ETHNOMETHODOLOGY: A CRITICAL REVIEW

Paul Atkinson

Department of Sociology, University College, Cardiff, Cardiff CF1 1XL, United Kingdom

Abstract

The paper reviews recent publications in ethnomethodology (EM) from a sympathetic but critical perspective. It is agreed that EM has made major contributions to sociological theory and to the empirical investigation of everyday life. A number of major reservations are made, however. The author suggests that some contemporary versions of EM—conversation analysis in particular—have an unduly restricted perspective. They give rise to a sociology which is behaviorist and empiricist, and which does not reflect the interpretative origins that inspired EM. Far from being a coherent and homogenous movement, the author suggests, EM is marked by inconsistency.

INTRODUCTION

This is a particularly appropriate time to review the recent past, current status, and future prospects for ethnomethodology. It is 20 years since the publication of Harold Garfinkel’s pioneering collection of essays (Garfinkel 1967). The recent republication (1984) of that volume is one of the many public signs of ethnomethodology’s firm status as a contribution to sociological inquiry. Its practitioners remain relatively few in number, but their collective output has been vigorous, marked by an unusually high degree of coherence. Its influence has been felt within the academic community beyond the relatively narrow confines of ethnomethodology per se. Considerable cross-fertilization has occurred between ethnomethodology and some branches of linguistics, while sociologists working within a broadly “interpretative” tradition have frequently incorporated insights and concepts from the ethnomethodological canon (though not always to the ethnomethodologists’ own satisfaction).

The sense of a mature and self-confident tradition has been encouraged by a
number of recent developments. First, several textbooks present a singularly authoritative view while rendering the approach accessible to the wider community (Heritage 1984, Sharrock & Anderson 1986, Benson & Hughes 1983, Livingston 1987). These have supplemented earlier introductory texts and primers (Leiter 1980, Handel 1982, Mehan & Wood 1975) and display the volume and variety of ethnomethodological work now published or in progress. Secondly, a growing number of well-edited collections of empirical papers have provided outlets for a substantial volume of research (Psathas 1979, Schenkein 1978, Atkinson & Heritage 1984, Sudnow 1972, Turner 1974, Button & Lee 1987). Ethnomethodological and ethnomethodologically informed papers have increasingly found their way into major journals of sociology, linguistics, and related fields, and several special issues have been devoted to the publication of ethnomethodological papers (e.g. Sociological Inquiry 50, Nos 3–4, 1980; Human Studies 9, Nos 2–3, 1986). As part of that progress, a portion of the previously unpublished work of Harvey Sacks has seen the light of day. Before his untimely death in 1975, Sacks exerted considerable influence through collaboration, teaching, and the circulation of transcribed lecture notes. His former collaborators and students have edited a number of papers for publication (e.g. Sacks 1978, 1979, 1984a,b). These have made aspects of Sacks’ thought more widely accessible and have progressively revealed him to be an observer of rare insight and imagination. Moreover, in very recent years Garfinkel himself has acted as general editor for a series of volumes reporting ethnomethodological work (Lynch 1985, Liberman 1985, Livingston 1986, Garfinkel 1986). Ethnomethodology continues to be greeted with mixtures of incomprehension and hostility in some quarters, but it is unquestionably a force to be reckoned with when it comes to the theory, methods, and empirical conduct of sociological inquiry.

The influence of ethnomethodology has been felt widely. In addition to studies of naturally occurring mundane occasions, ethnomethodologists have increasingly contributed to our understanding of social life in institutional settings: legal processes (e.g. Atkinson & Drew 1979, Lynch 1982, Maynard 1984, Pollner 1974, 1975, 1987); pedagogic encounters (e.g. Cicourel et al 1974, McHoul 1978b, Mehan 1979); medical consultations (e.g. Fisher & Todd 1983, West 1984, Heath 1986); scientific work (e.g. Garfinkel et al 1981, Lynch 1985, Livingston 1986); political meetings and confrontations (e.g. J. M. Atkinson 1984; Molotch & Boden 1985, Heritage & Greatbatch 1986). The considerable influence of ethnomethodology has been established beyond its own adherents by virtue of those contributions to more traditional areas of empirical sociology. The findings of ethnomethodology have contributed directly to our broader understanding of organizations, diagnoses and assessments, the social production of ‘facts’ and the construction of written and spoken accounts. They bear directly on the analysis of rationality,
practical reasoning, the achievement of everyday reality and normal appearances, moral assessments and categorizations, and intersubjectivity.

The boundaries of ethnomethodology are difficult to draw, even for the purposes of an essay such as this. While there is a readily discernible “core set” (Collins 1985) of practitioners in North America and Europe, whose work is densely cross-referenced and collaborative, there is also a considerable penumbra of authors who base their work directly on an ethnomethodological inspiration, yet do not appear in the corpus of the core set. Likewise, there are major figures who have been associated at one time or another from the earlier years and yet are not unequivocally granted legitimacy as core members, or whose work has proved more eclectic. The contribution of Aaron Cicourel is a case in point. Although he contributed to the early popularization of ethnomethodology and his critique of sociological methodology has been cited with remarkable frequency (Cicourel 1964), his later work, with its special emphasis on cognitive competence (e.g. Cicourel 1973, 1980) is not widely regarded as conforming to the mainstream of ethnomethodology.

In the current state of its development, “ethnomethodology” in general is not easily distinguished from the more specific development of “conversation(al) analysis.” The latter pays special attention to the fine-grained analysis of naturally occurring spoken interaction (and, more rarely, to the coordination of spoken and nonverbal activities); this is the most consistent and productive area in the tradition. Not totally distinct from other domains of ethnomethodological work (much of which draws on its findings), conversation analysis has its own characteristic set of concerns and procedures. Here there is no attempt to summarize and review all the detailed findings of conversation analysis. However, in certain respects conversation analysis has diverged from ethnomethodology’s original inspiration. In particular a tension exists between the specific treatment of conversation’s sequential order and more general interests in mundane reasoning.

This review is focused primarily on recent publications. Nevertheless, it is helpful to refer from time to time to earlier formulations and origins. This is done to assess the relationship between earlier inspirations and contemporary realizations of the program, as well as to pay some attention to some recent reformulations of ethnomethodology’s foundations. The general perspective of this review is sympathetic but not uncritical. Appreciative of its achievements I nevertheless argue that certain limitations and ambiguities exist within the program as a whole. The net effect of that may be to convey too negative a tone. The achievements of ethnomethodology are largely taken as read and not summarized here, while critical debate is engaged. For recent reviews that summarize many contributions, the reader in referred to Heritage (1984, 1987) among others.
The current state of ethnomethodology may be explored in part by examining some of the most recently published work by Garfinkel and his collaborators. These studies have been focused on the performance of "work" in a variety of settings. They exemplify the empirical thrust of contemporary scholarship in the field. A much-quoted source and justification for this program of work is referred to as "Shils' complaint" (cf Garfinkel et al. 1981, Heritage 1984, p. 301). In essence, this points out that in many if not all conventional treatments of "work" and similar organized activity, sociologists have paid remarkably little attention to the everyday practices of work itself. While close attention may be paid to such matters as organizational rules (both formal and informal), the development of occupational careers and identities, the negotiation of divisions of labor and so on, there is little or no equivalent concern for occupational practices themselves. In contrast, therefore, this aspect of the ethnomethodological program seeks to provide a detailed, naturalistic account of competent practice within specific domains of socially organized action. It addresses what Garfinkel and his colleagues refer to as the "quiddity," the "just whatness," of work itself.

There are clear continuities between this recent collection of work and earlier studies. Garfinkel's own study of clinic records (1967), for example, emphasized the situated production and reading of case notes in the constitution of the clinic's work. Likewise, Zimmerman's research on the routine practices of a welfare agency (1969a,b, 1971) concentrated on the "practicalities of rule use" that were enacted in the agency members' everyday actions. While taking a somewhat different empirical focus, some features of the recent work studies parallel the ethnomethodological corpus of conversation analysis. There is an underlying preoccupation with the temporal features of mundane occupational practice. Work is explored as an array of material objects and their manipulation, distributed in space and time. The work studies have included considerable attention to the practical accomplishment of science and mathematics. Lynch, Livingston and Garfinkel (1983) epitomize the approach. Their discussion is based on a synthesis of several studies and is sustained by the "attempt to rediscover the problem of social order in and as the real-world detail of scientific praxis" (Garfinkel 1983: p. 205). Scientific work is thus explored not in terms of its logic, epistemology, or paradigms, insofar as they imply cognitive resources and relationships antecedent to scientific work itself. Their approach to reasoning focuses rather on what they call its "worldly observability": "This means that reasoning is displayed in the midst of orders of intersubjectively accountable details."

The order of such social activity is largely a temporal order. Scientific phenomena, the authors argue, are built out of the sequential arrangements of
talk, and the sequenced tasks of handling experimental materials, equipment, and so on. The competence of the scientific worker is thus portrayed as the "embodied" knowledge of the skilled craft worker. A student experiment by Schrecker is reported to show, for instance, how the instructions for conducting an experiment are enacted only through an exploration of the physical equipment and its spatial arrangement. The experiment was found not to reside in the prescriptions of written instruction nor to be represented in what the authors call "canonical descriptions." Rather, "the sense of what the instructions instructed was found by turning to that lab bench and bodily engaging a complex of equipment to perform chemistry's events" (Lynch et al 1983: p. 212). Essentially the same approach is exemplified in the authors' study of the discovery of a pulsar (Garfinkel et al 1981). Scientific discovery here is treated as a practical worldly activity of scientists, rather than as a matter for philosophical legislation.

This style of analysis of scientific work is sustained in Lynch's monograph on a research laboratory (1985), which has a strong emphasis on the temporality of lab work. Lynch describes how the laboratory's members operate with a sense of projects that interrelate sequences of activity. The organization of scientific work is described as a complex array of time-space relationships. The comprehensibility of such work is to be found in that very organization. Although the ethnomethodologists do not discuss the issue explicitly, their emphasis on reason as practical and observable worldly conduct establishes a clearly pragmatist flavor to their accounts of knowledge production.

Lynch, Garfinkel, and their colleagues seek to distance themselves from the constructivist view of scientific knowledge. They erect an oversimplified model of the constructivist position, claiming that it represents a "philosophy that remains endlessly embedded in academic arguments about science with no attention being paid to the endogenously produced variants and argument that constitute the technical development of ordinary scientific inquiry" (Lynch et al 1983, pp. 224–25). In a similar vein, Lynch (1985, p. 274) implies that all constructivist accounts treat scientific knowledge as "essentially artifactual." In contrast Lynch claims to treat the identification of artifacts in the laboratory as a practical concern for its workers—and hence as a situated, observable matter rather than an issue for general epistemological principles.

The authors' self-characterization is useful in highlighting some important features of this ethnomethodological program. Although they sometimes exaggerate the contrasts with comparable work in the sociology of science (e.g. Latour & Woolgar 1979, Knorr-Cetina 1981), their emphasis on observable activity and its sequential arrangement is distinctive. As Latour (1986) points out, Lynch's monograph is not "about" science at all. He is indifferent to any debates about the nature and status of scientific knowledge; he makes virtually no attempt to identify any distinctive characteristics of
scientific work. This monograph about activities in a laboratory is, Latour suggests, “not about science, but about ‘some people at work’.” Indeed it would appear that precisely the same general set of observations could be made about any domain of human endeavor. Any one of them could be represented in terms of sequences of observable activities. In this vein, the social accomplishment of work has an extremely restricted denotation. Its orderly character is not to be found in occupational cultures and social institutions. Rather, the work is said to be “self-explicating.” That is: “The activities are reflexive, self-organizing, organized entirely in situ, locally” (Livingston 1987, p. 10).

The most striking writing in this vein is Livingston’s analysis of the work of mathematical reasoning (1986, 1987). By taking the reader through a proof of Goedel’s theorem, Livingston attempts to demonstrate that mathematical rigor resides in the local sequences of actions produced by mathematicians. Here is the most radical exemplar of all ethnomethodological studies of work: matters which are classically treated as context free and independent of human agency are here represented in terms of their detailed real-world enactment. Livingston’s blow-by-blow recapitulation of a mathematical proof is a vivid example of the analysis of quiddity in occupational and scientific work. It epitomizes the ethnomethodologists’ emphasis on sequences of practical activities in describing the “just whatness” of competent work.

There are, therefore, self-imposed limitations that are extremely radical in their consequences. In the emphasis on description of work “from within” and the absence of any sociological preoccupations, the analyst seems bound merely to recapitulate the observed sequences of activities with little or no framework for selection, or for the representation of those activities in any other discourse (cf. Latour 1986). The radical stress on observable detail risks becoming an unprincipled, descriptive recapitulation devoid of significance. The stance advocated by Garfinkel, Lynch, and Livingston is reminiscent of the French nouveau roman of authors such as Alain Robbe-Grillet: minute descriptive detail is assembled in a hyper-realist profusion, until the reader loses any sense of meaning. Livingston’s mathematical reworking is likely to strike the sociological reader in precisely that way.

The stance is sometimes justified by appeals to canons of natural science which are themselves problematic. The studies-of-work program is claimed to aspire to a state productive of

... simple observational sciences capable of depicting the mundane details of ordinary human activities with the same measure of precision and having similar mechanisms for resolving disputed claims as were achieved, for example, by the naturalists of the nineteenth century. (Heritage 1984, p. 301)

But such appeals are surely inadequate. First, they are based on unduly limited appeals to natural-scientific discourse. While it is perfectly true that
close, precise observation is integral, the achievements of science transcend empiricist and inductivist description. Given the contribution of a Darwin, a Wallace, or a Mendel, the restricted emphasis on observation is ironic. It is doubly ironic given the indifference or agnosticism expressed by authors like Lynch concerning the distinctiveness of science per se, as opposed to the distinctive practices of any particular domain of work.

What is clear is that contemporary ethnomethodology (in this manifestation) has shifted dramatically from a concern with social action in any classical sense of that term. The move from action to activity is indicative of an ambivalent relationship with phenomenological or verstehende antecedents. Peyrot’s treatment of ethnomethodology’s critics renders activity central (Peyrot 1982). Throughout his discussion he emphasizes ethnomethodology’s primary concern for relations between activities rather than for their meaning. Organized action is thus portrayed as a concatenation of signs, not in terms of the relations between signs and their referents as in most interpretative versions of sociology. Peyrot’s remarks are embedded in a discussion which seeks to deny any interpretative or phenomenological concerns. It is in that context, therefore, that Zimmerman (1978) distinguishes ethnomethodology’s current practice from its origins in phenomenology, as does Psathas (1980) in his discussion of the convergences and divergences between phenomenology and ethnomethodology. In these contemporary versions of ethnomethodology, meaning is granted only the most restricted of functions. It refers to the location of activities in sequential environments; “Action is intrinsically meaningful, not because it is meaningful outside of any concrete situation, but because it is always embedded in a concrete situation” (Peyrot 1982, p. 272). According to this perspective ethnomethodology is defined as the study of “the organization of everyday activity.”

From this point of view, therefore, social order is appropriately investigated through the study of sequential activities, such as queues, traffic flows, conversational turn-taking (cf Livingston 1987). The orderliness of social life is represented as the “order” in which things take place. Despite its origins (or at least its reconstructed logic) in a debate with Parsonian concerns for the problem of social order and action, and a phenomenological interest in members’ practical reasoning, contemporary writing by some ethnomethodologists has transformed these into an approach with a more structuralist and behaviorist flavor. As I argue below, that approach is to be contrasted with other strands in the ethnomethodological program which continue to embody more hermeneutic and interpretative concerns.

SEQUENCE AND LANGUAGE

The centrality of sequence is, of course, most apparent in the contemporary wing of ethnomethodology that addresses itself to “conversation(al) analysis.”
Here social action and social order are most transparently treated in terms of temporal concatenations. In this and the following section no attempt is made to produce a comprehensive review or to duplicate Schegloff's contribution (q. v.).

The analysis by ethnomethodologists of spoken interaction has resulted in an impressive corpus of work. It was pioneered by Sacks, Schegloff, and Jefferson (e. g. Sacks et al 1974; Sacks 1972a,b; Schegloff 1968, 1972, 1973; Schegloff & Sacks 1973; Jefferson 1972). Many of the empirical findings of conversation analysis show how parties to talk do organize and are sensitive to its sequential arrangement: its openings and closings, turn-taking, overlapping speech and so on. The local organization of conversational materials is displayed through attention to the phenomenon of adjacency of utterances (Sacks 1972b, Schegloff 1972). The sense of an utterance and its interactional force are dependent on its relations with prior and following turns. These observations concerning the organization of turns-at-talk have been developed by numerous studies of conversation which have elaborated the close relationship between sequential arrangements and their moral consequences for speakers and hearers (cf Pomerantz 1978, 1984; Davidson 1984; Jefferson & Schenkein 1977; Drew 1984; Schegloff 1980; Jefferson 1974, 1979, 1980; Goodwin 1981).

The formal, sequential properties of talk have also been investigated in a range of organizational settings. The patterns of turn-taking and rights to speak in classrooms (e.g. McHoul 1978b, Mehan 1979), courtrooms (e. g. Atkinson & Drew 1979) and clinics (e. g. West 1984, Heath 1986) display the asymmetry of such interactional settings. The situated rules of talk embody the differential distribution of power and authority. Likewise, relations between the sexes are enacted through unequal participation in talk (West 1984, West & Zimmerman 1977).

In general, conversation analysis has proved a major contribution to the investigation of the interaction order. It has illuminated many institutional and organizational settings. It has drawn attention to the detail and complexity of everyday life, and to the delicacy with which participants monitor the unfolding conversation as they collaborate in its production. Nevertheless, there are some general reservations to be entered.

Sacks (1984a, p. 26) declared that his use of conversational materials was based not on the attachment of special status to those data, but on the fact that they could be used for repeated, detailed scrutiny. Despite this apparent treatment of conversation as having no more significance than an objet trouvé, subsequent practitioners have indeed attempted to invest transcribed conversational recordings with a special significance.

It would be tempting, but misleading, to assume (pace Sacks) that this focus on language stemmed from theoretical interests shared with a variety of
interpretive sociologies, such as phenomenology, hermeneutics, or symbolic interactionism. Those perspectives treat the faculty of speech as a primary, socially shared resource in the construction of meaning and the constitution of everyday reality. The ethnomethodological treatment is different, however. Conversation is regarded as a form of collaborative conduct. There is much less concern with the explication of meaning than with the discovery of competences or methods whereby speakers generate orderly sequences of activity. (See Hammersley 1986 for a full discussion of the ambiguities of action and competence models.)

There is reference to such interpretative frameworks as "the definition of the situation," but this is used to refer primarily to conversationalists' orientations to sequential propriety (cf Heritage & Atkinson 1984, p. 6). Members' interpretive work is spoken, collaborative activity thus refers, in this context, to the structural properties of the talk itself. As in the study of work, sequential activity is explicated in terms of its endogenous, self-explicating features. In keeping with this general approach motive is not addressed in understanding social action. Language is not treated here as a medium for intentional, motivated social action.

In Garfinkel's original formulation of ethnomethodology's distinctive contribution, he portrayed the homunculus of Parsonian sociology as a "judgmental dope." There was a clear rejection of any assumption that social actors' knowledge of the everyday world was irrelevant or trivial. The phenomenological treatment of the life-world, on the other hand, gives special emphasis to the commonsense understandings of practical actors. Their knowledge is thus to be treated seriously. Indeed, everyday knowledge is regarded as foundational. In drawing on both Parsons and Schutz, Garfinkel's early inspiration was to reject the judgmental dope image in order to focus attention on the skillful and artful, methodical work put into the production of social order. In the intervening years, however, some versions of ethnomethodology have returned to the judgmental dope as their model actor. Intentionality and meaning have been all but eliminated. This is forcefully argued by Yearley (1984: pp. 93–94) and Bruce & Wallis (1983). The latter have given rise to a lively debate on the issue (see also Bruce & Wallis 1985; Sharrock & Anderson 1984). Intentionality and motive are not admitted as sources for interpreting social action. The ethnomethodologists insist that motivational accounts are themselves a form of action, in the tradition of Mills (1940), Scott & Lyman (1968), or Blum & McHugh (1970). Hence, the study of motivated action is translated, as Yearley (1984) puts it, into the study of "the anatomy of accounts." Where motives are admitted, they operate only in terms of the actor's competence in the production of orderly sequences of talk, or in terms of unacknowledged ad hoc accounts of interactants' motives in the explication of particular action scenes. In the absence of motives
and interests, it is not surprising that some commentators revert to a functionalist mode of explanation of social order. Heritage (1984, p. 265) for instance argues that:

there is a 'bias' intrinsic to many aspects of the organization of talk which is generally favorable to the maintenance of bonds of solidarity between actors and which promotes the avoidance of conflict.

Contemporary justifications of ethnomethodology, therefore, in their stress on activity and sequence have modified the connotations of action and order. If the model actor is not quite the Parsonian ignoramus, then at least he or she is frequently represented as a mere exponent of sequenced activities. In the case of conversation analysis, the complex phenomenological and sociological problems of mutual understanding between actors are rendered through the mutual monitoring of utterances for their completions, overlaps, and the like. There is no doubt that conversation analysis has succeeded in demonstrating some remarkably delicate and fine-grained patterns of behavior in collaborative talk. Whether it has preserved the sociological spirit of the program is another matter.

The contemporary treatment of "indexicality" is a case in point. It is well known that the issue of indexicality was a key one in Garfinkel's early work. It refers to the inescapable and primordial fact that natural language use is dependent upon contextual features. It is a radical development of logicians' and grammarians' treatment of the class of indexical terms such as "here" and "there" which depend for their reference entirely on the context of their use. In some early misunderstandings of the work it was thought that indexicality referred simply to the supposed need to study meanings in context—by reference to some sort of holistic ethnography. Likewise, as Heritage (1984, p. 136) points out, the heterodox but influential contributions of Cicourel and Douglas were read to imply that indexicality rendered sociological research impossible. But Garfinkel's early observations concerning indexicals (not in itself an idea original to ethnomethodology) were to stress action. If the use of language implies the use of "indefinite resources" to arrive at an adequate sense of certitude, then speakers and hearers have to work at contextualizing talk. The identification of indexicality as an irreducible and inescapable feature of everyday life thus focuses attention on members' practical reasoning, embodied in the documentary method of interpretation. Subsequently, however, the treatment by some authors has shifted in subtle but significant ways. Peyrot's essay (1982) on some common misconceptions concerning ethnomethodology spends some time on indexicality. He quite rightly recapitulates the misconception that indexicality is a problem of ambiguity or unintelligibility. But in doing so he underplays the implications for actors' judgments and practical reasoning. In common with other contemporaries
Peyrot emphasizes sequences of activities as the primary focus. He argues that indexicality is not a problem for actors because "each indexical action participates in the organization of activity of which it is a constituent part and obtains its definite, particular features by virtue of its participation in that ongoing setting of organized activity" (p. 270), and he stresses that analysis is concerned with the relation of activity to activity, not the relation of action to meaning: "In the case of conversation, even though it is a language activity, ethnomethodology is concerned with the organization of the activity carried on through talking, not with the referent of that talk" (p. 269). This formulation of the topic means that the context of talk's significance is limited to its sequential environment.

Reference has been made from time to time to ethnomethodology's recognition of the temporality of social activity. This is derived, in part at least, from the Parsons and Schutz background. Schutz in particular had emphasized the temporal framework in which actors' understandings are located. The retrospective-prospective nature of interpretation, whereby the present is understood in relation to an unfolding horizon of past and future, is a major contribution to interpretative sociology. In Schutz and other phenomenologists, time and time-consciousness are fundamental to the actor's experience of the life-world. In contemporary ethnomethodology—conversation analysis in particular—the temporality of social life has again been assimilated to the one-dimensional form of sequence. Time enters into this version of social life primarily through the step-wise, turn-by-turn construction of conversational order. Time becomes equated with the concatenation of activities. The retrospective-prospective nature of understanding is admitted in the specific and restricted sense that the significance of utterances is dependent upon their sequential relationships. Schegloff & Sacks (1973), for example, emphasized the importance of such sequential relationships: "By an adjacently produced second, a speaker can show that he understood what a prior aimed at, and that he is willing to go along with that. Also, by virtue of the occurrence of an adjacently produced second, the doer of a first can see that what he intended was indeed understood, and that it was or was not accepted. . . . It is then through the use of adjacent positioning that appreciations, failures, correctings, et cetera can themselves be understandably attempted" (pp. 298–98).

There can certainly be no doubt as to the empirical observations concerning the relevance of sequential placement in talk. Nevertheless, this is a limited view of the temporality of social life. The emphasis on sequence does little justice to social actors' complex temporal relations and time-consciousness or to contemporary sociological treatments of time (e.g., Bergmann 1981, Giddens 1981, Jaques 1982, Schoeps 1980). Coulter (1983) has criticized the restricted treatment of sequence and meaning. He points out that Sacks,
Schegloff, & Jefferson (1974), and work derivative of that influential paper, rely on an unduly restricted view of the retrospective-prospective nature of meaning. They identify the status of a prior turn/utterance by reference to subsequent turns. But, as Coulter points out, this is inadequate, as the analyst is not entitled to adopt as determinate the next speaker's apparent analysis of a prior turn. Coulter criticizes the conversation-analytic approach as an oversimplification and concludes that "sequential location cannot tell the whole story, no matter how much of an advance such a focus may be over the decontextualizing propensities of certain sorts of speech-act analyses" (p. 370).

LANGUAGE, PRESENCE, AND DESCRIPTION

Although Sacks had claimed no special status for conversational materials, it is clear that mundane conversation has been accorded a special primacy in much subsequent work. Mundane conversation is treated as standing for a privileged domain of everyday existence. The social activity of unremarkable talk stands, in ethnomethodological research, in a position equivalent to "commonsense." The special status accorded both reflects a persistent preoccupation, and a problem, in the tradition.

Ethnomethodology has inherited the phenomenologists' construction of a primary reality in the ordinary world as experienced in the "natural attitude" (cf Pollner 1987). Other domains of experience and reality are depicted as derivations from that primary and privileged sphere. The ethnomethodological realization of that position has resulted in the following characteristics of the program. First, despite methodological maxims to the contrary, the domain of commonsense has frequently been invoked by fiat, used as an interpretative device while simultaneously constructed by the discourse of ethnomethodology itself. This point has been made from within the ethnomethodological fold as well as from without (McHoul 1982). Secondly, from its inception, ethnomethodology has rested on a distinction between commonsense (or lay) and scientific (or analysts') understandings. This issue is so fundamental that some reference to the early literature is on order. Sacks' (1963) early paper on sociological descriptions was very influential. He charged that sociology's construction of the objects of its discourse was flawed. Introducing the distinction between topic and resource, he argued that conventional sociological descriptions were confounded with commonsensical cultural knowledge which remained tacit. The charge, then, was that unexplicated resources were drawn on. Sacks contrasted that state of affairs with an ideal science which aims to produce a literal description of its subject matter. Ethnomethodologists claimed subsequently to have expunged the flaws of conventional sociology by topicalizing unexplicated resources.
Tacit cultural knowledge was to be brought to consciousness and transformed into the topic of inquiry.

Now it is not at all clear what could possibly count as a literal description in the sense intended here by Sacks (however ironically or ideal-typically it is meant). And it is certainly the case that no actual science as practiced (as opposed to its reconstructed logic) operates without tacit knowledge. But, as we have seen in relation to the studies-of-work program, science has been appealed to by ethnomethodologists in a strangely uncritical fashion at times. Garfinkel (1960) appears to draw a sharp categorical distinction between science and practical (everyday) rationality. Sciences are characterized as operating:

in such a way (1) that they remain in full compatibility with the rules that define scientifically correct decisions of grammar and procedure; (2) that all the elements be conceived in full clearness and distinctness; (3) that the clarification of both the body of knowledge as well as the rules of investigation and interpretive procedure be treated as a first priority project; and (4) that the projected steps contain only scientifically verifiable assumptions that have to be in full compatibility with the whole of scientific knowledge. (Garfinkel 1960, p. 76)

Nobody could possibly object to the ideal of rendering concepts and procedures as clear and coherent as possible (and as far as is practicable, a qualification which Garfinkel seems to discount). As a heuristic principle, attention to the otherwise neglected features of everyday life has clearly been productive. Yet it is not necessary, nor self-evidently valid, to appeal to a positivist rhetoric of science and so stipulate a radical disjuncture between practical and scientific reasoning, between the mundane and the technical. There is a double irony here: Garfinkel’s appeal to science is itself rhetorical, and ethnomethodological studies of science have thoroughly dissolved the distinctiveness of scientific work anyway. The implication that there can be an absolute distinction between topic and resource was misplaced, as was the corollary that conventional sociological descriptions should somehow be suspended sine die until the foundational work had been accomplished. This objection has been voiced by McHoul (1982), who argues that ethnomethodology’s apparent search for presuppositionless description would be fruitless and misguided, while Pollner (1987, p. 149) has also argued that “mundane reason is essentially and unavoidably part of the investigative attitude of ethnomethodology.”

It is a corollary that ethnomethodology should construct the purely mundane in contrast to the scientific ideal. Mundane conversation is the prime candidate for the privilege of untrammeled, untheorized everyday activity. In practice, its position derives not from Sacks’ claim that it just happened to be available for inspection, nor from the centrality of language to social life in general. (The two possible justifications seem odd when thus juxtaposed in
any case.) There is a strong assumption that the transcribed materials of conversation approximate to an unmediated, literally represented social reality. One is reminded of Derrida’s critical analysis of the privileged presence of speech in the human sciences (Derrida 1976). For the conversation analyst, mundane conversation has the authenticity of presence, literally transcribed. (For a somewhat different juxtaposition of Derrida’s deconstructive turn and ethnomethodology, see Frank 1985.)

It is this attitude which has contributed to the distinctively empiricist and inductive rhetoric of much contemporary ethnomethodology—conversation analysis in particular (e.g. Wootton 1981). It is the extreme outcome of what Mehan & Wood (1975) identify as the logico-empirical (as opposed to the hermeneutic-dialectic) tendency in the ethnomethodological project. In fact, however delicate the transcription devices used (and conversation-analysis does not normally employ great delicacy with regard to most features of speech), the materials are inescapably theorized representations. The selection of linguistic features for representation, and the very construction of an object of description as conversational are strongly implicative. The plausibility of conversation analysts’ descriptions necessarily depends upon the reader’s everyday competence in deciphering standardized orthographic representations of naturally occurring speech. The latter is not a literal description, but depends on the reader’s reception of the ethnomethodological text. (For the necessarily theoretical character of transcription, see Ochs 1979.)

Arguably, it is the strongly empiricist and inductivist strand which has led to a particular confusion within conversation analysis. Again, the critique comes from within ethnomethodology. Coulter (1983) has pointed out a failure to distinguish between contingent and a priori structures in sequential analysis. He argues that conversation analysts have typically resorted to arguments concerning the distribution and co-occurrence of utterance types (cf e.g. Schegloff 1968, 1979), whereas many of the structural components of conversation have conventional relationships and form a logic for conversational sequences. It is a matter of convention that answers follow questions, not a contingent issue. While the detailed analysis of transcribed materials may be valuable heuristically, conversation analysis should not confuse the conventional and the contingent. Coulter’s remarks suggest that criticisms of authors such as Goffman from within ethnomethodology for not relying on inductive analyses of a large corpus of data are overemphasized.

NARRATIVES, TEXTS, AND RHETORIC

Many ethnomethodologists pay special attention to mundane spoken activity, but there are others attend to written sources. This is one aspect of ethnomethodology that shares empirical and analytic concerns with other
major areas of social and cultural studies, that is, the study of methods whereby spoken and written accounts or texts are produced and read. Here, apart from the relations with other aspects of ethnomethodology, the work relates directly to contemporary issues in literary theory and linguistics.

Although some branches of ethnomethodology have privileged spoken interaction, there is no reason why written materials should not be subjected to equivalent analyses. McHoul (1978a,b,c, 1980, 1982) has made a sustained contribution in this field. His work includes experimental "breaching" and naturalistic studies of reading as a process of practical reasoning. For instance, in a manner analogous to Garfinkel’s and McHugh’s experiments on reality-definition (Garfinkel 1967, McHugh 1968), McHoul examines how readers use the documentary method to make sense of a randomly generated poem and a scrambled section from a novel. The studies recapitulate the general orientation that finds members’ methods to be demonstrably rational and methodical. They also remind us of the specific issue that written texts—like spoken utterances—cannot unequivocally determine how they shall be received. Reading is an active undertaking. The reader’s task is to work on the text (however unself-consciously).

The approach is reminiscent of literary theories such as reception theory (e.g. Iser 1978) which also stress that the meaning of a literary (indeed, any) text is to be found not in the words, but in the interactive process between text and reader. While reception theory per se does not exhaust the field by any means, many versions of literary theory pay close attention to the complex interplay of cultural resources and textual conventions which enter into the production and consumption of literary works. Many of those current perspectives derive from quite different intellectual origins—most notably structuralist and poststructuralist inspirations—although the structuralist tendency in ethnomethodology implies convergence in practice. The critical and ethnomethodological emphases converge at many points. In their various ways they stress the documentary method by which meaning is read into the text. As but one specific example, critical theorists have commented upon the propensity of readers to naturalize texts which do not correspond to the conventional canons of realism, by finding commonsense naturalistic frameworks with which to interpret them (cf Culler 1975). There is a direct parallel here with ethnomethodologists’ observations of members’ methods for deriving sense from problematic, disordered textual materials. Garfinkel’s early work on the competent reading of fragmentary case-notes in a clinic is a classic case in point (Garfinkel 1967). Garfinkel demonstrated how the clinic personnel were able to read into the notes what they knew of clinic routine, and so find evidence of clinic work in the notes.

Ethnomethodologically inspired or influenced analyses have also been used to reveal the conventionality of scientists’ written texts. In terms of its
conventional status, scientific writing is commonsensically understood to approximate to "degree zero" writing (Bazerman 1981). But it can be thought of more productively as a highly contrived medium of persuasion. The natural scientific text, such as a research paper, is not a neutral descriptive representation. As a number of authors have demonstrated, papers depend upon an array of rhetorical devices to portray their findings as factual and well-founded. Woolgar (1980), for instance, analyzes a series of devices in scientific papers which accomplish the orderliness of the text and provide for ways of reading them as accounts of discovery. The textual organization of the paper itself, the conventions available to both reader and writer, provide implicit instructions for the reading. The readers can discover a series of reported events to be purposeful and rational; the discovery is itself found to be the outcome of a coincidence of inevitable occurrences and the researcher's actions. In a similar vein, Gilbert & Mulkay (1980) document how natural-scientific papers render their accounts meaningful by means of a restricted repertoire of rhetorical devices. Those accounting methods sustain a traditional conception of scientific knowledge production while eliding exogenous modes of explanation. (Gilbert & Mulkay's extensive treatment of scientists' talk will be returned to below.)

Of course, the ethnomethodological version of critique can equally be applied to the texts produced by sociologists. It is congruent with the program's orientation to sociological inquiry that the work of sociologists (and other social scientists) should be a topic of scrutiny. Sociologists inescapably employ rhetorical conventions in their construction of plausible descriptions and arguments. (For a sustained treatment of the topic from the perspective of classical rhetoric rather than ethnomethodology, see Edmondson 1984; for comparable discussions of anthropology, see Clifford & Marcus, 1986; in economics, see McCloskey 1985.) In an ethnomethodological vein, Anderson (1978) provides one analysis of the textual construction of sociological categories and arguments. He shows how each text constructs its rationality and plausibility through a series of conventional devices.

As McHoul (1982) points out, the perspective can also be turned to the texts of ethnomethodology itself. Indeed, his own work has a double intent: ethnomethodology of reading and a reading of ethnomethodology. He reminds us, for instance, that Garfinkel's original write-up of one of his experiments (1967, p. 89) "leaves open to readers' procedural knowledge of 'how such things are done' the matter of connecting up the data and the findings" (McHoul 1982, p. 16). More generally, ethnomethodology is as much a domain of conventionalized textual practices as any other.

The ethnomethodologists' analyses of methods for the production of plausible descriptions and arguments have some clear affinities with the descriptive machinery of classical rhetorics, as well as structuralist cultural theory. The
prescriptive models of the rhetoricians can equally be thought of as a repertoire of methods for the practical accomplishment of accounts, and the invitation of sympathy from the hearer/reader. Atkinson’s analysis of political rhetoric is one of the few where such a parallel is noted explicitly (J. M. Atkinson 1978). Drawing on the conversation-analytic approach, Atkinson has dissected political oratory, with special attention to the invitation of applause. (His is a modern restatement of the use of *clap-trap* in its original sense.) He shows how political speakers construct their speeches to project points where applause is elicited. The technology of permanent recordings has now transformed the classical canons of rhetorical maxims into a detailed, descriptive account.

Atkinson’s ethnomethodological analysis of politicians’ rhetoric underscores the potential convergence with that branch of anthropology known as *ethnopoetics*. The latter—admittedly a minority concern among anthropologists to date—pays close attention to the verbal arts (e.g., of narrative performance) that characterize a given culture. Ethnomethodology’s close attention to practical rhetoric, coupled with more anthropologically inspired perspectives (derived from the ethnography of speaking and ethnomethodology itself), suggest important and powerful ways of understanding occupational and organizational accomplishments. For recent examples of work in this vein which derive at least partial inspiration from ethnomethodological sources, see Pinch & Clark (1986) and Pithouse & Atkinson (1988).

Reference here to rhetoric should not be taken to imply merely decorative, epiphenomenal matters. The ethnomethodological treatment of rhetoric, narratives, and accounts stresses the extent to which they do not merely describe reality but actively constitute realities through those selvesame descriptions. This property—the “reflexivity” of accounts—colors all ethnomethodological analyses; the general area has been reviewed succinctly by Heritage (1983). The perspective derives directly from Garfinkel’s original formulation of the relationship between actions and accountings and their mutually elaborative character (Garfinkel 1967). It is exemplified in the now classic study by Wieder (1974) of the convict code. Wieder’s ethnography of a halfway house showed how a set of maxims, of an indefinite and general nature, was used as a framework of description and injunction in making sense of the organizational setting for its inmates. The convict code produces and sustains the institutional reality it comments on and regulates.

The flexibility and indefiniteness of accounts mean that they may be used to express and reconcile what (to the observer) appear otherwise to be discrepant versions. This is particularly well exemplified by Gilbert & Mulkay (1984) in an analysis of natural scientists’ accounts of their work. The authors show how two alternative versions of scientific discovery are expressed. The scientists describe discoveries in terms of the standard, positivist rationality
and the inexorable progress of scientific knowledge. At the same time they trade in accounts expressing the contingent nature of discoveries—couched in terms of personalities and local circumstances. The sociologist confronted by discrepant versions is not to adjudicate between them in search of the "right" account which supposedly corresponds to real events. He or she is not to try to use the accounts as evidence of "what scientists really do." Rather, as with the analysis of written texts, the point is to understand how the accounts are constructed and enacted. Gilbert & Mulkay's analysis of the reconciliation of discrepancy is a special case of a topic highlighted by Pollner (1974, 1975, 1987), who draws attention to some general accounting methods whereby discrepant evidence or experience about the world can be resolved. In yet more general terms, these accounting devices permit the preservation of normal appearances as they permit actors to accomplish ordinary realities (Sacks 1984b).

FACTS, ASSESSMENTS, AND DIAGNOSES

In common with phenomenologists and other interpretative sociologists, ethnomethodologists have reversed the Durkheimian maxim. They enjoin us to "treat facts as social accomplishments." Since the type case for sociological facts is suicide, the ethnomethodological approach is best exemplified in relation to that topic. J. M. Atkinson (1978) has made the most important and sustained contribution, though the topic appeared in Garfinkel's original essays and Sacks' early work. The focus here is on the methods whereby the relevant actors arrive at the conclusion that a deceased person took his or her own life. Douglas (1967) had developed a broadly phenomenological analysis of "definitions" of suicide, but emphasized the extent to which the linguistic categories are flexible, even indeterminate, in reference. Atkinson, on the other hand, stresses the methodical nature of the practical reasoning involved.

The production of facts and categorizations as the outcomes of practical reasoning was one of the earliest topics in ethnomethodology. For instance, Sudnow on "normal crimes" (1965) or death (1967), Cicourel on juvenile justice (1968), and Cicourel & Kitsuse on educational assessments (1963) were among the precursors. All emphasized the work that goes into the production of assessments and organizational outcomes. They also contributed, of course, to the emergent critique of conventional sociology and the debate concerning the use of data from official sources. The latter debate and controversy perhaps obscured from many readers of the period an appreciation of the positive contributions made.

The social production of natural facts and categories was fully documented in a number of contrasting studies in Garfinkel's early corpus. He himself commented on suicide as a topic of practical reasoning; the study of clinic
case-notes illuminates the construction and reading of factual records; and the case study of Agnes, a transsexual, develops the theme of the accomplished character of sex and gender. (The latter theme is fully developed in an ethnomethodologically informed study by Kessler & McKenna, 1978.)

In more recent years investigators have used the ethnomethodological framework to describe the ascription of medical and psychiatric diagnoses, measures of intelligence, and the assembly of legal decisions. These studies are not normally intended to promote an ideological critique of those facts and assessments. Coulter’s (1973, 1979) work on insanity ascriptions contains a telling critique of antipsychiatry, for instance. Nor do they promote a subjectivist account of reality construction. Rather, they seek to explicate the organized practices whereby facts, assessments, and the like are put together, shared, transmitted, justified and so on. Some commentators find in ethnomethodology’s treatment of reality-production a critical sociology, however. Smith (1974a,b) in particular suggests that the ethnomethodological investigation of everyday reality constitutes an examination of ideology, while Bandyopadhyay (1971) suggests that ethnomethodology shares with marxism a commitment to the demystification of social reality. Smith’s explicit use of ethnomethodology in a critical context is unusual. The majority of ethnomethodologists pay little attention to that aspect of sociology and profess relativistic indifference toward any particular social or ideological discourse (Chua 1977).

Lynch’s study of work in a scientific laboratory is typically agnostic as to the legitimacy of scientific discourse or its ideological character. He explores how the scientists in question seek to determine what it is that they have observed (Lynch 1985). He describes the shop talk of scientific workers as they set about evaluating the quality and usefulness of the data they produce. His study exemplifies the ethnomethodological perspective in that the work of making a scientific fact is located in the very specific, local circumstances of talk and practical action in the laboratory. For example, he shows how objectivity and verification are asserted or contested. The scientists’ facts and findings are sustained not in abstract epistemological discourse, but through everyday spoken interaction and physical activity.

CONCLUSION: TENSIONS AND RESOLUTIONS

No review of this length can do full justice to the range of empirical studies in ethnomethodology, nor to the diversity of positions within that school. For, contrary to public opinion, perhaps, it is not a homogeneous field. A series of specific themes and criticisms has been selected for review. It has been argued that contemporary styles of ethnomethodology have made major contributions to the sociology of everyday life in a variety of mundane and organizational settings. Conversation analysis has helped to map out a distinctive and
original field of inquiry (though it cannot claim exclusive rights in that regard). Whether or not conversation analysis and analogous approaches stressing the sequential organization of activities remain faithful to the full implications of Garfinkel’s and Sacks’s original inspiration is debatable, however.

It has been argued that in conversation analysis the hermeneutic-interpretative strand has been suppressed in favor of a more narrowly empiricist, even behaviorist element. It is instructive to contrast contemporary conversation analysis with the contributions most recently published under the aegis of Garfinkel himself (1986), although their composition is of an earlier date. Here the emphasis on hermeneutics is much more evident. In contrast with most accounts of conversation analysis, Pack’s (1986) paper on the development of a notational system for transcribing lectures pays much more explicit attention to the theoretical work such transcription implies. The author is more attentive than most ethnomethodologists to the nature of semiosis and at least some theorists of the sign (Schutz, Morris, Ducasse, and Wild): oddly, nearly all of contemporary semiotics is omitted. (For discussions of ethnomethodology and semiotics, see MacCannell & MacCannell 1982; for a parallel discussion of symbolic interactionism and sign-theory, see Stone 1982.) Likewise, Eglin’s paper on alchemy and occult knowledge (1986) explicitly adopts a more hermeneutic approach to language. The intellectual framework for these papers harks back to Schutz and Heidegger, while most of contemporary ethnomethodology is paid remarkably little attention.

There is, especially in Girton’s ethnography of Kung Fu (1986), an emphasis upon embodied knowledge, and the ways in which competence resides in physical capacities rather than prescribed rules. This observation is repeated in the studies of science (e.g. Lynch 1985, Schreider, cited in Lynch et al 1983). (For earlier accounts of scientific actions which refer to tacit embodied knowledge, see Polanyi 1958 and Ravetz 1971.) Sudnow’s account of the embodied knowledge of piano-playing (1978) also draws upon ethnomethodological and interpretative traditions.

These accounts are, as I have suggested, much more hermeneutic/interpretative in character and stand in contrast to more empiricist tendencies. The two strands identified by Mehan & Wood (1975) have tended to separate out in contemporary versions of ethnomethodology. Some manifestations of conversation analysis in particular have little explicit regard for the original sociological and philosophical rationale (and reluctance to engage in programmatics hardly absolves the practitioners). By contrast, relatively few authors have explicitly developed the philosophical perspectives. Notably among them, Coulter (1973, 1979, 1983b) has consistently attempted to develop a version of ethnomethodology grounded in ordinary language
philosophy, while recognizing the diverse philosophical allegiances espoused in the movement in general (1983b, p. 7). Coulter’s emphasis on the logical grammar of mental predicates contrasts his approach sharply with those which account for social action and order in terms of members’ cognitive competences or mental states (e.g. Cicourel 1973). Coulter documents how mental illness, for instance, is attributed, as a particular case of the “social construction of mind.” His approach thus sharply contrasts with those that treat motives and emotions as internal states of mind. Coulter’s sustained work on the attribution of mental states is paralleled by Jayyusi’s (1984) sophisticated development of Sacks’ early work on categorizations (e.g. Sacks 1972b, 1979). Sacks had focused on the implications of descriptions and classifications in everyday uses of language. Jayyusi’s monograph explicates some of the ways in which moral ascriptions (concerning rationality, competence, responsibility, and ethical evaluations) are couched in typifications and categorization devices. She writes of the political implications of how persons and events are described (for instance, in the context of mass media converage of events). Coulter and Jayyusi deal, respectively, with cognitive and moral ascriptions or predicates in ways derived from the philosophy of language and philosophy of mind. Coulter’s position has made him a highly articulate critic of empiricism from within ethnomethodology; despite a shared interest in the use of language, he and the conversation analysts derive divergent analyses.

There is, therefore, a degree of variety—even ambiguity—in the epistemological underpinnings of ethnomethodology in general. It is, of course, always possible to take account of empirical observations without subscribing wholeheartedly to the epistemological justification claimed for them, and despite ethnomethodologists’ frequent claims to exclusivity. Silverman (1985) drawing on Dingwall (1981) has argued that ethnomethodologically informed ethnography is an especially important development in contemporary qualitative sociology. There is no doubt that in addition to strictly ethnomethodological and conversation-analytic studies, many empirical investigations have drawn eclectically on their insights. Ethnomethodological influence has spread widely in the sociologies of medicine, education, law, work, organizations, and so on. I have argued elsewhere (P. Atkinson 1985) for an increasing rapprochement between ethnography and conversation analysis. The act of defining mutually exclusive paradigms is sterile, while debates across paradigms are equally fruitless on many occasions. The repeated debates concerning ethnomethodology and symbolic interactionism are a case in point. Attempts to establish common ground frequently falter in the light of mutual misunderstandings and reaffirmations of competing dogmas.

It would be unfortunate, however, if a continuing paradigm mentality prevented the very real and important contributions of ethnomethodology
from reaching the audience they deserve. The future impact of ethnomethodology in the social sciences is not likely to be fostered by repeated attempts to define a totally separate enterprise. Its growing links with sociolinguistics and its influence on eclectically informed interpretative work will ensure for ethnomethodology a continued importance within the human sciences. Ethnomethodology must now confront the various epistemological strands that co-exist within it, rather than claiming a more homogeneous and internally consistent program than in fact exists. In this review I have tried to draw attention to some areas for such critical attention. That should not be interpreted as implying wholesale rejection of ethnomethodology. On the contrary, this essay is predicated on a recognition of ethnomethodology’s contribution to sociology. The foundations laid by Garfinkel and Sacks have resulted in a radical reappraisal of sociology’s subject-matter and procedures. They and their colleagues, collaborators and students have developed a program of work which increasingly demands serious attention from all sociologists.

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