Formalization and its Discontents

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Preliminary note
The present paper was prepared and submitted for consideration in the ASA program in January 2018, EMCA Section, August 14, 2018, and some minor revisions were made in August 2018. Due to the timing of the original drafting, it only briefly mentions the 'Rebuttal' issue of *Discourse Studies* 20(1), 2018, guest edited by P. Drew. The 'rebuttal issue' was a response to 'The epistemics of Epistemics,' an earlier special issue of *Discourse Studies* 20(5), 2016; M. Lynch & D. Macbeth (guest eds.).

Some further sources:
Anderson, RJ and Sharrock WW (1984) Analytic work: Aspects of the organisation of
        conversational data. Journal for the Theory of Social Behaviour 14: 103–124.
E. Schegloff, Reflections on quantification in the study of conversation. ROLSI 26(1), 1993: 99-
        128.
Watson, Rod (2008) Comparative sociology, laic and analytic: Some critical remarks on
        comparison in conversation analysis. Cahiers de praxématique 50: 197-238.
M. Lynch, "Garfinkel, Sacks, and Formal Structures ...", *Human Studies* (in press).
Other relevant sources are available on this site under "Documents".

Abstract
Ethnomethodology (EM) and Conversation Analysis (CA) take a distinctive analytical position toward social actions: they hold that ordinary social actions exhibit and make practical use of formal structures independently of professional social science methods for observing, describing, and reforming structures of social action. Two trajectories of ethnomethodological research depart from that position: one, characteristic of CA, produces ever-more-refined formal descriptions of constitutive practices exhibited in and through embodied communicative actions; the other, characteristic of Garfinkel's ethnomethodological program, explicates practices of formal analysis wherever they are performed for organizing, administering, and researching social affairs, while remaining indifferent to the scientific status and instrumental value of the formalizations in question. The present paper argues that the two trajectories are not incompatible, in principle, but that current efforts to take on board social science methods for coding, quantification, and experimentation confuse the distinctive mode of formal analysis in CA with what Garfinkel called "constructive analysis," and are eroding what was is distinctive about CA in the interest of solidifying its professional status in the social sciences.

Introduction

In their collaborative paper, "On formal structures of practical actions,"[1] Harold Garfinkel and Harvey Sacks (G&S) discuss formal structures in two distinct, but deeply interrelated, ways. First, they observe that "formal structures" are produced and recognized as constitutive features of practical actions. They pay particular attention to what they call "formulations" of actions in conversation made by participants in conversation, and point out that such formulations of "what we are doing" are themselves actions that do more and other than they say in so many words. So, for example, when a cross-examiner complains in open court that the witness is not answering the question, this may be heard as an objection designed to leverage a more revealing response from the witness and/or to persuade the court that the witness is being evasive; it is not a detached observation that an overhearing analyst can take as an indication that the witness has, in fact, failed to answer the particular question. Second, G&S discuss professional sociologists' efforts to identify, describe, and analyze formal structures as *cases* of formulating, and not necessarily *special* cases. Their placement of professional sociological methods at the same level as the practical activities that sociologists study was a provocative deflationary move. Among other things, they appeared to be denying

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[1] Harold Garfinkel and Harvey Sacks, 'On formal structures of practical actions', in J. C. McKinney and E. A. Tiryakian (ed.), *Theoretical Sociology: Perspectives and Development* (New York: Appleton-Century-Crofts, 1970), 337-366; republished in H. Garfinkel (ed.), Ethnomethodological Studies of Work (London: Routledge & Kegan Paul, 1986), 160-198. Page citations are to the 1986 publication. For further discussion of this paper see Michael Lynch, "Garfinkel, Sacks and Formal Structures: Collaborative origins, divergences and the history of ethnomethodology and conversation analysis," *Human Studies*, forthcoming.

sociology its hard-won, but still tenuous, status as a social science with a rigorous methodology; a methodology that, within the practical limits of the current state of the art, enables the sociologist to transcend the limits of common sense knowledge of social structure. Worse, in their discussion of "formulating" in ordinary conversation, G&S assert that formulations (whether produced as participants' 'asides' in the course of ordinary conversations or as summary descriptions of findings from professional social scientific research) are context-bound, no less so than the particular actions they formulate. Having said this, however, Garfinkel and Sacks do not conclude that social actions make up a disorderly mess or an ineffable mystery until rendered by methodic procedures into analyzable data. Instead, they assert that the "doing" – the performative accomplishment – of ordinary actions is endogenously ordered, and that the order produced in and through that accomplishment *subsumes* formulations and provide them with their locally organized sense.

A question that Garfinkel and Sacks leave open – one that remains a fraught topic in ethnomethodology and CA (EMCA) – is the place, or places, of formal analysis. Do, or should, those of us who take up Garfinkel's and Sacks' initiatives when we study social actions engage in formal analysis? Can we do otherwise? And, when we do so, are we not engaging in what Garfinkel called "constructive analysis" – an array of research practices from which he distinguished ethnomethodology's program? In this paper, I cannot hope to provide definitive answers to these questions, but I do hope to address the difficulties they raise for current work in EMCA. And, at the very least, I believe that those questions should be kept alive, rather than dismissed for being residues of an earlier era that no longer have much relevance to current research.

A parting of the ways?

Some prominent conversation analysts, as well as a number of linguists and cognitive scientists who selectively invoke conversation analysis (CA) as a *method*, have lately initiated efforts to integrate CA with quantitative, experimental, and other established social science methods.[2] Others have been inclined to resist such 'progress', and I count myself among them. The question of whether CA is compatible with formal analytical methods deployed in sociology, social psychology, linguistics, cognitive science, etc., depends upon one's conception of CA. For those of us who hold that CA was, and should still be, included as part of ethnomethodology's program, the question also turns on our conception of ethnomethodology (EM). One version of the relationship between CA and EM is that they have been on separate trajectories for quite a long time, with CA becoming a program of formal analysis and EM offering a way to study how formal analysis is deployed in practice in and beyond the social and administrative sciences. Accordingly, CA has become more integrated with linguistics and EM with ethnographies of analytical practices in and beyond the sciences.[3] What preoccupies me in this paper is the observation that CA's version of formal analysis (as exemplified in the work of many, though

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[2] A few of the many examples of such efforts include: Tanya Stivers, "Coding social interaction: A heretical approach in conversation analysis?" *Research on Language and Social Interaction* 48(1), 2015: 1-19; Kobin H. Kendrick, "Using conversation analysis in the lab." *Research on Language and Social Interaction 50*(1), 2017: 1-11; and Lisa Mikesell, Galina B. Bolden, Jenny Mandelbaum, Jeffrey D. Robinson, Tanya Romaniuk, Alexa Bolanos-Carpio, Darcey Searles, Wan Wei, Stephen M. DiDomenico & Beth Angell, "At the intersection of epistemics and action: responding with *I know*," *Research on Language and Social Interaction* 50(3), 2017: 268-285.

[3] Greiffenhagen et al. suggest the term "methodography" for investigations of actual occasions in which social scientists engage in discursive and practical methodological work (Christian Greiffenhagen, Michael Mair, & Wes Sharrock, "From methodology to methodography: A study of qualitative and quantitative reasoning in practice," *Methodological Innovations Online* 6(3), 2011: 93-107, available at http://journals.sagepub.com/doi/pdf/10.4256/mio.2011.009).

not all, of its currently active practitioners) has gradually shifted away from what was once radically different from prevailing social science methods and has become more thoroughly integrated with them. I believe that this shift threatens to lose what was once distinctive, if not essential, to EM and CA as research programs.

Sacks proposed from the outset of his use of recorded conversations as material for investigating the local production of communicative actions that such actions were unrelentingly ordered and that such order could be described in a formal way. He is explicit on this point in the posthumously published "Notes on methodology" compiled and transcribed by Gail Jefferson from Sacks' MA thesis and other early writings and lectures.[4] Among the remarks preserved in that compilation is an outline on some of the "central findings" of his early research.

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[4] Harvey Sacks, "Notes on methodology," in J. Atkinson & J. Heritage (eds.), *Structures of Social Action: Studies in Conversation Analysis* (Cambridge: Cambridge University Press, 1984), pp. 21-27. The compilation of writings and lectures from different times inadvertently creates the retrospective illusion that, as early as his MA thesis, Sacks envisioned what later came to be called "conversation analysis". For a discussion of materials in the Sacks archive, see Richard Fitzgerald, "The data and methodology of Harvey Sacks: Lessons from the archive," *Journal of Pragmatics* (2018), https://doi.org/10.1016/j.pragma.2018.04.005 (in press). Fitzgerald (p. 8) presents an early quotation from Sacks on the "domain" he proposed to investigate: "The domain is one which we have come (those who are pursuing it) to call ethnomethodology." Fitzgerald presents this as a reminder that, when Sacks wrote these remarks in the 1960s, *ethnomethodology* was the name for a unified domain that included Sacks' early studies of conversation, among other topics. In the edited compilation published nine years after Sacks death, the sentence is rendered: "The domain is one that those who are pursuing it have called ethnomethodology/conversation analysis" (Sacks, "Notes on methodology," p. 21). Prior to Sacks' death in a road accident in 1975, it was common to speak of what he, his colleagues, and students were doing as "conversation*al* analysis." To some practitioners the 'al' suffix more clearly signaled that the analysis in question is endogenous to the very production of conversation, and that the job of the professional analyst was to explicate that production.

The detailed ways in which actual, naturally occurring social activities occur are

subjectable to formal description.

Social activities — actual, singular sequences of them — are methodical occurrences.

That is, their description consists of the description of sets of formal procedures

persons employ.

The methods persons employ to produce their activities permit formal descriptions of

singular occurrences that are generalizable in intuitively nonapparent ways and are

highly reproducibly usable. (p. 21)

Although Sacks' transcribed lectures have an informal quality and display wide-ranging

interests, his published writings are highly formal in the way they are set out, such as in the

1972 publication based on his dissertation.[5] While the 1974 turn-taking paper with Manny

Schegloff and Gail Jefferson, unlike Sacks' 1972 paper, was not formatted with numbered

paragraphs, it also presented a highly formal account of the recursive operations of an

impersonal machinery; a "rule set" for allocating turns at talk in ordinary conversation.[6] In

many ways, the turn-taking paper established a set of phenomena, an observation language,

and a disciplined empirical approach that became canonical for CA.

Garfinkel frequently upheld CA as the crown jewel of ethnomethodology. But, while he

was effusive in his acknowledgements of his own and ethnomethodology's debt to Sacks, he

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[5] Harvey Sacks, "An initial investigation of the usability of conversational data for doing sociology," in D. Sudnow (ed.), *Studies in Social Interaction* (New York: Free Press, 1972), 31-74.
[6] Harvey Sacks, Emanuel A. Schegloff, & Gail Jefferson, "A simplest systematics for the organization of turn-taking in conversation." *Language* 50(4), 1974: 696-735.

was never fully on board with CA's version of formal analysis.[7] There are some, seemingly

slight, differences between Garfinkel's references to "naturally organized ordinary activities,"

and Sacks' "actual, naturally occurring social activities." Sacks' formulation is widely used in CA,

and while Garfinkel's phrasing was not picked up in CA, it more explicitly provides "activities"

with a praxiological contingency in contrast to a naturalistic objectivity. And, unlike Sacks,

Garfinkel did not unambiguously propose that ethnomethodology would give *formal*

descriptions of such activities. Instead, he proposed to provide a "manual" that would engage

with the practices described in order to explicate their embodied, instructively reproducible

production "from within"; a production he proposed was inextricably situated in local contexts

of action. The term "manual" framed ethnomethodological studies as *instructions* for

assembling the practices under examination rather than as descriptions of the formal

properties of analytical objects.[8] It can be argued, though with more ambiguous textual

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[7] In an unpublished manuscript, Garfinkel subjects "latter-day CA," which he attributes to developments in CA following Sacks' untimely death in 1975, it to withering criticism. The manuscript was redrafted several times between the late 1980s and early 1990s, with different co-authors listed on different drafts. To my knowledge, Garfinkel wrote the manuscript with little or no direct input from his listed co-authors. The authors listed in one version, dated 1989, were Harold Garfinkel, Eric Livingston, Michael Lynch, Douglas Macbeth and Albert B. Robillard, and the manuscript was titled, "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." The remarks on "latter-day CA" are in Appendix III.

[8] Garfinkel used this expression frequently in the 1970s and 1980s, and even proposed a series of volumes with the title, "A Manual for the Study of Naturally-Organized Ordinary Activities," to be published by Routledge & Kegan Paul. Garfinkel circulated versions of the prospectus and outlines for the multi-volume set. It was never published, but for a time it enjoyed a virtual existence, and is cited in, among other places, John Heritage's summary chapter on "Ethnomethodology," in Anthony Giddens & Jonathan Turner's (1987) *Social Theory Today* (Stanford, CA: Stanford University Press, 1987), pp. 224-272, at p. 268. Also see M. Lynch

evidence, that Garfinkel's "actual" was never other than an existentially engaged 'objectivity' (a practical 'thinghood' akin to the Heideggerian 'ready-to-hand'), whereas Sacks' "actual" often seemed to refer to an objective phenomenon ('present-at-hand') that enabled singular instances to be assimilated into generalized collections and subjected to an analysis that would yield counter-intuitive, and yet verifiable (stable, reproducible) findings. However, the difference between their conceptions of "actual", "objective", and "formal" organizational phenomena, and of methods appropriate for their recovery and analysis, was subtle, unsettled, and unsettling. Although he often treated "formal analysis" as a phenomenon that ethnomethodology investigated (a topic), rather than a methodological resource, Garfinkel occasionally traded in formalisms, such as a symbolic formula he dubbed the "rendering theorem" and the various forms of bracketing he used in an expansion of a "glossing" procedure he and Sacks attributed to I.A. Richards.[9] (It remains unclear to me how seriously Garfinkel took such constructs.) One thing is clear, however, which is that he surely subscribed to Sacks' identification of "formal structures" as constitutive features of social actions. In their collaborative paper, Garfinkel and Sacks (G&S) say the following about ethnomethodology's understanding of "formal structures":

> ... by formal structures we understand everyday activities (a) in that they exhibit upon
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> analysis the properties of uniformity, reproducibility, repetitiveness, standardization,
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> typicality and so on; (b) in that these properties are independent of particular
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> production cohorts; (c) in that particular cohort independence is a phenomenon for

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(2002), "From naturally occurring data to naturally organized ordinary activities: Comment on Speer," *Discourse Studies* 4(4): 531-537.

[9] "Richards' gloss" is discussed in the appendix of the "Formal Structures" paper.

members' recognition; and (d) in that the phenomena (a), (b), and (c) are every

particular cohort's practical, situated accomplishment. (p. 163)

G&S then note that (c) and (d) mark the difference between ethnomethodology's treatment of

formal structures and the prevailing treatment in sociology.[10] In later writings and lectures,

Garfinkel returned again and again to this list, refining it, recasting it, and often assigning (a)

and (b) (and sometimes [c] as well) to Durkheim's treatment of social facts as *things*.   If, as

Garfinkel and Sacks assert (d) marks the difference between ethnomethodology and the

prevailing treatment in sociology, the question then is whether "formal analysis" is suitable for

studying the "practical, situated accomplishment" of social structures. This is a vexed question

that is key to understanding the apparent difference between Sacks' and Garfinkel's programs.

Formal analysis

The collection of Garfinkel's later writings, edited and introduced by Anne Rawls, presents

"formal analysis" (FA) in an extraordinarily broad way, in association with what he calls "the

worldwide social science movement." Garfinkel also makes clear that this "movement" extends

well beyond the academic social sciences, as formal analysis is "ubiquitously done in all

administered societies".[11] Although Garfinkel's reference to the "worldwide social science

movement" seems rather grandiose, it is no more grand than familiar theoretical references to

modern rational society, and more specific accounts of the administrative disciplines that

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[10] G&S set up this passage by saying that "ethnomethodological indifference" is the key to understanding that, and how, formal structures are "practical ongoing achievements". In other words, the recognition that formal structures are produced independently of particular cohorts in a way that is independent from, and indifferent to, sociology's methods is constitutive of those structures – or, rather, of the activities in which those structures are accountable.

[11] Harold Garfinkel, *Ethnomethodology's Program: Working out Durkheim's Aphorism*. Edited, with an introduction, by Anne Rawls (Lanham, MD: Rowman & Littlefield, 2002), at p. 121.

Foucault chronicles as arising in the late 18th and early 19th centuries, which were epitomized by Bentham's design for the panopticon.[12] "The worldwide social science movement," also has a thematic resemblance with critical theories of "audit" society and culture. If, as suggested in Garfinkel and Sacks's paper, formal structures are every particular production cohort's practical accomplishments, ethnomethodology would constitute a genealogy of social structure. For Sacks, the practices through which social structures are accomplished *in situ* are amenable to formal analysis. Presumably, however, such analysis would somehow differ from prevailing social science treatments.

Some distinctive features of CA proposed in programmatic writings by Sacks, Schegloff and other prominent conversation analysts are: (1) an extraordinary attention to recorded and re-examinable details of naturally occurring interaction, as opposed to coded summaries, imagined linguistic specimens, and other rendered data used in social science; (2) an indifference to preconceptions of the political or social significance, historical importance, and theoretical meaning of particular events and arenas of action that are brought under analysis; (3) a constraint upon professional analytical characterizations that they engage with the demonstrably relevant, locally produced, and locally recognized analytical categories (including membership categories, action categories, and linguistic categories) through which parties constitute singular courses of interaction; and (4) a commitment to describing *interactional* orderings that subsume individually produced sentence structures, lexical and non-lexical tokens, gestures, and other constituents of conjoint actions. If, as Sacks proposed from the

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[12] Michel Foucault, *Discipline and Punish: The Birth of the Prison* (New York: Random House, 1979).

outset, CA is a program of formal analysis, then it is a different *kind* of formal analytic program

than what prevailed a half-century ago, and arguably still prevails, in the social sciences. But, a

key question that is particularly poignant at present is how does (or ever did)

ethnomethodology and CA stand with regard to so-called "conventional" social science?

When positioning ethnomethodology as an "incommensurable" and "asymmetric

alternate" to the "worldwide social science movement," Garfinkel stridently insists that he is

not proposing a rival or critical discipline. And, yet, almost in the same breath, he observes that

the worldwide social science movement's methods are "everywhere accompanied by

incongruities."[13] At this point, he appears to be speaking about the social sciences, though a

similar case could be made with respect to the use of administrative metrics and other formal

analytical measures used in managerial activities ranging from the assessment of academic

'productivity' to the estimation of baseball players' market value.[14] Terms such as "curious*

incongruities,"[15] which in earlier writings Garfinkel called "curious absurdities," appear to carry

unmistakably critical connotations. In the context of long-standing criticisms of formal social

science methods (criticisms that have been part of sociology from the very outset of its

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[13] Garfinkel, *Ethnomethodology's Program*, 121-122.

[14] The connection of such metrics and the social sciences is an interesting topic. For example, the widely used "impact factor" that assigns numerical weight to journals based on citations to constituent articles, arose from bibliometric measures developed in the sociology of science, but many measures used in sociology derive from government and other organizational fields. On baseball metrics, see Christopher Phillips, *Scouting and Scoring* (Princeton, NJ: Princeton University Press, in press).

[15] Ibid. The asterisk is a mark that Garfinkel in his later writings would sometime append to a term to identify it as a "tendentious use" to distinguish what he meant with the term from more common usage; the latter to be held in suspense while the sense of the former gradually dawns on the reader in light of what Garfinkel proposes to say later. A skeptical reader may view such usage as an instance of critique-under-denial; that is, a deniable use of critical terminology.

establishment as an academic discipline), an invitation to stand aside from a commitment to formal analysis in the interest of examining its actual deployment – in order to elucidate the "curious* incongruities" between formal methods and their actual, situated deployment – seems tantamount to developing a critique. When Garfinkel's program is directed close to home at social science research practices, it becomes especially difficult to suggest that no criticism is intended or implied by talk of an "incommensurable" research program.

The critical edge in Garfinkel's treatment of formal analysis is especially obvious when he speaks of *constructive analysis* or *constructive analytic theorizing*. He does not draw a clear distinction between constructive analysis, constructive analytic theorizing, natural theorizing, formal analysis, classic studies, or classic methods, but as he makes clear in the following passage, ethnomethodology endeavors to avoid each and all of them:

> EM methods are more methods of avoiding formal analysis than methods of research. But they do place requirements on the researcher. The policy requires that the tasks of inquiry and argument provide for the practical objectivity and the practical observability of structures of practical action and practical reason, in and as of ordinary activities, while exercising an indifference to the policies of natural theorizing, withholding the corpus status of formal analytic descriptive facts, avoiding the design and administration of generic representations and their methodologized dopes, and in related ways making no use of the methods of constructive analysis.[16]

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[16] Garfinkel, *Ethnomethodology's Program*, p. 171.

But what exactly are these methods of constructive analysis, and how is it possible to avoid

making use of them? Some further specificity can be found in a passage in the "Formal

Structures" paper in which Garfinkel and Sacks discuss indexical expressions:

> Sociology's methods of formal analysis are differently disappointed by these
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> expressions: their definiteness of sense is without structures that can be demonstrated
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> in the *actual* expressions with the use of available mathematical methods, to specify a
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> sense, definitely. In a search for rigor the ingenious practice is followed whereby such
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> expressions are first transformed into ideal expressions. Structures are then analyzed
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> as properties of the ideals, and the results are assigned to actual expressions as their
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> properties, though with disclaimers of 'appropriate scientific modesty.'[17]

In other words, various constructs are built into research methods and "instruments" such as

fixed-choice questionnaires or interview protocols in order to provide sufficiently ordered data

to permit systematic analysis. These would include indicators, indexes, coding schemes,

definitions of variables, hierarchies of rules, binary distinctions and four-fold tables, models,

simulations, ideal types, and generalized metaphors. Their use in the social sciences cuts across

the distinction between qualitative and quantitative methods, and encompasses the entire

division of labor from data collection and data analysis through interpretive theorizing.

Given such a plethora of methods that ethnomethodology would avoid, it is fair to ask,

what else *could* ethnomethodology do?[18] Instead of taking up that question, I will address a

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[17] Garfinkel and Sacks, "Formal structures," p. 158.

[18] Ilkka Arminen concludes that Garfinkel's proposal to avoid all varieties of formal analysis
invites the impossible. The challenge of rescuing that possibility is a serious one, but I will not
take it up in this paper. Ilkka Arminen, "Scientific and 'radical' ethnomethodology: From

more tangible question that pertains to CA: given that CA is committed to a program of formal analysis, how does that program differ, if at all, from what Garfinkel called "constructive analysis"? One reason for turning to CA rather than addressing the broader question of the possibility of avoiding formal analysis altogether is that in the past half-century CA has provided a fund of studies and a degree of specificity on the question of formal structures that informs what an answer to that question might look like.[19]

Forms of formal analysis

As noted earlier, Sacks explicitly proposed that social activities are methodically produced, and that they are amenable to formal description. CA developed, and continues to develop, as a progressive program of formal analysis. With increasing confidence, many current practitioners present CA as a social science and seek to further integrate its "methods" with established social science procedures such as coding, variable analysis, and experimentation. Given such a trajectory, it is worth reconsidering what once seemed so radical about what Garfinkel and Sacks, both separately and together, proposed. Given his explicit commitment to formal analysis, how was the program that Sacks envisioned different in kind than familiar methods of constructive analysis and constructive analytic theorizing? The remainder of this draft will

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incompatible paradigms to ethnomethodological sociology," *Philosophy of the Social Sciences*, *38*(2), 2008: 167-191.

[19] At last year's ASA meeting, Eric Livingston and I suggested that CA provides a kind of foundation for what ethnomethodology has come to, and could come to: Michael Lynch & Eric Livingston, "The conversation analytic foundations of ethnomethodology," presented at the 112th Annual Meetings of the American Sociological Association, Montreal, 14 August 2017. The title is an inversion of an earlier paper, and points to a reflexive dialogue over the years between EM and CA: Michael Lynch, "The ethnomethodological foundations of conversation analysis," *Text - Interdisciplinary Journal for the Study of Discourse* 20(4): 517–532.

sketch out how I propose to address this question, and I refer readers to some recent papers that delve into aspects of the question.[20]

The conundrum that any effort at formal analysis faces can be given a name, and has been given the name "the characterization problem".[21] This is the problem of how to identify and elucidate lived-orders of practical action without first converting them into representations that systematically miss those orders. Most programs of formal analysis have no interest in recovering lived-orders of practical action in fine detail, and are far more interested in assimilating them into conceptual frameworks and theoretical schemes, and yet according to the "Formal Structures" argument, such constructs rest upon a foundation of unanalyzed orders that pervade their own practices: "The unavailability of formal structures [of practical actions] is assured by the practices of constructive analysis for *it consists of its practices*."[22]

One standard practice of constructive analysis that Garfinkel and others addressed is *coding* interview responses, records, and other forms of "raw data" in social science research. Typically, such as in the versions of Bales' coding scheme that have been adopted and updated for decades, a finite set of categories is used to render transcripts of recorded interactions into tractable data. Coding is often an intermediate step for setting up quantitative analysis, but it

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[20] For starters, see "The epistemics of Epistemics," special issue of *Discourse Studies*, guest edited by Michael Lynch & Douglas Macbeth, Vol. 18(5), 2016: 493-620, with contributions by Jonas Ivarsson, Oskar Lindwall, Gustav Lymer, Michael Lynch, Douglas Macbeth, & Jean Wong, and commentaries by Graham Button & Wes Sharrock, and Jakob Steensig & Trine Heinemann. Shortly after I drafted the present paper for submission to the ASA's program, a "rebuttal" issue of *Discourse Studies*, guest edited by Paul Drew was published (Vol. 20(1), 2018), in which contributors to the issue defended 'epistemics' against the criticisms in the earlier special issue. Responses to the rebuttal issue are available online, under "Documents" in the Radicalethno.org website: http://radicalethno.org/documents.html
[21] See Eric Livingston, *Ethnographies of Reason*. London & NY: Routledge, 2008, p. 227.
[22] Garfinkel & Sacks, "Formal Structures," pp. 178-179, emphasis in original.

also is widely used in qualitative research, as well. Often, coding is built into the design and administration of written questionnaires, in which the literary instrument provides respondents with standard questions and scaled answers to check off. Garfinkel's study of his research assistants' work of coding clinic records,[23] performed a gestalt switch on coding by treating coding practices non-instrumentally as a phenomenon of interest, rather than as a means for producing analyzable data. In line with the policy of ethnomethodological indifference, Garfinkel's coding study draws no conclusion about the validity of the data the research assistants produced. However, the elaboration of the "*ad hoc* practices" they performed, when contrasted with the explicit protocol they were instructed to follow could easily be assumed to call the resultant data into question. Similarly, when Garfinkel takes up Mannheim's "documentary method of interpretation" as a theme for explicating how subjects in his counselor experiment managed to make narrative sense of random yes-no answers to their questions to an unseen counselor, it is difficult not to conclude that Mannheim's method is likely to yield highly variable and possibly contradictory narratives (where the very possibility of discerning definite contradictions is itself problematic).[24] Few sociologists at the time that Garfinkel wrote about such (ethno)methodological practices, and few today, would be willing to settle for the implication that professional methods partake of *ad hoc* judgments and produce arbitrary stories. Similarly, it was difficult to ignore the critical implications, when Garfinkel and other ethnomethodologists characterized the 'subjects' of sociological research as though they

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[23] Garfinkel, *Studies in Ethnomethodology*, pp. 18ff. The experiment had some resemblance to classic social psychology experiments, but Garfinkel presented it less as a test than as a phenomenological demonstration.

[24] Ibid., Chapter 3.

were research associates whose contributions to sociological analysis were relied upon in interviews, questionnaires, and self-reported forms to provide accounts of social circumstances, attitudes, and organized actions. Criticism was implied as long as sociological methods were understood to surpass common sense judgments of fact and circumstance with scientifically validated findings.

In programmatic writings in CA, particularly but not exclusively by Sacks and Schegloff, the difference between constructive analytic methods and CA's procedures assumes a technical specificity. By this I mean that Sacks and Schegloff devoted specific attention to the relationship between professional analytical characterizations and the situationally relevant, singularly exhibited activities under study; activities that themselves were credited with being performative analyses. They drew an explicit contrast between Bales' interactional categories and the locally-relevant and contextually exhibited relevancies that arise in singular instances. They drew this distinction, not between "formal analysis" and a largely unspecified means of getting access for formal structures of practical actions; instead, they drew it between conventional social science research methods and a distinctive version of formal analysis in CA. They referred to the distinctive features outlined above – the use of recorded naturally occurring activities; indifference to preconceptions of significance; an effort to ground analytical characterizations by reference to locally exhibited relevancies; and an interactional rather than individual framing of characterizations – as technical properties and criteria such as the next-turn-proof procedure, which ties analytical characterizations to the evident way that parties treat one another's moves on particular occasions of talk-in-interaction. The overall

commitment in what Rod Watson has called "ethnomethodological CA"[25] is that the "raw data"

for studies of social actions are constituted by members, whose mastery of natural language

includes analytical competencies.

The question that remains to address in an expansion this paper is whether the formal

analysis currently practiced in much of CA significantly differs from what is, or was,

characteristic of ethnomethodological CA. To adumbrate what I intend to argue at length in the

expanded paper, unlike the formal analysis that Sacks and Schegloff developed, current

proposals and efforts to exemplify a "mixed methods" approach display the following features:

(1) a circular relationship between characterizations of singular instances in published papers

and (to readers hidden) collections from which the instances are drawn; (2) a general

superficiality of coded 'objects' and the sequential 'positions' in which they are placed, which

misses (and often misconstrues) the local sequential and pragmatic *sense* and *relevance* of the

actions in which they occur; (3) a conflation of linguistic features of speakers' utterances with

interactionally organized and embedded *practices*; (4) a treatment of sequential relevancies

and contingencies as correlated pairs of speakers' actions and normatively induced responses;

and (5) the formatting of transcripts and the extraction of published extracts to construct first-

and second-position relationships that feature in the analysis.[26]

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[25] Watson, Rod "Comparative sociology, laic and analytic: Some critical remarks on comparison
in conversation analysis," *Cahiers de praxématique* 50 , 2008: 197-238.
[26] Some of these features are elaborated and substantiated at greater length in M. Lynch,
"Confirming illusions: A literary practice in 'latter-day' conversation analysis," presented at
"Beyond epistemics: A one-day workshop at Manchester Metropolitan University, 7 June
2018." A draft is available at: http://radicalethno.org/documents/lynchconfirming.pdf . See
especially pp. 18ff.

Conclusion

The particular concern addressed in this paper was whether current efforts to integrate CA's "methods" with coding procedures and variable analysis (see note 2, above) are commensurate with what Garfinkel and Sacks proposed a half-century ago. The paper put aside the question of whether such efforts are compatible with Garfinkel's (arguably enigmatic) ethnomethodological alternative to what he called "formal analysis". Instead, the paper began to address the question whether arguments made by Sacks decades ago in favor of a version of formal analysis, and by Schegloff more recently, are compatible with what a substantial amount of research in and around CA is now doing.[27] Although Garfinkel and Sacks proposed a policy of "indifference" toward formal analysis, their discussions of what they called "constructive analysis" held unmistakably critical implications for practices of coding, the use of indicators and models, and other conventional research practices for producing analyzable data. In further work adumbrated in this paper, I intend to go into detail on the question of whether efforts to mix CA's "methods" with practices of constructive analysis sacrifice what is distinctive of ethnomethodological CA in favor of compatibility with the established social sciences.

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[27] See, for example, E.A. Schegloff, "Reflections on quantification in the study of conversation," *Research on Language and Social Interaction* 26(1), 1993: 99-128; E.A. Schegloff, "One perspective on *Conversation Analysis: Comparative Perspectives*," in J. Sidnell (ed), *Conversation analysis in comparative perspective* Cambridge: Cambridge University Press, 2009, pp. 357-406; and E.A. Schegloff, Commentary on Stivers and Rossano: "Mobilizing Response," *Research on Language & Social Interaction* 43: 38-48. Schegloff's criticisms of these moves in CA are presented in Doug Macbeth & Jean Wong, "CA and its heresies," presented at Seventh Meeting of the Language and Social Interaction Working Group (LANSI), New York, New York, October 2017.