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Ethnomethodology and the new *Methodenstreit**

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"But I've gotta use words when
I talk to you."

T. S. Eliot, *Sweeney Agonistes*

Ethnomethodology should not be mistaken for just one more variety of microsociology. It is much more: it represents a comprehensive challenge to some of sociology's most general assumptions. In the past, the superficial similarity of the ethnomethodologists' interest in small scale interaction has led to comparisons with the work of Goffman and the symbolic interactionists (Gouldner 1971, Denzin 1971, Gidlow 1972), but ethnomethodologists have vehemently rejected such parallels (Douglas 1971:21, Zimmerman and Wieder, 1971). The emergence of ethnomethodology, as both Bauman (1973b:31) and Goldthorpe (1973) recognize, represents a renewal of the *Methodenstreit* which flared in German sociology around the turn of the century. The ethnomethodologists reject deductive explanation of the "covering law" kind in social science, emphasize the necessity of grasping the "meanings" which social interaction has for those participating in it, and are preoccupied with the uniqueness of every social situation.¹ Though recast in subtle forms, the old arguments for a rigid distinction between the natural and social sciences are easily recognizable.

The philosophical roots of ethnomethodology are two-fold. Ethnomethodologists themselves most prominently claim intellectual ancestry from the social phenomenology of Alfred Schutz.² Yet Schutz has other sociological followers – most notably Berger and Luckmann (1967) – whose work represents a much less drastic break with the interests of "conventional" sociology, and from whom ethnomethodologists are at pains to emphasize their differences. What is really more distinctive of ethnomethodology is its debt to Wittgensteinian "ordinary language" philosophy. The nature of these twin debts will be examined below.

Of ethnomethodology as a school of thought within sociology, Harold Garfinkel is usually acknowledged to be the founder. Although ethnomethodology did not attract very wide attention among sociologists generally until a spate of publications by Garfinkel and his fol-

¹ To speak of 'the ethnomethodologists' as I do in this paper involves a necessary oversimplification. They have many differences among themselves, and some writers often identified with the school (Douglas for example) would not, or would no longer, apply the label to themselves. And Sacks and his group now stress the 'scientific' character of their analyses.

² Hindness (1972) and Bauman (1973a) have cast doubt on whether Schutz's work represents an accurate interpretation of Husserl's phenomenology.

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lowers appeared in the mid-1960's,³ Garfinkel himself (1968) has traced its basic ideas back as far as 1945, when he was involved in a study of how juries arrive at their decisions. The term "ethnomethodology" was invented much later as a conscious analogy with the terms "ethnoscience", "ethnobotany", "ethnomedicine" and so on, as they are used by anthropologists. It was meant to signify that just as native classifications of plants or illnesses can be taken not as science but as data to be analysed by the social scientist, so can the "methods" employed by participants in social situations.

The substantive research carried out by ethnomethodologists and their critique of "conventional" (or "absolutist" or "constructivist" or "positivist") sociology are closely related, but a few points can be made about their style of research. A feature of Garfinkel's early studies was his stratagem of disrupting familiar situations in order to expose the background assumptions and expectations, the stock of knowledge and "methods", which participants unconsciously deploy in them.

"Procedurally it is my preference to start with familiar scenes and ask what can be done to make trouble. The operations that one would have to perform in order to multiply the senseless features of perceived environments; to produce the socially structured affects of anxiety, shame, guilt, and indignation; and to produce disorganized interaction, should tell us something about how the structures of everyday activities are ordinarily and routinely produced and maintained.

... my studies are not properly speaking experimental. They are demonstrations, designed ... as 'aids to a sluggish imagination.' I have found that they produce reflections through which the strangeness of an obstinately familiar world can be detected." (Garfinkel, 1967:37-38)

A famous example was that in which students were asked to behave as a lodger in their own homes (Garfinkel 1967:41-44), to the bewilderment and often anger of the rest of the family. Sudnow's *Passing On: The Social Organisation of Dying* (1968) is an example of a larger-scale study, involving the analysis (but not in this case the disruption) of the minutiae of interaction in hospital wards.

More recently, two fairly distinct (though not mutually exclusive) trends have appeared in ethnomethodological investigations. One is the study of behaviour within organisational settings, especially organizations dealing with people - as patients, as delinquents, as recipients of welfare payments and so on - focusing particularly on the way in which the clients are categorized and official records manipulated "for good organizational reasons".⁴ The second trend, in which Harvey Sacks is the outstanding figure, has been towards the development of a new kind of sociolinguistics - the analysis of actual tape-recorded conversations.⁵ The rationale for these two tendencies is apparent in

³ Volumes which have attracted particular attention include Garfinkel (1967), Cicourel (1964, 1968), Douglas (1967), McHugh (1968), Sudnow (1967). Useful collections of shorter papers by ethnomethodologists are Douglas, ed., (1971), Sudnow, ed. (1972) and Turner, ed. (1974). Ethnomethodology's eventual transatlantic passage to Britain was marked by the appearance of Filmer, *et al.* (1972).

⁴ See Garfinkel, 'Good Organisational Reasons for "Bad" Clinic Records', (1967: 186-207), as well as Cicourel (1968), Zimmerman (1969).

⁵ For examples of conversational analysis, see Sacks (1972a, 1972b) and Schegloff and Sacks (1973).

the light of the ethnomethodologists' critique of "conventional" sociology.

The Ethnomethodological Critique of 'Conventional' Sociology

In his *Method and Measurement in Sociology* (1964), Aaron Cicourel launched a thoroughgoing critique of the whole paraphernalia of sociological research methods. On the first page, he wrote:

"My basic assumption is that the clarification of sociological language is important because linguistic structure and use affects the way people interpret and describe the world. Since sociologists have evolved their own theoretical terminologies and frequently discuss, on the one hand, in these varying terms the language and substance of each other's theories and on the other hand the language of persons in everyday life whose behaviour they are interested in explaining and predicting, it is quite likely that the syntax and meaning of these languages will become entangled."

For their view of what would constitute acceptable sociological theorising, the ethnomethodologists rely on Schutz's distinction between first and second degree constructs. Schutz contended that we experience the external world of things, people and events as typifications, classifications and categorizations. The corpus of everyday knowledge handed down to us also includes an armoury of practical actions, "efficient recipes for the use of typical means for bringing about typical ends in typical situations" (Schutz, 1964:14). But any individual's stock of everyday knowledge taken as a whole is to a greater or lesser degree incoherent, inconsistent, and fragmentary. Perfect clarity is neither attainable nor necessary, for it serves essentially practical purposes. The sheer diversity of practical actions is an obstacle to coherence and, moreover, while the practical recipes "work", producing the desired and expected results, there is little incentive for conscious pursuit of clarity. For the social scientist, this poses a paradox. He has to give clear and consistent explanations of a subject-matter which itself is often unclear and inconsistent. Schutz suggested that this is achieved by the construction, for scientific purposes, of typifications of the typifications subjects make for their practical purposes – by building "constructs of the second degree".

The ethnomethodologists, however, argue that "conventional" sociology generally fails to carry out this procedure. As they put it, it tends "to use common sense knowledge of social structures as *both* a topic and resource of inquiry" (Garfinkel, 1967:31). Many of the "facts" which sociologists tend to accept as data have been created in everyday situations for practical purposes. They are not facts but

"accomplishments". As Zimmerman and Pollner (1971:82) express this argument,

"Sociology's acceptance of lay member's [*sic*] formulation of the formal and substantive features of sociology's topical concerns makes sociology into an eminently *folk discipline* . . . Insofar as the social structures are treated as a given rather than as an accomplishment, one is subscribing to a lay inquirer's version of those structures."

To this extent, it is argued, "conventional" sociology is merely on a par with any person's understanding of his everyday situation – a "first degree" corpus of knowledge "competitive" with everyday practical knowledge.

The thrust of this view can be seen in repeated criticisms of the use of official statistics in sociology (Cicourel 1968, Kitsuse and Cicourel 1963, Douglas 1967). Douglas takes a case of classic concern to sociology, that of suicide rates, and argues that they reflect the *interpretation* of deaths by coroners. Their judgments cannot be reduced merely to a formal rule of categorization – the rules are always interpreted, and Douglas purports to show that the interpretations of different coroners are inconsistent with each other. Cicourel extends similar reasoning to the field of delinquency, showing how the unconscious background assumptions of probation officers and police lead them to find delinquency where they expect to find it and to be more ready to categorize acts by certain types of people than others as delinquent. The conclusions are similar: aggregate statistics are meaningless, or at most reflect only the operations of the official bureaucracy, because identical "real world events" may be placed in different categories, while quite different events may be placed in one and the same category.

Sociologists have no doubt frequently used official statistics too uncritically. As Hindess (1973) argues, it is disastrous to impose alien categories and then to reify the resulting statistics, as he shows to have happened dramatically in the classification of the agrarian population in the Indian Census of 1951. Categories are theoretical constructs, the choice of which must be justified as appropriate. Given this, however, a great deal can be done by the framing of detailed instructions and training of enumerators, interviewers and officials to minimise the inconsistent application of categories. But, as Hindess points out, the ethnomethodologists do not show much interest in the possibility of estimating the degree of error present in statistics or in that of substantially improving them.

Why these possibilities do not interest them may become a little clearer in the light of Pollner's critique (1974) of Becker's version of the labelling theory of social deviance. The theory is summed up in Becker's statement that "The deviant is one to whom that label has successfully been applied; deviant behaviour is behaviour that people

so label" (Becker, 1963:9). Pollner, however, accuses Becker of not following this principle consistently, and thereby confusing the common-sense actor's model of deviance with a sociological one. For the common-sense actor, deviance is an objective quality of another's action which causes and justifies an appropriate reaction to it by the community. On the other hand, Pollner writes, "Viewed sociologically, the deviant character of the act is not intrinsic to the act but depends upon or, more emphatically, is constituted by the *subsequent* response of the community" (1974:29, my italics). Becker employs both of these perspectives to produce his cross-classification of types of deviant (Fig. 1). Pollner, on the other hand, draws on his study of an American traffic court – where, as he points out, many of the convicted feel aggrieved because their common-sense knowledge tells them that the same offence is continually being committed without the offenders being apprehended.

	Obedient Behaviour	Rule-breaking Behaviour
Perceived as deviant	Falsely Accused	Pure Deviant
Not Perceived as deviant	Conforming	Secret Deviant

Figure 1. Types of Deviant Behaviour (Becker, 1963: 20)

"... not all those who violate rules are categorised as deviant and not all those who are categorised as deviant have violated a rule. Presumably (i.e. in perfect conformity with the criterion of rule violation), if all the faults of police and judicial processing were removed so as to provide for homogeneous categories, then labelling would be superfluous. Rule violation and application of the label 'deviant' would coincide and the deviant would in effect be he who has violated a rule and only incidentally he who has been labelled deviant." (Pollner, 1974:3)

Since this condition is not met, Pollner rejects the idea of "real deviance" as a matter of no relevance for sociologists, as being in effect an unknowable *Ding an Sich*. Becker's classification is just the same as that employed by judges (who for this purpose count as laymen using commonsense practical reasoning): "conforming" and "pure deviant" behaviour correspond to a just acquittal and a just conviction.

The deeper roots of the ethnomethodologists' critique of "conventional" sociology lie in their views on the nature of social interaction and the use of language within it. Underlying most sociology they see what Wilson (1971) calls "the normative paradigm". This involves two related ideas, that interaction is rule-governed, and that deductive explanation is appropriate to sociology. Deductive explanation involves logically deducing the facts to be explained from a number of theo-

retical premisses and given empirical conditions. This necessitates that "each description entering into deductive explanation must be treated as having a stable meaning that is independent of the circumstances in which it is produced" (Wilson 1971:71). For this to be true in the explanation of social interaction, behaviour has to be seen as governed by rules, which Wilson defines as "a stable linkage between the situation of an actor and his action in that situation" (1971:60). What this really amounts to, he contends, is that in order to treat various instances as examples of the same category of social situation, sociologists have to assume a degree of "cognitive consensus" – to assume that actors agree in recognizing situations at different times and in different places as being instances of "the same kind" of situation, and therefore behave similarly.

In contrast, underlying the work of phenomenological sociologists is "the principle of the contextual determination of meaning" (Douglas, 1971:37). Of course, "conventional" sociologists recognize that the same item of behaviour (a lie, for instance) can have different meanings in different contexts. Ethnomethodologists, however, take an unusually radical view of the interpretative nature of the process of social interaction. They see actors as continually groping towards a definition of the situation at hand, and situations are subject to continuous redefinition. In particular, all conversations contain numerous expressions which are not explicitly defined in a particular situation, and participants have to achieve "operational" or working definitions of all such "indexical" expressions which are valid only in the situation at hand.

"Indexicality" is a key term in ethnomethodology. It is taken from the work of the logician Bar-Hillel (1954), "who defines an indexical expression as one that depends for its meaning on the context in which it is produced" (Wilson, 1971:68fn.). Bar-Hillel gives as examples of indexical expressions the statements "It's raining" and "I'm hungry", as contrasted with a non-indexical statement such as "Ice floats on water" which has universal validity. Abercrombie (1974) has pointed out that Bar-Hillel argues only that indexical expressions depend for their *reference*, not their general meaning, on the context in which they are produced, and that this argument does not justify the much stronger principle of the contextual determination of meaning. The meaning immediately becomes clear when the reference is made evident. Garfinkel (1967:25–26, 38ff), on the other hand, illustrates his view of indexicality with an excerpt from a highly elliptical conversation between a husband and wife, together with their much longer expansion needed to make it intelligible to an outside observer. Yet "conventional" sociologists remain unconvinced that this is a matter of vast importance. They take it as a commonplace that people in close relationships anticipate a large stock of relevant knowledge on the part of the other, making possible elliptical conversation, but that participants could explain the reference of their conversation to an outside observer if

necessary and thereby make its meaning manifest, and *that this is sufficient for purposes of sociology*.

Nonetheless, the ethnomethodologists stand by their view that meaning is utterly dependent on unique situational contexts.

"It is apparent that in the interpretive [*sic*] view of social interaction, in contrast with the normative paradigm, definitions of situations and actions are not explicitly or implicitly assumed to be settled once and for all by literal application of a pre-existing, culturally established system of symbols. Rather, the meanings of situations and actions are interpretations formulated on particular occasions by the participants in the interaction and are subject to reformulation on subsequent occasions." (Wilson, 1971:69)

Similarly, Zimmerman and Pollner (1971:97) argue that the features of social situations are "unique to the particular settings" and therefore "may not be generalised *by the analyst* to other settings". That the new *Methodenstreit* is but the old reborn is here very evident. Bauman (1973b: 34) succinctly comments that those who argue for the uniqueness of socio-cultural events are

"convinced that the uniqueness of what they see and describe is an attribute of the phenomenon described and not of the very low level of particularity they have deliberately chosen or unknowingly inherited."

But given their insistence on the 'irremediability' of indexicality, they consider the interpretative view of social interaction incompatible with the logic of deductive explanation. McHugh *et al.* (1974: 3) go so far as to say that "Speech never 'solves' this problem, if by solve is meant to remedy, because any imaginable solution (any new speech) is itself a new version of the same old problem". *Chacun à son solipsisme*. It is difficult to see how this extreme position is compatible with Schutz's views on the use of second degree constructs.

Ethnomethodology and Schutz

Since ethnomethodology stands in opposition not only to "conventional" sociology but also to the work of other sociological followers of Alfred Schutz, I want to draw attention to an ambiguity in Schutz's work and to the ethnomethodologists' interpretation of it.

In a generally very clear statement of his position (1962: 48-66), Schutz spelled out the points on which he was in agreement with Ernest Nagel, who may be taken as representative of all that ethnomethodologists dislike in the logical positivist philosophy of science.

"I agree with Professor Nagel that all empirical knowledge involves discovery through processes of controlled inference, and

that it must be stateable in propositional form and capable of being verified by anyone who is prepared to make the effort to do so through observation – although I do not believe as Professor Nagel does, that this observation has to be sensory in the precise meaning of this term [i.e. Schutz rejects behaviourism – SJM]. Moreover, I agree with him that ‘theory’ means in all empirical sciences the explicit formulation of determinate relations between a set of variables in terms of which a fairly extensive class of empirically ascertainable regularities can be explained. Furthermore, I agree wholeheartedly with his statement that neither the fact that these regularities have in the social sciences a rather narrowly restricted universality, nor the fact that they permit prediction only to a rather limited extent, constitutes a basic difference between the social and natural sciences, since many branches of the latter show the same features.” (1962: 51)

Schutz, then, clearly embraces the deductive mode of explanation for the social sciences. Of course, he goes on to argue (convincingly, I think) that Nagel misunderstands the nature of *Verstehen*. He explains that it has nothing to do with introspection but, just like commonsense experience of the everyday world, is a result of processes of learning and acculturation. And *Verstehen* is “by no means a private affair of the observer which cannot be controlled by the experience of other observers” (1962: 56). So Schutz here endorses the assumption of a degree of cognitive consensus rejected by Wilson.

In the same paper, Schutz proceeds to reiterate his familiar point that because social reality already has meaning for people living within it, the social scientist must work with constructs founded upon the everyday constructs of the daily world. This notion of constructs of the second degree is the source of great difficulty. For one thing, it is not entirely clear whether Schutz thought that all or only *some* of our sociological constructs must be second order. The ethnomethodologists certainly believe that all of them must be. On the other hand, Schutz remarks at one point that “all scientific explanations of the social world can, *and for certain purposes must*, refer to the subjective meaning of the actions of human beings from which social reality originates” (1962: 62, my italics). This seems to imply that Schutz would agree with Gellner (1960) that, for example, “to understand the class structure of a society one must not only know what rank, etc., mean in it, but also how many people occupy each grade, and this is a matter of counting, not understanding”.⁶ We have already noted the ethnomethodologists’ objections to counting.

More importantly, Schutz repeatedly argued that “constructs of the second degree” must conform to the “postulate of logical consistency”, but also to the “postulate of adequacy”. By the latter he meant:

⁶ Bryant (1970) elaborates Gellner’s point and, significantly, is attacked by Walsh (in Filmer *et al.* 1972: 41 ff.).

"that each term in such a scientific model of human action must be constructed in such a way that a human act performed within the real world by an individual actor as indicated by the typical construct would be understandable to the actor himself as well as to his fellow men in terms of commonsense interpretation of everyday life." (1962: 64)

But this statement is deeply ambiguous. If Schutz means only that social scientists' theories must be able logically to subsume the practical "recipes" of commonsense, and so to relate generalising propositions to descriptions of how actual people behave in actual situations, then this is nothing exceptionable. On the other hand,

"if what he means entails that the commonsense view of the social world must be immune from correction and modification by the discoveries of social science, then what he asserts is not only false but deprives the social sciences of part of their genuine importance. Our commonsense beliefs about society are not only often false, but are also sometimes incorrigible at the level of commonsense." (Emmet and MacIntyre, 1970: xiv.)

The ethnomethodologists seem clearly to opt for the second of these two interpretations. This is the burden of their criticisms that "conventional" sociology is merely a first degree of "folk" sociology, and that its vocabularies and theories seek "to remedy the indexical properties of members talk and conduct" (Garfinkel, 1967: 11). On the other hand, I believe there is evidence that Schutz intended his remarks in the contrary sense. First, he often referred to "sets of motives, goals and roles" (1962: 64) as examples of valid second degree constructs – and these are precisely the kinds of concepts distrusted by ethnomethodologists (see for example McHugh *et al.*, 1974: 21–46, Cicourel, 1970). Second, in both his early and late work (1972: 244–46, 1962: 64–65), he spoke appreciatively of the progress of economic theory – for example, models of perfect and monopolistic competition – which claims great generality and is poles apart from the ethnomethodological style of investigation. Third, in his comments on Nagel at least, he makes it clear that his quibble is not so much with how social scientists have in the main done social science (though of course he had specific criticisms), but with Nagel's way of representing that procedure.

It is true that elsewhere Schutz can be read to give more support to the ethnomethodological position. For instance, in a paper (1964: 64–88) taken by Garfinkel as the basis for the influential last chapter of *Studies in Ethnomethodology*, Schutz was at pains to emphasize that we should not speak simply of a concept or ideal-type, but must specify the problem for which it has been constructed. Here Schutz is essentially criticising what Helmut Wagner (1964) was later to call the "falsity of the displacement of scope" (of concepts and theories from their

micro- or macro-level of origin to the other) but rather broadens the issue. Somewhat Delphically he says, "To be sure it must be admitted that the term 'level' applies strictly only to whole systems of problems; nevertheless, the consequences are, in principle, the same." However, Schutz's conclusions are broadly the same as in the paper previously mentioned. He admits that a great part of social science can be performed "at a level which legitimately abstracts from all that happens in the individual actor" (1964: 84-85), though he regards it as a kind of intellectual shorthand. Where "real scientific work" is done, however, the scientist will always have the option of shifting to the level of the individual actor. This condition is clearly met in even so apparently "abstract" and generalising a branch of social science as macroeconomic theory, and it is all that is required to meet the postulate of adequacy.

On the whole, although Schutz undoubtedly presents problems of interpretation, it seems to me that writers like Berger and Luckmann are more closely his followers than are the ethnomethodologists. In practice, the ethnomethodologists' criticisms of "conventional" sociology seem to be supported by appeals to certain arguments of Wittgensteinian philosophers.

Ethnomethodology and the Wittgensteinians

In its modern form, the case against generalising, deductive explanation in social science has repeatedly been stated by Wittgensteinian philosophers, and the ethnomethodologists often appeal to the writings of philosophers such as Winch (1958, 1964), Louch (1966) and Austin (1961, 1965) to support their own arguments.

Winch's basic argument is that the reasons people give for their actions, their explanations of their motivation, cannot be taken as the causes of those actions, because the relationship between a reason and an action is internal and logical, not external and contingent. Reasons are not therefore anything distinct from their supposed effects. This view is echoed loudly in the work of McHugh *et al.* (1974: 27-28). They see "motive" as a commonsense formulation, the acceptance of which by sociologists contributes to sociology's status as a "folk discipline". They claim that the "conventional" sociologist "still conceives of motive as a practical actor's concrete report of his state of mind: he only shifts his focus from the state of mind to the talk, by treating the talk as some sort of public indicator of the mind" (1974: 3). McHugh *et al.* conceive of reasons as the "*ad hoc* reconstruction of the concrete practice of following a rule" (1974: 86). Again, this rests on the view that rules, always having to be re-interpreted in every new situation, cannot give any clear guidance to action. Reasons, in fact, are seen as *post facto* justification for actions, and bear no relation to what brought them about. "It is something like a logician who wants to do a formal proof. The rules of logic do not provide a correct answer, they provide

a guide to how answers should be defended. Reasons should be seen in a similar way." (1974: 88)

This position, seems, however, inevitably to lead some ethnomethodologists to try to get at meanings entirely through what is externally available, so that paradoxically they end up in virtually a behaviourist position.⁷ The distinguished behaviourist B. F. Skinner has written:

"What is said by a sentence is something more than what the words in it mean. . . . The impulse to explicate a meaning is easily understood. We ask 'What do you mean?' because the answer is frequently helpful. Clarifications in this sense have an important place in every sort of intellectual endeavour. For the purposes of effective discourse [read: 'interaction in the everyday world' – SJM] the method of paraphrase usually suffices; we may not need extra-verbal referents. But the explication of verbal behaviour should not be allowed to generate a sense of scientific achievement. One has not *accounted for* a remark by paraphrasing 'what it means'." (Skinner, 1957: 8–10, his italics)

The resemblance to ethnomethodological views on indexicality and the unacceptability of reasons as causes of actions is striking.

Of course, "conventional" sociology rejects this position by assuming, along with Schutz, that it is possible to interpret motivation through a process of *Verstehen*. This is essentially the same as the commonsense process of ascription of motives, though an attempt is made to systematize and clarify sets of motives for sociological purposes – a procedure again sanctioned by Schutz. The ethnomethodologists, however, seem sceptical about the possibility of attaining such clarity because they consider *polymorphism* to be inevitable and all-pervasive both in the concepts of everyday life and within the sociological realm.

What is meant by polymorphism? Gellner (1968) sees it as one of the main pillars of the "linguistic philosophy" of the Wittgensteinians. The principle is perhaps most easily explained by means of the distinction between a concept and a homonym. An example of a very obvious homonym is the word "racket", which can refer either to a kind of criminal activity, to an implement used in the game of tennis, or to a medieval musical instrument. In this case there is no connection between the three meanings, but in the case of polymorphism, which Wittgensteinians see as much more pervasive than simple homonymity in the ordinary use of language, a word has a variety of related though far from identical meanings. As Gellner (1968: 50–51) pointed out, traditional, pre-Wittgensteinian philosophy assumed that

"a concept, if clear, has one criterion (which may of course contain a number of conditions in conjunction or even as alternatives), and that if on the other hand there is a multiplicity of sets of criteria, we are already dealing with homonyms."

⁷ Any resemblance between ethnomethodologists and behaviourists may seem strange, since the former see the latter as the very embodiment of 'positivism'. However Day, in two interesting articles (1969a, 1969b) has argued that there are strong affinities between, on the one hand, Skinnerian behaviorism and phenomenology, and on the other hand, between Skinner and Wittgenstein.

But this clarity is precisely what is denied by "linguistic" philosophers, who strenuously emphasise complexity, variegation, blurred boundaries and transitions between concepts. Put more precisely:

"The 'polymorphus' diagnosis offered of this situation by Linguistic Philosophy runs as follows: a term may very well apply to a whole class of objects A, B, C etc., in such a way that A does have something in common with B, and B with C, but not necessarily A with C. There may, as it is put, be a 'family likeness' such as occurs when a member of a family is recognizably one of it, yet no two members need have any features in common." (Gellner, 1968: 49)

This is the classic Wittgensteinian idea of "family likeness", and something very close to the principle of polymorphism is clearly a key part of the ethnomethodologists' case. So far as I can trace, only Coulter (1973) has actually used the word polymorphism, but the principle is perfectly evident in ethnomethodological criticisms of official statistics and discussions of indexicality. Obviously it would be 'futile to deny that there are imperfections both in social statistics and in sociological vocabulary (consider the term "role", or the Minoan labyrinth of terminology used in the study of social stratification). Sociologists need not deny it, so long as they remain convinced, as the ethnomethodologists are not, that in social science, just as in natural science, progress is always possible through the process of criticism towards greater consistency, coherence and clarity.

But a second pillar of "linguistic" philosophy identified by Gellner is the "Argument from the Paradigm Case" (APC for short). This is the argument that words mean what they are used to mean. If we deny that a word means what it is customarily said to mean, we must either be ignorant of the language, or be intent upon reforming or correcting it. Both the "linguistic" philosophers and (as we have seen in their views on the "irremediability of indexicality") the ethnomethodologists claim this is absurd. The ethnomethodological variant of the APC can be seen in this passage from Garfinkel:

"Characteristically, formal investigations have been concerned either with devising normative theories of symbolic usages, or while seeking descriptive theories, have settled for normative ones. In either case it is necessary to instruct the construing member to act in accordance with the investigator's instructions in order to guarantee that the investigator will be able to study their usages as instances of the usages the investigator has in mind. But following Wittgenstein, person's [*sic*] actual usages are rational usages in *some* 'language game'. What is *their* game? As long as this programmatic question is neglected, it is in-

evitable that person's usages will fall short. The more will this be so the more are subject's interests in usages dictated by different practical considerations from those of investigators." (1967: 70, his italics)

Plausible as the APC may appear, Gellner (1968: 37) argues that it is basically silly; it confuses connotation and denotation. The fact that people in a given society have a use or a range of uses for a term (such as "miracle") in no way establishes that their suppositions about the workings of the world are correct. The APC is a manifestation of an ancient philosophical tendency, against which Popper (1972: 24) argues:

"When Kant said our intellect imposes its laws upon nature, he was right – except that he did not notice how often our intellect fails in the attempt: the regularities we try to impose are *psychologically a priori*, but there is not the slightest reason to assume that they are *a priori valid*, as Kant thought."

Science can and must seek to correct commonsense.

Conclusions

Though it is beyond the scope of this paper to argue adequately for what I believe to be the superiority of realism over phenomenology, logical positivism and Wittgensteinianism as an epistemological basis for social science, Popper's remark does point towards some general conclusions which can be drawn from the foregoing discussion of ethnomethodology.⁸

The fundamental flaw in the ethnomethodologists' position, it seems to me, is their two-level conception of knowledge, for which they draw support both from Schutz and the Wittgensteinians, and which entails the view that there is a "commonsense world" which sociology cannot and must not attempt to correct or "remedy", but only to analyse. In consequence, as raw material for analysis, they call for "literal descriptions" of social interaction and talk (see Wilson 1971: 71ff, Filmer *et al.*, 1972: 107–115). Along with logical positivists like Carnap, they accept the possibility of a pretheoretic "observation language" (see Hindess, 1973: 51–58). Popper, on the other hand, argues that all observation, both laymen's everyday observation and scientific observation, is "theory-impregnated". It is not possible to know or describe anything without employing some general concept. There is no sharp discontinuity between everyday and scientific knowledge; propositions from commonsense are the starting point of all science, and scientific knowledge evolves only by these being systematically exposed to the process of criticism. In this limited sense, the ethnomethodologists'

⁸ Readers interested in Popper's acceptance of commonsense realism but rejection of the commonsense theory of knowledge should consult particularly his *Objective Knowledge* (1972).

charge that sociology is a "folk discipline" is correct. To admit this, however, they allege,

"... assumes sociology to be a disciplined investigation that is fully competitive with members' relaxed investigation. . . . Such an assumption leaves unexplicated members' methods for analysing, accounting, factfinding, and so on, which *produce* for sociology its field of data." (Zimmerman and Pollner, 1971: 83)

The latter conclusion is, I think, untrue. There seems little reason why the new interests in linguistic behaviour and what might be called the "ultra-micro-analysis" of social interaction cannot be handled within other epistemologies, and indeed Sacks and his colleagues (Sacks 1972a, 1972b, Schegloff and Sacks, 1973) have already produced generalisations from conversational analysis which seem quite capable of being absorbed into orthodox sociology.

Ethnomethodologists often approvingly quote W. I. Thomas's famous dictum that "if men define situations as real, they are real in their consequences". But they have a strong tendency to abbreviate it in effect to "if men define situations as real, they are real". That social reality must remain akin to the shadows of the prisoners in Plato's cave seems to me to be the drift of, for example, Pollner's criticism of Becker, mentioned above. Such a view seems to me to stand in the way of asking the questions of most interest to both sociologists and laymen. For Thomas's dictum in its untruncated version leads to the study of the unintended consequences of social actions,⁹ which is the very basis of sociology's interest and importance. And this stems precisely from the construction of sociological theories combining propositions about participants' perceptions of the situation with propositions contributed by sociologists about the dynamics of the wider structures in which participants are enmeshed but can only partially or inaccurately perceive from their individual vantage points.

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⁹ Merton (1968: 475-90) takes Thomas's dictum as the point of departure for his classic discussion of the 'self-fulfilling (and self-contradicting) prophecy'. But the idea of unintended consequences of social action can be interpreted much more broadly (see Mennell 1974: 168-171) so that investigating them is arguably sociology's very *raison d'être*.

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