Handbook of Social Theory

Fundamentals of Ethnomethodology

10.4135/9781848608351.n19

[p. 249 ↓]

Chapter 19: Fundamentals of Ethnomethodology

An Alternate, Asymetrical Sociology

Ethnomethodology does not sit comfortably in a book like this, for it neither is nor has a theory in the conventional sociological sense, any more than it has a specific or distinct methodology. One way of summarizing ethnomethodology, though not necessarily one that would easily receive the assent of its practitioners, is that it is an attempt at assembling what Ludwig Wittgenstein was apt to call 'reminders'. As such, they serve to clarify our understanding by drawing our attention to things that we already know, but which we are inclined to overlook, or to exile from our attention when we undertake to theorize.

Despite attempts to include it within the 'social action consensus' (as, for example in Colin Campbell's misguided *The Myth of Social Action*, 1996), ethnomethodology remains unreconciled to the prevailing situation in (what remains of) sociology—an unregenerately dissident tendency. It is not dissidence for its own sake, but the result of deep and thorough divergences in the idea of what sociology can be. Invited to see ethnomethodology's influential and beneficial effects, in those recent efforts at theoretical review and synthesis produced by such prominent figures as Jiirgen Habermas, Anthon Giddens, Pierre Bourdieu, Jeffrey Alexander and Randall Collins, one can only see, rather, that the essential point has constantly been disregarded, if indeed it has been noticed at all. Forcefully reaffirming ethnomethodology's incongruous character, Harold Garfinkel, the founder of the enterprise, deems it an incommensurable, alternate, asymmetrical sociology (Garfinkel and Wieder, 1992).

One might take Holmwood's (1995) comment about the way theorists have now come to appreciate that members of the society are 'knowledgeable' participants in the same enterprise of theorizing with their professional sociological counterparts as an acknowledgement of, especially, ethnomethodology's critique of the 'cultural dope', of the tendency of sociological theories to portray the members of society as more naïve than they actually are. However, rather than as an appreciation of any such achievement, this could also well seem a direct inversion of the actual point, for, of course, Garfinkel insists that ethnomethodology does not set out to make matters better or worse than they are 'ordinarily cracked up to be' and, in rejecting the 'cultural dope' does not want to make persons out to be more or less smart than they are 'ordinarily cracked up to be'. The correct interpretation of ethnomethodology's lesson is that professional sociological theorists and ordinary members of the society have much more in common than the traditional (professional) sociological contrast between analyst and member makes out. However, that is not because ordinary members have been found to be engaged in theorizing comparable to that conducted in the professional mode, but, instead, because the professional sociologists are (without acknowledgment) much more like the members than they take themselves to be— themselves extensively involved in operating as members immersed in the order of 'practical sociological reasoning'. *Practical* sociological 'theorizing' is not directed toward issuing versions of 'how the society is in general' but involves drawing upon 'common sense understandings of social structures' in order to decide 'what is happening here?', 'what are **[p. 250 ↓]** those people doing?', 'how did things end up this way?' This is not at all comparable to the kind of (upper case) theorizing to be found in those volumes of monumental size, if not significance, that aspire to be the contemporary equivalents of Parsons' *The Structure of Social Action*. It is, however, the kind of (lower case) 'theorizing' that sociological theorists and researchers routinely, ubiquitously and unreflectively do when they speak concretely about the ordinary affairs of the society. There the 'common sense understandings of social structures' come obscurely and ambiguously into play as supplements to the upper case theorizing and the methodological glosses of professionalized enquiry[1] (cf. Bittner, 1974: 70). Further, that reasoning is not 'sociological reasoning' in any specialized and distinctive sense, but is itself integral to and inseparable from the practically saturated concerns in whose service it is done, for example, reasoning during litigation or during jury deliberations, reasoning in the laboratory and observatory over the course of experiments and

observations, the following through of the detailed course of mathematical reasoning, reasoning in the management of household arrangements, in the preparation for spiritual and meditative disciplines, in the prevention of accidents in vehicle repair, in the calculation of business costs and profits, in the asking of questions in court, and so on, *ad infinitum*.

To put it another way, the interest resides in the competencies that are required to execute practical affairs, competencies that, of course, range from those that are ubiquitous and commonplace and which 'anyone' might be expected to do, to those of a much more specialized kind and over which only a few might be expected to possess full mastery. Possession and application of these competencies require a grasp upon the fact that they are socially organized, and the effective conduct of these affairs involves the practical management of the course of action in and through a socially organized setting. Consider, Garfinkel's masterful exposition of the competencies of those who work at the determination of suicidal deaths for a Suicide Prevention Centre (SPC):

> The work by SPC members of conducting their inquiries was part and parcel of the day's work. Recognized by staff members as constituent features of the day's work, their inquiries were thereby intimately connected to the terms of employment, to various internal and external chains of reportage, supervision, and review, and to similarly organizationally provided 'priorities of relevances' for assessments of what 'realistically,' 'practically,' or 'reasonably' needed to be done and could be done, how quickly, with what resources, seeing whom, talking with whom, talking about what, for how long, and so on. Such considerations furnished 'We did what we could, and for all reasonable interests here is what we came out with' its features of organizationally appropriate sense, fact, impersonality, anonymity of authorship, purpose, reproducibility – i.e., of a *properly* and *visibly* rational account of inquiry. (1967: 13)

These considerations, including *inter alia* office policy, departmental administration, case load, scheduling, budget and collégial relations, are not conceived as extrinsic to the work of investigating suicidal death. They are rather matters which are integral to

forming the course of action that will comprise the investigation and determination of the nature of the death, and that will provide grounds for assessing the adequacy of that enquiry's results. There are numerous other considerations that also enter in: the investigation will consist in some significant part in talking to people, but *which* people, and in what order? Talking to them about what topics? Employing what techniques for getting them to talk at all, or to talk in the way required to make what they have to say useful material for the enquiry? There is also the matter of knowing what to make, with respect to the enquiry's purposes, of what they will or can be brought to say, to know how these conversations individually and cumulatively count as evidence pointing one way or the other to conclusions about the nature of the death under investigation. As well as:

Knowing how to report—that is, how to present orally or to write up—what has been done in ways that allow one to be left alone to get on with one's own work, to be taken, to be understood by others on the basis of what one says and writes, indeed to have done what one claims to have done, knowing how to write up what has been done so that the conclusion reached will be seen to follow plausibly from what has been reported, being open to other obvious interpretations, are all further matters that are involved in being able to be demonstrably competent in one's work. It is the grasp upon these innumerable, multifarious, detailed, specific and localized considerations, and of the ways in which they figure as grounds for further inference and action, that makes up the sociological competence of the occupants of the coroner's office, the competence which enables them to *see* the phenomenon of a person dead by suicide in the first place. Suicide is, *in this sense*, an *organizational* phenomenon. 'In this sense' here covers the fact that the topic is suicide, the official, legally established fact, the basis on which insurance payments will be made or withheld, decisions as to whether to set criminal investigations in train and so on can be made, and that the ascertaining of this legally established fact is the regular work of the coroner's office and associated investigators. *In this sense*, suicide is nothing other than precisely the investigator's competence applied in accordance with legal standards, office policy, professional good practice and the like in the pursuit of an enquiry to a defensible and acceptable conclusion (the latter itself being relative to circumstance and to exigencies which are not necessarily amenable to anticipation or control by the investigator). The circumstances invoked are the realities, as suicide investigators know them, of the work

of identifying suicidal deaths, and the suicidal death is *both* the origin and the outcome of the investigative process conducted under and through those circumstances, with those circumstances being managed by the investigator in assembling the course investigation and in respect of decisions as to what to conclude thus far and what to do next.

Not a Theory

Space does not allow me to go into the reasons for and the gross inadequacies in the treatment meted out to the supposed 'agency' position in the one-sided dialectic characterizing recent and contemporary debates about 'structure' and 'agency'. The direct way to highlight the difference as it affects ethnomethodology is to point to the deep ambiguity which cloaks the relationship between the sociological analyst's and the member's point of view. Barely, but simply, one can see the failure to appreciate the character and dimensions of the problem (from ethnomethodology's point of view) in the way that Giddens, Bourdieu and others attempt to resolve their problems by giving 'the member's point of view' an appropriate location within their theoretical scheme, providing acknowledgement of the 'subjective' or 'agency' aspect. As conceived by ethnomethodology, the problem is not one that originates or can be resolved *within* the theoretical framework that attempts an analytical resolution of the issues attending the 'privileging' of the professional sociologist's point of view, for this framework presupposes that there is a clear, sharp or deep distinction between 'the analyst's' and 'the member's' points of view. The question which is raised is not 'Where to place common sense understandings' relative to 'social structural influences' in the determination of persons' conduct?' The question is instead: how much of the freight of *sociological analysis* is carried by the 'common sense understandings' which are tacitly, unrefiectively and unsystematically deployed and depended upon in the analyst's purportedly analytical work? It is not easy to find any place in the sociological literature (outside of ethnomethodology's own writings) where this matter is even raised, let alone discussed and dealt with.

Trying to compact the intricate complexities of the reasoning that propels ethnomethodology's dissidence into the space of this short chapter requires real simplification, so for simplicity let it be that ethnomethodology's core problem is: how

to track the course of sociological inferences? Rephrased: how to do sociology in a way which allows such tracking? The capacity to undertake such tracking would allow the identification of the points at which 'commonsense understandings' are involved in moving the reasoning forward. *If* this ambiguity is seen to be problematic, then there is a question as to whether it can ever be resolved. At least, whether it can ever be *resolved within the terms of sociological theorizing as those are programmatically conceived within the discipline*. It ought to be obvious that ethnomethodology does not aspire to the formation of *theory*, and that it is not a trivial matter for it that it does not. The undertaking of theorizing in the professionally approved mode would perpetuate, rather than obfuscate the structure of sociololgical inference, **[p. 252 ↓]** of the *inferential* interplay between analyst and member status.

Not a Methodology Either

However, this does not make ethnomethodology's concerns a set of distinctively methodological ones, engendering a priori deliberation to determine valid procedures for enquiry. It means, rather, that the very idea of what a subject matter for sociology might be is utterly changed. The topic becomes that of *reasoning*-sociological reasoning. This does not, however, mean that reflection on the reasoning of sociological analysts has been substituted for the examination of the life in the society, for one of ethnomethodology's crucial features is its insistence that ordinary members of the society are just as much *sociological* reasoners as are those professionally identified as sociologists. One *can* reflect upon the reasoning of professional sociologists, but *qua* concern with practices of sociological reasoning, such reasoning has no *special* status: it comprises merely one topic amongst others, exhibiting the ways of 'practical sociological reasoning'. The result: motivation for attention to the instantiations of the array of practices comprising *practices of practical sociological* reasoning. Neither more nor less.

Ordinary Social Order

The relation to the questions that standardly motivate sociological theorizing is now quite attentuated. None the less, there is a continuity, which may be found through the problem of social order. However, even in this connection there is as much discontinuity as continuity, for the problem is transformed. Again, crudely:

[p. 253 ↓] This is anything but an attempted reduction of social order to subjective states, as is often critically supposed, being, instead, an attempt to re-identify the phenomena of social order, to identify them in the forms in which they are practically encountered and managed. Talk of 'practical sociological reasoning' should not be mistaken for allusions to inward cognitive computations, but to the depiction of *embodied* and social-organizationally embedded practices. It is risky to lose sight (in the way that Finn Collin, 1997 does) of the fact that sociological reasoning is situated within the world of daily life which itself has anything but a vaporous character (cf. Bittner, 1973). I have tried to suggest the notion of 'reflexive' as it figures significantly in ethnomethodology is less a concern that one's methods of enquiry should apply to oneself just as they do to those who are the subjects of one's studies, than with the fact that activities are embedded in the same social order that they produce.[2] Remember that the enquiry into the 'production of social reality' equates with the assignment of (for brevity) factual status to certain phenomena—such as that this person really is dead and really died by his or her own hand. The point about such a determination is, of course, that the enquiry which undertakes to establish the fact is plainly located within an organizational setting whose ways and features are clearly the real worldly conditions of carrying out the enquiry. The ostensible problem which Collin thinks he finds is a consequence of overlooking the implications of the treatment of *time structures* as matters to be handled in a principled way and, therefore, with the character of *sociological enquiry (lay and professional)* as conducted in real time.

Collin (1997) reads ethnomethodology's writings as a self-defeating portrayal of social reality as being constructed out of nothing, as though the 'negotiation' of factual status were conducted in a vacuum. Unfortunately, the appearance of an impossible conception—reality negotiated out of nowhere and from nothing—is only a result of

the neglect of the emphasis noted above upon the irreducibly embedded character of social actions, and of their reflexive relationship to their setting. Were the extent to which the assignment of factual status is conducted under the auspices and by means of the organized ways of a social setting—after all, what else does the above sketch of investigations into suicides show?—fully appreciated, then the apparent difficulty would instantly evaporate. Ethnomethodology's purpose is not to make determinations on its own behalf of the factual status of matters that members address, such as, for example, the nature and cause of an alleged suicidal death, or to second-guess the correctness of some astronomers finding a pulsar (Garfinkel et al., 1981). The sociologist's examination of those occasions—the determination of cause of death, of the reality of a discovered pulsar—is one step removed from such determinations, itself seeking to highlight the practices through which the parties assure themselves that they *really have* found out how this person died or where a sought-after pulsar is located.[3] The study of the 'night's work' of a team of astronomical observers as they work through the series of observations that results in the discovery of a pulsar shows how it is not possible for anyone to say, at the outset of the observations, how they would turn out, nor, as the realization that the observations were possibly showing the presence of *something*, whether that something was really there, whether the apparent observations were a result of having found something or were merely an effect of the technologies being used for the observations, and whether, further, if the observations were authentic, the phenomenon being observed was actually the thing being sought. It was only over the course of their work, making their observations and reasoning about the significance of these, that they could eventually arrive at the conclusion that they had indeed discovered phenomenon that was theoretically possible, but had not yet actually been found. *In this sense*, the 'pulsar' is a product of their work, something whose existence is established, whose reality is affirmed through the observatory-sited activities of assembling, of building up in an accumulating series, and construing the corpus of materials that comprise a series of astronomical observations (Garfinkel et al., 1981).

Ethnomethodology concerns itself, then, with phenomena as they are encountered from within local social settings, and therefore in terms of the way in which they are 'known' as something to be dealt with in that setting (as the pulsar is something to be found through the use of the technologies and skills of observatory practice).

The phenomena which are brought under ethnomethodology's review are often quite commonplace, such as, for example, 'traffic'. The objective is to identify the phenomenon from its very midst, in the way that, for example, traffic is prevailingly (but not exclusively) encountered either from the driving seat, on the road and *en route*, or equally commonly, in the course of pedestrian movement. Again, however, there must be no illusion of reductionism which would insist that traffic is *only* the affair of vehicle and pedestrian movement. Clearly, 'traffic' also features (for example) as an affair of system management. No one of these involvements is to be privileged over any of the others, for they instantiate for ethnomethodology the social order as a *local* production; each instantiates **[p. 254 ↓]** different localities within which the production of orderly traffic movements can be achieved. The two uses of management in this paragraph should not be conflated. The driver-at-the-wheel who is managing the traffic is doing so in the sense of handling or coping with the traffic situation that he or she currently occupies, and it is this kind of management—the practical handling of social affairs— with which ethnomethodological studies are occupied. Those who work as managers —such as someone who, say, directs the Highways Agency or supervises the air traffic control system—represent in ethnomethodology's view just more instances of practical management of everyday affairs rather than a distinct species of it. The study of such managers would be contending with the phenomena of highway or air traffic as, no doubt, those that are found on the desk, in the meeting, in the finding of experts to assure policy etc.[4] The local production of order is undertaken from one or another of a multiplicity of local sites, sites *within* the course of affairs that the activities are managing.

Ethnomethodology, then, deliberately eschews upper-case theorizing of the sort which has lately reasserted itself in the "Return of Ground Theory". It agrees, further, that deep difficulties which sociological projects have conventionally and continuously faced (though the recognition that this is so is periodic and cyclical) are due to the disjunction of theory and research. That theory and research stand in a deeply problematic relation is hardly surprising when they are construed as distinct activities which are to be pursued in near-complete autonomy from one another. Why confront this problem in its terms? Why not disregard the traditional distinction of 'theory' and 'method' and engage in 'theorizing' in conjunction with the conduct of research? Why not, further, notice the difference in the 'logic' of enquiry (as formulated by Abraham Kaplan, 1998)

⑤SAGE knowledge

between logic-in-use and reconstructed-logic, and abandon a concern for the latter, being concerned with the *actual* methods for 'collecting data', exclusively with the logic-in-use in the actual enquiry. 'Collecting data' is in scare quotes only to emphasize that collecting data *is the very same thing as* finding out about social structures.

Canonical Characteristics of Conversation Analysis

One of the few things which nowadays stirs the blood of ethnomethodologists is the controversy over whether 'conversation analysis' is or is not subsumed under, affiliated with, or even originally indebted to ethnomethodology (cf. for a provactive discussion 'Molecular Sociology' in Lynch, 1993: 203–57). The *historical* merits of insisting upon or denying the existence of such a relationship are one thing, the advantages of using conversation analysis (or at least, the work laid out in Harvey Sacks' (1992) *Lectures on Conversation)* as an exemplification of ethnomethodology's ideas are another. And perhaps by registering some of the reservations about what I'll call 'latter-day CA', I might do even more to clarify what it is that, whilst appreciating that *in other respects* Sacks' meticulous reasoning follows through on ethnomethodology's project ideas, gives rise to contemporary dissatisfactions.

If you are interested in tracking where, and in what fashion, specific commonsense understandings enter into the course of sociological reasoning, then there is every advantage to beginning with both materials and problems which are as simplified as possible. Beginning with tape-recordings of (often telephone) conversations as the *whole* of one's 'materials' will seem perverse from most methodologically informed sociological points of view, but then, it needs to be remembered that Sacks' point of view reciprocated the impression of strangeness about sociology's more usual ways of setting about things. How can we, without knowing something about the identities of these people, their placement in social systems and so on, possibly begin to understand what they are doing in their talk? But suppose we do know things about the identities etc. of the conversationalists, what advantage to us will that be if those identities etc. are introduced into the course of (professional) sociological reasoning in an undisciplined way? The investigative indifference to the possession of such

'ethnographic' riches hardly equates with a denial of the fact that, for example, that the speaker is a police officer ever matters. The question is not one of substance but of procedure: why make presumptions that identities etc. *must* matter, and how they must do so? Why not begin *without* such biographical information and see how far one can get *without* it? and see if one finds it unnecessary to draw upon such information, or whether one finds that one cannot proceed further without such information, specifically what it is that makes such information cogent to the investigation at just this point.

If one works with audio recordings and their transcriptions, what is the data? The point of the enquiry is to find this out, to determine what is on the recording, what is in the transcription. Examining the data is not a preliminary to analysis, it *is* the undertaking of the analysis. The examination of the audio recording must first **[p. 255 ↓]** address the questions: what are these persons saying/doing, and on what basis can one make those identifications?

Note, first, for the conversations that are used as materials, these are commonly transparent to the understanding of the investigator. The talk involved is pretty characteristically plain talk, about commonplace affairs.[5] Much of what is said can be understood without knowing *anything* 'biographically' about the speakers independently of the recording. The talk, as plain talk, makes sense: it does not engender bewilderment—what are they saying, what could they possibly mean, what on earth are these people doing? They are (plainly) arranging to meet for coffee, one is updating the other on what happened at work today, they are telling jokes and stories, they are soliciting assistance from the police, they are getting into an argument about leisure preferences and so on. That is *evidently* what they are doing. No special competence is required to figure out these materials. No specialized kind of expertise, medical, or legal is required for this, and certainly no professionally provided *sociological* expertise is required either. The basis for following the talk and grasping its sense is the fact that these matters are ones that pretty much *anyone* can grasp (anyone who is an ordinary, wide awake member of the society, that is) and it is, of course, upon the basis of one's *common sense understandings* of how *both* talk *and* the everyday affairs of the society are organized that one can make such construals.

However, the point is not to gloss these conversations, listening to/reading through the records to arrive at a neat, concise summary of what the talk amounts to. It is, rather, to examine the conversation in a step-by-step way, to make specific identifications of just what is being said, what is being done by what is being said, and what is being contributed to the conversation by what is being done/said at any specific point. A step-by-step examination of the ways in which the materials are construed for what they are is required. The conversational sequence is treated as a real time construction, one that is put together by the parties to it as it goes along. The alignment between the analyst's and the participants' understandings is a critical consideration, the participants being assumed to act on the basis of knowledge available to them, and the identification of their actions turning, then, upon what they could have known at the moment at which the utterance was spoken. The operation of what Garfinkel termed the 'prospective/ retrospective' determination of sense requires attention to the difference between the sense that can be assigned to an utterance at the moment of its production, and the subsequent modification of that sense in the light of what is said/done subsequently. The treatment of the talk as a real time activity involves a precise tracking of the ways in which individual utterances contribute to the emerging sense of the conversational exchange. The conversational sequence is one which builds-up, with the nature of the identity of any present utterance being tied in multifarious ways to its predecessors. Being tied, furthermore, very much to the specifics of prior utterances.

The recognition that the organization of the talk is done through its specificity acknowledges for the case of conversation the general point that real worldly practical action is invariably a matter of contending with the 'just this' quality of its circumstances. The notion of 'constraint' has been widely adopted as the criterion of demarcation between sociological approaches, between the supposed sides of 'structure' and 'agency' particularly. It is ethnomethodology's misfortune to have been deemed to have fallen on the 'agency' side of this inadequately conceived divide, with (in its terms) the principal failure of the 'agency' approach being its incapacity to acknowledge the extent to which action is constrained. A polarization of positions around the issue of constraint as such is wholly inappropriate, though there may be reasons for differentiation in terms of the kind of interest that is taken in constraint and in the kind of constraint that is of interest. The interest of classical sociological theory is in the question of whether or not the agent is free to act or whether he or she is limited in freedom to act, or is perhaps

largely (if not wholly) propelled by causal determination: how free is the agent to do this action rather than that one? Admittedly a crude way of presenting the issue, but sufficient to allow the suggestion that the issue of interest to ethnomethodology (most meticulously reflected in conversational studies) is what goes into the performance of an action: just how is one (here, now, in this place, with these people, engaged in the doing of this activity, having so far done these things, and having all this yet to do) to do the next thing that one does? Just what words, said in what fashion, accompanied by what other doings, in those circumstances, will serve to carry out the action that is on the agenda? Posing the question this way does not entirely reject the question of how actions are selected, but it *demotes* it, in favour of a deeper (that is, otherwise only presupposed) concern with the formation of the course of action: what specifically counts in real world conditions as the doing of action X or Y?

There are, then, multifarious (I might even say, tight) constraints upon what words, gestures, movements etc. can be put together, under these circumstances etc., so as to perform whatever **[p. 256 ↓]** action is to be undertaken. Amongst these constraints, there are, of course, those which pertain to the interactional organization of the occasion and to its collective construction. The formation of the action-to-be-done needs also to be considered from the way in which it contributes to the further organization of the relationships (whatever these might be) with other parties, and to the doing of whatever is required for the continuation of the organization of the occasion that the relationships and actions are carrying through. Saying just what words in just what ways could execute the step of (say) 'answering the police dispatcher's enquiries in the correct manner and in sufficient detail to ensure the initiation of the kind of police action I seek in response to my complaint and thereby close out my telephone call to the emergency number?'

Conversation analysis has a predominant focus on those constraints on the formation (the 'design' as it is often called) of conversational contributions that derive from the turn-taking character of conversations: 'sequencing' considerations most generally, and centrally those involved in the sequencing of turns at talk. The organization of conversation involves the more-or-less alternation of turns at talk (that is, more or less, one person talks at a time, though sometimes several do, and sometimes no one does) and therefore presents the issue of how many turns at talk does it take to do a given action? In other words, the issues arise around accessing the conversational 'floor'.

Patently, then, the talking needs to be done in ways that enable taking and keeping 'the floor'. The speaker's utterance may need to be constructed so that it effects either

These and a plurality of similar and related questions are not ones that can be answered in the abstract, and in advance, for they require that one look into actual cases to see how, in specific instances, these are done. Moreover (certainly in the beginning), it is not possible simply to pluck out instances of relevant occurrences and examine these for, given that the identity of any utterances hinges upon its contexting circumstances, one must examine much more of the conversational sequence (perhaps the whole of it) to be assured that the correct identification of the utterance's character has been made.

The conversational exchange has provided a perspicuous instance for conveying the idea of self-organizing activities, though, it must be stressed that conversation is not to be conceived as thereby more self-organizing than other activities.

Conversation analysis emphasizes the way in which the conversation which is put together through a succession of alternating turns at talk by different speakers is jointly assembled into a unified structure. The individual utterances are produced so as to comprise unified sub-parts of the conversation, such as, for example, talk unified around a single topic, with these sub-parts being further unified into a whole: the conversation has a beginning-to-end structure. Amongst the constraints which govern the formation of any single turn at talk, then, are those that:

The building of the conversation's organization, whilst it may be projected and anticipated, is none the less something that is constructed over the course of the conversation itself, something that is worked out in and through the talking. Though points (1) to (5) above are cast **[p. 257 ↓]** in terms that apply to a single person, it must be recalled that the production of the unity of the conversation is a collective affair, is done together amongst the participants, and is not, therefore, determined by any single participant. I have preferred the term 'collectively' to 'collaboratively' so as to avoid all implication that the construction of an orderly conversation precludes the possibility of controversy, dispute and conflict, whilst simultaneously stressing conversation's remorselessly interactional character.

Whilst the conversation has an overall organization, this is something that is managed on a turn-by-turn basis, with the form of the conversation's structural sub-divisions and their relation to its overall architecture being worked on as it goes along. The management of the relation between immediate past, the present and the projected next turns provides the place for the local work upon the organization of the conversation's course, for addressing practically, in the formation of the utterance, the issues of the continuation of the current phase of the conversation, the re-orientation of the direction of the course of this phase or its possible closure etc. Nothing about the conversation's course is fixed in advance, for that eventual course is to be worked on in the conversation itself: working out where the conversation is going, where it ought to go, and how to get it to go in the way that it should, *is* holding the conversation, not something done distinctly from talking through the conversation itself.

Plainly it is the case that the purely improvised character of conversations does not parallel the organization of many other kinds of occasions, for these involve doing activities that have been worked out in advance or that follow through routinized and standard courses of action. However, it is not by virtue of its entirely on-the-spot character that conversation provides a perspicuous example. The fact that other occasions are standardized and practised does not obviate the need for examination of their real time organization. The parties to such routinized occasions must none the less reciprocally find where they mutually are in the developing sequence, ready themselves for the anticipated next steps, form and implement the specifics of the action that will place that action in its appropriate, timely and proper place within the interactive progression of the occasion.

Recently, however, the issue of conversation analysis' ethnomethodological provenance has become a disputed issue, and there has been the expression, from the ethnomethodological side, of doubt about conversation analysis' *bona fides*. The controversy around this point is both complex and tangled, and I can only superficially diagnose the source of these doubts. There can be little doubt but that the publication of a systematizing paper setting out a synthesizing scheme for the depiction of conversational turn-taking provided the turning point (Sacks et al., 1974). The production of this paper signalled great success for conversation analysis, bringing together the detailed analyses of innumerable features of conversation's turn-taking organization within a concise summation. At the same time, it made, or brought to

fruition, a transition in the way in which the relation between conversational participant and the conversational analyst was conceived. The work of conversation analysis was becoming more a matter of administering a developed and formal analytical scheme to the further study of conversational materials. The possession and use of the scheme seemed to engender a distinct, specialized and professional competence that distinguished the analyst from the participant. The phenomena in conversation that were being sought and found appeared to be those which could only be noticed and identified by those operating with the guidance of the scheme. Thus, there was a divergence from the initial concerns of conversation analysis with the identification of those phenomena which anyone (at least anyone who could carry on a conversation, preferably in the appropriate natural language) can find, into an interest in phenomena in conversation that can only be found through the use of a specialized investigative apparatus.

I do not here comment on the rights and wrongs of the disagreements on this issue but mention it only to reinforce the point which has provided a central rationale for refusing the stock sociological concerns with the formation of theories and methods, namely, the determined insistence that the phenomena of interest are ones that 'anyone' (with the appropriate practical competences) can find. From this point it follows that ethnomethodology has no phenomena that belong distinctively to it, and that it has no need to develop a theory and method as means toward finding those phenomena, and for discerning their order. It has no version of social reality to offer on its own behalf. It offers only instigation to, and perspicuous examples in, recovering what, in one or another part of the social order, inhabitants already 'know' in the form of practical mastery of their everyday affairs (with 'everyday' being a relative term).

Fundamentals of Ethnomethodology

I have sought to draw out as clearly as possible the very distinctive nature of ethnomethodology's central concern, which has generated a **[p. 258 ↓]** pioneering exercise in developing a concern that arguably must underlie any rigorously thorough sociology but which has been—thus far – overwhelmingly distinctive to ethnomethodology, namely, what does it take to carry out a course of activity and, in

doing this, to carry out the society's routine, worldly affairs?[6] Ethnomethodology itself and the arguably conjoined field of conversation analysis have been dominated by their two founding figures, Harold Garfinkel and Harvey Sacks, and it is in their work that the basic conception and direction of the approach (as sketched out here) has been formed, with a number of other researchers following through on their initiatives. These researchers have generated a large corpus of studies, under Garfinkel's direct influence, of a wide variety of practical affairs, and under Sacks' influence, of aspects of turn-taking in conversation,[7] though the guiding principles have remained fairly constant. I have sought to draw out some of those central principles here, trying to spell out those that provide the rationale for ethnomethodology's insistence that it does not appropriately fit within the conventional sociological categories of either theory or method. It is not motivated by the aspiration to make *discoveries* about the nature of social phenomena, but to undertake the *recovery* of what is already known—but is 'known' in the form of competent mastery of practical affairs—to the members of society. In its preoccupation with this, it remains unique.

Notes

1 Bittner says (with organization theory specifically in mind, but making a more general point) that 'in general, there is nothing wrong with borrowing a commonsense concept for the purposes of sociological inquiry. Up to a certain point it is, indeed, unavoidable. The warrant for this procedure is the sociologist's interest in exploring the commonsense perspective. The point at which the use of commonsense concepts becomes a transgression is where such concepts are expected to do the analytical work of theoretical concepts. When the actor is treated as a permanent auxiliary to the enterprise of sociological inquiry at the same time that he is the object of its inquiry, there arise ambiguities that defy clarification.' Ethnomethodology's exercise could be seen as entirely engaged in attempting to achieve clarity with respect to the relationship between analytical and commonsense concepts.

2 This last requirement is assuredly satisfied by ethnomethodology's abjuring of claims to a proprietary methodological apparatus.

3 The requirement for 'unique adequacy' is one which calls for the use of hybrid competence, calling upon investigators of specialist competences to possess those competences, in order that they may themselves and autonomously make factual determinations of the character of what is done, achieved, found. Eric Livingston acquired mathematical skills in order that he might reflectively rework Godel's proof (Livingston, 1986). It is, however, Livingston's mathematical, not his sociological, competence which assures him of the soundness of the proof. His ethnomethodological reflections are on the ways in which mathematicians, himself now included, assure themselves of the soundness of proofs.

4 Lynch and Bogen (1996) studied the major public 'spectacle' of the Iran-Contra hearings. The fact that theirs was an analysis of a major national affair did not cause them to retract ethnomethodology's study policies. That this was such a national affair made no great analytical difference.

5 In which respect they are scarcely a paradigm of the full range of social phenomena, especially those situations which involve specialized competences and which are extensively opaque to anyone lacking the relevant competence.

6 It would be quite wrong to read into the expression 'to carry out the society's affairs' a preference for the study of compliant conduct, for compliance and defiance surely both number amongst 'the society's affairs'. The differences between them do not entail differences in the mode in which they may be analysed.

7 For a catalogue of writings up to 1990 see B.J. Fehr's bibliography in Coulter, 1990, which features some 3000 items.

Wes Sharrock

References

Bittner, Egon, (1973) 'Objectivity and Realism in Sociology' , in George Psathas (ed.), Phenomenological Sociology . Chichester: Wiley, pp. pp. 109–25

Bittner, Egon, (1974) 'The Concept of Organisation' , in Roy Turner (ed.), Ethnomethodology . Harmondsworth: Penguin, pp. pp. 69–81

Campbell, Colin, (1996) The Myth of Social Action . Cambridge: Cambridge University Press. http://dx.doi.org/10.1017/CBO9780511583384

Collin, Finn, (1997) Social Reality . London: Routledge.

Coulter, Jeff, (ed.) (1990) Ethnomethodological Sociology . Aldershot: Elgar.

Garfinkel, Harold, (1967) Studies in Ethnomethodology . Englewood Cliffs, NJ: Prentice-Hall.

Garfinkel, Harold, and Wieder, D. Lawrence, (1992) 'Two Incommensurable, Asymetrically Alternate Technologies of Social Analysis' , in Graham Watson, ed. , and Robert Seiler, (eds), Text in Context . London: Sage. pp. pp. 175–206

Garfinkel Harold, Lynch Michael, and Livingston Eric 'The Work of a Discovering Science Construed with Materials from the Optically Discovered Pulsar' Philosophy of the Social Sciences vol. 11 no. 1 pp. 131–58 1981

Holmwood John 'Feminism, Epistemology and Postmodernism: What Kind of Successor Science?' Sociology vol. 29 no. 3 pp. 419–20 1995

Kaplan, Abraham, (1998) The Conduct of Inquiry . NJ: Transaction Books.

Livingston, Eric, (1986) The Ethnomethodological Toundations of Mathematics . London: Routledge.

Lynch, Michael, (1993) Scientific Practice and Ordinary Action . Cambridge: Cambridge University Press.

Lynch, Michael, and Bogen, David, (1996) The Spectacle of History . Durham, NC: Duke University Press.

Sacks, Harvey, (1992) Lectures on Conversation , vol. 2 vols. Oxford: Blackwell.