Radical Ethnomethodology¹

Position paper for Meeting at MMU, 22-23 June 2016

Michael Lynch

Cornell University

Introduction

The aim of this meeting is to discuss and promote *radical ethnomethodology*. In response to this agenda, you might wonder "Isn't ethnomethodology already radical?" The answer is a qualified *yes*. A half-century ago, ethnomethodology was introduced as a radical alternative to existing disciplinary programs in the social sciences, and the denunciations it drew from leading sociologists and social theorists helped to accentuate its radical identity. At the time, Harold Garfinkel and Harvey Sacks emphasized that ethnomethodology and conversation analysis differed fundamentally, not only from sociology but also from intellectual traditions dating back to ancient Greek philosophy. In a rather grandiose pronouncement in an unpublished manuscript, Garfinkel presented ethnomethodology's agenda in contrast to an "agreement between the Athenians and the Mancunians," about the technical conditions for the "revelation of truth in the world." Where the ancient Athenians turned to the "achievements of deep thinkers," the moderns of Manchester found revelation in "the labs and factories":

On one matter both agreed: whether in the universals of thoughtful proofs or in the mysterious effectiveness of crafts and shop techniques the revelation of truth in the world was not to be searched for already in and as of the deepest, most familiar, most unremarkable lived possession of ordinary everyday activities. (Garfinkel et al., 1988: 71-72)

In contrast, ethnomethodology would eschew the privileges of both the deep thinker (though Garfinkel certainly was one) and the technical expert while descriptively coming to terms with the intelligible production of ordinary activities. Harvey Sacks put this agenda in a disarmingly simple way:

I want to propose that a domain of research exists that is not part of any other established science. The domain is one that those who are pursuing it have come to call ethnomethodology/conversation analysis. That domain seeks to describe methods persons use in doing social life. It is our clam that, although the range of activities this domain describes may be as yet unknown, the mode of description, the way it is cast, is intrinsically stable. (Sacks, 1984: 21)[2]

In this passage, Sacks mentions two distinguishing features of this domain: first, that it consists of "methods" that are used not only by social scientists, but also and primarily by persons who produce the myriad activities that social scientists study; and, second, that the domain consists of *intrinsically stable* methods available for systematic description.

---

[2] This passage originally extracted and edited from Sacks' masters thesis at UCLA (n.d., cited in Sacks, 1984).

Both Garfinkel and Sacks proposed an agenda that conflicts with a commonplace academic and administrative presumption, which is that specialized analytical methods, disciplined efforts at measurement and assessment, and extraordinary reflexive insight are necessary for cutting through the dross of everyday activity, ordinary language, and commonsense reasoning in order to elucidate the causal forces and ideological formations that drive those activities. That presumption comes with the territory when advanced students in a social science are trained in specialized analytical methods and when they struggle to attain a degree of theoretical insight and technical mastery that surpasses the banalities of everyday reasoning. It also is expressed in programmatic efforts to see through the ideological 'truths' that mask social reality.

As Manny Schegloff once expressed it, Sacks' proposal that order was necessarily and constitutively exhibited without relief in everyday activities served "as a buffer against the potential for academic and theoretical imperialism which imposes intellectuals' preoccupations on a world without respect to their indigenous resonance" (Schegloff, 1997:165). This observation did not amount to a call to abandon disciplined research in favor of a *Lebensphilosphie*. Instead, it raised strict constraints on what could possibly count as an adequate characterization of social actions.

I assume that all this is familiar to those of you attending this meeting: one reason I invited you is that I assumed that you would not need to be reminded about ethnomethodology's original agenda. However, my qualified *yes* to the question "Isn't ethnomethodology already radical?" has to do with a conviction that a

reminder is in order, not only for those of us who might take (or renew) an interest in ethnomethodology and for others who are not at all interested in it but feel free to mischaracterize its agenda, but equally for some of the currently leading figures associated with ethnomethodology and conversation analysis.

The Agenda: Resisting the Normalization of Ethnomethodology

A few of points of clarification may be in order before going into further detail about our agenda.

First, I am taking ethnomethodology and conversation analysis together, rather than placing them in opposition to each other. Rod Watson (2008: 234) has used the expression "ethnomethodological conversation analysis"[3] to characterize current research in CA which, in one or another way, continues to develop the agenda enunciated by Sacks in the above quotation. That agenda did not die with Sacks, and sustaining it is much more than a matter of making perfunctory reference to CA's origins with ethnomethodology. However, much of what passes for CA today, even when decorated with citations to Garfinkel and Sacks, has reverted to what Garfinkel called "constructive analysis" and (as in the quote above from Schegloff) a privileging of expert analysis over and against the organization of everyday actions.

Second, at the moment, it is easy to become confused about what is, or once was, "radical" about ethnomethodology and CA. In one sense, what I am calling "radical", others have called, or would call, "classical" or "orthodox." Twenty-five years ago, Melvin Pollner (1991) suggested that ethnomethodology and CA had

---

[3] Watson attributes the phrase to George Adoff.

abandoned "radical reflexivity" in favor an empirical engagement with the everydayness of its subject matter. In my home field of science and technology studies, it is common to cite ethnomethodology as an early source of inspiration for ethnographic "laboratory studies" in the 1970s and 1980s. However, more recent decades have seen various "turns" toward novel ontologies and 'imaginaries', poststructuralist treatments of culture, politicized (as well as professionalized 'policy') agendas, and so on. And, recently in CA, and pulling in a different direction, Paul Drew (2012: 63) noted that "epistemics" (Heritage, 2012a,b) represented a novel, radical, and profound departure from prior work in CA. In a related development, Tanya Stivers (2015) proposed a "heretical" move (which, she noted, has been underway for quite awhile) toward coding conversation analytic data, and presenting "distributional" findings that pave the way for quantitative analysis. To avoid potential confusion about what is or is not "radical," I would suggest that these recent developments are anything but radical, as they invite and perform variations on the theme of what Garfinkel called "constructive analysis," and revert to familiar theoretical postures and privileges.

Third, and related to this last point, through the efforts of some leading proponents of ethnomethodology and CA, a degree of "normalization"[4] has set in. Ethnomethodology is being aligned with intellectual traditions of classic theory and interpretive sociology, while CA is sometimes taken to be an exemplary "normal science" with a well-established method of inductive empiricism. Viewed in light of such efforts, it may appear that it is no longer necessary to propose, as Garfinkel and

[4] Thanks to Graham Button (personal communication) for suggesting the theme of "normalizing Garfinkel."

Wieder (1992) did, that ethnomethodology and CA are *incommensurable* with all other social science and analytical programs. Indeed, it may appear that ethnomethodology and conversation analysis long ago passed through a contentious and fractious phase and are now blessed with a level of maturity and civility, both within the field and between it and the academic profession at large.

Taking these and related developments into account, it seems that "radical" ethnomethodology is threatened by the devoted work of some of the leading proponents of ethnomethodology and/or CA. Early on, and for as long as they remained alive and active in the field, Garfinkel, Sacks, Schegloff and many others battled against 'friendly' efforts to integrate ethnomethodology with social psychology, cognitive science, symbolic-interactionist sociology, and psycholinguistics, among other fields and subfields. They also resisted efforts to formalize the "method" of ethnomethodology, to correlate its "micro" findings with "macro" factors, or to articulate one or another intellectual lineage for its "theory". Today it seems that, far from resisting assimilationist moves toward "conventional" sociology, linguistics, or cognitive science, some of Garfinkel's and Sacks' prominent successors are leading the way, and they appear to have little stomach for engaging in debate about their initiatives.   This meeting is more of a caucus for those of us who may be inclined resist such "normalization," than an effort to stage debate with others who are promoting it.

In the past decade, radical ethnomethodology has lost many of its leading practitioners through death and retirement from active work. (Others who are retired or are close to retirement haven't necessarily stopped working!)

Ethnomethodology and conversation analysis seem likely to continue to exist, if not thrive, for at least another generation or two: professional associations, journals, and a few university centers and clusters are sustaining research in the field. However, it is not clear that the radical initiatives that gave rise to ethnomethodology and CA will remain lively for much longer, except perhaps as matters of historical interest.   At this moment, it may seem pointless to resist the normalization of ethnomethodology. Needless to say, the likelihood is dim for initiating a radical 'turn' or 'return' under present conditions in the profession, but the aim of this meeting is more modest. At the very least, we can endeavor "to put the alternative view on the record and keep it there," as Hutchinson et al. (2008: 93-94) proposed. We also can, and I hope will, discuss possibilities for ethnomethodology (radical or not) that would be novel and interesting, and not simply nostalgic for what ethnomethodology once was and/or critical of what it has become.

The sections that follow present and briefly outline topics of discussion for this two-day meeting. They are meant to touch off further topics of discussion, rather than to circumscribe them.

Ethnomethodology and Social Theory

Ethnomethodology is commonly taken to be a sociological theory, to be compared and integrated with other major theories or "paradigms". It is also commonly described as a "method". Although Garfinkel sometimes would acknowledge (as though in a confessional way) that his work was programmatic — a matter of *theorizing about* the agenda of ethnomethodological studies — he also suggested

that ethnomethodology was not comfortably situated in any of the lines of classical or contemporary theory. While acknowledging his theorizing, Garfinkel also evinced a "resistance to theory" (Hutchinson et al., 2008: 92) that runs against the grain of the widespread insistence in our academic milieu that tacit "theory" lies behind all perception, thought and action; an insistence that supports an academic milieu that would have the endless task of the reflexive scholar be to explicate the theoretical underpinnings (rather than *conceptual sense*) of ordinary social actions.

Among the tendencies that mark ethnomethodology as "radical" is the unabashed effort to pursue descriptions of observable, actual, real-worldly practices, but without taking on the baggage of empirical social science, and also without regressing into hyper-generalized interpretative glosses. Although Garfinkel was very well versed in social theory and philosophy, and expressed a particular affinity with existential philosophy, he advised his students to "misread" the phenomenologists' texts as though they were speaking directly and perspicuously of the detailed organizational and interactional phenomena the students took up in their studies (Garfinkel, 2007). (And, of course, Garfinkel himself is widely misread, though often not in a way he would recommend.)

Garfinkel's treatment of theory (as well as his treatment of method) has been smoothed out by commentators, some of whom are closely identified with ethnomethodology and Garfinkel. These commentators have articulated Garfinkel's and ethnomethodology's relations to classical traditions of sociology, both as criticisms of, e.g., Parsons, and as original extensions of Weber's and Durkheim's seminal theories (Heritage, 1984; Hilbert, 1992; Rawls, 2002). Such efforts to locate

ethnomethodology within traditions of social theory have had apparent support in some of his own recent writings, in which he pays homage to Durkheim's aphorism, "the objective reality of social facts is sociology's fundamental principle." However, his reformulation of that "principle" as a "phenomenon" makes for anything but a smooth ride:

> For ethnomethodology the objective reality of social facts, in that and just how it is every society's locally, endogenously produced, naturally organized, reflexively accountable, ongoing, practical achievement, being everywhere, always, only, exactly and entirely, members' work, with no time out, and with no possibility of evasion, hiding out, passing, postponement, or buy-outs, is thereby sociology's fundamental phenomenon. (Garfinkel, 1988: 103)

Garfinkel's relationship (or various relationships throughout his long career) to social theory will be the focus of a number of questions in our discussion of "radical ethnomethodology." These questions are closely associated with ethnomethodology's relation to what Garfinkel sometimes called "constructive analysis."

Ethnomethodology's Relationship to Constructive Analysis

Over the years, Garfinkel used a variety of terms to contrast ethnomethodology's program with other analytic programs in, and well beyond, sociology. These terms include but are not limited to the following: constructive analysis, constructive analytic theorizing, formal analysis, formal analytic sociology, and classic studies. The terms are not necessarily synonyms, though in some contexts Garfinkel seems to use them interchangeably. In earlier work (1967) he tends to speak more

frequently of constructive analysis, whereas in later work (2002, 2007) he tends to speak of formal analysis and classic studies. Sometimes he refers more specifically to formal analytic methods, models, and other constructs in the social sciences, but at other times (including in the same texts) he refers very broadly to a "worldwide social science movement"; an entire historic "movement" with ancient roots that is far from limited to Western science or academic research. In the latter, such formal methods are found

> ... in sciences, in all sciences, in the natural sciences, in the mathematical sciences, in the social sciences, in every branch of engineering and technician support, as well as in endless arts, trades, tricks, skills, occupations, jobs, and sciences of practical action and practical reasoning, everywhere, in all sciences from A to Z, from correctly consulting the *I Ching* for good advice in meeting life's serious adversities, to carrying out the Azande's poisoned chicken oracle, to astrology, to surgical laparoscopy, to orchestral conducting, to up-close magic" (Garfinkel (2007: 44).

Despite the breadth, and apparent all-inclusiveness, of his list of formal methods in scientific and non-scientific research, he distinguishes ethnomethodology's research 'methods' from all of them: "EM [ethnomethodological] methods are more methods of avoiding formal analysis than methods of research." He notes, however, that ethnomethodology's (non)methods "do place requirements on the researcher" (2002: 171). And, he has repeatedly asserted that they "are incommensurable with methods of formal analysis" (2007: 14).

Garfinkel used the terms "formal analysis" and "classic studies" in an extremely broad way, not only in reference to quantitative methods and so-called "classical theory" in sociology, but also to an indefinitely large array of academic and non-academic programs. Although Garfinkel's writings may lead us to wonder where in the world there might be space to do anything other than formal analysis, he certainly did not include ethnomethodology among the "classic studies".

Garfinkel sometimes uses the term "incommensurable" to characterize ethnomethodology's relation to "formal analysis" in and beyond the sciences. I take it that he is not likening ethnomethodology to a Kuhnian paradigm that would replace, or even provide an alternative to, formal analysis, or even formal-analytical sociology. At least, it would not be an alternative that would replace one method of formal analysis with an arguably improved method. The relation is more along the lines of a gestalt switch: an alternation between figure and ground, except that unlike the illustrative duck-rabbit, the alternation is *asymmetrical*. It also is not a relationship between a science and a metascience, since ethnomethodology would presumably provide descriptions of specific instances through which particular "methods" are accomplished, rather than general principles and rules of method. This "alternate" would pair formal accounts of method with particular performances of action that realize (or illuminatingly fail to realize) the methodic actions adumbrated by such accounts.[5]

---

[5] Garfinkel did not cite Wittgenstein's later writings in connection with his pronouncements about incommensurability, but there are parallels between his 'attitude' toward professional sociology and Wittgenstein's toward professional philosophy. Both were indifferent to the project of reforming or correcting 'common sense' with constructed logical languages or (in the case of sociology)

Questions to discuss at our meeting which arise from ethnomethodology's relationship to formal analysis include: If Garfinkel's program involves an avoidance of formal analysis, then how can ethnomethodological descriptions be produced in a methodic way, taken up, taught, and so forth? What could possibly count as a finding, or a methodic practice in ethnomethodology? Is it necessary for ethnomethodological methods to be more methodical than the activities those methods aim to bring into view? Are ethnomethodology's methods always "found methods" rather than a priori rules and principles? This leads us to the topic of how Garfinkel's radical agenda implicates work in CA.

Ethnomethodological Conversation Analysis

Ilkka Arminen (2008) contrasts "radical" ethnomethodology (exemplified by Garfinkel's program) with "scientific" ethnomethodology (exemplified by CA). His version of the former is a (perhaps deliberate) parody, suggesting that Garfinkel envisions a program that would describe ordinary action and reasoning without adding anything to what was observed, and "with no pre-conditions and no empirical noise from signifying practices" (Arminen, 2008: 170). In brief, enacting the program would require a null point of observation, from which the ethnomethodologist would produce descriptions eschewing all theory or specialized sources of interpretation. Arminen asserts that CA, contrary to its origins with ethnomethodology, "seeks to supersede common sense through a sensitive analysis of (verbal) coordination of social actions that surpasses the everyday understandings of actions" (Arminen, 2008: 184). Moreover, in his view (citing

explanatory models, and both were more interested in explicating practical actions in ordinary as well as professional settings.

Heritage [1984] to support that view), "CA is ultimately a general theory of social action" that addresses the classical sociological concern to reconcile social action and social structure.

Aside from the stark contrasts Arminen draws between "scientific" CA and "radical" ethnomethodology, his account of CA conflicts with many of the programmatic remarks made by Sacks and Schegloff. Nevertheless, he is far from alone in drawing what Garfinkel called "latter day CA" away from its origins with ethnomethodology. By the late 1980s (and, actually, much earlier), Garfinkel had grown ambivalent toward CA, and this ambivalence was evident in his writings on the subject. He frequently and publicly expressed unabashed enthusiasm for CA as the most exemplary body of ethnomethodological studies. For example, Garfinkel (1988: 107) cites "the extraordinary collection of studies on conversation," and refers to "[h]undreds of published studies [which] have established the existence of a domain of phenomena that was unknown and unsuspected until it was collaboratively developed by Harvey Sacks, Emanuel Schegloff, and their colleagues." At roughly the same time, however, he wrote the following in a paper prepared for publication in Boden and Zimmerman's (1991) volume *Talk and Social Structure* (the paper eventually was not included in that volume[6]):

---

[6] The manuscript has yet to be published. I do not know why it was not included in the Boden & Zimmerman volume, but reasons can easily be imagined: one is the length of the paper and its five appendices and bibliography (essentially a short book in itself); a related reason was delay in Garfinkel's completing and revising it; and still another is the fact that Garfinkel directly attacked the very program of "institutional talk" that the volume represented. Garfinkel informally distributed the manuscript in different versions, and with different co-authors listed after his name, in 1988 and 1989. Clearly, he was the author who wrote the piece, and the others were 'courtesy authors'.

Latter day CA which, since Harvey Sacks' death [in 1975], insists upon coded

turns' sequentially organized ways of speaking of talk and structure, makes

talk out as structure's mandarins: ruling it, insiders to everything that counts,

dreaming science, all dignity, pedantic, and corporately correct. These ways

make talk out as really the just what all concerns with structure could have

been about, and, to the point of these remarks, the just what

ethnomethodological concerns with structure could have been about.

(Garfinkel et al., 1988: 65)

It would be all too simple to draw a bright line between CA before and after

Sacks' death, although with hindsight it can be said that Garfinkel clearly foresaw

the direction that his successors in the Department of Sociology at UCLA have taken

CA. There also is no shortage of material, particularly from Sacks' transcribed

lectures, to support the idea that Sacks entertained a much broader and richer

agenda than "latter day CA" ever developed. But, as noted above,

"ethnomethodological CA" did not end with Sacks' death. Although one could easily

suppose that Garfinkel in the above passage targeted Schegoff as the chief

"mandarin" who led CA along the formal-analytical path, it is also the case that

Schegloff authored numerous articles over the years that criticized attempts by

others to integrate CA's program with "macro" sociology, speech-act theory, critical

discourse analysis, cognitive science, and Goffman's interactionism (Schegloff, 1984,

1987, 1988, 1991, 2005, 2009, 2010). He consistently supported Sacks' effort to

exploit the advantages of tape recording and playback to develop precise

descriptions of the coordinated production of social actions. Unlike Sacks, however,

who (at least in an early lecture) professed to treat recorded conversation as "incidental" and as "simply something to begin with" for studying actual performances of social activities (Sacks, 1984: 25-26), Schegloff steered CA toward a more focused "technical" analysis of "talk-in-interaction." Though his analysis took its point of departure from the vernacular production of actions achieved in and through interaction, under his guidance CA developed professional distance from its *lived* phenomenon. Like Sacks, Schegloff recommended the use of tape recording, playback, transcription and analysis, not only to gain detailed access to the vernacular production of actions by "freezing the object of inquiry," but unlike Sacks he proposed that the procedure would "allow the analyst to shed, at least partially, the relentless interference of vernacular familiarity in the analytic depiction of actions" (Schegloff, 1996: 166). He elaborated further that "[t]he same commonsense knowledge of the culture, and the semiotics, pragmatics, and discourse structure of a language that helps to constitute our cultural and linguistic competence blinds us and impedes our capacity to get at the constitution of action *technically*" (ibid.).[7] There is a remarkable contrast here, which I hope we shall discuss at length at our meeting, between Schegloff's assertion about the *interference* arising from our vernacular competence and the idea from the

---

[7] In the course of this account of CA's technical advantages, Schegloff (1996: 166) makes an interesting comparison with widely used coding procedure developed by Robert Bales: "although much of Bales's data was recorded on audiotape after the challenge of coding the stream of behavior in real time was recognized as problematic, the audiotape was erased for reuse as soon as the 'acts' had been coded. The results of the coding were taken to be 'the data,' leaving actual, recorded conduct as a kind of scientific detritus." I'm reminded of a twist on a familiar proverbial phrase that James Wilkins (then of the Centre of Criminology, University of Toronto) liked to recite (circa 1979): "throwing out the baby and leaving the bathwater for analysis."

philosophy of ordinary language that confusion arises from academic efforts to treat vernacular concepts as technical objects.[8]

Despite "dreaming science", and as a key part of that dreaming, Schegloff repeatedly addressed the problem of relevance: a key conceptual and methodological issue initially identified by Weber, and later articulated by Schutz, and given a distinctive 'Wittgensteinian' orientation to the observability of public actions in ethnomethodology. Eric Livingston uses a related term "the characterization problem" to bring into focus how an analyst demonstrates that a characterization recovers the production of an action. Livingston (2007: 243) distinguishes ethnomethodology's orientation to the problem from that of "disciplinary sociology," which utilizes specialized concepts, theories, and methods to delve into a "hidden order" — hidden in the production of the actions analyzed.

A major methodological concern for CA was, and remains, how to align the academic analyst's characterizations with the actions of the parties in a recorded conversation. Schegloff (1996: 171) explicitly recognizes that the "professional analyst" is in the position of an overhearer, despite having the advantage of being able to replay the recording repeatedly to listen carefully and transcribe the details. Instead of supposing that the overhearer's problem is a matter of having to guess what is going on 'in the heads' of the participants while they talk, Schegloff addresses the characterization problem by reference to public displays evident in and through the analysis of recorded materials. In line with Garfinkel's conception of accountability, Schegloff treats the production and recognition of conversational

---

[8] I'm grateful to Doug Macbeth for pointing out this contrast.

order as reportable-observable; i.e., necessarily part of the intelligibility of language-use-in-interaction. As a check on the interpretative license available to the overhearing analyst, he specifies "technical" constraints, such as the "next turn proof procedure," on how to characterize the endogenous organization of conversational sequences.

Recently, some of CA's leading proponents have taken the field much further in a constructive-analytic direction. This is not an entirely new development, as comparative and "distributional" observations based on collections of (arguably) similar conversational fragments have been in use for decades (see Watson [2008] for a critical discussion on the topic). However, in recent years a 'hardening' has become evident, as spokespersons for CA have advocated "formal coding" and quantitative analysis of "distributional evidence," rather than the apparently more informal characterizations of trends found through examination of collections (Stivers, 2015). In some instances, CA is presented without qualification as an "inductive" or "data-driven" method (e.g., Stevanovic and Peräkylä, 2012: 302), while CA procedures for confirming that an overhearing analyst's characterizations recover the relevant features of actions are denigrated as "ad hoc" (Heritage, 2012a: 2). Levinson (2013: 105) completely inverts CA's effort to establish vernacular accountability and demonstrability, when he asserts that much of the work in CA about adjacency pairs and so forth makes use of "loose" and "intuitive characterizations of the actions embodied in turns," adding that these characterizations are "largely based on our knowledge as societal 'members' or conversational practitioners." He adds that this "loose hermeneutics is the soft

underbelly of CA," which gives others (presumably linguists) the impression that CA is "a branch of the occult" (p. 105). The upshot is an evident divorce between the "intuitive" or "vernacular" accountability of conversation and the analytical characterizations of an *expert* armed with technical training in an empirical science.

At the same time, latter-day CA has become increasingly compatible with analytical social science, as much of the research now draws from Goffman's emphasis on information control, and from established notions of communication as information exchange between persons (e.g., Heritage, 2012a, b). It is not only that CA is reconciling itself with conventional social science, but also that it has gradually drifted away from being a distinctive approach to the vernacular accountability of ordinary actions.

A group of us have examined a recent trend (indeed, a juggernaut) in CA, involving a rapprochement with linguistics and the treatment of "epistemics" in conversation as a solution to the "problem" of action-formation (Heritage, 2012; Levinson, 2013).[9] This work will be discussed at our meeting, to raise questions about the current prospects of ethnomethodological CA.

Microfunctionalism and "Pollner's plenum"

---

[9] An invited panel on the subject was convened at the 2015 IIEMCA meeting in Kolding, Denmark. Members of the panel were Jonas Ivarsson, Oskar Lindwall, Gustav Lymer, Michael Lynch, Doug Macbeth, Wendy Sherman-Heckler, and Jean Wong, and papers arising from the panel, along with commentaries by Graham Button and Wes Sharrock and Jacob Steensig and Trine Heinemann, are scheduled for publication in a special issue on "The Epistemics of Epistemics" of the journal *Discourse Studies* (Vol. 18, No. 5, to appear in October 2016). The papers represent an effort over the past two and a half years to come to terms with CA work on the topic.

In their contribution to a special memorial issue of *The American Sociologist* dedicated to Melvin Pollner, Heritage and Clayman (2012) affiliate Pollner's (1987) work on mundane reason and reality disjunctures to particular conversational practices, such as adjacency pair organization and the "preference" for agreement. They characterize these as "some of the elements in a large plenum of practices that are appropriately understood as methods of reducing the likelihood of major fissures in the fabric of social relations and of social reality itself of the kind that Pollner describes as reality disjunctures" (106).

Perhaps inadvertently, while suggesting that Pollner's plenum was an assemblage of ordered practices, Heritage and Clayman slide into a Parsonian conception of what they call, after Goffman, "the interactional order." Garfinkel (1988: 105) speaks of "Parsons' plenum" as being the "full" and "concrete" counterpart of Talcott Parsons' formal theory of social action. In a way, "Parsons' plenum" is analogous the early-modern natural philosophical doctrine of space completely filled with matter that defied all efforts to demonstrate its material presence and organization. Garfinkel asserts that for Parsons there is "no order in the plenum," by which I take him to mean that Parsons deemed it necessary — not only for analysts but also for actors — to impose an abstract, normative order of common culture on the moment-to-moment flux of everyday activities. But, whereas Parsons' actor presupposes this order, and internalizes its categories and rules as "reality", the analyst discerns its historicity and socially relative organization (in his own way, Parsons was a social constructionist). Garfinkel sharply distinguishes Parsons' plenum, the intelligibility of which rests on a

foundation of rules and categories in common culture, from ethnomethodology's 'discovery' of orders in, and as, the concerted production of actions in situ, and he gives conversation analysis[10] major credit for the elaboration of an important domain of such everyday order.

Heritage and Clayman (2012) read Pollner's account of the incorrigible presuppositions of mundane reason as an insightful analysis of the cultural underpinnings of conversational order. Although, like Garfinkel, they suggest that the constituents of a "large plenum of practices" are already ordered in and through their local production, they credit Pollner with insight into the fundamental, normative underpinnings of those practices. Accordingly, adjacency pair organization "embodies a simple conversational norm that, upon the production of a first action (for example, a greeting, question, request etc.), a recipient should respond with a corresponding second action. This norm is one of the most enduring in human society, and is unquestionably treated as an incorrigible feature of social life" (Heritage and Clayman, 2012: 101). They go on to elaborate that departures from the norm (e.g., not returning a greeting) do not count against the norm, but are understood as evidence of various sources of failure and insult. "Across all these accounts [of failure, etc.], the conclusion that is simultaneously 'protected' and presupposed is that the rule itself has a continuing existence and relevance [as] … a presumptive basis of social interaction, pristine and inviolate" (101). (Note the

---

[10] Garfinkel (1988) refers to "conversational analysis," rather than "conversation analysis" (which had already become the conventional term for the field). He was not alone in preferring the former term, as for him as well as some others in the field it more clearly suggested that "analysis" was endogenous to the local production of conversation as well as to the professional study of recordings and transcripts.

slippage from a practical organization, to a fundamental norm that underlies it, and then to an inviolable, presupposed rule.) Finally, they assign to this rule a fundamental stability and relevance for structuring a moral order and maintaining civility and solidarity. "Conversational interaction is informed by an enormous and interwoven body of norms that are strikingly stable. … It is clear to us that the fundamental processes that Pollner described in *Mundane Reason* are central to the mechanisms through which interactional structures are maintained, protected from erosion, and stabilized across centuries and even millennia" (p. 102).

To speak of Pollner's plenum is perhaps unfair to Pollner, as the distinction between the microstructures in the plenum and the cognitive and normative basis for their functional stability arises from Heritage and Clayman's reading, which they acknowledge ironically is a "view from the suburbs". Pollner (1991) had used an analogy with "moving to the suburbs" to criticize the loss of "radical reflexivity" that he attributed to recent trends in ethnomethodology and CA. Pollner's radical reflexivity is akin to the classic notion of self-reflection: a deep, self-critical insight into the fallible underpinnings of commonplace (including the savant's own) presumptions to know 'reality'. He contrasts it with the "endogenous reflexivity" that had become established as a topic in ethnomethodology and with CA's efforts to describe how actions relate to their relevant circumstances while publicly displaying those relevancies for further inference and action. Heritage and Clayman evince no interest in radical reflexivity, but they credit Pollner with having unearthed a teleological process that underpins the stability and moral force of concerted interaction and social solidarity. As Heritage and Clayman observe, this is

a "Durkheimian" view of "a society ... defined by the extent to which its members share a set of ideas in common." They liken it to an "ecosystem" or "bubble like the earth's atmosphere only made up of cultural stuff: ideas, beliefs, knowledge, and assumptions, together with maxims and practices of working with them" (106). Although Pollner's plenum is not bereft of order (as Heritage and Clayman characterize it, it consists of specific organizations of interaction identified by CA), it is underpinned by a deeper culture of shared presuppositions and beliefs, remarkably akin to the Parsonian notion of common culture that Garfinkel (1967: 68ff.) holds responsible for the production of the "cultural dope" in social theory.

Although Pollner should not be held responsible for "Pollner's plenum," his notion of "radical reflexivity" competes for the attention of those of us who hanker for a radical ethnomethodology. Pollner surely aimed to avoid the empiricist and realist tendencies he attributes to CA, but his version of reflexivity (arguably) reverts to a classic variant of self-reflection, while missing the novelty and devastating implications of Garfinkel's reflexivity; implications that are devastating for social science, while leaving quotidian social life as it was. Like Alan Blum and Peter McHugh (1984), whose version of self-reflection was akin to his, Pollner was an astute scholar and observer, and it is especially challenging for such a scholar to explicate how Garfinkel's reflexivity is radical; indeed, more radical in its way than the revived self-reflection that is celebrated in present-day social and cultural studies. It is radical in the way it *somehow* breaks free (or proposes the possibility of breaking free) of the notion that action, thought, and perception are "laden" or

"framed" by ideologies, systems of belief, "discourses", and "imaginaries" that operate at a level that is both broader and deeper than what we (think we) know.

Pollner's radical reflexivity demands relentless criticism of our own tendencies to posit a world that exists, in itself, as though untouched by how we posit it. Garfinkel tended to link that demand to the classic doctrine of subjective constitution. Although *social* constructionism places constitutive accent on social conventions rather than cognitive dispositions, it also tends to inhibit any talk of "natural occurring" or "naturally organized" activities and of methods for explicating them. A topic to discuss in relation to the alternative "radical" moves in and around ethnomethodology, is how to preserve the topic of "naturally organized ordinary activities" without falling into the usual social science traps: abstracted empiricism, regressive self-reflection, and endless critique.

Although Arminen appears to advocate a "scientific" program of the sort that Pollner consigns to the suburbs, his critique of radical ethnomethodology is Pollnerian in the sense that it emphasizes the absence of any reflexive account of ethnomethodology's mundane interpretive underpinnings. Arminen also holds CA accountable for its own lack of attention to the ethnographic understandings that inform detailed interpretations of recorded data, and he recommends a hybrid form of scientific ethnomethodology designed along the lines of workplace studies. Ethnography, or "analytic ethnography" (Garfinkel et al., 1988: 51ff.), is itself a subject of dissatisfactions and 'deconstruction' (Sormani, 2014; Button et al. 2015) for a radical ethnomethodology, and this too will be a topic of discussion at our meeting.

What now?

Much of what I have said in this position paper is backward looking: holding present-day ethnomethodology and CA accountable to the programmatic writings and exemplary studies by founders of those fields. During the time I knew him, Harold Garfinkel was relentlessly forward looking – too much so, in fact, as indicated by his continually revised outlines for multi-volume sets that would surpass his own legacy and adumbrate the next breakthrough. Nowadays, grandiose visions may seem out of place and out of time, but there remains the question of what a radical ethnomethodology might amount to. There is, of course, endless work to be done to present correctives and reminders to our colleagues in the social sciences, and (as suggested here) to our erstwhile fellow travelers. This agenda for social science is very much in line with the series of Mind and Society meetings, which continue to sustain the agenda of Wittgensteinian/ordinary language philosophy in the face of the confident, upwardly mobile trajectory of cognitivist philosophy. But is there, or can there be, a more "positive" (albeit not "positivist") agenda to be pursued? A consistent injunction in ethnomethodology and CA is to produce "studies" and not scholarly expositions and critiques. And, while programmatic arguments in, for and against, and on behalf of ethnomethodology are abundant, there also are many studies in hand. If, however, much of the current work in ethnomethodology and CA seems to have taken a wrong turn, played itself out, and/or contented itself with mere applications and scholarly ruminations, then the question is what else is there to do *now*.

For the scholars among us, suggestions for rebooting ethnomethodology might be found in the warehouse of unpublished materials that Garfinkel left behind, including numerous tape recordings of conversations with Harvey Sacks and many other notable figures in ethnomethodology and sociology. There also are relatively undeveloped suggestions in published writings. Garfinkel's recent writings (e.g., 2002, 2007) include many challenging suggestions about developing "hybrid studies," studies of "instructed actions," and the "unique adequacy requirement," which we can discuss at our meeting. Many tangible suggestions and exemplars also can be found in Harvey Sacks' lectures. Although the lectures have been in print for decades, first as informally circulated mimeos and later as published volumes (Sacks, 1992), much of what Sacks said and wrote has yet to be pursued in depth.

Garfinkel and Sacks' (1970) collaboration also provides a basis for considering how to reconcile Garfinkel's (1991) aim to *respecify* formal analysis with Sacks, Schegloff and Jefferson's (1974) demonstration of the *formal analyzability* of conversation. Although, as mentioned above, some of us have concluded that the recent interest in "epistemics" in conversation is misbegotten, it would be worth discussing how to address the exquisite detail exhibited in CA in connection with the occasioned actions glossed under the rubric of "work".

It seems likely, however, that something other than putting ethnomethodology and CA "back on track" is needed. At this point, rather than lay out my own half-baked thoughts about a "track" that we would need to discover, my preference is to leave this topic for extended discussion at our meeting.

Preparations for the meeting

This meeting will be held in Manchester for several reasons, the most important of which is to draw upon the relatively large number of people in the area who know, care about, and have made contributions to radical ethnomethodology. The plan for this meeting is to have two different types of session on consecutive days. The first day is all-day seminar designed to incubate discussion of "radical ethnomethodology" along the lines suggested in this position paper. A limited number of participants were invited to attend the seminar.   The seminar was not set up as an 'inclusive' meeting in which 'representatives' of different factions in ethnomethodology would present and debate their points of view. Instead, it is an attempt to articulate reminders and to envision possibilities for developing an understanding of ethnomethodology and CA that remains fairly widespread in Manchester, but which is rare and endangered in the "worldwide social science movement" at large. And, sadly, it is becoming increasingly rare and endangered within professional ethnomethodology and CA.

The second day of the meeting is more open, and is designed along the lines of previous Mind and Society meetings at MMU, with a series of talks followed by lengthy discussion periods. The two sessions are linked, as the seminar discussions on the first day should feed into the discussions during the second day. Questions to be addressed include, but are not limited, to the following four clusters:

(1) What is radical ethnomethodology, and what sort of work exemplifies it? Is there a point anymore in this day and age for a radical ethnomethodology? Has it been superseded by more promising developments both within and beyond ethnomethodology?

(2) How is radical ethnomethodology related to social theory and philosophy of social science? Is it a distinctive contribution to traditions of social theory or, as Garfinkel says about method, is it a 'theory' that attempts to avoid formal theorizing? If that latter, how can such avoidance be possible? Given the prevalence of notions of theory ladenness and culture ladenness in the social sciences and humanities today, how can a descriptive approach to "naturally organized ordinary activities" withstand critiques to the effect that it is "constructed" (as opposed to being crafted)?

(3) Can ethnomethodology avoid engaging in "constructive analysis," and if so how? Related to this question are further questions raised above about CA's efforts to align professional analysts' characterizations with participants' orientations.

(4) In line with the final section above on "what now," we should as "what next"? Clearly, it is far from "radical" at this point to rehash what Garfinkel and Sacks said decades ago, or to harangue our colleagues for missing the point and backsliding into 'conventional' social science (though they may deserve such harangues). When discussing "what now/what next," we can draw upon work that we are doing now, rather than imagining "blue sky" alternatives.

These are tentative formulations of our questions. Feel free to suggest other

questions and topics, and I look forward to our discussions in Manchester.

References

Arminen, Ilkka (2008) Scientific and "radical" ethnomethodology: From
       incompatible paradigms to ethnomethodological sociology. *Philosophy of the
       Social Sciences* 38(2): 167-191.
Blum, Alan, and Peter McHugh (1984) *Self-Reflection in the Arts and Sciences*.
       Atlantic Highlands, NJ: Humanities Press.
Boden, Deirdre, and Don Zimmerman (Eds.) (1991) *Talk and Social Structure:
       Studies in Ethnomethodology and Conversation Analysis.* Berkeley, CA:
       University of California Press.
Button, Graham, Andy Crabtree, Mark Rouncefield, and Peter Tolmie (2015)
       Deconstructing Ethnography. Heidelberg: Springer.
Drew, Paul (2012) What drives sequences? *Research on Language and Social
       Interaction* 45(1) 61-68.
Garfinkel, Harold (1967) *Studies in Ethnomethodology*. Englewood Cliffs, NJ: Prentice
       Hall.
Garfinkel, Harold (1988) Evidence for locally produced, naturally accountable
       phenomena of order, logic, reason, meaning, method, etc. in and as of the
       essential quiddity of immortal ordinary society, (I of IV): An announcement
       of studies. *Sociological Theory* 6(1): 103-109.
Garfinkel, Harole (1991) Respecification: Evidence for locally produced, naturally
       accountable phenomena of order*, logic, reason, meaning, method, etc. in and
       as of the essential haecceity of immortal ordinary society (I) an
       announcement of studies. In: G. Button (Ed. *Ethnomethodology and the
       Human Sciences*. Cambridge: Cambridge University Press: 10-19.
Garfinkel, Harold (2002) *Ethnomethodology's Program: Working out Durkheim's
       Aphorism*. Lanham, MD: Rowman & Littlefield.
Garfinkel, Harold (2007) "Lebenswelt origins of the sciences: Working out
       Durkheim's aphorism," *Human Studies* 30: 9-56.
Garfinkel, Harold, Eric Livingston, Michael Lynch, Douglas Macbeth, and Albert B.
       Robillard (1988) Respecifying the natural sciences as discovering sciences of
       practical action, I & II: Doing so ethnographically by administering a schedule
       of contingencies in discussions with laboratory scientists and by hanging
       around their laboratories. Unpublished manuscript, Department of Sociology,
       UCLA.
Garfinkel, Harold and Harvey Sacks (1970) On formal structures of practical actions.
       in J.C. McKinney and E.A. Tiryakian (Eds.), *Theoretical Sociology: Perspectives
       and Developments*. New York: Appleton-Century-Crofts(pp.337-366.
Garfinkel, Harold and D. Lawrence Wieder (1992) Two incommensurable,
       asymmetrically alternate technologies of social analysis. In: Graham Watson,

R.M. Seiler, eds. Text in context: studies in ethnomethodology. Newbury Park: Sage: 175-206

Heritage, John (1984) *Garfinkel and Ethnomethodology*. Cambridge: Polity.

Heritage, John (2009) Conversation analysis as social theory. In: Bryan Turner (Ed.), *The New Blackwell Companion to Social Theory*. Oxford: Blackwell.

Heritage, John (2012a) Epistemics in action: Action formation and territories of knowledge. *Research on Language and Social Interaction* 45(1): 1-29.

Heritage, John (2012b) The epistemic engine: Sequence organization and territories of knowledge. *Research on Language and Social Interaction* 45(1): 30–52.

Heritage, John and Steven Clayman (2012) "Melvin Pollner: A view from the suburbs." *The American Sociologist* 43(1): 99-108.

Hilbert, Richard A. (1992) *The Classical Roots of Ethnomethodology*. Chapel Hill: The University of North Carolina Press.

Hutchinson, Phil, Rupert Read, and Wes Sharrock (2008).*There is No Such Thing as a Social Science*. Hampshire, UK: Ashgate.

Levinson, Stephen C. (2013) Action formation and ascription, Chapter 6 in Jack Sidnell and Tanya Stivers (Eds.), *Handbook of Conversation Analysis*. Blackwell, 103-130.

Livingston, Eric (2008) *Ethnographies of Reason*. Ashgate, Hampshire, UK.

Pollner, Melvin (1987) *Mundane Reason: Reality in Everyday and Sociological Discourse.* Cambridge: Cambridge University Press.

Pollner, Melvin (1991) Left of ethnomethodology: The rise and decline of radical reflexivity. *American Sociological Review* 56(3): 370-380.

Rawls, Ann (2002) Editor's introduction to Harold Garfinkel, *Ethnomethodology's Program: Working out Durkheim's Aphorism*. Lanham, MD: Rowman and Littlefield, pp. 1-64.

Sacks H (1984) Notes on methodology. In: Atkinson JM and Heritage J (eds) *Structures of Social Action*. Cambridge: Cambridge University Press, pp. 2-27.

Sacks H (1992) *Lectures on Conversation*, Volumes I & II, Oxford: Blackwell.

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

Schegloff Emanuel A. (1984) On some questions and ambiguities in conversation. In: Atkinson JM and Heritage J (eds) *Structures of Social Action*. Cambridge: Cambridge University Press, pp. 28–52.

Schegloff, Emanuel A. (1987) Between micro and micro: Contexts and other connections. In J. Alexander, B. Giesen, R. Munch and N. Smelser (Eds.), *The Micro-Macro Link*. Berkeley and Los Angeles: University of California Press, pp. 207-234.

Schegloff Emanuel A. (1988) Goffman and the analysis of conversation. In: Drew P and Wootton A (eds) *Erving Goffman: Exploring the Interaction Order*. Oxford: Polity, pp. 89-135.

Schegloff Emanuel A. (1991) Conversation analysis and socially shared cognition. In: Resnick LB, Levine JM and Teasley SD (eds) *Perspectives on Socially Shared*

Cognition. Washington, D.C.: American Psychological Association, pp. 150-171.

Schegloff Emanuel A. (1996) Confirming allusions: Toward an empirical account of actions. *American Journal of Sociology* 102(1): 161-216.

Schegloff Emanuel A. (1997) Whose text? Whose context? *Discourse and Society* 8: 165-187.

Schegloff Emanuel A. (2005) On integrity in inquiry ... of the investigated, not the investigator. *Discourse Studies* 7: 455-480.

Schegloff Emanuel A. (2009) One perspective on *Conversation Analysis: Comparative Perspectives.* In: Sidnell J (ed), *Conversation Analysis: Comparative Perspectives.* Cambridge: Cambridge University Press, pp. 357-406.

Schegloff Emanuel A. (2010) Commentary on Stivers and Rossano: "Mobilizing response". *Research on Language and Social Interaction* 43(1): 38-48.

Sormani, Philippe (2014) *Respecifying Lab Ethnography: An Ethnomethodological Study of Experimental Physic*s. Hampshire, UK: Ashgate.

Stevanovic, Melisa, and Anssi Peräkylä (2012) Deontic authority in interaction: The right to announce, propose, and decide. *Research on Language and Social Interaction* 45(3), 297-321.

Stivers, Tanya (2015) Coding social interaction: A heretical approach in conversation analysis? *Research on Language and Social Interaction* 48(1): 1-19.

Watson, Rod (2008) Comparative sociology, laic and analytic: Some critical remarks on comparison in conversation analysis. *Cahiers de praxématique* 50: 197-238.