Philosophical Culture: An Ethnomethodological Inquiry into Formal Reasoning* and his 2013 *More Studies in Ethnomethodology* to the recent book *Tasting Coffee: An Inquiry Into Objectivity* (2022).

To repeat, the main goal of our article is not to provide a methodological assessment of the differing approaches of EM, CA and the CS but, rather, to elaborate how EM/CA can be seen as a productive 'alternate' that respecifies – i.e., somewhat relying upon for its development – the constitutive topics and approaches of CS. We also aim to show how an orientation to the socio-historical fabric and inter-institutional links illuminates core issues and methodological discussions that remain relevant in multidisciplinary, anthropological, and ethnographic research.

Sacks, Garfinkel, and the Chicago School: Influences and Divergences?

Harold Garfinkel (1917–2011) and Harvey Sacks (1935–1975) are generally considered to be the founders of EM and CA – two interrelated sociological approaches aiming to conduct endogenous studies of practical actions and practical reasoning as situated accomplishments of social order (Garfinkel & Sacks, 1970; Garfinkel, 1967, 2002, 2022a; Sacks, 1967, 1992). Although they both worked closely with many collaborators (Carlin et al., 2025), Garfinkel and Sacks are commonly credited as central figures in EM and CA (Lynch, 2019). One way to open a discussion of the "thorny nest of issues" (as Carlin[12] wrote in his response to us) of the possible resonances between EM/CA and CS is thus to look at whether, to what extent, and *how* these two groundbreaking American sociologists reflected on the field research associated with Chicago.

Rawls (2022a) notes that Garfinkel's earliest sociological orientations were informed by "the interests in interaction inspired by Znaniecki, Thomas, Cooley, and Mead" (p. 93).[13] Garfinkel encountered CS during his MA studies at the University

[12] Andrew Carlin teaches at Ulster University in Educational and Information Sciences. His contributions cover the ethnomethodological study of library and information sciences, science communication, bibliography and book history, and the history of EM, including introductions and reappraisals of the works of Edward Rose. With K. Neil Jenkings, he is co-editing the Routledge series "Directions in Ethnomethodology and Conversation Analysis".

[13] In other texts, Rawls points out that some of the early CS writings of Albion Small and Louis Wirth, by prioritizing Georg Simmel's conception of sociology, also misinterpret Durkheim by portraying society as the result of an aggregate of individual actions. EM, according to Rawls, follows Durkheim and does not subscribe to the individualist vision of society: "Durkheim agreed with Comte on the general premise that social facts are the special object of sociology, but disagreed with him on what social facts are and how they come about. Comte had emphasized the role of social values in shaping individual behavior, while Durkheim adopted a more radical view, according to which the individual herself is a social product and is therefore also a social fact." (Rawls, 2018a, p. 457, machine translation) For EM, on the contrary, 'individuals' do not have any ontological or epistemological priority but are 'social objects' whose production is to be explicated: "It is necessary to give up the idea that properties of the individual are organizing features of social practice and to focus, instead, on the orders of practice themselves." (Rawls, 2006, p. 89).

of North Carolina from 1939 to 1942. His early influences also included Talcott Parsons[14] and his students, such as Wilbert E. Moore, for whom Garfinkel conducted a study of Bastrop (TX) in 1942. Moore believed stability in modern society depended on shared norms and values, but Garfinkel found that Bastrop was never stable in this way (Garfinkel, 2008, Appendix 5). Instead, Garfinkel argued that interaction characteristics were more important for understanding the town than formal institutions. This approach aligns with CS in focusing on who is doing what with whom, where, and how, as constitutive of local social organization (Rawls, 2008, pp. 95–100).

Despite these early resonances, direct references or comments on CS are not easy to find in Garfinkel's published or unpublished work.[15] One of the rare occasions is in his early paper (Garfinkel, 1956), where Garfinkel suggests that sociological theories can be re-stated in accordance with a "praxeological rule", i.e., "any and all properties whatsoever of a social system that a sociologist might elect to study and account are to be treated as technical values which the personnel of the system achieve by their actual modes of play" (p. 191). He gives four examples, among which is also A. R. Lindesmith's theory of drug addiction:

> Lindesmith's theory of the determinants of drug addiction would be stated as follows: To increase the rate of drug addiction, teach persons receiving the drug which symptoms of distress are due to the withdrawal of the drug. (Garfinkel, 1956, p. 192)

Lindesmith received his PhD at the University of Chicago in 1937 under the direction of Herbert Blumer. The book that Garfinkel cites (*Opiate Addiction* from 1947) was also an important inspiration for Becker's early research on marihuana users in the 1950s (Plummer, 2003, p. 22). However, this does not indicate more than Garfinkel considering Lindesmith's work to be available for such a praxeological "respecification" (to use a later term; see Garfinkel, 1991) as much as other sociological theories, thus perhaps serving as a contrastive backdrop for the nascent EM.[16]

[14] Contrary to common view, Garfinkel's EM was not devised as a negative reaction towards Parsons' sociology. Garfinkel finished his PhD in 1952 under Parsons' supervision, and they worked together until the 1960s (see, e.g., Rawls & Turowetz, 2019). For example, Parsons explicitly acknowledged Garfinkel's "very able" assistance in revisiting the pattern variable scheme (Parsons, 1979/1980, p. 12). Overall, Garfinkel took Parsons very seriously – he wrote a detailed manuscript on his theoretical system (Garfinkel, 2019b), and considered *The Structure of Social Action* to be a "wonderful book" in which EM's origins are to be found (Garfinkel, 1991, p. 11). See also footnote 3 above.

[15] In conversation with us, Heath also noted that he cannot "remember a single occasion" where Garfinkel would have made a "critical comment of Chicago or the people associated with Chicago at all". In his written response to our questions, Carlin notes that "Znaniecki was an early shared interest for Garfinkel and Rose" but remains skeptical about an "influence" between CS and EM. Indeed, as noted by Heath, during his studies in Manchester (UK) there was a "really thorough discussion of the Chicago School within the undergraduate programme" coupled with a "strong emphasis to differentiate EM from CS ethnography".

[16] Garfinkel started using the term ethnomethodology in 1954 (Rawls, 2002, pp. 4–5).

Other minor clues can be found in unpublished materials. According to the correspondence available at the Garfinkel Archive in Newburyport, in 1964, Garfinkel and Bittner submitted the manuscript of "'Good' organizational reasons for 'bad' clinic reports" to the journal *Social Problems* for publication. Becker seemed to be involved in recommending it to the editors for consideration, and later also discussing the revision with Bittner.[17] However, findings of this kind – while historically significant – merely suggest collegial support, rather than substantive similarities or differences.

The only place where Garfinkel comments in a substantive way on the work of CS is a recently published interview conducted in 1980 (Wiley, 2019), suggesting an enrichment of CS's ethnographic sensibilities through phenomenological inspirations. Garfinkel compares "construction" and "production" as ways of accounting for the lived accomplishment of social order. He notes that Chicago sociologists missed the relevance of phenomenology and "didn't really catch on to how radical the notion of the natural attitude could be" (Wiley, 2019, p. 172). The writings of Husserl, Heidegger, Merleau-Ponty, and Schutz provided Garfinkel (and intermediately Sacks: see Lynch & Eisenmann, 2022) a way to gain access to the structural production details of social life that one can only get "a *sense* of" in ethnographies (Garfinkel, 2002, p. 221; original emphasis). As summarized by Lynch in conversation with us, Garfinkel was "much more enamored of phenomenology" and he may have viewed "the Chicago School as kind of phenomenology lite" which "didn't have the grip on his … intellect that phenomenology did."[18]

More concrete points of possible connections are available in the work of Harvey Sacks. Goffman was on his PhD committee, and Goffman's *œuvre* can be seen as an important source of inspiration for the development of CA (see, e.g., Clayman et al., 2022, pp. 263–265). Despite Goffman removing himself from the committee in 1966, citing the "circular reasoning" in Sacks' work (Silverman, 1998, p. 28), and making somewhat disparaging remarks regarding later CA in *Forms of Talk* (1981), there was clearly a mutual respect and a good degree of overlap in their interests in the organization of language-in-use, if not in how to pursue it (see also Goffman, 1983a; Schegloff, 1988). Sacks' archives at UCLA contain extensive notes on Goffman's books such as *Behavior in Public Places* (1963; henceforth BPP), where Sacks critically commented on aspects such as the unreflected use of language for

[17] In the end, the paper was apparently rejected by *Social Problems* and published only later as chapter 6 in *Studies* (Garfinkel, 1967).

[18] It might be the case that the roots of CS were more deeply influenced by phenomenology than Garfinkel acknowledges; however, a detailed discussion of this topic is beyond the scope of our paper. We can only note that Simmel was in touch with Husserl (Liberman, 2018), and Heidegger referenced Simmel as one of his sources of inspiration (Jalbert, 2003). Another aspect that would require a more thorough consideration is the relationship between pragmatism and phenomenology, and the impact of both of these philosophical traditions on CS and EM/CA (cf. Meier zu Verl & Meyer, 2024).

ethnographic descriptions – as Garfinkel and Sacks (1970) later put it, language was a *resource* and not a *topic* – by Goffman.[19] For example, following a quote from BPP (p. 33), Sacks notes: "one thing about Goffman is his persistent activity of choosing among the ordinary meanings of ordinary words the meaning that his use is to have, e.g., pp. 36–37; as though he can choose; that ambiguity is a property which in a way requires and certainly permits choice." Similarly, following a quote from BPP (pp. 96–98), Sacks observes: "the fact that the English language permits the use of these nouns and pronouns in the various combinations above should not lead us to imagine that an analysis of what any of them might mean, and how they be employed, sociologically speaking, has been presented; and not merely an exploitation of the language." Directly inspired by Garfinkel (as also reflected in Sacks, 1963), Sacks' unpublished early notes provide a rich and still valid critique of the way social science is ordinarily done.

Sacks' archives at UCLA also contain an intriguing grant proposal submitted in the first half of 1974, entitled "An Inquiry into the Systematics of Conversational Interaction." Goffman was principal investigator and Gail Jefferson, Marjorie and Charles Goodwin are listed as co-investigators.[20] Sacks is mentioned as a referee and as an important source of inspiration, and the grant appears to propose a combination of Goffman's and Sacks' approaches. It aimed to collect large amounts of naturalistic audio and video data, transcribe it, and to develop and consolidate a field of study dealing with "conversational interaction" on the basis of Goffman's programmatic recognition of the importance of turn-taking (Goffman, 1964, pp. 135–136) and the work later done by Sacks et al. (1974).[21] As for the research focus of the project, the proposal states: "(…) In general terms, the unremittingly counter-intuitive aspect of interaction which presents itself again and again is that conversationalists orient to procedures, and to procedure-constructional lexical and kinetic components in extraordinarily fine detail. Briefly: people do not merely all go to vote on a specific day of the year, but they inhale, say 'uh', glance at each other, produce mid-word interruptions, in equivalently massively and anonymously legislated fashion." (p. 5).

For our present purposes, these materials document the extent and rigor of engagements with Chicago sociology by Sacks and his colleagues. According to Schegloff (1992, p. xlv, fn. 30), Sacks' "fondness for ethnography crossed

[19] On the distinction between *topic* and *resource* with regard to ethnography, see also Heritage and Maynard (2022): "The point here was that interpretive sociologists' observations about roles, statuses, and other social structures, however well-documented ethnographically and set in correspondence with actual conduct, need appreciation as reflexive accomplishments rather than resources for explaining observed conduct." (p. 27) Rather than taking professional roles or institutional statuses for granted as factors explaining situated conduct, EM/CA aims to analyze how roles, statuses and other features of social structures are maintained and used by participants in producing organized social activities (see also Zimmerman & Pollner, 1970; Manning, 1970; Halkowski, 1990).

[20] We are not sure whether the proposal was funded for the proposed period of September 1974 to May 1977, but we are not aware of any outputs or publications funded by it.

[21] See Fitzgerald (2024) for an account of the development of the "turn-taking paper", and also more generally on the richness of archival resources available at the Sacks archive at UCLA.

disciplinary boundaries, and he collected original issues of the volumes produced by the founding 'Chicago School' of sociological field workers". In one of his earliest lectures in 1964, Sacks remarks how researchers at the time were "returning to detailed ethnographic work," citing Gans' *The Urban Villagers* (1962) as a notably recent effort in that vein – and we will revisit this link below. Speaking of developments in linguistic anthropology, Sacks states that his "own relation to that stuff is fairly tangential in some ways", but does acknowledge CS's ethnographic tradition as foundational, expressing his aim to move beyond it with more concrete descriptions:

> Instead of pushing aside the older ethnographic work in sociology, I would treat it as the only work worth criticizing in sociology; where criticizing is giving some dignity to something. So, for example, the relevance of the works of the Chicago sociologists is that they do contain a lot of information about this and that. And this-and-that is what the world is made up of. The difference between that work and what I'm trying to do is, I'm trying to develop a sociology where the reader has as much information as the author, and can reproduce the analysis. If you ever read a biological paper it will say, for example, "I used such-and-such which I bought at Joe's drugstore." And they tell you just what they do, and you can pick it up and see whether it holds. You can re-do the observations. Here, I'm showing my materials and others can analyze them as well, and it's much more concrete than the Chicago stuff tended to be. (Sacks, 1992, Vol. I, p. 27)

As far as we know, this section is the most explicit and extended reflection on CS in Sacks' published work. By raising the contrast between ethnography and nascent CA through the notion of 'concreteness', we also open up some of the intriguing epistemological issues discussed in the remainder of this paper. We have, therefore, used this quote in our discussion sessions with senior colleagues as a "conversational *coat hanger*" – an approach that allows a bit of knowledge to be used in conversations with expert practitioners to elicit further comments, insights, and second stories (Garfinkel, 2022a, pp. 33ff).[22]

Despite some differences in Garfinkel's and Sacks' approaches, the key point that they were both making about ethnographic studies of CS relates to treating members' practical knowledge seriously as an analytical resource, explicating it in its situated and embodied details, and demonstrating the constitutive features of social activities by concrete empirical materials that are available for repeated inspection. Both Garfinkel and Sacks were problematizing what they called "constructive analysis" (Garfinkel & Sacks, 1970; see Button et al., 2022), which creates a putative

[22] See also the editor's introduction to Garfinkel's book (Lynch, 2022, p. 7). It seems that this interviewing procedure is tied to "Rose's gloss" (Garfinkel & Sacks, 1970; cf. Carlin, 2006): "… an exercise that Professor Ed Rose sometimes uses when his host is chauffeuring him around the city, showing him the sights. Rose looks out the window and says 'My, that place has changed.' Rose searches his host's reply for what he can have been or might further be talking about. He has a way from being able to *talk* though he doesn't know what he might be talking about to learn about the city and its changes. He has never seen the place nor has he been in that city before" (Garfinkel, 1965, pp. 6–7).

replica of a worldly phenomenon through a set of established scientific methods (e.g., statistical procedures, or thematic coding), and then conducts theorizing or explanation on the scientifically constructed version of the world rather than dealing with the original phenomenon. Such a procedure "gets you two objects you're looking at, when in fact you're only inspecting one" (Garfinkel, 2021, p. 28). As Sacks wrote in an unpublished critical note on a paragraph from T. C. Schelling's *The Strategy of Conflict* (1960): "As though an ideal world were something like the real world, as though it were a subject matter. But it is not; to begin this way gives you no idea what it is like to theorize about a real world (even as a training); for here you control everything; even the sense of the term 'explanation'."[23]

As discussed above, Sacks primarily distinguished his work from CS via the availability of the materials to the reader such that they could "see for themselves" the claims that any analysis made. It is worth highlighting, however, that Sacks valued the observational grounds of CS ethnographies and their inclusion of the 'this and that' of the everyday life of the settings described. Despite Sacks' development of CA being best known for working with transcripts produced from recordings, Sacks prioritized the *availability* of social practices and phenomena to observation. Indeed, in the same 1964 lecture where he discusses CS, Sacks remarked: "The tape recorder is important, but a lot of this could be done without a tape recorder. If you think you can see it, that means we can build an observational study" (1992, Vol. I, p. 28). While not suggesting Sacks was 'doing ethnography,' it is important to highlight that his early work involved constant fieldwork, noticing the organizational character of everyday settings and developing analyses from that starting point, whether recordings were used or not (see Sacks, 1992, Vol. I, pp. 81–94). The tape recorder then allowed for capturing the "this and that" of social life, displaying an "order at all points" that was documented in transcripts, revealing details that could not be imagined (Sacks, 1984a, p. 25) or noticed through unmediated observation (Sacks, 1992, Vol. I, p. 28).

This epistemological distinction is significant for the relation between the two approaches. CS ethnographies viewed the 'culture' of a setting to be known or found out among aggregated instances, collected over time, and often triangulated with other data. Sacks and EM/CA understood 'culture' as constitutive and hence 'accountable' in all instances that reproduce the selfsame nature of the 'setting': "A culture is not … to be found only by aggregating all of its venues; it is substantially present in each of its venues" (Schegloff, 1992, p. xlvi). Sacks' analyses of cases and 'fragments' were not, then, intended to represent 'a culture' but to explicate how culture is being procedurally accomplished, in situ (Hester & Eglin, 1997), and to account for its know-ability in the first place (Sacks, 1992, Vol. I, pp. 469–471). In short, EM/CA is not concerned with a synecdochical relationship of 'micro' and 'macro'. Conversely, CS ethnographies often operate with the notion of the sociocultural 'microcosm,' where a local setting reflects and represents the broader

[23] Harvey Sacks Papers (UCLA), collection number LSC.1678, box number 8, folder "Suicide". The note was undated, but seems to be written in the early 1960s when Sacks was employed at the Suicide Prevention Center in LA.

organization of a city or 'urban life' in general; the goal of 'making the strange familiar' turns on this relation between generality and particularity, recognisability and description. Nevertheless, making 'strange' ordinary and making 'ordinary' strange are not mutually exclusive – although an analysis can likely accomplish one at a time. These points of distinction require further elaboration, for which there is no room – instead, we conclude this section with a quote from our conversation with Bergmann:

> The contrast to the Chicago School and Symbolic Interactionism was in this way the attention to details, the occupation with small episodes, in a certain way, to deal with analysis from there, which do not remain there, but from there you can come to generalizations. I actually experienced that as a difference to Symbolic Interactionism. That you can get to generalizations and theoretical statements not only via Hegel and not only via Durkheim and so on, but that you can also arrive there via the analysis and the exact analysis and reach a more structural analytical point, actually, from the materials and arrive at generalizations from there.

These issues allow us to lay out the structure of our further discussion in order to disentangle some of the substantive distinctions and laterals between EM/CA on the one hand, and CS on the other – if only as a relatively loosely defined "school of activity" (Becker, 1999, p. 10) which may serve as a "a rhetorical device, a gloss used to include and exclude" (Carlin's written response to us). As noted earlier, we do not seek here to produce a particular 'version' of the CS but to explore the forms of CS fieldwork and ethnography that were engaged with by those who developed EM/CA. We see this loosely defined body of activities as connected by the prioritization of direct observation along with different workings out of Blumer's insistence that interactionists and ethnographers should strive to describe and understand the "*empirical reality-in-process*" (Morrione, 2021, p. 39, original emphasis). In the remainder of the article, we explore some of its similarities and differences with EM/CA's "analytic mentality" (Schenkein, 1978) by focusing on the nature of 'fieldwork' and 'the field', on the character of 'data' and their 'concreteness' in 'detail', and on generalization in concepts and broader implications of individual studies. We will show that CS and EM/CA approaches share the central focus on doing fieldwork and closely engaging with the social practices investigated. However, while in CS sociology there is a tendency to approach local phenomena as instances and proxies for larger social structures, for EM/CA, a key point is that "concreteness [is not] handed over to generalities" (Garfinkel, 1991, p. 15).

Doing Fieldwork and 'the Field'

Notwithstanding the materials presented above, there appears little evidence that CS directly influenced EM/CA's advocacy of fieldwork and appreciation of empirical details. Fieldwork was, however, crucial for Garfinkel, Sacks, and later EM and CA researchers, who sought detailed descriptions of the diversity of social life. For the first generation of EM and CA, Sacks exemplified the importance of fieldwork

and ethnography. Grounded in his critical appreciation of CS and Goffman's work (as shown in Sacks' notebooks at UCLA), Sacks conducted fieldwork at the Suicide Prevention Center in Los Angeles from 1963 onwards as part of his PhD (Schegloff, 1992, p. xvi), gaining access to the center's phone calls.[24] Among the scarce literature references in Sacks' *Lectures* are works by Barker and Wright (e.g., 1951). Roger G. Barker, director of Midwest Psychological Field Station at the University of Kansas, developed 'ecological psychology' through observational studies in urban Chicago and surrounding towns. He also edited *The Stream of Behavior* (1963), which organized detailed observations of children's behaviors into a temporal sequence of episodes.[25] Such close involvement with observational materials resonates with the approach proposed by Garfinkel and Sacks (1970).

As Lynch put it, "there's a kind of inevitable relationship" between EM and CS, resulting in them being mutually significant within the broader sociological domain of 'qualitative research'. It seems that there was also an expectation that the appreciation between EM and CS might go both ways. In the 1960 s, Garfinkel gave Becker and Strauss access to his unpublished manuscripts – anecdotally, Wes Sharrock told us: "What I got from Becker and Strauss was that Garfinkel regarded them as likely recruits, … he thought they were close enough to be persuadable, in a way." Here, "close enough" can be heard in relation to the sensibility of CS fieldworkers, as briefly outlined above. A sensibility that understood studying social reality as a matter of describing interactional processes that provide for the emergence of social phenomena, rather than the theoretical construction of structures that were seen to influence peoples' behaviors within it (Blumer in Lofland, 1980). Indeed, for Blumer, "[t]his is a complete inversion of what is involved … [instead] there are people engaged in living, in having to cope with situations that arise in their experience, organising their behaviour and their conduct in the light of those situations they encounter, coming to develop all kinds of arrangements which are ongoing affairs … The metaphor I like to use is just 'lifting the veil' to see what is happening" (pp. 261–262). Note, here, that the veil is one woven by professional sociology, rather than referring to the notion that there is a 'hidden' social reality that might be accessed through any degree of theoretical or methodological sophistication (cf. Livingston, 2008). As such, fieldwork is not a methodological 'choice' as it is often portrayed in textbooks but, rather, an unavoidable form of inquiry if one wants to get close to the ways in which the world is produced. And in this regard, the CS ethnographies became emblematic. As Garfinkel proclaims, in a short recognition of CS's "analytical sociology's description": "My God! Look at all the beautiful stuff that they can now see going on, and it is just there waiting for me" (Garfinkel, 2022b, p. 145). These mutual resonances were developed by later generations of EM and CA practitioners, yet the concrete sense of 'fieldwork' and 'field' may differ in the EM/CA and CS contexts. One must be careful about assuming similarities merely through the use of the same terms. As Carlin cautioned in his response to us by

[24] The tape recordings later constituted the data for his "turn-taking paper" (Sacks, 2004; Sacks, Schegloff & Jefferson, 1974).

[25] For more on Barker's ecological psychology, see also Scott (2005).

pointing out the "randomness of the word ethnography": "'ethnography' covers a multitude of approaches, yet a textbook version of sociology lumps them together. (…) All they share in common is the word 'ethnography'." Therefore, in continuing to trace the connections and laterals of the CS and the development of EM and CA a central matter to be discussed is the notion of 'the field'.

The doing of 'fieldwork' is integral to the ethnographic dimension of EM (e.g., Sudnow, 1967; Wieder, 1974) and to the early understanding of CA as a "natural observational science" (Sacks, 1992, Vol. I, p. 803; see also Lynch & Bogen, 1994). Classically, it involves a researcher venturing into the field, often a distant location, to collect materials for later examination back home. Moreover, these 'field specimens' undergo transformation during the collection and preservation process (see also Mlynář & Rieser, 2025). As Barker and Wright (1951) noted: "… parts of the original have been altered and other parts have been lost … A pressed flower in an herbarium is not the same as a flower in bloom. It is useful for botanists nonetheless" (p. 1). But, in the end, as highlighted by Bittner (1973) in his reflections on ethnography, "for the field worker things are never naturally themselves but only *specimens* of themselves" (p. 121, original emphasis).[26]

Without reviewing the large corpus of literature on what constitutes 'the field' for CS ethnography, and also without wishing to do that literature a disservice, we use a distinction introduced by Burgess and Bogue (1964, p. 7) for the study of "natural areas". 'The field' may be, on the one hand, conceived of as a place that the ethnographer goes to – a geographical location that requires 'trips', allows for creating 'maps', and captures how society is embedded in its "spatial patterns". On the other hand, 'the field' can be seen as "cultural life" that is joined in order to document the ways in which the social is constituted in that setting or, indeed, by moving with the members, capturing the social-cultural knowledge related to a local community. Despite the shared orientation towards 'fields' in these two senses, the treatment of them might also be where CS and the development of EM/CA differ.

Field as a 'Location': Mapping Places and Following Maps

E. W. Burgess recalls that a few years after *Introduction to the Science of Sociology* (1921), he and Park began a course in Field Studies. Meanwhile, in his Social Pathology course, he had students make maps of various social problems. This led to the realization of a definite pattern and structure in the city, with many social problems being correlated (Burgess & Bogue, 1964, pp. 3–4):

> In every course I gave I am sure there were one or two students who made maps. (…) The students made maps of any data we could find in the city that could be plotted. This phase might also be called, "Discovering the Physical Pattern of the City." We were very impressed with the great differences between the various neighborhoods in the city, and one of our earliest goals

[26] See also Anderson and Sharrock (2013) for an appreciative discussion of this paper by Bittner, "Objectivity and realism in sociology".

> was to try to find a pattern to this patchwork of differences, and to "make sense of it." Mapping was the method which seemed most appropriate for such a problem. (Burgess & Bogue, 1964, pp. 5–6)

Looking for background variables such as income that would conform to the patterns of urban development is reflected on a hand drawn map which is believed to be the earliest illustration of a theoretical model developed by Burgess to explain the social organization within urban areas.[27] Goffman's treatment of the presentation of self, arising initially from fieldwork in lodgings on the Shetland Island of Unst, is a case in point (see Smith, 2022): what was found there can be seen again and again in other settings. Indeed, the earlier CS ethnographies, and the monographs mentioned by Sacks – *Plainville, Street Corner Society, The Gang, The Irish Countryman* – were exemplary of Park's well-known, reported, instruction to "go out and get the seat of your pants dirty in real research" (Becker, 1954, pp. 154–155). The value of these ethnographies is precisely in documenting the forms of life that were emerging in the various everyday settings of the rapidly changing Chicago. As such, they can be read as providing a rolling record of changes in the organization of urban life and its impact upon relationships between individuals and groups and space and mobility.[28] The ethnographic descriptions contain the stuff of everyday life of those settings and take the reader *to* them. As Doug Macbeth[29] wrote in his email response to our invitation to talk about CS:

> As for Sacks' praise, when I read it I immediately thought of 'Taxi Dancing' for obvious reasons... a thickly embodied public moral [and breathing] setting/spectacle produced between strangers who barely talked, I'd bet.

Such ethnographies were, then, the beginnings of the "peopled ethnography" championed by Gary Alan Fine (2003). By the same token, the value of novel fields, carefully observed, presents its own limitations. Recurrently, the 'local life' of the setting is understood through the concerns of sociology, often at the expense of the detail of the production of the organizational forms found there. Indeed, Fine's subtitle is "developing theory from group life", a matter we will turn to below. As Macbeth (in his response to our invitation) continued:

> But the place was striking more than the treatment. Cressey's interests were pretty normative, available to the same critique of Becker's piece on jazz musicians. In fact, the normativity, taken to new, ignored and not quite usual places, seems to be the thread that holds a lot of the sense of the phrase 'Chicago

[27] See Burgess' hand-drawn map as an illustration of his theoretical model of urban zones, in *Mapping the Young Metropolis, The Chicago School of Sociology, 1915–1940*: https://www.lib.uchicago.edu/collex/exhibits/mapping-young-metropolis/mapping-chicago/

[28] In the study of urban life, early CS sociology took much inspiration from the work of Georg Simmel (see Tomasi, 1998).

[29] Douglas Macbeth is emeritus professor of education at The Ohio State University. His contributions include analyses of 'detail' in ethnomethodological studies (Macbeth, 2022a, 2022b) and his extensive studies of classroom order, interaction and understanding. His recent work includes engagements with the 'epistemic analytic framework' (Lynch & Macbeth, 2016) and reading Sacks' *Lectures* (1992).

School' together, from the 30s into early Goffman. The setting is what holds our attention.

Rather than enriching the *comédie humaine* (Hughes, 1970) and hence theory through re-discovering novel and unknown social patterns of different urban settings in a changing society, EM sought to flip the question to uncover the shared methodic basis through which "immortal ordinary society" (Garfinkel, 1991) can be re-discovered in different settings, again and again, by engaging in a range of tutorial exercises, including, for example, the production of a phenomenal field through the work of finding one's way, using informal maps drawn in the reader-users' presence (Psathas, 1979). These "occasioned maps", though sketchy, are understood through the audio-visual practices accompanying their production: "Their study requires audio- or videotaped recordings of the occasions on which they are produced since the visible-on-paper product is not understandable apart from the situated occasion of its production" (Psathas, 1979, p. 205). Garfinkel identified "traveling's practices in … embodied details of those practices. The map's properties of order* are exhibited in and as directly and unmediatedly observed territorial organizational things" (Garfinkel, 2002, p. 130). Both Psathas and Garfinkel grounded their work in phenomenology, respecified for sociological studies. According to Garfinkel, in their orientation to social construction, CS and symbolic interactionism missed the ordinary production of the lived social world and familiar practices. As pointed out by Bittner (1973), the spirit of CS urban ethnographies is indeed tied to a "realism" and "total immersion in the life studied" (p. 117), but is nevertheless always a description of otherness: "The paramount fact about the reality bounded by an ethnographic field work project is that it is not the field worker's own, actual life situation" (p. 121). This brings us closer to the second sense of 'field' we outlined above – when conceived as a community and its culture.

Field as a 'Culture': Becoming a Member and Reflexive Membership

For CS, reporting how the social is constituted by a set of individuals is generated through becoming a member and immersing the ethnographer into social situations and their ecologies. As Goffman (1989) advised fieldworkers in a discussion panel contribution in 1974:

> [Participant observation is a technique] of getting data, it seems to me, by subjecting yourself, your own body and your own personality, and your own social situation, to the set of contingencies that play upon a set of individuals, so that you can physically and ecologically penetrate their circle of response to their social situation, or their work situation, or their ethnic situation, or whatever. So that you are close to them while they are responding to what life does to them. (p. 125)

To subject 'oneself', the fieldworker, to the culture in and of the field, one has to become a member to the extent that their traits as a person, body and personality have to be opened up for alteration by the field's culture. Goffman sees no place for compromise or reservation, though there may be room for imperfection:

> People don't like to cut their hair, for example, so they retain something of their own self, which is nonsense. On the other hand, some people try to mimic the accents of the people they're studying. People don't like to have their accents mimicked. So you have to get a mix of changing costume, which the natives will accept as a reasonable thing, that isn't complete mimicry on the one hand, and that isn't completely retaining your own identity either. (p. 128)

The experienced sociocultural or class-related otherness by subjecting themselves to the others' identity provides CS ethnographers a methodological estrangement to report about marginalized, unusual, and hardly accessible urban communities or spaces. Although it is a policy for an EM researcher to gain the adequate cultural competence in the field (Garfinkel & Wieder, 1992), that adequacy is not to warrant the researcher's description as *the* local group members', but to embed the EM inquiry into a local culture and a setting in vivo. The ethnomethodologist achieves a cultural membership to produce a reflexive description that manifests the practical and natural language competencies which staff witnessable naturally-organized activities.

In this sense, notions such as 'individual', 'group', and 'society' live in their members' competence of reproducing and recognizing yet another perspicuous setting (Garfinkel, 2002) in the in-course-ness of any given phenomenally-available organization. An instructive case is Carlin's (2023) discussion of his UK fieldwork with a *Big Issue* seller, Tommy, in studying the self-explicating and self-replicating order of "pavement cultures" and the visibilities of urban poverty (p. 142). The 'field' may be a setting – Carlin did *go* to a place in order to make his observation – but the 'field' explicated in the study incorporated Tommy's area knowledge (Bittner, 1965/2013) and his methods for seeing and accomplishing the order of the pavement as relevant for his work of selling the magazine. In this 'praxeological ethnography', then, practice, method, place, and knowledge are approached as organized moment-by-moment, as a gestalt contexture. Entering the field is more of a practical apprenticeship; leaving it, or not, is a matter which remains tied to the competencies gained which themselves reveal 'the field' in the first instance. Scientific experiments (Lynch, 1985); pick-up basketball (Macbeth, 2022b); playing checkers (Livingston, 2008); changing a truck tyre (Baccus, 1986); the debates of Tibetan monks (Liberman, 2004); and, of course, queuing (Garfinkel & Livingston, 2003) or an ordinary conversation in which a story is told (Sacks, 1984b), are emblematic and productive field sites for EM and CA.

Rather than spatially locating 'otherness', the approach of EM/CA seems to be closer to a self-estrangement (in contrast to CS's policy of 'making strange familiar'), based on learning to see one's own society and culture 'differently.' Garfinkel's breaching exercises were designed to do precisely that, invoking the seeing of the 'ordinary' as 'strange' (see Sormani & Vom Lehn, 2023). In Sacks' parallel project, the estrangement is achieved by repeated listening to the recording and its transformation into a textual object, the CA transcript (Mlynář & Rieser, 2025). As Baccus (1985) formulated it, "[t]o study ethnomethodology is to change the way you think about the world" (p. vi). This brings us directly to the theme of teaching – an intriguing point of intersection between CS and EM/

CA, often highlighted in our conversations with senior colleagues. In addition to the utility of CS studies for introducing the sensitivities required for teaching EM/CA to undergraduate students, as related to us both by Lynch and Sharrock, there might be some more fundamental intersections.

Regardless of differences in conceptualizations of 'the field', fieldwork was central to the trade as well as the pedagogy of EM and CA and remains so in current practice. In their teaching, Hughes, Garfinkel, and Sacks seem to be alike in focusing on working with their students, and much of their transformative influence then comes out in studies written by their students, related to how researchers work with each other to learn the craft of fieldwork and analysis. Teaching, in this regard, has to do with in situ observations of social life – as Heath put it in our conversation: "to teach students how to do this stuff is to get them out in the field as soon as possible and work with them on the stuff that they do". Heath (1984) highlights Hughes' paper titled "Teaching as Fieldwork", which is a plea for learning about occupational culture with and from students, developing a sense for and focus on what kind of actual work goes on in interaction. One example from a student is how sensitive one has to be in fitting coats on men (Hughes, 1970, p. 15).

As Heath also pointed out, there was certainly a preoccupation for Hughes in encouraging students to do fieldwork, as well in his other works like *Men and their Work* (1958) or *The Sociological Eye* (1971), which was described by Carlin in his response to us as, for him, "the single most important text", a "*primus inter pares*". In our conversation, Heath concluded his observations on this theme in this way: "Here, in our PhD programs, they seem to normally want students to spend a year reading around the topic before they go into the field. And I think we managed to get ours out in the field within six weeks, they also have to do some video-recording so they have preliminary materials to work on. Whatever it is, it almost doesn't matter as long as they're going out to do some fieldwork, to be quite frank." Such an early exposure to fieldwork, central also in Garfinkel's (2002) insistence on what he called "tutorial exercises" and "propaedeutic cases", develops a detailed orientation towards the produced structures of action and concreteness of social objects that tend to be glossed over in ethnographic descriptions and narratives.

Even when the focus of analysis is on video materials and detailed transcriptions thereof, a good deal of fieldwork still stands behind both the organization of the camera, and the organization of understanding the practices recorded (see Mondada et al., 2024). This fieldwork is essential in accessing "witnessable phenomena that are not available to recording" (Carlin, 2023, p. 149). Indeed, as Meehan put it in our conversation, *being there* is important, as "there is something about the rhythm, the other sensual aspects to understanding settings" and "the video data and audio data can only take you so far". This praxeological apprenticeship in the local accomplishment of the phenomenal field is fundamental. As Heath remarked: "once you go into these specialized workplaces … fieldwork, ethnography, is absolutely fundamental to doing work. … if you go into these complex environments, like an operating theater or a control room, how do you even decide what to film without knowing something about what's going on?" Hence, 'fieldwork' is indispensable for all stages of EM/CA inquiries – preparation, capturing, and analysis – in order to

gain familiarity with social activities in recovering the "discoverable phenomena of order" (Garfinkel & Wieder, 1992, p. 181).

Data, Concreteness, and Detail

One aspect of the 'familiarity' of social activities that EM and CA highlight is that orderly actions are often tied to routine selections among multiple possibilities. This usually goes unnoticed, but unexpected actions can upset or confuse people (Garfinkel, 1963; Watson, 2009). What we call 'moral' is built into our actions and words, and these social aspects are found in naturally occurring data. One way to develop an empirical focus in EM is to start doing CA, as Meehan did in the 1970s with Anita Pomerantz, one of Sacks' students, as he recalled when talking with us:

> Anita walked in and said: "This is a class in personality and social structure. And basically my approach is whatever personality might be and whatever social structure might be, I reckon that we would likely find this in data. And so what we're gonna do this semester is look at data." And this was music to my ears because we were actually systematically looking at conversational data. She didn't assign Harvey [Sacks]'s work to read.... We hardly read anything. We just basically did data sessions that whole term. [...] So what happened there is that Anita just kind of introduced me to the world of conversation analysis by *doing* conversation analysis. [...] And it really appealed to me. … I liked the way it was grounded in observations, the observation of what people are actually doing.

We also heard of a similar episode involving Pomerantz, from about the same time, in our conversation with Bergmann:

> It was a seminar by Anita Pomerantz, where maybe there were 15 participants and we had a transcript and a video recording by Chuck Goodwin, who was just about to finish his dissertation. And then it was decided that we didn't want to watch the video, but rather start only with audio and transcript and so we spent an entire term on this audio and transcript. At the end of the term, we just came to the end of the first page of the transcript. … So, the intensity of the analysis, where it wasn't just about fantasizing something but that was actually driven by analysis, that was also mind blowing for me, I would say. … In contrast to Chicago School and symbolic interactionism, where in this way the attention to details, the occupation with small episodes, in a certain way, to deal with analysis from there, which do not remain there, but from there you can come to generalizations.

As the final part of this quote suggests, despite the appreciation of close attention to recorded data, it is also clear that transcribing audio or video for analysis can only take one so far. This does not only have to do with aiming "to cast your local stuff in general forms" (as put by Sharrock in our conversation), but also with other sensibilities regarding the achieved character of the studied environments. Being able

to blend in and spend a lot of time with the practitioners, such as police officers in Meehan's case, does not only give a sense of what is going on, but also creates opportunities for what Bittner called "spilling the beans" – when members open up to the investigator and talk about what is ordinarily not talked about. To return to our conversation with Meehan:

> … that kind of captures how I see where ethnography is needed, where the video data and audio data can only take you so far … particularly the work of getting involved in being on the street, being in the rhythm of a shift, riding a whole shift, going in and out, preparing beforehand, understanding what it takes to do that, and by the way, showing up not just when it's convenient for you, but you know, showing up night after night after night, go work those four nights, go work the midnight shift, show up there, and you know what, like Egon [Bittner] said, they're going to spill the beans because they can't hide … from the observer.

Uncompromising immersion in fieldwork, as described by Meehan, enables researchers to be "taken seriously" by practitioners (see Garfinkel, 2022a) and to understand the structure of actual events. Sacks noted: "If you can't deal with *the actual detail of actual events* then you can't have a science of social life" (Sacks, 1992, Vol. II, p. 26; emphasis added). We suggested that Sacks found reflexive structures in the concreteness of "actual detail of actual events" lacking in the already-aggregated events of CS ethnographies. In our discussion with Heath, this aspect became central: Chicago work often focuses on "elaborating … conceptual frameworks" like license, mandate, or career in institutional settings, while EM/CA focuses on "details of the structure of the activities … within those settings and within those environments." To explore this potential tension between CS and EM/CA, we turn to the notion of 'detail' and its relation to competent membership.

'Detail' in, as, and of 'Data'

In a chapter on Sacks in a recent handbook on Goffman, Hoey and Rawls (2022) note that the main difference between Sacks and Goffman is "the degree to which they treat social objects made in interaction as fragile" (p. 377). For CS, social order rests on shared and fixed, albeit adaptable, symbolic meanings. By contrast, "for Sacks and Garfinkel," write Hoey and Rawls (2022), "all words, gestures, and actions are fragile, indexical, and must be placed in sequences of action in an orderly way – to acquire meaning" (p. 377). When we talked with Lynch, he also noted that by the 1970 s, Garfinkel's "suspicion of signed objects" was related to the hardening of "differences with Goffman and with the whole Chicago School tradition and beyond Chicago School", "differentiating from the emphasis on signs and symbols and symbolic interaction" and from the vision of sociology as being "all about interpretation."

Heritage and Maynard (2022) write in a similar direction:

> Chicago School and related ethnographies elevated meanings and understandings to an abstracted conceptual realm by way of roles, rules, statuses, and

> other terms as the means for analyzing patterns in everyday life. Contrastively, [in EM/CA], there is a member-produced, *organized coherence in the details of the practices by which participants make a setting's features visible and available to one another* as the taken-for-granted structures, including roles, rules, and the rest, which they confront as externalities. (p. 28; emphasis added)

The notion of *detail* is key, as is how 'data' – such as fieldnotes, interviews, photographs, or recordings – may provide access to the concreteness of lived social activities. As Lynch pointed out in our conversation, Sacks was "following an intuition that there was order in detail that both linguists discarded as production … and the sociologists [were] figuring out that if you get into more detail it just gets into being a mess." Importantly, the detail of orderly social action cannot be imagined or remembered, it can only be discovered or recovered by doing praxeological studies. The relevance of detail is established by its constitutive character in the given courses of situated action. The ultimate (and often elusive) aim of EM/CA studies is to "pursue the social by looking deeper and deeper into *the observable details* of the activities in which people are engaged" (Livingston, 2024, p. 20, emphasis added). Such constitutive detail is not avoidable or reducible and cannot be theoretically conjured; it can only be encountered in actual doings.[30]

Macbeth (2022a) notes that "familiar notions of 'detail' tend to stand on behalf of an additive or pointillist conceptualization of an image progressively revealed" (p. 89): the traditional and everyday notions of detail take it to be something 'small', out of which 'larger' things are assembled. This is also the case of contemporary CA, as Macbeth argues, where detail means expanding the technical accuracy of the transcript (cf. Clift & Raymond, 2018). It presupposes an external objectivity of CA's 'data', and the criteria of an appropriate level of detail or analytical precision might become detached from the lived production of the examined phenomena. For EM/CA, however, details are not necessarily 'small', and they are not emblematic of a pursuit in 'micro-sociology'.[31] EM's details are not countable in the sense of having 'many details', or providing 'too much' (or 'not enough') of them – they cannot "be rendered as a list of attributes" (Macbeth, 2022b, p. 63). Missing detail does not leave negligible gaps in a picture painted in broad strokes, but it means losing the phenomenon at its very core. Detail in EM/CA is related to the work of glossing

[30] "Orderings of ordinary activities are unimaginably extensive phenomena; they are essentially *other* than we do imagine or could ever imagine them to be, and they await discovery" (Garfinkel, 2022b, p. 155, original emphasis; see also Maynard, 2012; Ferencz-Flatz, 2023).

[31] It has to be noted here, in order to avoid some common misunderstandings, that EM/CA is not a variant of 'micro-sociology'. EM/CA does not take for granted that there is an immanent contrast between 'micro-' and 'macro-level' social order, or a need for establishing a separate domain of a 'meso-level' order (such as "interaction order" in Goffman's [1983b] sense). Contrastively, EM/CA moves beyond the duality of micro and macro (see, e.g., Schegloff, 1987; Hilbert, 1990; Coulter, 1996) and it is indifferent towards its disciplinary relevance – or, to put it more precisely, it acknowledges it as relevant as long as it features as a relevant distinction in members' own practical reasoning and practical activities.

and *etcetera* – any description can always be expanded, and more can always be added.[32] But for what practical purpose? For EM, this has to do with the routine recognizability of social objects in their produced accomplishment. Detail for EM then might be a matter of "adequate looking" rather than the granularity of reality. Garfinkel notes that in conventional social sciences, phenomenal detail is inevitably "reduced by being rendered as the properties of signed objects" (Garfinkel, 2022a, p. 74). EM/CA's task is not to explain or clarify everyday phenomena but to lay them out as known by skilled practitioners, remaining faithful to "the lived orderliness of practical action" (Garfinkel, 2022a, p. 71). In a 1962 archival recording available at the Garfinkel Archive in Newburyport (MA), Harvey Sacks says: "I don't take it that my job is to clarify. My job is to show how it is that it's clear." In this regard, EM and CA are not motivated by the aspiration to 'uncover' some hidden aspects of social phenomena, but to undertake the recovery of what is already known – but is "'known' in the form of competent mastery of practical affairs – to the members of society" (Sharrock, 2001, p. 258; see also Livingston, 2008). The 'concreteness' of recovered detail, in this sense, is related to the reproducibility of the phenomena: as Meehan put it in conversation with us, EM/CA aims to be "descriptive enough to preserve enough of the detail such that … you could describe, reproduce, what is the practice under examination". And the detail is already naturally available to members in "the of-course-ness of things", to use a phrase that Meehan attributed to Garfinkel. For EM, the familiarity of action is the subject matter, in the "developing detail*" of organizational things – while "ensembles in detail* are the makings of such things, and all things of action, meaning and structure" (Macbeth, 2022b, p. 61).

Empirical 'Concreteness' as Membership

Let us now ask a question that moves closer to the discussion of 'data' and their adequate analyzability. How can details of practical actions and reasoning be accessible in empirical settings? So far, we have been considering the 'concreteness' – that Sacks may have lacked in ethnographies of CS – as tied to descriptive constitutive detail of social activities. In his response to our questions, however, Carlin proposed an alternative – Garfinkelian – reading:

> I have always taken Sacks' point as highlighting that however careful the observation, however sensitive the ethnographer's descriptions of people's activities, however apposite descriptions might be (writing style, sentence construction, word selection, etc.) – each and every written description is a gloss. It cannot provide the procedural detail, *how* something was done. That requires a methodology (*not a method*) – a logic for the preservation of people's natural

[32] The notion of *etcetera* was central in early discussions of EM by Garfinkel and Sacks. It aims to capture that adequate description is not a consequence of precision or eliminating indexicality, but one that explicates the grounds of competent membership that are "known-or-knowable-in-common", "without-respect-for-the-requirement-of-specific-explication" (Garfinkel, 1962, p. 6; see Eisenmann et al., 2024b, pp. 2727–2728).

> language activities, whether these activities require specialised skill and craft competencies or the membership competences owned by people in society. It also requires the analyst to share these competences in order to recognise them.

The requirement for analysts to share members' competences to recognize and explicate them leads to the *unique adequacy requirement of methods* in EM/CA research (Garfinkel & Wieder, 1992; Hofstetter, 2024; Liberman, 1999; Smith, 2024). In the context of this article, the unique adequacy requirement is mainly relevant with regard to the issues with the form of sociological description that Garfinkel called "ethnographic reportage." Garfinkel's dissatisfactions with "just so stories" provide a further point of comparison in the relationship of CS and EM/CA.[33] He criticized conventional sociology for the "missing what" – the lack of detailed accounts of actual working practices. Garfinkel would often explain the "missing what" by referring to "the Howard Becker phenomenon": "According to Garfinkel, by reading Becker's studies of jazz musicians, we learn a lot about where jazz musicians work, what they earn, who they work with, and so on. However, we do not learn how they actually play their music, alone or together. The depiction of the work itself is missing from Becker's account, indeed all sociological studies of work…" (Greiffenhagen & Sharrock, 2019, p. 256). Becker's (1951) study was treated by Garfinkel as a continuation of Hughes's work (Garfinkel, 2022b, pp. 143–145).[34]

In a discussion that echoes Sacks' critical notes on Goffman's *Behavior in Public Places* (see above), Garfinkel (2022b) points out that "[a]lthough ethnography can deal with the missing what, it does so in the peculiar fashion in which persons, who know what the ethnographer is talking about, can hear him talking about just *that*, and can talk about the matter in a similar manner." (p. 149) However, studies of EM/CA's phenomena that are "grounded in ethnography are essentially unable to encounter, address, formulate, or settle issues of structure." (p. 150) In a series of manuscripts recently published as *Studies of Work in the Sciences* (Garfinkel, 2022a), the tension between EM and ethnography became central. Garfinkel writes

[33] Ethnography has a controversial standing in EM/CA that has been subject to numerous discussions (e.g., Sharrock & Anderson, 1982; Wowk, 1984; Moerman, 1988; Pollner & Emerson, 2001; more recently, Eisenmann et al., 2024a, and the special issue they introduce). However, most scholars seem to agree that ethnographic fieldwork of some kind is a necessary prerequisite for ethnomethodological studies.

[34] As an EM alternate that accounts for the "missing what" (see also Button et al., 2015), Garfinkel would cite Sudnow's studies of playing jazz piano (Sudnow, 1978, 1979, 2001), where he "made use of his own competence as a jazz pianist to systematically unravel and describe the embodied ways in which he accomplished the work of jazz improvisation" (Tolmie, Benford & Rouncefield, 2013, p. 229). In our conversations with senior colleagues, the phrase "Howard Becker phenomenon" has been recurrently seen as somewhat "cruel" (by Heath and Lynch). Criticizing the phrase, Carlin pointed out that "Becker's project was different than Sudnow's project", but is unnecessarily evaluated in terms set by EM, condensed to a "shabby epithet". Moreover, Heath remarked that he does not recall such an account being given by Sudnow himself as a motivation for his studies, though Garfinkel (in an unpublished manuscript) attributed the phrase to Sudnow. There seems to be no evidence that Sudnow developed his study as a direct response to Becker's work.

that the "work-site practices" – the phenomena of EM/CA's interest – are "'hidden' in and as their apt and familiar efficacy", and that they are:

> … only available to practitioners in and as of embodied work-site equipmentally-affiliated 'skills.' […] In any actual case they are unavailable to 'situationally' disengaged, let alone to *a priori*, analytically reasoned reflection. Nor in any actual case are they available to introspection, to ethnographic reportage, to the analysis of ethnographic documentation or to documented arguments except, and at best, as documented conjectures. They are done in detail* and they consist of what detail* could be in technical, material contents. (p. 88)

In a 1980 seminar discussion, published in the same volume (edited by Lynch), Garfinkel specifies what is the issue with ethnographies: "… there are the resources of ethnography with which then to propose some stories of how the work was done. Trouble? You have the story, though it will not respond under any interrogation that you can imagine to the task of formulating and solving as a matter of structure what the … practice consisted of as a production." (p. 109) The "body of competent practice", as put by Macbeth (2022b), "needs to be your own to find *in* those competent affairs the sense of EM's regard for *detail**" (p. 61).

What, then, under such constraints, is EM/CA's 'data'? One upshot of the preceding discussion seems to be that rather than following pre-established and reified methodological procedures for 'data collection' – making videos, transcripts, fieldnotes, etc. – it is crucial to locate the identifying features in and of the phenomenal field. What counts as 'data' is always setting- and description-specific (Schwartz, 1979/2002), consisting of materials that serve as "elaborate reminders of mundane phenomena" (Coulter, 1983, p. 374) and later as an inseparable part of their analytic reconstruction. However, as we will see in the next section, ethnographic work such as the one cultivated by CS traditions can perhaps also become a springboard to a detailed praxeological analysis.

"Everyone Gets a Chance to Talk": Ethnographic Observations and Issues of Structure

The most direct view we have of Sacks analytically operationalizing his respectful criticism of CS ethnographies is a lecture delivered in the Fall of 1967 entitled "Everyone gets a chance to talk". At the heart of the discussion – which is yet another example of Sacks' broad and deep reading of anthropology and ethnographic studies – is a consideration of a specific claim about a marker of community offered by Herbert Gans in the classic *The Urban Villagers* (1962; henceforth TUV); that, in the course of an evening where the group had gathered, "everyone gets a chance to talk". We recount the central observations of this lecture here to demonstrate and exemplify the sorts of productive problems that Sacks was able to draw out of ethnographic descriptions and, indeed, his unique orientation to questions of "what the world is made up of".

Sacks' lecture (1992, Vol. I, pp. 701–710) concerns a two-page excerpt from TUV – supplied to students ahead of the class – describing a regular and informal gathering of a peer-group in the "kitchens and living rooms of the West End apartments." Gans' report remarks on the general organization of the gatherings; on the "separation of the sexes" in different rooms or ends of the same table, the permissible topics covered by the group, and their function in community life. The talk, writes Gans, primarily features shared experiences and topics of interest: celebrations, births, and bereavements, and current events, from which flows further talk about similar events in the past that "drift(s) into talk about the good old days." In a manner echoed in Duneier's *Slims' Table* (1992), the talk also features "reports – and judgments – of deviant behaviour", the giving of advice, and the discussion of "problems common to the group." Certain topics are said to be off limits; Gans notes surprise at what he conceived of as the "most pressing problem – redevelopment and relocation – received relatively little attention." The conversation is dominated by gossip and reports about people connected to the group who are not present, a genre of talk examined by Bergmann (1993).

By way of analysis, Gans goes on to describe the "functions" of the exchange of news, as a means for members to keep up-to-date not only on the news itself, but on the current and potential membership of the group. As well as "entertainment," the talk is said to provide for "social control" whilst also introducing "new ideas that might be useful to the group", and, in particular, the utility of "experts in a culture that suspects or rejects the professional expert provided by the outside world." The flow of gossip and information is seen to "hold the network of peer groups together" and "do without formal community organizations." A key organizing principle of the talk is that "while there are people who dominate the conversation, and others who contribute little, *there is generally an opportunity during the long evening for everyone to talk*, either to tell a story or to deliver an opinion" (emphasis added).

Even taken in isolation, the two-page account is certainly "good enough" to produce an image of the community and its talk concerned, as it is, with matters directly relating to the group and its constitution. A reader can appreciate that group life can be organized in this way. The description "rings true" (Lemert, 2003). From this starting point of the acceptability of the report, Sacks' critique is not one that sets out to replace Gans' observation with other accounts of urban small group life, differently organized, as did consequent critiques of TUV. Sacks does not dispute the "possible facts" (p. 702) presented. Instead, Sacks proceeds to provide a treatment of the reported conditions of the organization of the group's talk as an opportunity for an examination not of the "functions" of the talk, but the local organization of the talk such that the claim "everyone has a chance to talk" could be reasonably produced in and through the orderliness and ordering of the talk itself. In typical manner, Sacks' observations are both seemingly obvious and immensely insightful. Although multiple problems and potential topics are teased out of the description, we consider three here.

Sacks' first trouble is that "we ought not to assume that 'everyone' and 'each one' are equivalent. 'Everyone' might be some smaller population than, say, 'each person present.'" The second related observation is that "not anything anyone may say may constitute 'saying something over the course of an evening'" (p. 701); a given

utterance might not be counted as 'saying something.' With these observations in hand, Sacks goes on to consider the matter of the eligibility of topics of news among a group consisting of kin and friends as produced *by* the categorial relationships of the participants to one another.[35] Viewed in this way, the membership of the group of (potential) speakers renders it "*obligatory*" that news such as weddings, deaths, and births are announced to the group. Hearing relation-relevant news from another source might be grounds for a complaint; "How come you didn't tell me?" The sharing of such news is not, then, incidentally done. Both "having something to say" and *having* to say it is conditioned by the relationships of the persons present. It is this *obligated* talk that, in turn, is generative of equally obligated talk relative to the news; for example, offers of condolences or congratulations. As Sacks notes: "… while Gans points to these sorts of things, i.e. that there is talk of 'news', and kinsman and friends are present, how it happens that those things *produce* some or any or all of the talking is not at all stated." (p. 703).

Sacks' treatment of Gans' report continues with a second question; namely, "How is it that such a matter as 'separation of the sexes' is relevant for anyone or everyone coming to talk in a conversation?" (p. 703). Again, Sacks is concerned with the techniques and orientations of the participants to their participation as generative of next turns and rounds of talk; that is, through the production of appropriate topics for "mixed company" as well as actual sequences of talk that provide for participation at just that point (e.g. a 'norm' providing for husbands and wives not doing disagreement in public, and the appropriateness of conversation between those not married to each other in the presence of their partners). Whilst this can be read to be an analysis 'of its time' in relation to the 'rules' that Sacks refers to, the significance is the way the 'rules' *display* the operational relevancy of categories of present potential speakers which, in turn, contexturally-condition who gets to talk, about what, and when, with whom. "Everyone gets a chance to talk" is thus both achieved and constrained in and through *relational* categories and their sequentiality. That is, "sex composition and everyone getting a chance to talk are not unrelated, and are not-unrelated in systematic ways, and need to have nothing to do with 'favourite topics of talk'" (p. 704). Which renders 'having a turn to talk' as a members' concern rather than, say, a structural matter of gender relations as conceived by conventional sociology.

A third issue is the way in which the possibility of "everyone gets a chance to talk" turns on matters of next speaker selection, for example, whether a question is one that "selects a next, or one that doesn't select a next." Open questions possibly enable "anyone" to respond, but of course, that slot can only be occupied by one person, whereas other conversational turns produce a "round"; of greetings, of

[35] It seems the interest in categories – one of the major lines of Sacks' work (e.g., Fitzgerald & Housley, 2015) – may also have some of its roots in the work of CS. Chapoulie (1996, pp. 23–24) notes that one of Hughes' starting points was an idea that the categories of ordinary language reflect the viewpoint of categories used by specific people, and that those categories impose on the researcher the 'practical' viewpoint of the people who use them, and their perception of social phenomena – not only the implicit value judgments made by the people studied, but also the carving up of social reality that those judgments imply.

telling jokes, and so on, in which 'anyone' might contribute across multiple sequential occasions. And then there is the noticing that someone has *not* spoken, yet. Significantly, Sacks notes that the *location* of the group-talk is yet another condition. That the meetings take place in someone's home produces contexturally-relevant categories of 'host' and 'hostess' who may have as their business "the business of surveying the conversation to see that everyone is getting a chance to talk, or everyone's having a good time, or participating" (p. 704) which in turn raises a series of considerations for group-talk of politeness and appropriate contributions, checking on participation, and so on. Sacks also points to how the news can itself be generative of next turns at talking such that they retain the topic of the first telling by producing a "characterisable next story" (p. 706).

We have included the discussion of this lecture as a way of explicating the different possible questions that arise from observational studies. At the heart of this is, as mentioned above, the resource/topic distinction that lies is central to the divergence of EM and CA from the CS. While Gans' descriptions of aspects such as the arrangement of seatings, topics of talk, and conversational dynamics ("the talk shifts back and forth") provides resources that furnish the lived details, his formal observation of these events as 'generally an opportunity for everyone to talk' which is functionally salient to the group, Sacks treats the reportage as an opportunity to explicate the contexture of Gans' own seeing that "everyone gets a chance to talk". The lecture provides a clear view of the ways in which the descriptions presented in ethnographic studies can be taken in good faith as *possible* descriptions – a generative topic illuminating the organizational structures that render a scene describable. In this way, ethnographic reports are not attached to established sociological interests, but offer an instructable catalogue of topics for EM/CA investigations (see, for example, Dennis, 2024).

In Sacks' hands, the trouble is not that the ethnography is not 'detailed enough.' Indeed, as we hope to have made clear above, the solution is not to "bug the world" with microphones and video cameras to capture things that rarely happen or, indeed, 'what really happened' in a naive empirical sense (Garfinkel, 2022a, p. 110). Instead, Sacks is offering a wholesale reorientation to *participants'* orientations to local features, constituent elements, observable and ethnographically reportable detail of the talk *as* the detail of that talk and its production. The concern with detail – ethnomethodologically conceived – is not, then, a matter of 'more data' and certainly not reducible to a crude empiricism. Rather, Sacks' questions are themselves born of a different sense of what might figure as organizational features. Sacks does not ignore questions of who is participating in the talk but brings them to the fore, as *participants*' concerns displayed in the ongoingly assembled, contingent, ordering of their talk. Nor does he ignore central sociological themes – gender, community, morality, obligation. Instead, Sacks returns these 'themes' as topics of inquiry to the very grounds of their production: the categorial and sequential ordering of the group's conversation. For Sacks, then, that "everyone gets a chance to talk" is not to be taken as evidence of the sort of community that Gans' wants us to see but, instead, the beginnings of inquiries into the doing of group life that starts and ends with the way in which that group life is demonstrably done. In this sense, the "this and that" of ethnographic reports can be treated concretely in relation to specific

courses of action or activity. And of course, the ordering of the evenings' talk reported by Gans turns out to get organised in much the same way, through much the same techniques as group talk in all sorts of locations and contexts gets done. As we discuss in the next, and concluding section, this is a quite different sense of generalization from that of the CS and, indeed, sociology in general.

The significance, however, is not to find Sacks' remarks as 'findings' but, rather, reminders of potential explication of the inexhaustible topics and *problems*, yielded by an attention to the organization of the setting, warranting further attention. As Becker's discussion of jazz musicians, for all its great contributions, may still be seen as 'missing' the practical work of playing jazz together, Sacks shows how Gans' analysis of the group's talk, despite the important findings it provides, may be seen as 'missing' the work that the ordering of the talk does for the setting. Nevertheless, one should not read 'missing' as necessarily advancing a claim of superiority. EM/CA is "asymmetrically alternate" (Garfinkel & Wieder, 1992) to sociology and remains "indifferent" (Garfinkel & Sacks, 1970) to its disciplinary pursuits: "Ethnomethodology is NOT a corrective enterprise. It is NOT a rival science in the worldwide social science movement" (Garfinkel, 2002, p. 121).

"You Have to Get Out There and See What Is Actually Going On"

As noted in the introduction, the call for papers for this special issue occasioned a curiosity. The co-authors of this article were assembled around an immediate recollection of the quote from Sacks' (1992, Vol. I, p. 27) appreciative critique of the CS ethnographies and an interest in further exploring what his notion of "more concrete" might have meant. Moreover, we were motivated to understand just how those who have been central to the development of EM and CA in different contexts understood the relationship to CS and how it figured in their own work. Beyond the connections and contrasts we have traced across the article, we do not arrive at a clear position on the relationship between the Chicago School and EM/CA. We have certainly aimed to avoid contriving one from the materials available to us. As noted already, responses to our invitations that declined to comment or said, simply, that there was no relation to speak of, were as valuable as those we have included above.

Across this article, we have drawn on a range of sources – publications, archival materials, and conversations with key figures in the field – to describe the 'working relationship', insofar as that relationship existed, between CS and what was often disparagingly referred to as the "Californian Sociology" of Garfinkel, Sacks, and their collaborators and students. Inexorably, that discussion has tended toward tracing lines of departure, rather than connection, that mark the distinctiveness of EM and CA. At the same time, we intend to have described, if not fully demonstrated, how the points of connection are significant in the broader intellectual context in which EM and CA were developed. In a mainstream sociological landscape dominated by grand theories and quantitative methodologies, the turn to fieldwork, and an attention to the actual settings in which social life is accomplished, in whatever form, with the intention to furnish faithful descriptions of what is found there, provided initial points of contact which continued in various forms of institutional

support and intellectual exchange in an often hostile context (e.g., Coser, 1975; Gouldner, 1970). In that context, while there may be no direct philosophical or methodological influence of CS on EM/CA, both share a commitment to describing the practices and experiences of members in their settings. Viewed from the distance of a disciplinary perspective, it is little wonder that the CS and EM/CA might be considered fellow travelers in approaching the study of social life from roughly the same direction (some have even, erroneously, claimed Goffman as an ethnomethodologist – e.g., Attewell, 1974). This connection is also, perhaps, expressed in a shared "open exploratory spirit" (Faris, 1967, p. 130) of inquiry that emerged in the milieu of the US sociology of the 1950 s and 1960s. Both EM and the Chicago School share an eclecticism of method, albeit for markedly different reasons. As Becker (1999, p. 10) summarized, the legacy of CS is its openness to various ways of doing sociology. Despite shared orientations to fieldwork and observation, the distinction remains stark, especially regarding understandings of *field*, *detail*, and *adequate description*. A further distinction, and perhaps *the* key distinction, is in relation to the products and insights generated through that fieldwork.

Reflecting on Sacks' quote as our source of inspiration, and the discussions with our interlocutors, the distinctions between CS and EM/CA revolve around 'detail,' which is not just about data availability. To put things simply, Gans having a transcript of the urban villagers' talk would not produce the same sorts of insights as those generated by Sacks from the ethnographic description, and we can only wonder as to what Sacks would have produced from a recording of the same talk. This is not, as we hope to have made clear, to straightforwardly pitch EM/CA 'against' the CS ethnographies. But, rather, to demonstrate what more is possible with an ethnographic sensibility as a point of departure, rather than a destination in its own right. Of course, in the face of the 'grander' contributions of CS to understandings of the organization of cities and urban life, of deviancy and labelling, and of economic dealings, the contributions of EM/CA can seem trivial. Both Garfinkel and Sacks acknowledged this apparent triviality in different ways. For Garfinkel, this turned on the sense that members' practices are utterly familiar to them if not available to them for their explication (Garfinkel & Livingston, 2003). This holds for mundane settings such as the queue as it does for more specialized practices, and the pay-off of EM is demonstrating the significance of what is hidden in plain sight. For Sacks, the aim was to develop not a method of inquiry but a novel sociology grounded, in one statement, in the "science of the seemingly trivially obvious", in the pursuit of identifying the methodic practices that could be demonstrated to be in operation in such a way that meant that "we're dealing with something real and powerful. And not just grossly powerful, like it provides for the rate of industrial development, but it provides for little tiny things that God might have overlooked, perhaps" (1992, Vol. I, pp. 237–238). The *concreteness* of EM and CA does not, then, lie in the materials worked with alone but, rather, the ways in which those materials are understood to exhibit, and are understandable through, the lived availability of members' methods. And this requires a particular sort of training, or maybe un-training. As, Laurier (2010), writing of video materials, analysis, and theory, remarks: "Using video will not allow us to ground our theories or anyone else's, despite Glaser and Strauss's famous promise that we could ground theory through empirical work. The promise

of this sort of empirically guided study is that it may help give us a brief rest from theorising…" (p. 116).

We have, throughout this article, necessarily described the stark differences between the CS and EM/CA, and we do not wish to close by erasing them. We might move toward a conclusion by returning once more to Sacks' remarks on the attention to the "this and that" that "the world is made up of" that the approaches share and the significance of that orientation for contemporary sociology. Both approaches strive to produce a descriptive fidelity to the means through which social reality is achieved; a fidelity for phenomena which cannot be produced from the desk or from the bookshelf and which can only be discovered in 'the field'. Both approaches, in different ways, demonstrate that the circumstantial detail of social life, in its myriad forms and dynamicity, cannot be replicated, reproduced, or recovered through simulation, modelling, or any conceivable sophisticated forms of synthetic or computational 'mock-ups' (Garfinkel & Sacks, 1970, p. 363). What we have aimed to do here is follow this study-policy in our discussion of the CS in relation to EM/CA. We have not set out with a priori definitions of the CS, nor do we offer any in conclusion. What we intended to show is how what the CS was and is for EM/CA emerges in the course of the relevancies of those ethnographies filled with "this and that" for pursuing detailed studies of social life. As such, we do not intend this article as a direct juxtaposition of two 'perspectives' nor as a methodological or epistemological assessment of what they offer. Instead, we offer the discussion presented in this article as exactly that: an informative exercise in tracing a relationship between 'close enough' ways of working in studying the social that 'grew up' in post-war American sociology. Rather than coming down in favour of one approach over another – although it may be clear where our favour and that of our conversation partners lies – we have aimed to present the reader with an analysis and discussion relating to just how we might go about observing and describing "what the world is made up of". A discussion we hope will continue on from what we have presented here and, perhaps, in a more systematic manner, using other materials and methods for doing so. Indeed, we think any exercise in the mapping of intellectual traditions, understood as a landscape, which traces meaningful connections and divisions, shows future directions of travel better than hard lines drawn between assumedly monolithic ways of doing sociology. For now, however, we might give the last word to Herbert Blumer (Lofland, 1980). When asked in 1969 about the horizon for sociology by Carl J. Couch, and whether there was "anything in the future in terms of ethnomethodology in your assessment, in terms of conflict stuff that's coming out, in terms of the old traditional Marxism?" and whether there was to be a return to the empirical science approach of the '30 s and '40 s, Blumer responded:

> Well, I should say in *spirit*, yes. And I very vehemently defend the fact that it should go back to this in spirit, in line with what I was trying to suggest before is the procedure of an empirical science and understanding its empirical world. Instead of just viewing that world through a whole array of preestablished images and setting up your research in terms of those images, your research procedures, you have to get out there and see what is actually going on. Now this was the merit, it seems to me, of what was being done in Chicago. (p. 274, original emphasis)

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