Ethnomethodology

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The 1967 publication of Harold Garfinkel's *Studies in Ethnomethodology* marked the institutionalization of 'ethnomethodology' as a theoretical viewpoint in the social sciences. While ethnomethodology has since been the subject of extensive debate and discussion, this essay argues that certain key features of Garfinkel's work have generally been unsatisfactorily grasped by commentators and critics. Briefly, the argument will be this. Ethnomethodology has typically been assimilated to that general viewpoint that takes social interaction to be rule-generated. While this reading roughly fits the work of one prominent ethnomethodologist, Aaron Cicourel (section I), Harold Garfinkel's view is quite different in critically important respects (section II). Cicourel and Garfinkel exemplify two quite distinct orientations within ethnomethodology, and the differences between the two can be seen as grounded in divergent intellectual foundations (section III). It will be argued, however, that the Garfinkelian foundations have important defects (section IV). A concluding section contains some caveats concerning the main arguments of the essay (section V).

I

It has become commonplace to speak of social interaction as 'rule-guided' or 'rule-governed'. This characterization of interaction is designed to display the belief that rules play some active part in the production of the activity.[1] These rules are construed as, so to speak, 'operating procedures' for interaction; persons are seen as actively (if tacitly) following rules in deciding what action to perform and how to articulate that performance. To say that interaction is a rule-governed activity in this sense is to say that interactants are guided by, take note of, pay attention to rules in their production of action and utterance. A description of these rules underlying interaction is taken to be an explanation of how interaction comes to be, an account of the production of interaction.

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There is an important second facet of this standard view: the recognition that rules not only guide one's own actions, but also provide guidelines for making sense of (interpreting) the actions of others. Grimshaw (1973), for example, indicates that in characterizing behavior as rule-governed he means 'that "natively competent" members of a social group don't behave randomly; that their behavior is derived from shared understandings which simultaneously govern their own behavior and their interpretations of the behavior of other members' (p. 99, n. 2). It is, on this view, just because interactants enjoy shared rules or understandings that they can both adequately make sense of others' actions and articulate their own performance in a way that ensures a smooth social production (i.e., interaction). 'Making sense' of others' actions and utterances is taken to be a mental or cognitive process which follows certain sense-making rules or procedures. The process of sense-making is usually seen to centrally involve one's expectations about others' actions; Cushman & Whiting (1972), for example, suggest that 'rules function as guide posts to direct and indicate shared patterns of expectation' (p. 227).

While this general approach is widely represented among students of language and social interaction, it is nicely exemplified in the position advanced by Harré, 1972, 1974a, 1974b). In their view, 'social psychology must make room for attempts to unravel the modes of generation of social behaviour "within" the person' (Harré & Secord, 1972, p. 133), since 'explaining behavioural phenomena involves identifying the generative "mechanisms" that give rise to the behaviour' (p. 9). They treat 'rules as the substance of the generative mechanism of sequences of action' (p. 72), arguing that 'it is the self-monitored following of rules and plans that we believe to be the social scientific analogue of the working of generative causal mechanisms in the processes which produce the non-random patterns studied by natural scientists' (p. 12). But they also stress that 'a rule . . . can be thought of not only as guiding action but as determining expectation. Knowing a rule is not only to know what one should do, but it also gives one some ground for expectations one may have of the behaviour of other people who accept the rule' (p. 182). Thus rules are 'future directed; they not only guide action, but also determine expectations concerning the actions of other persons' (p. 12).

Both facets of this standard conception of the rule-governedness of interaction are typically attributed to ethnomethodologists generally and to Garfinkel particularly. Psathas (1973), for example, suggests that Garfinkel's studies 'provide the recipes for performance. . . . A

social scientist's report on how to find one's way in the city, how to give directions, how to locate objects in space when deprived of a visual sensory field—based on how ordinary persons in the everyday world actually do these things—can be used as a set of instructions, rules, or operating procedures for performing these same activities' (p. 12). Here Psathas cites Garfinkel's (1967, pp. 104–15) 'Some Rules of Correct Decision-Making that Jurors Respect,' characterizing it as 'a study which presents some rules of decision-making used by jurors in daily life' (p. 21, n. 9). Truner (1974) similarly describes the juror study as elucidating 'the "decision rules" jurors employed in reaching a verdict' (p. 237). Harré & Secord (1972) believe that 'the ethnomethodology of H. Garfinkel . . . has a good deal in common with the kind of study we would call ethogeny' (p. 172), which they define as the 'discovery and identification' of 'the generative "mechanisms" that give rise to behaviour' (p. 9). More particularly, they suggest that Garfinkel shares with them the belief that rules are the primary generative mechanisms (see pp. 150, 172).

Ethnomethodologists are also typically seen as elucidating the assumptions and rules governing the mental activity of making sense of the world, and as in general having mentalistic concerns. For example, Bauman (1973) writes that 'ethnomethodologists insist on one's right to discuss mental entities' (p. 20); Perinbanayagam (1974) describes Garfinkel as 'trapped . . . into a psychologistic epistemology' (p. 531); and Goldthorpe (1973), who takes ethnomethodology to be centrally concerned with 'mental states' (p. 456), refers to the ethnomethodologists' 'insistence on the crucial but neglected role of cognitive processes in social interaction' (p. 454). Similarly, Gellner (1975) takes ethnomethodologists to task for their 'subjective' stance, characterizing ethnomethodology as standing 'squarely in the Idealist tradition' (p. 434); and Mayrl (1973) describes ethnomethodology as 'essentially a psychologism' (p. 26), arguing that the position 'leads quite logically to idealism and ultimately solipsism' (p. 27). And Coser (1975), who says that 'ethnomethodology aims at a descriptive reconstruction of the cognitive map in people's minds which enables them to make sense of their everyday activities and encounters,' suggests that 'in some versions of ethnomethodology, inter-subjectivity is consciously neglected so that one ends up with a veiw of individual actors as monads without windows enclosed in their private and unshareable universes of meaning' (p. 696).

In short, the usual reading assimilates ethnomethodology to the standard rule-account of interaction (for further examples of one or another facet of this reading, see Psathas, 1968; Denzin, 1970; Gouldner, 1970, pp. 390–5; Gidlow, 1972; Cushman & Whiting,

1972, p. 230; McSweeney, 1973; Mennell, 1974, pp. 51–61; Gamson, 1974; Meltzer, Petras & Reynolds, 1975, pp. 75–81). Ethnomethodologists are seen as describing the 'generating rules' for social interaction (the rules that produce the situated actions of everyday actors) and as being centrally concerned with the cognitive processes and 'cognitive rules' involved in the mental process of sense-making.

What I wish to suggest is that this reading is only half right. My argument is this. Ethnomethodology is not a unitary viewpoint. Crudely and broadly put, there are two general schools of thought in ethnomethodology, one exemplified in the work of Harold Garfinkel, another in the views of Aaron Cicourel. The standard rule-account reading roughly fits Cicourel's approach, but it cannot accommodate Garfinkel's.

Now in fact there are more than just two competing variants of ethnomethodology. Indeed, the variety of 'ethnomethodological' views is so great that most commentators would not disagree with Coser's (1976, p. 33) characterization of ethnomethodology as 'protean.' Still, I think that a focus on Cicourel and Garfinkel as exemplary representatives of the two primary divergent strains can prove useful in illuminating matters.

Cicourel's view can be sketched as follows. Cicourel is concerned with 'how language and meaning are constitutive of the way in which everyday social interaction is assembled and represented' (1974a, p. 7). He notes that while most sociologists rely heavily on concepts such as 'status', 'role', and 'norm' in explaining social interaction, these sociologists do not elucidate the basic cognitive processes underlying actors' invocations of roles, norms, etc. Cicourel suggests that, in addition to the 'surface' rules concerning particular roles or norms, there exist 'basic' rules (or 'interpretive procedures') which apply across social situations and upon which the actor draws in formulating lines of action within the constraints of surface rules. As Cicourel (1972) writes:

> The actor must be endowed with mechanisms or basic rules that permit him to identify settings that would lead to 'appropriate' invocation of norms, where *the norms would be surface rules and not basic to how the actor makes inferences about taking or making roles.* The basic rules of interpretive procedures are like deep structure grammatical rules; they enable the actor to generate appropriate (usually innovative) responses in changing situated settings. (p. 244)

Thus Cicourel suggests that 'a more refined conceptual frame for understanding norms will have to specify basic rules as a set of invariant properties governing fundamental conditions of all interac-

tion' (1972, p. 250). These basic rules would describe 'the nature of minimal conditions that all interaction presumably would have to satisfy for actor and observer to decide that the interaction is "normal" or "proper" and can be continued. The acquisition and use of interpretive rules or procedures over time amounts to a cognitive organization that provides a continual sense of social structure' (p. 250).

Cicourel offers several examples of interpretive procedures (basic rules). One is the 'reciprocity of perspectives' rules, derived from Alfred Schutz's (1962, pp. 11–13) work. This rule has two parts. 'The first part instructs the speaker and hearer to assume their mutual experiences of the interaction scene are the same even if they were to change places. The second part of the rule informs each participant to disregard personal differences in how each assigns meaning to everyday activities; thus each can attend the present scene in an identical manner for the practical matter at hand' (Cicourel, 1972, pp. 251–2; see also Cicourel, 1970, pp. 147–8). Thus in following this rule 'both participants will adopt the idealized standpoint of assuming reciprocally shared experiences, and the same principle for assigning meaning or relevance to their immediate environments' (Cicourel, 1974a, p. 86).

A second interpretive procedure is the 'et cetera' assumption (and a closely related idea, that of the 'retrospective–prospective sense of occurrence'). The et cetera rule requires that 'a speaker and hearer "fill in" or assume the existence of common understandings or relevances of what is being said on occasions when the descriptive accounts are seen as "obvious" and even when they are not immediately obvious' (Cicourel, 1970, p. 148). Cicourel (1972) notes that 'this et cetera rule and its sub-routines permit the speaker–hearer to make normative sense of immediate settings by permitting temporary, suspended, or "concrete" linkages with a short-term or long-term store of socially distributed knowledge' (p. 252). The 'retrospective–prospective sense of occurrence' notion is one of the sub-routines of the et cetera rule (though sometimes the 'retrospective-prospective' idea receives separate treatment; see, e.g., Cicourel, 1970, p. 149). Cicourel (1974a) describes this sub-routine as follows:

> Vague or ambiguous or truncated expressions are located by members, given meaning contextually and across contexts, by their *retrospective–prospective sense of occurrence*. Present utterances of descriptive accounts that contain ambiguous or promissory overtones can be examined prospectively by the speaker–hearer for their possible meaning in some future sense under the assumption of filling in meanings now and imagining the kinds of intentions that can be expected later. Alternatively, past remarks can

now be seen as clarifying present utterances. The filling in and connecting principles enable the actor to maintain a sense of social structure over clock and experienced time despite deliberate or presumed vagueness and minimal information conveyed by participants during exchanges. (p. 87)

A third interpretive procedure is the 'normal form' rule:

> Interaction participants presume normal forms of acceptable talk and appearances, or if discrepancies appear, attempt to normalize the action scene. The rule provides the actor with a basis for rejecting or reducing a range of possible meanings to a collapsed typification of the social structures. The rule instructs the actor to reject or recognize particular instances as acceptable representations of a more general normative set. (Cicourel, 1972, pp. 252–3)

The point here is that competent interactants 'recognize and employ normal forms in daily interaction under the assumptions that all communication is embedded within a body of common knowledge or "what everyone knows" ' (Cicourel, 1970, p. 149). Cicourel (1970) notes that the reciprocity of perspectives rule and the et cetera rule both presume 'the existence of certain normal forms of acceptable talk and appearance upon which members rely for assigning sense to their environments' (p. 148). The 'normal form' assumption essentially instructs the actor to (a) articulate his actions in accordance with these socially shared normal forms, and (b) hear and see the actions of others in terms of these same normal forms (see Cicourel, 1974a, pp. 86–87).

Notice that all these interpretive procedures are 'cognitive instructions' to actors—these are cognitive equipment, 'socially relevant cognitive properties' (Cicourel, 1974a, p. 168), things ' in the actor's head' (even though the person may not be overtly conscious of them). The interpretive procedures are used both to generate action and to cognitively make sense of the actions of others. Thus, for example, Cicourel (1974a) writes that 'the reciprocity principle instructs the actor to impose an idealized interchangability of standpoints during interaction and to follow a similar procedure for assigning meaning or relevance' (p. 86). This same general cognitive emphasis is evident when Cicourel, in the course of discussing the interactions of deaf children, emphasizes the importance of acknowledging 'the existence of an internal representation system that is not dependent on a normatively organized external representational system' (1976, p. 158; for similar indications of clear mentalistic concerns, see Cicourel, 1974b, p. 14, and Cicourel, 1975).

The upshot of all this is that the usual reading of ethnomethodology, which assimilates that view to the standard rule-account of interaction, fits Cicourel's approach rather well. Cicourel is concerned with 'the psychological and sociological cognitive processes presupposed in language acquisition, use, and change' (1974a, p. 93), and thus he treats rules as generative mechanisms embedded in a cognitive structure, to the end of understanding how 'interpretive procedures and surface-rule competence generate behavioral displays' (1970, p. 144).

But can all ethnomethodologists be seen in this way? Most commentators and critics, as we have seen, treat all of ethnomethodology as a species of the usual rule-account of social interaction. But I will argue that Harold Garfinkel's work exemplifies a version of ethnomethodology which is very different from Cicourel's version, and that Garfinkel does not hold anything like the standard view of the rule-governedness of interaction.

As a prelude to displaying Garfinkel's approach, consider the following bits of evidence which, I think, cast doubt on the usual reading of Garfinkel's work. Garfinkel (1967) scornfully describes the way in which sociological theories frequently make the person into a 'cultural dope', 'the man-in-the-sociologist's-society who produces the stable features of the society by acting in compliance with pre-established and legitimate alternatives of action that the common culture provides' (p. 68). While Cicourel is surely not Garfinkel's target here, this comment can easily enough be seen as indicting the sort of view in which the stable social order is thought to be produced by shared action-generating rules (with heavy normative force) which culminate in certain 'normal forms' of interaction. In the same vein, Garfinkel (1967) suggests that 'portraying routine actions as those governed by prior agreements' is undesirable, as this 'theorizes essential phenomena out of existence' (p. 73). Again, this statement seems rather at odds with the usual rule-account and its emphasis on such mechanisms as 'implicit contracts' and 'tacit conventions'. And finally—to cast doubt on Garfinkel's alleged mentalistic concerns—Garfinkel holds that 'meaningful events are entirely and exclusively events in a person's behavioral environment. . . . Hence there is no reason to look under the skull since nothing of interest is to be found there but brains' (1963, p. 190). Garfinkel's statement is manifestly inconsistent with the cognitive emphasis of the standard rule-account. But if Garfinkel doesn't hold the typical rule-account, what is his view?

II

In describing Garfinkel's approach it will be useful to begin with Simmel's question, the question that motivates much of sociology: how is social order possible? Clearly, the standard rule-account is one sort of answer: the orderliness of the social world and of action within it is explained by shared action generating rules. Garfinkel's approach is rather different. The orderliness, facticity, objectiveness of the social world is not something 'out there' to be analysed or explained in anything like the usual sense. Rather, that the social world is seen as (and see-able as) ordered is taken to be an accomplishment of the persons (the members of the sociocultural group) living in it. As Wilson (1970) puts it, 'the member's sense that the features of the social order are objective and real is an accomplishment of the members on that same occasion' (p. 79).[2] On this approach, 'the objective structures of social activities are to be regarded as the situated, practical accomplishments of the work by and through which the appearance-of-objective-structures is displayed and detected' (Zimmerman & Pollner, 1970, p. 103). In fact, Garfinkel writes that 'the prevailing topic for ethnomethodological study' is 'the objective reality of social facts *as* an ongoing accomplishment of the concerted activities of daily life' (1967, p. vii).

The question then is, what kinds of 'concerted activities of daily life' accomplish this apparent orderliness of the social world? An alternative formulation of this question elucidates the term 'ethnomethodology': what kinds of *methods* do members use in accomplishing or producing a sense of order? Just as 'ethnomedicine' refers to the commonsense conception, knowledge, and use of medicine among members of a sociocultural group, so 'ethnomethods' refers to members' methods—methods for producing the appearance of orderliness, rationality, objectiveness in the social world (see Garfinkel's remarks in Hill & Crittenden, 1968, pp. 5–11). 'Ethnomethodology', obviously enough, is the study of members' methods.

In general, these methods are conversational practices—features of talk, elements of conversation. It is through talk that the social world is made to be, and seen as, orderly. 'The apparent orderliness and coherency of the scenes of daily life are matters that members are continually and unavoidably engaged in recognizing and making recognizable to each other. . . . Over the course of interaction members persuade and otherwise make evident to each other that events and actions directed toward them are coherent, consistent, planful, connected, and the like' (Zimmerman & Wieder, 1970, p. 290). Garfinkel & Sacks (1970) put it this way:

> Persons, because of the fact that they are heard to be speaking a natural language, *somehow* are heard to be engaged in the objective production and objective display of commonsense knowledge of everyday activities as observable and reportable phenomena. We ask what it is about natural language that permits speakers and auditors to hear, and in other ways to witness, the objective production and objective display of commonsense knowledge, and of practical circumstances, practical actions, and practical sociological reasoning as well. . . . For speakers and auditors the practices of natural language somehow exhibit these phenomena in the particulars of speaking. (p. 342)

It is important to notice Garfinkel's emphasis here on overt behavior: there are certain things ('practices of natural language', conversational practices) *in* talk (or displayed in talk) which make that talk out to be orderly, which (more generally) makes social action and the social world out to be orderly. Indeed, Garfinkel (1967) suggests that 'in exactly the ways that a setting is organized, it *consists* of members' methods for making evident that setting's ways as clear, coherent, planful, consistent, chosen, knowable, uniform, reproducible connections—i.e., rational connections' (p. 34). All of these characteristics of social settings and of social action are *made evident* in behaviour, are describably ('accountably') present in overt action: 'Any setting organizes its activities to make its properties as an organized environment of practical activities detectable, countable, recordable, reportable, tell-a-story-aboutable, analysable—in short, *accountable*'(Garfinkel, 1967, p. 34).[3] This accountability of the social world is crucial to Garfinkel's view, for Garfinkel (1967) says that the 'central recommendation' of his studies is that 'the activities whereby members produce and manage settings or organized everyday affairs are identical with members' procedures for making those settings "account-able" ' (p. 1, italics added). The import of this identity is that the orderliness of the social world *consists of* just these overt, describable, accountable features. It is these features in behaviour which give the sense of coherence and order to the social world; persons engaged in interaction are continually

> engaged in [the] serious and practical work of detecting, demonstrating, persuading through displays in the ordinary occasions of their interactions the appearances of consistent, coherent, clear, chosen, planful, arrangements. In exactly the ways in which a setting is organized, it *consists* of methods whereby its members are provided with accounts of the setting as countable, storyable, proverbial, comparable, picturable, representable—i.e., accountable events (Garfinkel, 1967, p. 34).

One implication of this approach is that Garfinkel treats sense-making not as a cognitive activity, but as identical with accounting

(describing) practices. Attewell (1974) puts this point nicely: 'Garfinkel does not simply settle on an image of members on the one hand making sense of continuing situations and then on the other independently telling that sense. He sets up an important equivalence between the making sense of situations and the telling of that sense' (p. 183). Thus it is that there is nothing 'under the skull' of interest to Garfinkel. Everything that matters is present in overt behaviour, present in talk; sense-making *is* telling that sense, and sense-making is possible only because social settings and activities are organized and managed in ways that makes their orderliness evident and accountable, ways that give a sense of coherence and planfulness to the social world.

So Garfinkel holds that it is members' conversational practices (including accounting practices) that create the sense of orderliness in the social world, that make social order observable, sense-able. And Garfinkel takes these practices as phenomena for study: 'The study of common sense knowledge and common sense activities consists of treating as problematic phenomena the actual methods whereby members of a society . . . make the social structures of everyday activities observable' (1967, p. 75).

Garfinkel's concern with rules can now be elucidated. *Reference to rules* is one of the everyday conversational practices members employ in creating and sustaining the appearance of orderliness, comprehensibility, and rationality of action in the social world. Hence, 'actions in a bureaucratic setting, for example, are not of interest for the ways in which they are detected and displayed as actions-in-accord-with-a-rule' (Zimmerman & Pollner, 1970, p. 100). As Zimmerman (1970) puts it:

> The notion of action-in-accord-with-a-rule is a matter not of compliance or noncompliance per se but of the various ways in which persons *satisfy* themselves and others concerning what is or is not 'reasonable' compliance in particular situations. Reference to rules [is] a common-sense method of accounting for or making available for talk the orderly features of everyday activities, thereby making out these activities as orderly in some fashion (p. 233).

Garfinkel is thus concerned not with rules as 'generating methanisms' of action, but with the phenomenon of reference-to-rule as a conversational practice that creates and sustains the apparent orderliness of the social world. As he writes, 'all procedures whereby logical and methodological properties of the practices and results of inquiries are assessed in their general characteristics by rules are of interest as *phenomena* for ethnomethodological study but not other-

wise' (1967, p. 33). (It is important to note that Garfinkel says '*all* procedures', for conventional sociology, insofar as it displays the phenomenon of reference-to-rule, is thus a potential domain for inquiry. More on this shortly.)

This ordering, normalizing function of reference-to-rule sheds light on Garfinkel's (1967) statement that 'it is both misleading and incorrect to think of an agreement as an actuarial device whereby persons are enabled as of any Here and Now to predict each other's future activities' (p. 74). The standard rule-account of interaction, of course, treats agreed-upon rules in just this way. Garfinkel takes a rather different view: 'More accurately, common understandings that have been formulated under the rule of an agreement are used by persons to normalize whatever their actual activities turn out to be' (p. 74). The difference here is brought out in Garfinkel's work on jurors. Far from being (as suggested by Psathas, 1973, and Turner, 1974) a description of the generating rules for action, that work is a description of the standards (rules) to which jurors publicly refer in creating and sustaining the orderly appearance and character of their own activity. Garfinkel notes that most studies of decision-making treat persons as having a set of rules to follow in making a decision, and as correcting previous decisions in accord with these procedures as new information turns up. He suggests an alternative:

> In place of the view that decisions are made as the occasions require, an alternative formulation needs to be entertained. It consists of the possibility that the person defines retrospectively the decisions that have been made. *The outcome comes before the decision.*
>
> In the material reported here, jurors did not actually have an understanding of the conditions that defined a correct decision until after the decision had been made. Only in retrospect did they decide what they did that made their decisions correct ones. When the outcome was in hand they went back to find the 'why,' the things that led up to the outcome, and then in order to give their decisions some order, which namely, is the 'officialness' of the decision (1967, p. 114).

Thus Garfinkel (1967) suggests that 'the rules of decision making in daily life, i.e., rules of decision making for more or less socially routinized and respected situations, may be much more preoccupied with the problem of assigning outcomes their legitimate history than with the question of deciding before the actual occasion of choice the conditions under which one, among a set of alternative possible courses of action, will be elected' (p. 114). The order-producing function of reference-to-rule should be clear.

We can now also elucidate Garfinkel's (1967) otherwise curious statement that ' "shared agreement" refers to various social methods

for accomplishing the member's recognition that something was said-according-to-a-rule and not the demonstrable matching of substantive matters. The appropriate image of a common understanding is therefore an operation rather than a common intersection of overlapping sets' (p. 30). Garfinkel is concerned with the public accomplishment of reference-to-rule, and hence he objects to theoretical views that merely refer to 'shared agreement' without clarifying the public operations that accomplish the appearance of (and thus, in Garfinkel's view, constitute) this. Thus Garfinkel sees bringing routine actions under the aegis of 'prior agreements' as 'theorizing essential phenomena out of existence' (1967, p. 73).

I have already mentioned that insofar as professional sociologists explain human action by reference to rule, that referencing activity is a potential domain for inquiry by Garfinkel. This has puzzled some commentators. Thus, for example, Anderson (1974) writes:

> Harold Garfinkel's variant of ethnomethodology . . . contains the peculiar idea that the sociologist's picture of the rules that regulate social action does not, per se, have any superior standing over and above the model of the rules that the actors may have formulated for themselves. In other words, the sociologist's model is 'just another' folk model of social behavior. . . . [But] if it were just another folk model, what would be the point of the sociologist's enterprise? . . . This position by Garfinkel, it seems to me, is either a form of intellectual oneupmanship, a way of shocking some 'professional establishment,' or else it represents a particular form of that loss of self confidence and misplaced egalitarianism on the part of the elite that is frequently met with in American institutions of higher education. (p. 105, n. 6)

But Garfinkel's 'peculiar idea' can now be seen to be not at all peculiar—certainly not requiring the kinds of explanations that Anderson offers. Garfinkel's point is simply that both the naive social actor and the professional sociologist display the phenomenon of reference-to-rule, and thus both can be studied by Garfinkel; both are engaged in making the social world out to be orderly, and the public methods they employ in accomplishing this task are the phenomena Garfinkel wishes to investigate.

Now reference-to-rule is only one of a number of artful conversational practices that allow the social world (and talk and action within it) to be seen as orderly. Indeed, reference-to-rule can be seen as a species of a more general conversational practice called 'formulating'. Garfinkel & Sacks (1970) explain this as follows:

> One finds conversationalists, in the course of a conversation, and as a recognized feature of that conversation, *formulating* their conversation. . . . A member may treat some part of the conversation as an occasion

to describe that conversation, to explain it, or characterize it, or explicate, or translate, or summarize, or furnish the gist of it, or take note of its accordance with rules, or remark on its departure from rules. That is to say, a member may use some part of the conversation as an occasion to *formulate* the conversation. (p. 350)

These practices of formulating exhibit for conversationalists an orientation to making the conversational activity out to be orderly, coherent, accountably rational (see Garfinkel & Sacks, 1970, p. 355).

There are also conversational practices of greater subtlety than formulating. One of the ways in which the rationality, coherence, and orderliness of talk is displayed is by that talk's adherence to a variety of requirements, including (e.g.) a 'reciprocity of perspectives' requirement. For Cicourel (and Schutz) the 'reciprocity of perspectives' notion was conceived as a 'cognitive instruction' in the actor's head which served to generate overt behaviour. But for Garfinkel, 'reciprocity of perspectives' is instead something publicly displayed in overt talk, a conversational practice that gives the appearance of orderliness to the social world. By talk's adherence to this requirement, that talk—and the social world it engenders—is made out to be rational. Garfinkel (1967) discusses several such requirements, characterizing these as properties of overt behaviour (more specifically, properties of talk):

The anticipation that persons will understand, the occasionality of expressions, the specific vagueness of references, the retrospective–prospective sense of a present occurrence, waiting for something later in order to see what was meant before, are sanctioned properties of common discourse. They furnish a background of seen but unnoticed features of common discourse whereby actual utterances are recognized as events of common, reasonable, understandable, plain talk. Persons require these properties of discourse as conditions under which they are themselves entitled and entitle others to claim that they know what they are talking about, and that what they are saying is understandable and ought to be understood (pp. 41–2).

It is readily apparent that what Cicourel characterizes as 'cognitive procedures' Garfinkel treats as properties of overt behavior (see Mehan & Wood, 1975b, p. 196).

There are two interesting features of all these conversational practices. One is their reflexive character. That is, these practices do not stand apart from interaction in making that interaction out to be orderly, but are themselves accountable parts of that activity; these practices 'are carried on under the auspices of, and are made to happen as events in, the same ordinary affairs that in organizing they describe' (Garfinkel, 1967, p. 1). Thus, for example, if in the course of

a conversation a speaker summarizes that conversation, his formulating activity is itself part of that same conversation. 'His talk itself, in that it becomes a part of the selfsame occasion of interaction, becomes another contingency of that interaction. It extends and elaborates indefinitely the circumstances it glosses and in this way contributes to its own accountably sensible character' (Garfinkel & Sacks, 1970, pp. 344–5). This reflexivity is critical to the accomplishment of a sense of orderliness, for it is critical to the accomplishment of a sense of orderliness, for it is just because these practices are 'constituent features of the settings they make observable' (Garfinkel, 1967, p. 8) that an ongoing activity, while ongoing, can be made out to be coherent, rational, sensible, orderly. And members 'know, require, count on, and make use of this reflexivity to produce, accomplish, recognize, or demonstrate rational-adequacy-for-all-practical-purposes of their procedures and findings' (p. 8).

Garfinkel notes that a second interesting feature of conversational practices is their relation to the indexicality of talk. Now Garfinkel's discussion of indexicality has often been taken to mean simply that all interaction is highly contextual. After all, an 'indexical expression' depends for its sense upon the context in which it is used; 'objective expressions', on the other hand, do not (at least ideally) depend for their sense upon the particular context of usage, but rather are 'context-free'. So when Garfinkel notes that indexical expressions are recurring features of social interaction, he is heard as just saying that all interaction is very much context-bound (see, e.g., Douglas, 1970).[4]

But Garfinkel's point here goes beyond simply noting the contextual character of interaction. For Garfinkel, it is talk that largely constructs the appearance of an orderly and rational social world; this talk is largely indexical. Now, as Garfinkel notes, it is often thought that indexical expressions are in some respects inferior to objective expressions, especially where any kind of rational inquiry—logic, mathematics, science—is involved. But Garfinkel's point is just that indexical expressions can be seen to have rational properties, since it is these expressions that produce the ordered character of the everyday social world (and—here reflexivity enters—in fact are themselves part of the social world that is made observably ordered). Thus Garfinkel (1967) characterizes the accomplishment of the appearance of order in the social world as 'an awesome phenomenon' consisting in part of 'members' uses of concerned everyday activities as methods with which to recognize and demonstrate . . . the rational properties of indexical expressions and indexical actions' (p. 10). Indeed, at one point he describes ethnomethodology as just 'the investigation of the

rational properties of indexical expressions and other practical actions as contingent ongoing accomplishments of organized artful practices of everyday life' (p. 11). So Garfinkel's point about indexicality is not just that interaction is contextual or that conversational practices are closely tied to the context of their use, but that indexical expressions somehow do have rational properties, since these expressions both engender an ordered social world and are themselves ordered by conversational practices.

To summarize: Garfinkel wishes to explicate how it is that the social world is made to appear orderly, rational, sensible, coherent. This apparent orderliness is seen as a practical accomplishment of interactants, achieved through the artful use of talk. Overt features of this talk (various conversational practices) create and sustain a sense of order and coherence in the social world, and indeed the orderliness and rationality of the social world *consist of* just these features.

III

Garfinkel and Cicourel obviously exemplify versions of ethnomethodology which subscribe to fundamentally opposed views on a number of important issues. The situation in ethnomethodology is thus quite unlike, say, that in psychological behaviourism, where Skinner, Hull, Tolman, and others shared certain fundamental beliefs (such as a belief in a positivistic philosophy of science and in significant phylogenetic continuities in behaviour) while differing over more detailed questions. In the case of ethnomethodology, the two leading theorists disagree about rather basic matters. While Garfinkel makes light of the 'cultural dope' of sociological theories, Cicourel emphasizes the 'normal forms' assumption; while Cicourel focuses on 'socially relevant cognitive properties', Garfinkel finds 'no reason' to investigate cognitive processes; and so on.

Now one might just see these as differing foci of interest. Indeed, construed somewhat more broadly, one might take Garfinkel and Cicourel as representatives of two different general orientations of the study of talk. For one of these orientations, talk itself is the phenomenon of interest: its structure, its character, its organization, the uses to which it is put, etc. In a sense, talk is seen as something to be explicated on its own grounds. For the other orientation, talk is to be elucidated by reference to mental states, processes, and events; the interactants' underlying cognitive organization of social events is what provides illumination of the nature of social interaction.

Drawn in this way, the distinction seems one of interest, not substance; that is, the two orientations seem not fundamentally incompatible.

But this construal is, I think, too generous. Rather, the differences between Garfinkel and Cicourel stem from quite divergent intellectual foundations, and reflect more than just differing interests. This section sketches those foundations, to the end of sharpening the contrast between Cicourelian and Garfinkelian ethnomethodology.

A useful place to start is the work of Alfred Schutz, for both Cicourel (1974a, p. 33) and Garfinkel (1967, p. 36, n. 1) cite Schutz's approach as an important element in their thinking. Schutz's work is based on the premise that the everyday social world (and action within it) is meaningful to the social actors living in it. The social scientists's task is one of understanding the processes by which this meaningfulness is constructed. In addressing this task Schutz turns to Edmund Husserl's phenomenological analysis of consciousness, arguing that ' if we are going to understand the concept of meaningful action . . . we must examine the formation and structure of those lived experiences which give meaning to an action' (1967, p. 41). Schutz uses Husserl's description of the nature of consciousness as a general framework to be fleshed out with materials specific to the everyday social world (see Schutz, 1962, p. 149; 1967, p. 44). Schutz's emphasis throughout his work is on the relation between structures of consciousness and the meaningfulness of the social world.

It should be obvious that Cicourel's work much better develops Schutz's basic inclinations than does Garfinkel's. Both Cicourel and Schutz look to consciousness as the ground of explanation of social phenomena, while Garfinkel denies interest in such inquiry. But if Cicourel shares (and Garfinkel does not) Alfred Schutz's general approach to the study of social interaction, where are Garfinkel's foundations to be located?

In a general way, I think, Garfinkel's work can be seen as sharing the basic orientation of that broad tradition in Anglo-American philosophy represented by the work of such philosophers as Gilbert Ryle and Ludwig Wittgenstein. In what follows I attempt to bring this out. I will be relying most heavily on Ryle's work, but I do not pretend to be giving anything like a careful description of his views. And I am not claiming that Garfinkel actually shares Ryle's substantive views, nor that Garfinkel thinks of himself this way. Rather I am trying to clarify Garfinkel's basic orientation, by showing how Garfinkel's approach parallels in broad outline certain well-known ideas in contemporary Anglo-American philosophy.

In his influential book *The Concept of Mind* (1949) Gilbert Ryle

examined the Cartesian conception of the mental and found it wanting. On this view, as Ryle describes it, there are thought to be two very different substances in the world, mental and material. The mental substance, mind, resides in the person, and each person enjoys a special privileged access to this 'secret grotto' of his that others do not. Events occur within this substance, and these mental events are the causes of overt behaviour. Ryle (1949) terms this general viewpoint 'the dogma of the ghost in the machine', arguing:

> In unconscious reliance upon this dogma theorists and laymen alike constantly construe the adjectives by which we characterize performances as ingenious, wise, methodical, careful, witty, etc., as signalizing the occurrence in someone's hidden stream of consciousness of special processes functioning as ghostly harbingers or more specifically as occult causes of the performances so characterized. They postulate an internal shadow-performance to be the real carrier of the intelligence ordinarily ascribed to the overt act, and think that in this way they explain what makes the overt act a manifestation of intelligence. They have described the overt act as an effect of a mental happening . . . (p. 50).

Ryle opposes this view, arguing instead that 'in describing the workings of a person's mind we are not describing a second set of shadowy operations. We are describing certain phases of his one career; namely we are describing the ways in which parts of his conduct are managed' (1949, p. 50). Thus it is Ryle's view that 'when we characterise people by mental predicates, we are not making untestable inferences to any ghostly processes occurring in streams of consciousness which we are debarred from visiting; we are describing the ways in which those people conduct parts of their predominantly public behavior' (p. 51).

In attacking the Cartesian 'dogma' Ryle charges the Cartesian with a number of logical or conceptual errors. One of Ryle's primary targets is the Cartesian's appeal to mental events as causes of behaviour, for in Ryle's view that appeal is founded on a misunderstanding of the role mental concepts play in our language. An example will help clarify matters.

Ryle notes that one belief frequently associated with the Cartesian outlook is a commitment to the 'doctrine of volitions', the view that the difference between voluntary and non-voluntary behaviour is that the former (unlike the latter) is caused by the occurrence of a special mental event, namely a volition or act of will. Ryle criticizes this view on a number of grounds: that the doctrine does not explain how a ghostly mental event can cause a concrete physical occurrence, that the doctrine involves an infinite regress (generated by the question 'are volitions voluntary or involuntary acts of mind?), and

so on (see pp. 64–7). His conclusion is this:

> In short, then, the doctrine of volitions is a causal hypothesis, adopted because it was wrongly supposed that the question, 'What makes a bodily movement voluntary?' was a causal question. This supposition is, in fact, only a special twist of the general supposition that the question, 'How are mental-conduct concepts applicable to human behavior?' is a question about the causation of that behavior (1949, p. 67).

Ryle attempts to display the confusion involved in this general Cartesian supposition by noting that in everyday life we can often perfectly well decide whether (e.g.) an action is voluntary—even though we don't have access to any ghostly mental occurrences within the person (which supposedly cause the behaviour), but only to observable events. 'Champions of the [volitions] doctrine should have noticed the simple fact that they and all other sensible persons knew how to decide questions about the voluntariness . . . of actions . . . before they had ever heard of the hypothesis of the occult inner thrusts of actions' (p. 67). Thus, Ryle argues, concepts such as 'voluntary' cannot refer to ghostly unobserved processes, but only to features of overt behaviour. Hence Ryle suggests that the Cartesian's treatment of mental concepts is misguided from the start: the Cartesian thinks these concepts refer to occult mental events which cause overt behaviour, and so he thinks that an explication of these concepts involves questions about causal processes, when in fact (on Ryle's view) such causal questions are not at all appropriate, since these mental concepts do not refer to unobserved ghostly processes but simply to features of overt behaviour.

Ryle takes this same general line in criticizing a number of elements of the Cartesian view. For example, in discussing perception he suggests that 'para-mechanical' accounts of the 'causal mechanisms' of perception are conceptually flawed, for the appropriate questions to ask 'are not questions of the para-mechanical form "How do we see robins?", but questions of the form, "How do we use such descriptions as 'he saw a robin'?" ' (1949, p. 225). That is, Ryle argues that because we are perfectly well able to use descriptions such as 'He saw a robin' even though we have not got access to any putatively hidden mental goings-on, our descriptions cannot refer to such unobserved processes and thus the Cartesian's causal-process answers are answers to inappropriate questions:

> When asked whether I do or do not see a tree, I do not dream of postponing my reply until an anatomist or physiologist has probed my insides . . . The question whether I have or have not seen a tree is not itself a question about the occurrence or non-occurrence of experimentally disco-

verable processes or states some way behind my eyelids, else no one could even make sense of the question whether he had seen a tree until he had been taught complicated lessons about what exists and occurs behind the eyelids. (Ryle, 1954, pp. 100–1; for a similar argument see Wittgenstein, 1967, §614)

'Voluntary action' and 'perception' are only two of the concepts Ryle thinks the Cartesian has misunderstood. Ryle suggests, for example, that to say someone acted intelligently is not to say anything about covert mental causes of (or accompaniments to) the person's behaviour, but only about public features of the person's action: 'Overt intelligent performances are not clues to the workings of minds; they are those workings' (Ryle, 1949, p. 58). So in general Ryle argues that the Cartesian goes wrong in supposing that our mental-concept words refer to ghostly causal processes, in supposing that to explain these mental concepts one must give some sort of causal-process story in which mental events are seen as the causes of behaviour. In fact, argues Ryle, everything we need to understand such concepts can be found in overt behaviour.

Now consider Garfinkel's work in light of Ryle's views. Garfinkel is not, as we have seen engaged in giving mentalistic causal explanations of how action is generated (see his remarks in Hill & Crittenden, 1968, p. 220). Indeed, Garfinkel finds nothing of interest 'under the skull'. Everything that matters is public, present in overt behaviour. What gives the appearance of orderliness to the social world is not (as Cicourel would have it) any set of cognitive processes generating behavior, but is rather the character of overt action itself: the conversational practices that are present—observably, describably—in behaviour. What makes social action rational, orderly, methodical is something publicly present in the behaviour, not something 'under the skull' that causes the behaviour.

Thus, for example, the orderliness and coherence of talk is something shown *in* that talk; requirements such as 'reciprocity of perspectives' are 'features of common discourse' which allow that discourse to be heard as 'common, reasonable, understandable, plain talk' (Garfinkel, 1967, p. 41). There is no need to examine cognitive processes to understand the orderly, methodical, rational, planful character of talk—these things are shown *in* the talk.

Again, the orderly character of jurors' decisions is not something that arises from any pre-programmed mental decision tree in which decisions are made 'as the occasions require' or 'as additional information turns up' (1967, p. 113). Instead, 'the outcome comes before the decision' (p. 114), and it is what publicly happens after the outcome that makes the outcome into an official decision; given the

outcome, the jurors 'went back to find the "why", the things that led up to the outcome, and then in order to give their decisions some order, which namely, is the "officialness" of the decision' (p. 114). And in the course of bestowing order on their actions, jurors had occasion to publicly refer to rules, thereby making their actions out to be orderly (see pp. 109–10).

Or again, more generally: 'The *recognizedly* rational properties of . . . common sense inquiries—their recognizedly consistent, or methodic, or uniform, or planful, etc. character—are *somehow* attainments of members' concerted activities' (1967, p. 10). What makes social action observably rational is something achieved by members in the course of interaction, something publicly and accountably achieved through overt conversational practices.

The parallel between Ryle and Garfinkel is, I trust, clear. Where Ryle argues that mental-process stories are unnecessary to explain our mental concepts, so Garfinkel suggests that cognitive stories are unnecessary to explain the appearance of social order. Where Ryle claims that everything we need to understand mental concepts can be found in behaviour, so Garfinkel holds that our sense of social order is to be explicated in terms of overt conversational practices. And thus where Ryle suggests that the 'intelligence' of intelligent action is not to be found in some mentalistic causal mechanism but instead in characteristics of behaviour, so Garfinkel argues that the 'orderliness' (or 'rationality' or 'coherence' or 'planfulness') of orderly social action is not to be found in cognitive processes but instead in features of overt social action.

A final striking example of the parallel is provided by Ryle's and Garfinkel's treatments of knowledge. Ryle, of course, does not see knowledge as something hidden in the person's 'secret mental grotto', but rather as something displayed in (and contained in) overt behaviour (see Ryle, 1949, pp. 28–9, 40–1, 44–5, 133). And Garfinkel echoes this view in the course of commenting on persons' 'knowing' social rules:

> That is a very interesting notion of 'know.' The 'know' here has to do not with what one might have in mind in some secret place. It is not a case of your having to calm a respondent or seduce him in order for him really to tell you. Then you would be illuminated on what he had been hiding all along. Instead, 'know' consists really in a structure of activity. That is what the 'know' consists of. It is not that the member has it somewhere in the nervous traces or that he has it according to a theory of personal action. (Hill & Crittenden, 1968, p. 47)

So to sum up thus far: There are two quite different orientations

that go by the name 'ethnomethodology'. Both are broadly concerned with the methods by which the social world is made to appear orderly, but from this general common concern the two orientations diverge sharply. Aaron Cicourel, in the tradition of Alfred Schutz, focuses on the cognitive operations underlying social interaction. Harold Garfinkel, following a vaguely Rylean line, centers on the ways in which the overt behavior of social actors accomplishes the appearance of orderliness in the social world. The two approaches are not simply different, but radically opposed. For Garfinkel mentalistic processes are not of interest, since conversational practices—those things that give the appearance of orderliness in the social world—can be done 'only and exclusively by competent speakers, who can do them only and entirely through the particulars of notational displays in natural language' (Garfinkel & Sacks, 1970, p. 353). Conversational practices *are* overt behavior, sensemaking *is* telling that sense, and hence to explain the appearance of social order there is no reason to look under the skull for any mentalistic causal mechanisms.

There are two parenthetical but important points that might be stressed at this juncture. One is this: The general Garfinkelian and Cicourelian orientations in ethnomethodology have been depicted here in their rather 'pure' forms. Most ethnomethodologists do not perfectly fit either of these views as described. Still, it may be useful to note that Zimmerman (1970), Zimmerman & Pollner (1970), Zimmerman & Wieder (1970), and Wieder (1974) all express Garfinkelian views. The work of the late Harvey Sacks is more difficult to classify. Sometimes Sacks expressed Garfinkelian views (see, e.g., Sacks' remarks in Hill & Crittenden, 1968, p. 48; and Garfinkel & Sacks, 1970). For the most part, however, he seemed to either adopt a vaguely Cicourelian stance or to avoid entirely talk of generative mechanisms in favour of simply describing regularities in the structure of talk (see, e.g., Sacks, 1965–72, 1972, 1973, 1974). The ambiguous status of Sacks' conversational analyses also plagues other conversational analysts (see, e.g., Schegloff, 1968, 1972; Jefferson, 1972; Sacks, Schegloff & Jefferson, 1974; Schenkein, 1978). The work of Mehan & Wood (1975a, b, 1976) is also rather ambivalent in orientation, though for the most part they seem to prefer a modified Garfinkelian line.

The other parenthetical point concerns the question of Garfinkel's phenomenological foundations. I earlier rather quickly dismissed the possibility of such foundations for Garfinkel's overtly non-mentalistic approach, even though Garfinkel claims that his work is based on that of Schutz, Husserl, and Gurwitch (see Garfinkel, 1967, p. ix).

And certainly in the usual interpretation of the views of Husserl, Schutz, and other phenomenologists, an orientation such as Garfinkel's would be seen as distinctly non-phenomenological. But, as Mehan & Wood (1975b) suggest, Garfinkel's views can be construed as a transformation of the constitutive procedures identified by phenomenologists: these procedures are now seen not as aspects of consciousness or as things privately held by persons, but as publicly available interactional features which constitute the orderliness of the social world. While a careful discussion of phenomenological constitutive principles and Garfinkel's unique treatment of them would divert attention from the central themes I wish to strike here, I would be remiss if I did not re-emphasize that my Rylean treatment of Garfinkel is one primarily designed to serve exegetical purposes. I make no claim that (e.g.) Garfinkel actually relies more on Ryle than on, say, Husserl. My aim here has not been to accurately describe the details of Garfinkel's views or his professed intellectual debts, but rather to clarify Garfinkel's basic orientation by displaying the broad parallels between that orientation and the kinds of views expressed by Ryle. Obviously, a more complete description of Garfinkel's viewpoint would involve considerable elaboration beyond the sketch given here, and would necessitate close attention to Garfinkel's specific use of the work of Schutz, Gurwitch, Husserl, and other phenomenologists. But it is precisely because Garfinkel transforms the phenomenologists' constitutive procedures into overt conversational practices that his basic foundations are best characterized as rather more Rylean than Husserlian.

The question I now want to raise, however, is whether Garfinkel's orientation can stand up to close conceptual scrutiny. I will suggest that it cannot.

IV

Garfinkel has complained that the term 'ethnomethodology' has become a 'shibboleth'; he explicitly disclaims responsibility 'for what persons have come to make of ethnomethodology' and suggests that 'the term may, in fact, be a mistake. It has acquired a life of its own' (in Hill & Crittenden, 1968, p. 10). Given the radically different approaches encompassed by the term 'ethnomethodology', Garfinkel's complaint seems justified. As an example of the extent to which the term has become overextended, Dreitzel's (1970) collection of essays claims on its cover to contain 'eight articles that demonstrate

what ethnomethodology is all about'. Yet of the authors represented, only two (Peter McHugh and Aaron Cicourel) have even faint associations with Garfinkel's work; and of these, one (McHugh) has explicitly denied being an ethnomethodologist (see Blum & McHugh, 1971, p. 99, n. 1), while the other (Cicourel), as we have seen, has developed his views in a way that departs fundamentally from Garfinkel's original approach.[5]

It should also be clear that many of the common characterizations and criticisms of 'ethnomethodology' are faulty, just because they do not take account of the radical differences between the Cicourelian and Garfinkelian variants. We have already seen for example, how Garfinkel's approach resists assimilation to the standard view of interaction as rule-generated. Another example is provided by the common characterization of ethnomethodology as a version of 'phenomenological sociology' (see, e.g., Psathas, 1973). Broadly construed, phenomenological sociology is an orientation that places central emphasis on 'the primacy of consciousness and subjective meaning in the interpretation of social action' (Natanson, 1962, p. 157). Yet Garfinkel's disclaimer of interest in things 'under the skull' rather clearly disqualifies him from membership in this general approach.[6] And again, the oft-heard objection that ethnomethodology is overly subjective, that it 'ignores everything but the interpretation of the individual actor' (McSweeney, 1973, p. 153; see also Mayrl, 1973, and Gellner, 1975) simply cannot be lodged against Garfinkel (see Zimmerman, 1976, pp. 8–10). Garfinkel's consistent emphasis on the ways in which the public, observable features of behaviour produce a sense of social order is manifestly not 'subjective'.

Given this situation, criticism can only be usefully leveled at particular ethnomethodological theorists, not at 'ethnomethodology' as a whole. In this section, I wish to suggest that Garfinkel's view involves a fundamental confusion. The confusion I wish to charge is one I believe Gilbert Ryle and Norman Malcolm have also perpetrated. That this should be so is not entirely surprising, given the earlier-described affinities between Garfinkel's approach and certain views prevalent in contemporary Anglo-American philosophy. As the point I wish to make is more clearly displayable in the work of Ryle and Malcolm, my criticism of Garfinkel will be clearer if discussed against the backdrop of their work.

As noted earlier, Ryle argues that the Cartesian is mistaken in thinking that the appropriate questions to ask about mental concepts are causal questions. Ryle's view is that this fundamental blunder flaws the Cartesian programme from the outset. So, for example, in

explaining how detecting a mosquito differs from having a shrill singing in the ears we do not want 'tidings or hypotheses' about paramechanical mental events occurring in some secret mental grotto. Instead, 'what we want to know is how the logical behavior of "he detected a mosquito" differs from that of "there was a singing in his ears" . . . ' (Ryle, 1949, p. 225). Ryle's focus is on what public, observable criteria we use in deciding whether to use one description or another, since it is these public events (not hidden, inaccessible mental causes) that mark the difference between the two.

The difficulty with Ryle's view is that there are here *two* equally legitimate—though quite different—questions. The Cartesian is interested in one, Ryle in another—and an answer to Ryle's question does not meet the Cartesian's needs. This is nowhere more nicely brought out than in Jerry Fodor's discussion of Ryle's view. Fodor (1968) suggests that Ryle's approach 'suffices to eliminate as conceptually disreputable not only "paramechanics" but also automotive mechanics, and thus surely shows that something has gone wrong' (p. 19). Fodor argues this claim as follows. We often characterize automobile engines as 'working' or 'functioning'. On Ryle's view, suggests Fodor, we use a word like 'functioning' to report that 'the behavior of the engine satisfies the norms, standards, and so forth commonly applied in evaluating the behavior of engines' (p. 20).

> But then it follows immediately that a theory of *how* engines function (that is, of the mechnical transactions that determine the difference between functioning and malfunctioning) must invariably be a howler since, by hypothesis, it is the satisfaction of norms and standards that determines that difference. How, then, are we to avoid blundering into the psuedo-science of automotive mechanics? How are we to avoid the grammatical misapprehensions that tempt us to seek mechanical explanations of the behavior of engines? We must systematically replace misleading questions like 'How do engines work?' with antiseptic questions like 'How do we use such descriptions as "This engine worked last Tuesday"?' (Fodor, 1968, p. 20)

The defect in Ryle's viewpoint which leads to this curious conclusion is nicely identified by Fodor: 'In the case of automobiles, at least, it is perfectly clear that inquiring into the conditions under which it is appropriate to say 'It works' (e.g., inquiring into the norms, standards, etc., a working engine is required to satisfy) is quite different from inquiring *how* it works, so the suggestion that we abandon the latter sort of inquiry in favor of the former is utterly gratuitous' (1968, p. 21). Put a bit more directly, we might say that Ryle confuses two distinct questions: (1) How do we tell whether, e.g., the engine is working (or the person is perceiving, or acting intelligently)? and (2)

What theoretical account (causal-process story, etiology) are we to give of the engine's working (or the person's perceiving or acting intelligently)?[7] The Cartesian is interested in the second question, Ryle in the first. But the Cartesian is not asking an inappropriate or illegitimate or conceptually flawed question—merely one different from the one in which Ryle is interested—and so Ryle's charge that the Cartesian has logically blundered cannot be accepted.

Ryle's confusion of these two questions is not an isolated mistake. Norman Malcolm, a prominent Wittgensteinian philosopher, has erected an argument designed to show that the talk in contemporary psychology of 'cognitive processes' is futile and misguided (Malcolm, 1971). He takes as an example of such talk the current work on 'recognition processes'. This work, Malcolm (1971) argues, assumes that 'whenever one recognizes something there is a process of recognition' (p. 386). Malcolm thinks this assumption conceptually faulty:

> The mistake here is easy to state but profoundly difficult to grasp. Recognizing someone is not an act or a process, over and above, or behind, the expression of recognition in behavior. But also, of course, it is not that behavior. . . . Your recognizing John in the crowd cannot be identified with your smiling at him and saying, 'Hi, John.' Imagine an eccentric who smiles at and says 'Hi, John,' to every tenth person he passes; and who has never seen this John before. Given those facts, his smile and utterance on this occasion would not be an expression of recognition. Thus it is the facts, the circumstances surrounding that behavior, that give it the property of expressing recognition. This property is not due to something that goes on inside. (p. 387; Wittgenstein's similar emphasis on surrounding circumstances can be seen in Wittgenstein, 1967, §§577, 587)

Malcolm suggests that 'if this point were understood by philosophers and psychologists, they would no longer have a motive for constructing theories and models for recognition, memory, thinking, problem solving, understanding, and other "cognitive process"' (1971, p. 387).

But Malcolm, too, confuses the two equally legitimate questions 'How do we tell?' and 'What theoretical account are we to give?' It is certainly legitimate to investigate how we tell whether someone is recognizing (i.e., to investigate that overt criteria we use in deciding the question), but this neither precludes nor constitutes a theoretical account of how recognizing happens (e.g., a cognitive-process story involving, say, matching operations).

And now to Garfinkel. Like Ryle and Malcolm, Garfinkel finds nothing of interest under the skull. The orderliness of the social world and the rationality and methodical character of social action *consist of* the overt, describable features of behaviour which produce

the sense of that orderliness and rationality. Thus, for example, the rationality of rational decision making is not to be explained by referring to any set of mental processes, but rather by pointing to overt features of the behaviour. Or again, Garfinkel's concern is not with any shadowy cognitive process of 'understanding', but with how participants in a conversation manage to make evident over the course of that conversation that they are understanding what is occurring (see Garfinkel & Sacks, 1970, p. 366). And 'knowledge' is not something in the actor's head, but something found in overt behaviour. Thus Garfinkel focuses exclusively on overt behaviour: on what there is in some piece of behaviour that makes it (accountably, describably) a case of 'understanding' or 'rational decision-making' or 'methodical action' or whatever.

But here again is a confusion of the two questions 'How do we tell?' and 'What theoretical account are we to give?' An answer to the question of how we tell that someone has understood (or is acting intelligently, or knows something, or is recognizing) neither precludes nor constitutes an answer to the question of what theoretical account we are to give of understanding (or recognizing, knowing, acting intelligently or rationally).

Now Garfinkel is surely right to this extent: One of the important ways in which we tell that the social world is orderly and rational, that persons are acting in rational and orderly ways, is by examining what people say (and how they say it, in what circumstances, and so on). Where Garfinkel goes wrong is in thinking that the orderliness of the social world *consists* in nothing more than this, that because this is how we *tell* that the social world is orderly that this is *how* the social world is orderly, that we need not look 'under the skull' here. For, as I have tried to bring out, the questions 'How do we tell?' and 'What theoretical account are we to give?' are two different and equally legitimate questions.

This criticism of Garfinkelian ethnomethodology does not entirely undercut Garfinkel's work, so much as it puts that work in perspective. The overt signs of orderliness are continually and unavoidably relied upon by social actors throughout the course of daily life. Garfinkel has succeeded in showing that there is here a hitherto largely unexplored domain of phenomena which is a legitimate field of inquiry. But the investigation of this area does not replace or preclude a Cicourelian sort of inquiry into the cognitive processes underlying social interaction.

And, while space does not permit a full discussion of the issue here, it might be noted that this same analysis can be seen as applying rather broadly to those theorists who have called for Rylean/Witt-

gensteinian approaches to the social world (e.g., Winch, 1958; Coulter, 1973; Rubenstein, 1977). These approaches, to the extent that they confuse 'how do we tell' with 'what account are we to give', unjustifiably and unhappily narrow the scope of investigations of the social world. By failing to acknowledge the distinctiveness and legitimacy of causal accounts of human behaviour, these approaches truncate the possibilities for understanding persons; for without attempts to identify the generative mechanisms underlying social behaviour, our understanding of that behaviour will inevitably be incomplete.

V

In this concluding section I want briefly to offer a few caveats and reservations about the main arguments of the preceding sections. Broadly put, these comments concern various points which, in the interest of sharpening the contrast between Cicourel and Garfinkel, were put rather too baldly or were bypassed entirely.

First, while Cicourel's views were described as fitting the 'standard rule-account' of interaction, in fact Cicourel's approach departs in several important respects from that standard explanation as described here. For one thing, Cicourel is careful to distinguish different sorts of rules, and his concern is explicitly with 'basic rules' as against the 'surface rules' which ordinarily receive the bulk of attention. And Cicourel stresses that action is not simply a matter of following previously known rules that mandate particular acts, but instead consists of the emergence of organized conduct (in the course of an ongoing, unfolding interaction) in ways that involve the employment of rules both as mechanisms for the interpretation and production of activity and as justifications for actions that have occurred. Moreover, Cicourel places greater stress on visual and nonverbal aspects of social interaction than is common in most standard rule-accounts of interaction (see, e.g., Cicourel, 1968, 1976). But even given these departures from the usual rule-based explanation of social interaction, Cicourel's views are still rather closer to the 'standard' rule-account than are Garfinkel's, and hence the argument in section I of this essay simply assimilated Cicourel to the usual rule-account without the qualifications just mentioned.

Second, Cicourel's and Garfinkel's views are (or at least at times have been) similar in a number of ways that have not been sufficiently stressed here. For example, Cicourel's *Method and Measure-*

ment in Sociology (1964) is rather more Garinkelian than Cicourel's later work. And even Cicourel's more recent work still shares with Garfinkel a focus on members' methods for making the social world appear orderly; Garfinkel and Cicourel may differ over the nature and character of these methods, but at this broad level they nevertheless have a common focus of attention. Indeed, both theorists specifically emphasize the retrospective–prospective sense of occurrence and the et cetera principle as members' methods—but in one case these methods are 'properties of discourse' (Garfinkel, 1967, p. 41) and in the other are 'socially relevant cognitive properties' (Cicourel, 1974a, p. 168). And, of course, both are interested in the nature of social interaction, though they pursue rather different concerns and differ over the extent to which reference to cognitive processes is required to illuminate the character of overt behaviour.

Finally, it might be noted that the analysis offered here suggests a possible reconciliation of Garfinkelian and Cicourelian ethnomethodology. Cicourel and Garfinkel certainly hold differing views, but in at least one way their work can be seen to be complementary, for while Garfinkel focuses on the Rylean 'how do we tell' question, Cicourel focuses on the Cartesian 'theoretical account' question. So while not overlooking their important differences, we still might be able to say that Cicourel's and Garfinkel's work could be neatly melded. None of this countenances the misreadings and unjustified criticisms described earlier, nor does it justify glib treatments of ethnomethodology as a unitary viewpoint. But at least the differences between variants of ethnomethodology can now be seen as grounded in differing—and not (*contra* Ryle) mutally exclusive—foci of interest.

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NOTES

[1] While my phrasing will not always reflect this, the usual rule-account of interaction makes no claim that interactants are necessarily aware of the rules they follow; rather, the suggestion is that we may usefully think of persons' actions as generated by rules even where those rules are 'tacit' (nonconscious). A related line was taken by Chomsky (1965) in defending the psychological status of generative grammar.

[2] The concept of 'member' is not treated clearly in Garfinkel's work. Usually the way the term is used it clearly refers to a person, and that is the way I shall use it here. However, Garfinkel & Sacks (1970) write that 'we do not use the term to refer to a person. It refers instead to mastery of natural language' (p. 342). Elsewhere Garfinkel says that 'member' means 'a course of activity, recognizable for its directionality' (in Hill & Crittenden, 1968, p. 119). But it will be more useful for us to treat these descriptions as characterizing what being a member comes to (rather than as defining 'member'). In any case I do not think anything crucial to my argument depends on this decision.

[3] To characterize something as 'accountable' is to call attention to its describability; this is a sense of 'accountable' quite different from the sense in phrases such as 'held accountable for'. This does not seem to have been noticed by, e.g., Gamson (1974), who confuses 'accountable' with 'justifiable'.

[4] The idea of indexicality has been used by some theorists (e.g., Wilson, 1970) as the basis of an argument to the effect that conventional sociology is fundamentally conceptually flawed. This does not seem to have been Garfinkel's intent in discussing indexicality (see his remarks in Hill & Crittenden, 1968, pp. 3, 130, and esp. 192, 193–4; and Garfinkel & Sacks, 1970, p. 361).

[5] The break between Cicourel and Garfinkel has been noted by Coulter (1971, 1974) and McSweeney (1973), though I do not think either realizes just how deep the split is.

[6] There is a highly metaphorical sense in which Garfinkelian ethnomethodology could be said to be 'phenomenological', in that it employs an 'epoche of social order' to be able to focus exclusively on the ways in which members' practices produce the *appearance* of orderliness; see Heap & Roth (1973, pp. 363–4).

[7] This elegant 'two questions' formulation I owe to Thomas Nickles.

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