

Reflections on the June 22-23 2013 meeting on Radical Ethnomethodology, at MMU
Michael Lynch

(1) The overall theme of 'radical ethnomethodology' apparently did not resonate with all (or perhaps even most) participants in the meeting. As expected, the theme was subjected to questions about what it could possibly mean, and gathering from comments I received there was not much resolution by the end of the meeting. One understanding of what I proposed with the theme is that ethnomethodology/conversation analysis is 'in trouble' and that it is necessary to go back to the 'roots' (especially as articulated by Garfinkel and Sacks) for reminders of what was/is 'radical' about it and is threatened by recent developments. In my position paper, I emphasized two distinctive programmatic features emphasized in early writings by Garfinkel and Sacks: (1) an indifference to the presumptive ability of the savant to transcend the limits of common sense; and (2) a proposal that singular occasions of social action are ordered and orderly in the course of their production. Together, these amount to a claim that there is no need to render the immediate production of social actions into models, analytical codes, standardized logical languages, or other constructs in order to make them analyzable.

The main impetus for my conviction that now is an especially apt time to rehearse these reminders is the study that several of us have been undertaking on 'epistemics' and related developments in CA. It seems that relatively few people who took part in the discussion shared our concerns with current developments in CA. Perhaps because of the locale and composition of our meeting in Manchester, many participants appeared to be indifferent to 'epistemics', and to any other development in CA or ethnomethodology that aims to integrate research and theorizing with established agendas in sociology, linguistics, and other social sciences. Some comments and questions suggested that there is nothing new about such efforts, and that they mainly concern those of us who view ethno/CA in relation to disciplinary sociology (see points 2 and 3, below). Moreover, as a few participants suggested, sociology has moved on from where it was in the 1960s and 1970s, so that ethnomethodology is no longer viewed as a threat to sociology, or as a program that is threatened by 'conventional' sociology. There was some disagreement about whether such dismissals of alleged 'threats' expressed complacency, or whether they indicated that ethnomethodology can thrive within and beyond academic sociology or any other social science discipline. (For critical writings about recent trends in sociology and related social sciences, I recommend some of the papers on 'radical reflexivity', 'portmanteau representations,' and other topics in the Wes Sharrock & Bob Anderson website: <http://www.sharrockandanderson.co.uk> .)

(2) A question that came up from time to time during discussions was whether ethnomethodology needs a "foil" of the sort provided by so-called "conventional" social science, and sociology in particular. Related to this was the suggestion raised by Bob Anderson of distinguishing "critique" from "criticism". Another way to put the question is, can we pursue studies of ethnomethodological *phenomena* without being bothered by what sociologists would do?

I suggest at least two ways to take up this question. One is in terms of Garfinkel's theme of "asymmetric alternates," where he treats formal accounts as counterparts to ethnomethodological descriptions of lived activities. Although criticisms (such as Hubert Dreyfus's of the limits of artificial intelligence programs), and critiques (such as Husserl's of Galilean science), point to the incompleteness of formal analysis and its dependency on practical reasoning and practical action, ethnomethodological descriptions neither vindicate nor undermine formal accounts of practical actions. Formal analysis is not so much a 'foil' in such studies as an invitation for ethnomethodological research into 'what else' might be at stake in the production of activities.

The second way to take up the question has to do with the investments in formal accounts of practical actions that are prevalent in the administration of such actions. The ethnomethodological 'finding' that social scientific methods, administrative metrics, and other formal schemes are 'first pair-parts' in *Lebenswelt* pairs puts them on a par with recipes, manuals, occasion maps, musical notation, and formal accounts of mathematical proofs: their limits and virtues are occasional and contingent. That they do not describe the actions of using them does not count against them, though it may conflict with programmatic arguments that treat formal accounts as primary and self-sufficient representations of reality, or as causes or 'drivers' of actions in accord with them. In such instances, ethnomethodological investigation can be aligned with critiques (and criticisms) of reductionist accounts – including and especially those that, arguably, ride under the banner of ethno/CA.

(3) Since, for many, "radical" implies novelty, and not just *any* sort of novelty, there were questions about what, if anything, is or could be on the agenda for ethnomethodology, radical or otherwise. Related to this is the question of whether my (and others') dissatisfactions might be symptomatic of the feeling that the low hanging fruit was harvested a long time ago, and that we're now faced with diminishing returns, with novelty being largely limited to moving more or less familiar ways of doing studies into new fields and taking up previously unexplored subject matter. The excitement about epistemics in some quarters of CA also might be taken as evidence that something – perhaps anything – novel is long overdue in the field. My own view of this – I'm unsure about how many others may share it – is that much of what Garfinkel and Sacks originally proposed has yet to be understood or developed in the social sciences, and (sadly) even in many current ethno/CA circles. Moreover, in Manchester, and largely centered around Wes Sharrock and his many colleagues and students over the past half-century, ethnomethodology has been infused with a distinctive Wittgensteinian emphasis. 'The Manchester school' (if I can be permitted to call it that) gives far more explicit emphasis to Wittgenstein, Winch, and to ordinary language analysis than Garfinkel or Sacks ever did in their published writings and lectures (though Sacks' early work on membership categories has been integrated with conceptual analysis by Steve Hester, Peter Eglin, Dave Francis, Lena Jayyusi, Jeff Coulter, and many others). A striking difference between 'the Manchester school', and the EMCA associated with sociology departments mainly, though not exclusively, in the USA is that the 'Manchester' studies remain indifferent (though not overtly hostile) to the conceptual and organizational rubrics of a (social) science. The implications of that approach, in my judgment, remain as radical as they ever were.