Garfinkel, Sacks and Formal Structures:
Collaborative origins, divergences and the vexed unity of ethnomethodology and conversation analysis

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Abstract
It is widely recognized that ethnomethodology (EM) and conversation analysis (CA) share a common origin. It also is widely recognized that in the past half-century they have developed along different trajectories, with CA establishing itself as a compact but robust science of talk-in-interaction and ethnomethodology remaining geographically scattered and epistemologically anarchic. In this presentation, I discuss the historical relationship between these two lines of research, with a focus on the collaboration between Harold Garfinkel and Harvey Sacks that culminated in the publication in 1970 of the paper "On Formal Structures of Practical Actions." From an examination of the paper and relevant archival materials, I argue that, contrary to what others have written, it was a genuine collaboration with lasting importance for both ethnomethodology and CA. I also argue that Garfinkel and Sacks' paper exhibits divergent conceptions of ethnomethodology's relation to "formal structures" as well as a remarkable and original effort to subordinate the privileges of academic analysis to an effort to describe and elucidate the critical implications of "members' methods of sociological inquiry."

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On this occasion we're celebrating two fiftieth anniversaries of the founding works in ethnomethodology: Harold Garfinkel's (1967) *Studies in Ethnomethodology* and Harvey Sacks' *Lectures on Conversation* (published in 1992, and transcribed lectures from Fall 1964 through Spring 1972). Both of these documents can, and should, be read by any of us who would want to gain insight into what we have been doing all along. They are not only of historical interest, though they are that too. A half-century is not a trivial passage of time in the social sciences. Depending upon the social science and where one finds its origins, 50 years is a good chunk of its history. So, ethnomethodology has a history, and that history is a significant part of the history of social science. Also, while the positive social sciences like to celebrate their own progress and to invest hopes in the future, they also find reason to return to the founders' writings.

Garfinkel is of course credited for being the founding father of ethnomethodology. And it's fair to credit him with that status, though he often joked that what he founded was a company of bastards. Sacks also is, of course, credited – and fairly so – with being the founder of conversation analysis, though during his lifetime it was often called 'conversation*al*' analysis – which some of us preferred as a way to highlight the way 'analysis' was endogenous to the production of conversation.

The notion of genius is not in vogue in social studies, in part because it bypasses what is often a collective achievement in favor of the uncanny qualities of an individual. Nevertheless, Garfinkel and Sacks were both often called geniuses. I recall a casual remark by Gail Jefferson to the effect that we were very fortunate to have two geniuses in our field: Harold and Harvey. She named a few others (including herself) saying, 'the rest of us are just smart'. I think she was being modest about herself, but in any event Garfinkel and Sacks were rare and strange individuals, and both often exhibited a degree of self-fashioning as geniuses.

At present, ethnomethodology and CA remain linked, but tenuously so, and they have been on different trajectories for quite a long time. (Of course, there are many exceptions to this generalization – as many of us here respect and deploy both approaches and attempt to use them creatively.) My connection to these and other lines of work in and around ethnomethodology does not go back for quite 50 years, but 45 years ago when I first took up the study of them, there already was some difficulty with maintaining compatibility between ethnomethodology and CA. I began working towards the PhD in social sciences at UC Irvine in 1972. Previously, I had been an uninspired (and uninspiring) student in sociology, and I went to Irvine on the recommendation of a professor who recognized that I was hankering for something different. Irvine was a new university – founded six or seven years before I went there – and the School of Social Sciences at that university was non-disciplinary. The organization theorist James March had a hand in that design, and one aim was to foster novel initiatives through the formation of clusters of interested faculty and students from different fields. The School of Social Sciences also provided a space for lines of work that were not widely recognized or welcomed in disciplines – lines of work like ethnomethodology. Sacks and David Sudnow held faculty positions there, and so did Craig MacAndrew, who had done work in ethnomethodology some years before. (Carlos Casteneda had taught classes there the previous year and I signed up for a graduate seminar on 'intentionality' that he was scheduled to teach – but he didn't show up.) Garfinkel was on a one-year visiting appointment during a sabbatical from UCLA at the time I entered the PhD program at Irvine, and I took the opportunity to take his seminar. He did show up, but before the end of the academic year he disbanded the seminar out of frustration with students who didn't do the work (a long story). However, he invited several of us to take his seminars at UCLA in the years that followed, and we took him up on that offer.

I had come to Irvine to study with Sacks, since I had heard he had developed an original way of working in social science, though I had little idea of what he did. I got off to a bad start when I went looking for him at the beginning of my first academic year at Irvine. His office door was open and there were two or three people standing inside talking with one another. They looked like undergraduates, and I turned to one of them – a small, long-haired guy wearing a blue-jean jacket (the hippie uniform), and asked him if Professor Sacks would be back soon. He announced "I'm Professor Sacks." I then recognized that the face that went with the undergraduate look was not an undergraduate face. After getting over the embarrassment, I asked if I could attend his graduate seminar, and he said that I could for one quarter (10 weeks), but that it was a research seminar for his serious students and I'd have to decide if I wanted to pursue it after that. If it wasn't for me, I should go elsewhere. Reflecting on that incident now, it's remarkable that Sacks was so obviously indifferent to recruiting students or 'selling' what he had to offer by emphasizing its universal appeal and value. We could think of a slogan: "Ethnomethodology: it's not for everybody; take it or leave it." (At around that time, Garfinkel would occasionally say, "ethnomethodology is for those who are into it.")

Garfinkel had a very different manner and mode of dress. A fellow student who was studying linguistics at Irvine told me that when he first saw Harold he thought he was the janitor. Aside from the seminar I took from him, I shared an office near the men's room, and Garfinkel would often chat on his way to or from it. My office mate was studying visual psychology, and had some interesting posters on the wall illustrating unusual perspectives. Harold admired them and gave me credit for posting them, and wouldn't hear my disclaimers. He also tolerated my naivety when, following a discussion with other students in the seminar, I reported that we were having difficulty with his vocabulary. I suggested that perhaps we could have a glossary, and he gave me a look of awe at the question and said "Why don't you collect the shibboleths, and give them to me." Soon after starting the task I realized its absurdity.

After ten weeks of Garfinkel and Sacks in separate seminars, I decided to pursue Garfinkel's line, and it was clear that it wouldn't be compatible with getting more deeply involved in Sacks' research group. At the time, his group consisted of Anita Pomerantz, Judy Davidson, JoAnne Goldberg, and Alene (Kiku) Terasaki. Others such as Gail Jefferson would drop in occasionally, another student who attended wasn't yet (or ever) fully admitted to the core group. (Gene Lerner started a year or two later, and I shared an office with him.) It was clear that these were very bright and dedicated students, and that their research was organizationally coherent: Pomerantz was completing a dissertation on second assessments and compliments; Terasaki was underway with a study of announcement and pre-announcement sequences; and others were investigating other sequences and sequential features. The seminar met in Sacks' laboratory (in the social science laboratory building), which had equipment for recording and playing back audio and videotape.

Both Sacks and Garfinkel, in different ways, were very demanding of their students, and they came down hard on them when they thought they were being insufficiently careful or dedicated. Garfinkel had very different demands than Sacks did, and to try to satisfy both of them would be a thankless task. Garfinkel, though infamously capricious and temperamental, was far more open to a variety of ideas for projects. I had done a research assistantship with a survey analyst before moving to Irvine, and I didn't enjoy being assigned specific tasks and topics, such as performing the routines of coding questionnaire results, analyzing the data, performing statistical tests, and so forth. There was a hint of such industry in Sacks's research group, and it wasn't something I wanted. It was unclear what Garfinkel expected of his students, but the field was wide open, if full of land mines. (There were hazards with Sacks' approach as well – I recall a conversation with a student who had explored doing CA, but decided for a more conventional approach to linguistics; he forthrightly said that, while it was interesting, he didn't want to take the chance with an approach that had such unclear career prospects.) Although I decided not to stay with Sacks' research group, I did take another seminar on video analysis a couple of years later, which Sacks opened up to students beyond his small research circle, since it was new stuff. I did get to know some of his students, particularly Terasaki, with whom I had many long conversations about her thesis research, CA more generally, and the relationship between 'Harold's' and 'Harvey's' work.

In the early 1970s, ethnomethodology in the USA was largely located in California, and mainly at four UC campuses: Irvine, UCLA, Santa Barbara, and San Diego. There were clusters of faculty and PhD students at each campus: Aaron Cicourel, Bud Mehan, Beryl Bellman, and Benetta Jules-Rosette at UCSD; Don Zimmerman, Larry Wieder and Tom Wilson at UCSB; Harvey Sacks and David Sudnow (and after Sacks died, Jefferson for a few years) at Irvine; Garfinkel, Schegloff, Mike Moerman, and Melvin Pollner at UCLA. There were, of course, other people and places in the US and UK: Manchester, Boston University, University of Colorado; Goldsmiths College; York University (Canada) and University of York (England) etc., but California certainly was central for ethno and CA. There were internal differences and cross-cutting alliances. Cicourel had already moved away from ethnomethodology into cognitive science; Wieder was allied with Garfinkel in the phenomenological wing; Zimmerman took to CA and began building a program of applied CA; and others became free agents and hybrids.

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As I mentioned earlier, for decades CA and ethnomethodology have been heading on separate tracks, though there remains a tenuous connection between them. The track that CA (or, at least, much of it) currently is on seems to be joining a main line that runs through linguistics, psychology and cognitive science. Earlier this year, the journal *Research on Language and Social Interaction* (ROLSI) published a special issue that proposed and exemplified attempts to merge CA with experimental psychology. In his introduction to the special issue, Kobin Kendrick observes that "[t]he question of whether CA as a field should welcome experimental and laboratory studies of interaction and embrace or permit their methods may strike some as heretical" (Kendrick, 2017: 2). Two years earlier, Tanya Stivers of UCLA also used the word "heretical" in the title of a paper: "Coding social interaction: A heretical approach" (Stivers, 2015). Both of them also present numerous caveats in their proposals, but as they point out they are announcing a fait accompli as well as advocating entirely novel moves.

I think this is an ironic situation. Garfinkel would sometimes make remarks about the way the social sciences would welcome us back to the church if we showed sufficient contrition and willingness to return to the fold. Kendrick's heresy is an abandonment of ethnomethodological CA and a return to the mother church. The metaphor of heresy also is objectionable in so far as it suggests a departure from an orthodoxy whose prohibitions and demands for purity ultimately are groundless.

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This move to embrace more conventional methods and conceptions of human science has been going on for several decades, despite the fact that Manny Schegloff has written numerous critical articles reminding others of the distinctive research agenda in CA as compared with the rest of sociology, socio- and psycho-linguistics, cognitive science, and speech-act theory (see, for example, Schegloff, 1984, 1987, 1992, 2010).

There also has been an effort to at least partially sever the historical linkage between CA and ethnomethodology, so that what CA has become is treated as entirely consistent with what it was from the start. In such a Whiggish history, CA is a method of "ordinary inquiry" as Thomas Wilson characterizes it in a 2012 article. Ordinary inquiry is standard-fare science (as Wilson puts it): "observing something and reporting about it in such a fashion that other people can in principle independently check up on it" (Wilson, 2012: 219). Accordingly, CA makes creative use of recording technology to discover and describe details of actual language-use in different settings in which talk is a major part of what participants do. In the recent issue of ROLSI proposing a CA and experimental psychology merger, new technologies are deployed (including MRI) to mediate the effort to bring conversation under technical examination.

Wilson makes a particularly interesting move in his historical account. Like many others, he acknowledges that CA was an offshoot of Garfinkel's ethnomethodology, and that Sacks initially drew upon ethnomethodology and his relationship to Garfinkel in the 1960s. I think it is becoming more common, especially as CA draws interest from students coming out of linguistics, psychology and cognitive science, to begin a history of CA with the 1974 publication of the turn-taking paper by Sacks, Schegloff, and Jefferson, and to place Erving Goffman alongside Garfinkel (or even to displace Garfinkel) as predecessors. Wilson doesn't erase Garfinkel from that history, but instead divides him in two; or, rather, he divides Garfinkel's ethnomethodological project into two phases. The first phase Wilson calls "classical" ethnomethodology, which consisted in a series of substantive studies (of Agnes, clinic records, jurors' reasoning, and the documentary method of interpretation) that were published in *Studies in Ethnomethodology* in 1967. Wilson points out that, with the exception of the first chapter of *Studies*, the chapters that made up the rest of the book were largely completed (and in several cases published) before 1962. As expressed in the first chapter of *Studies in Ethnomethodology*, and in his writings during the years that followed its completion, according to Wilson, Garfinkel entered a "radical" phase, a move that represented a return to some of the phenomenological interests expressed in his 1952 dissertation. The "radical" program, Wilson (2012: 220) argues, abandoned "ordinary inquiry". A decade earlier, in a review essay on Garfinkel's 2002 collection entitled *Ethnomethodology's Program*, edited by Anne Rawls, Wilson (2003) made a similar argument about the "radical" program expressed in that book. (For now, hold on to what this "radical" program might have involved; I'll return to it later.)

According to Wilson (2012: 224), CA, and Sacks' work in particular, drew upon Garfinkel's "classical" phase, and substantiated and greatly strengthened its weak empirical support with a large body of studies that built upon one another. If we were to draw an analogy with Darwin's diagrams, according to this genealogy CA split off from ethnomethodology during its 'classical' period, and then evolved and thrived as a robust social science, whereas Garfinkel's 'radical' program was maladaptive and became a dead-end. A problem for Wilson's chronology is that Garfinkel and Sacks collaborated on a series of draft papers "On 'setting' in conversation," for a presentation at the 1967 ASA meeting, which later was published in 1970 under the title "On formal structures of practical actions." The paper thus was drafted, redrafted, and published several years after the alleged split. Wilson acknowledges this anomaly, and addresses it in a long footnote (n. 18, p. 231). In the footnote, he points out, correctly, that the Formal Structures paper developed out of the earlier ('settings') draft prepared for the 1967 ASA meeting. He also says, correctly, that the paper incorporates and builds upon a lecture that Sacks gave earlier in that year (February 16, 1967). Wilson then makes a claim that I will contest: "the treatment of this material in 'Formal Structures' is incompatible both with Sacks's substantive ideas and his way of proceeding as evidenced throughout his work." He cites Schegloff's introduction to Sacks' lectures, in which Schegloff (1992: xlix) mentions that according to Sacks, 'Formal Structures' was Garfinkel's paper (interestingly, Schegloff mis-cites the title of the publication as "Formal properties of practical actions," and gives the date as 1972 [they are correctly listed in the references of his introduction]). Wilson also supports this claim with an interpretation of the paper (which I will turn to shortly). John Heritage (2016), in a forthcoming paper published online echoes this claim: "In referring to this as 'Garfinkel's paper', I follow Schegloff, Wilson and evidence from recordings of conversations between Garfinkel and Sacks regarding the paper, all of which converge in suggesting that Sacks played little role in its creation." Heritage agrees with Wilson that CA was established before Garfinkel's radical turn.

These recent histories bring CA into alignment with current developments in the field. This is far from unusual in disciplinary histories, and to an extent such historicizing is unavoidable. However, in this case, I had strong reasons and motives to doubt what Wilson and Heritage were saying. I have long regarded the formal structures paper as the most important single published paper in ethnomethodology. It's also one of the most challenging to read. I don't know how many times I have read it, but every time I read it I am rewarded with a different sense of its arguments and implications, as well as a different set of residual puzzles and confusions. The paper explicitly presents a *radical* argument to the effect that ethnomethodology and what later was called conversation analysis take a different analytical stance toward ordinary language use – and indexical expressions and indexical actions in particular – than the rest of sociology; a stance that, as Garfinkel and Sacks point out, also differs from traditions in linguistics and logic dating back to the ancient Greeks.

Wilson, in part, directs his argument about the formal structures paper to correct Wes Sharrock and me on a point we had made some years ago to the effect that Sacks and Garfinkel actively collaborated on the paper (Lynch and Sharrock, 2003: xix). For Sharrock and me, Garfinkel and Sacks's collaboration provided an original, and to an extent continuing, point of departure for ethnomethodology and CA that differs profoundly from the way the social sciences tend to handle human actions. Wilson 'corrects' us by asserting that "Garfinkel unilaterally appropriated and idiosyncratically transformed material from Sacks, but instead of citing Sacks as a source Garfinkel named him as coauthor. Thus the fundamental difference between their approaches was obscured and Sacks was made to appear as aligned with Garfinkel's developing radical views" (Wilson, 2012: 231).

The paper in which Heritage echoes this claim is a rebuttal to a special issue of *Discourse Studies* in which Doug Macbeth, Jean Wong, Jonas Ivarsson, Oskar Lindwall, Gustav Lymar, and Wendy Sherman-Heckler and I critically examined his writings (some of which are co-authored by Geoffrey Raymond) on 'epistemics' in conversation (Lynch and Macbeth, 2016). Like Wilson, Heritage was attempting to disaffiliate Sacks and CA from any connection with the 'radical' ethnomethodology that we were associating them with. Fortunately, I was able to pursue Heritage's claim that evidence from recordings indicate that Sacks played little role in the paper's creation. With generous help from Anne Rawls and Jason Turowetz, I was able to examine tape recordings and documents in the Garfinkel archive in Newburyport Massachusetts. The documents included six drafts of the paper in the weeks before the 1967 ASA meeting in which Garfinkel and Sacks presented the "setting" paper, as well as audiotapes and transcripts of sessions in which the two of them were preparing the paper.

It seems fitting for this 50th anniversary to discuss a collaboration between Garfinkel and Sacks that took place almost exactly 50 years ago, in the summer of 1967. I'm still in the process of going through this material (a tiny portion of the vast collection of materials in Newburyport), but one tape of a Garfinkel and Sacks meeting is especially telling. It is a recording of working session in which Garfinkel for the most part remains silent. After some preliminary conversation, in which Sacks offers to provide a way to link two substantive sections of the previous draft, he begins to dictate potential material for the paper while Garfinkel takes notes. The paper had already incorporated arguments and examples from Sacks' lectures in February and March of the same year. I would not be surprised if those lectures drew from earlier discussions with Garfinkel, though I presently have no evidence documentary of this.

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The formal structures paper defies easy summary, but some things are clear enough and widely recognized. One obvious thing is that it is an elaboration on the phenomenon of indexical expressions. The phenomenon is of interest in the paper in two related ways. One way it is of interest is that indexicals – linguistic expressions whose sense depends upon the immediate context of use and understanding – present trouble for a broad range of analytical programs. Indexicals present trouble for philosophical preoccupations with reference, for logical efforts to stabilize grammar and meaning, for sociological and anthropological attempts to establish the equivalency between actions and events on different occasions, and for efforts to devise machine translation programs. For such programs one 'remedy' is to translate indexicals to 'objective' expressions: to substitute spatial coordinates for 'here'; clock time specifications of 'now'; proper names for 'me'; and referential nouns for 'it' or 'that'. Another is to invent technical terms, logical symbol systems, indexes, and standardized codes. The other way indexicals are of interest in the paper is as an investigable phenomenon. The 'problem' that analytical programs in linguistics, philosophy, and Artificial Intelligence have had with indexicals is closely related to ethnomethodology's *incommensurable* interest in that phenomenon. Indeed, the relationship between the two itself is deeply integrated with the point the paper. As stated at the start of a draft (August 15, 1967):

> The heart of the argument: although members are omniprevalently doing [formulating for settings], that work does not make up what those ensembles of activities consist of as orders and as order-accomplishing practices. We are going to describe the practices that do make up those ensembles of practices as order accomplishing practices.

The material that Sacks dictates is an argument that elaborates and bridges two major sections of the paper. One section, which he refers to as the 'critique', has to do with the history of troubled interest in indexical expressions, and Sacks offers an instance from an ancient Greek fragment (the *Dissoi Logoi*) that provides the example "I am an initiate" to illustrate a statement whose truth varies depending on who says it and when. He also elaborates upon the persistent 'obsession' (as he calls it) in Western philosophy and social science with translating indexical expressions into analytically tractable propositions. He then offers ways to connect that section with the paper's discussion of the use of formulations in ordinary conversation, making the point that for the most part indexicals are used without apparent need for formulations to clarify their reference, and that a procedure of substituting 'precise' terms for indexicals does not recover how they are used in situ.

A small sample from a recording in August 1967 occurs after Garfinkel invites Sacks to dictate the bridging section. My transcription is rough, and does not include notations for the long pauses between dictated phrases:

> Sacks: Okay. We'll begin (off) the paper by noticing what amounts to an obsession. Uhm, which, we would figure pretty much any researcher in sociology has. And then, uh, offer some initial reflections on that. And then consider it much more extensively. If you take the case of members' talk, of producing sociological research turning on, based on, about members' talk, then what the obsession initially consists of is the concern, a concern that in various ways (is) satisfiable to clarify that talk

> in the interest of the research. So, for example, if someone were- if- if a member were to say, "It's nice to have you here with us." A researcher would find himself engaged in doing such things as giving that statement a name, telling us where "here" is, who "us" are, and things like that.

In the published paper, and in their prior drafts and conversations, Garfinkel and Sacks (1970) contrast their approach to previous analytical efforts to 'fix' indexical expressions by characterizing their stable meaning. They observe that participants in ordinary conversation sometimes use formulations: "saying-in-so-many-words-what-we-are-doing," as though the speaker is characterizing the action in an 'aside' from that action. But then Garfinkel and Sacks make an interesting and original move. Here is where they draw heavily on examples from Sacks lectures, particularly Sacks' February 16 lecture from earlier in that year. In that lecture, Sacks discusses the use of "indicator terms" to speak of the "setting" but without naming it as such. He was doing a study of recordings from a series of group therapy sessions, and was puzzling over the omnirelevance of the setting (group therapy) to the recorded exchanges among the participants. He presents this puzzle in relation to the obsession that linguists and logicians have had over the years (indeed for millennia) to translate indicator terms to proper names in order to establish their intended reference and truth: "… they're concerned to find, for the indicators, a formulation of 'some object,' 'some time,' etc., where that the use of the indicator in its sentence *intends* such a formulation goes without saying" (Sacks, 1992, Vol. 1: 518). In this case, Sacks was interested in whether or how references to "here" referred to the "setting" – group therapy session. He adds that the "whole business" – the obsession with referential specification – "was exploded by Wittgenstein" (ibid.). Aside from the preoccupations with reference in logic and linguistics, "setting" is of prime sociological significance as a way to analytically and functionally integrate concrete actions (in this case the recorded actions of participants) with stable institutional orders.

At this point in the February 16 lecture, Sacks breaks some new ground. He doesn't offer a solution to the structural-functionalist problem of integrating individual actions with social structures, nor does he deliver a critique of established theories of reference. He points out that the problem of specifying which aspects of the thing (or the time, place, etc.) that the indicator term references – the object, property, aspect, measured time or location – may obscure what the parties are doing with the term. It not that "What are you doing *here*?" can be replaced by "What are you doing in group therapy?" Or, rather, were the latter expression to be used, it would not be an equivalent utterance, even if it is 'correct' referentially. The point Sacks develops is that these indicator terms may have "stable use as a means of invoking an unformulated setting, and referring to uncategorically identified persons, and noticing uncategorized activities" as "their specific business" (Sacks, 1992: 520). To substitute "group therapy session" for "here", though perhaps defensible, is to miss what the parties are doing with "here" specifically, and not as a proxy for "group therapy session". This is related to what Garfinkel, Livingston and I in the pulsar paper discuss as the use of the pronoun "it" in a way that suspends the reference to, e.g., a pattern on a screen, an artifact, or an astronomical source – until later (Garfinkel et al., 1981: 157).

Briefly, I take the following points from Sacks' lecture (as well as other writings and lectures in and around 1967 and 1968), the drafts of the collaborative paper and Sacks' verbal input into it, and the final publication of the 'formal structures' paper:

(1) There is an incommensurable relationship between ethnomethodology's interest in

indexical expressions and actions, and standard analytical procedures in sociology, linguistics, and traditions in logic, ancient and modern. Pivotal topics for elucidating this difference are: 'understanding' (of language); 'meaningful talk'; and (methodologically) the problem of characterizing the setting in which such talk and action take place. In brief, Garfinkel and Sacks are proposing a radical difference between their analytical treatment of indexicals and that of the Western philosophical heritage.

(2) Garfinkel and Sacks take a persistent problem that logicians and methodologists attempt to bypass or resolve – how to formulate a setting; how to adequately specify what the agent of an action is *doing* – and instead of trying to solve it, they treat it as a phenomenon to be investigated in quotidian circumstances. (This is what I understand "ethnomethodological indifference" to involve.) In the case of "saying-in-so-many-words-what-we-are-doing," a key analytical feature of the phenomenon is the way it is embedded in the course of interaction. Accordingly, the ethnomethodological interest in the phenomenon is in how it is woven into a particular way of hearing, responding to, and exploiting opportunities that arise in the temporality of talking together.

(3) Sacks' "findings" about formulations are two-fold. First, he observes that indicator terms are used without the need to translate or remedy the properties that linguists and others find unanalyzable until given stable specifications of location, time, person, object, etc. Second, he argues that formulations are not simply representations of "settings" and "actions" that otherwise occur, but are themselves actions that, in any given case, may come across as gratuitous, embarrassing, pedantic, etc.

Somehow, and I'm genuinely at a loss as to how, Wilson reads Sacks's lecture and the formal structures paper to be contradicting each other. Referring to the difference between what is said in the paper and what Sacks says in a transcribed lecture, Wilson asserts:

> The difference is especially apparent in the discussions of formulations [in the formal structures paper]. Sacks noted that a formulation concerning a conversation made by a participant in the course of that conversation is not a neutral observation but rather, a move within the conversation itself: "there's no room in the world to definitively propose [without consequences] formulations of activities, identifications, and settings" ([Sacks] 1992a: 516). Consequently, he argued, participants use what he called "indicator" terms and deictic expressions, as a way of drawing attention to features of the current setting without having to make direct formulations (see also 1992a: 544-546). In contrast the claim in "Formal Structures" (353, 359) is that members use formulations as attempted remedies for such expressions. In short, in "Formal Structures" Sacks's argument is stood on its head and his central point is missed entirely. Other aspects of "Formal Structures" incompatible with Sacks's work include Garfinkel's emphasis on "ethnomethodological indifference" and the invisibility of members' practices to ordinary inquiry. (Wilson 2012: 231: n. 18)

Especially puzzling is Wilson's claim that the Formal Structures paper "stood on its head"

Sacks' observation that "there's no room in the world to definitively propose [without consequences] formulations of activities, identifications, and settings." Far from standing this observation on its head, the published paper explicitly and prominently includes it: "It seems that there is no room in the world definitively to propose formulations of activities, identifications, and contexts. Persons cannot be non-consequentially, non-methodically, non-alternatively involved in [saying in so many words what we are doing]" (Garfinkel & Sacks 1970: 359, brackets in original; also see p. 353).

Following what Garfinkel and Sacks (1970) say about formulations, it is not surprising that it turns out to be difficult even to identify what is or is not a formulation. They begin the section of their paper on formulations in conversation by noting that "it is an immensely commonplace feature of conversations that a conversation exhibits *for its parties* its own familiar feature as a 'self-explicating colloquy'" (p. 350). They add that a member (a master of natural language) "may treat some part of the conversation as an occasion to describe that conversation, to explain it, or characterize it, or explicate, or translate, or summarize, or furnish the gist of it, or take note of its accordance with rules, or remark on its departure from rules" (ibid.). In apparent contrast, Heritage (2016: 46, n. 21) asserts that formulations are "vanishingly infrequent in everyday conversation," though perhaps not in more specialized or "institutional" circumstances. But, whether "formulations" are frequent or infrequent in everyday conversation, or in interrogations, interviews, etc., the larger point is that conversations are "self-explicating." As Garfinkel and Sacks go on to say, such self-explication is produced in and as ordinary conversation, and not necessarily by means of formulations. And, as they also emphasize, when formulations *are* done they are *occasioned*, and to this it could be added that their identity *as* formulations is itself contingently tied to the circumstances of use. Another way of putting this point is that "formulations" do not comprise a well-bounded category; they are continuous with, and provide elaborations of, the inter-actions they formulate. Consequently, if you search through transcripts of "everyday conversations" in order to build a collection of formulations, you will find many or few depending on how you look.

Although Garfinkel and Sacks explicitly propose indifference to the matter of whether the formulations they discuss are done by "professional" or "lay" members, they make clear that formulations are frequently deployed in social science research to clarify, explain, characterize, translate, interpret, and summarize questionnaire and interview responses, written and oral testimonies, and recorded conversations. They also emphasize that ordinary conversations are formulate-*able* by competent masters of a natural language; that is, actions in conversation are intelligible and explicable even when the parties on a particular occasion proceed without explicit commentary or inquiry *about* what they are saying and doing. This ordinary competency provides social scientists with a vernacular resource for collecting and analyzing talk as "data". The problem, as Garfinkel and Sacks further point out, is that when social scientists, logicians and philosophers (and, now, conversation analysts) rely upon *formulating* (coding, paraphrasing, writing ethnographic descriptions, and various "glossing practices") to produce and aggregate collections of equivalent cases, their data have an unexamined relationship to the vernacular competencies that generate them: "In a search for rigor the ingenious practice is followed whereby [indexical] expressions are first transformed into ideal expressions. Structures are then analyzed as the properties of the ideals, and the results are assigned to actual expressions as their properties, though with disclaimers of 'appropriate scientific modesty'" (p. 339). Garfinkel and Sacks then provide a long list of professional sociological methods and summarize them under the heading “constructive analysis” (p. 340).

…

Allow me to draw this to a conclusion. First a caveat. I am not saying that there was no difference between Sacks’ and Garfinkel’s orientations, or between the programs they initiated. From the outset, Sacks envisioned the possibility of a science of practical actions that would elucidate formal structures exhibited in actions that are recognized and used by participants to constitute social orders. Sacks also was attracted to ‘machine’ metaphors, and he construed systems of rules as ‘machineries’. Garfinkel treated the possibilities for achieving formal analysis – discovering and analyzing structures or machineries – as a phenomenon rather than a goal for ethnomethodology. He was less concerned to build a distinct science that would develop its own program of formal analysis. Instead, he envisioned ethnomethodology as a novel program that would examine the situated production and use of formal analysis in situ, in distinct instances of lay and professional activity. The collaboration between the two that occurred 50 years ago was an instance of a fragile convergence between the programs that Garfinkel and Sacks were in the process of inventing.

Revisiting that collaboration can provide insight into the current ‘heresies’ that are being promoted in the name of CA. The problem with coding and quantification of linguistic and interactional objects is not one of “ecological validity” – providing categorical representations of collections of similar objects that correctly identify their “naturally occurring” counterparts. Instead, the problem is that such a program *misses the phenomenon* – the phenomenon being the locally organized achievements that give rise to more or less stable settings, meanings, and so forth. The problem is not one of inaccurate representation but of obscuring phenomena through the operations of constructive analysis.

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