

On Doing and Saying.

A Note on the Disjunctures of Criticism.

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Introduction

This Note is neither a defence of ethnomethodology nor a reply to its critics. It complains about the standards of critical analysis which have been applied, thus far, to ethnomethodology. Although papers critical of ethnomethodology are now very numerous, none of them, in our opinion, contains any consequential contribution to the debate over the relationship of ethnomethodology to other species of sociology. The objections which have so far been offered would not lead anyone committed to an ethnomethodological position to rethink their basic strategies. Indeed, little if any of the criticism would give reason for more than a moment's pause to those it criticises. It is not that ethnomethodology is incorrigible but that the attack to date has been ineffective largely through its own deficiencies. The critics rarely display any clear understanding of the tenets or implications of their object of criticism.

We shall argue that the following shortcomings are characteristic of the work of aspiring critics of ethnomethodology:

- 1) they are often disingenuous about the plight of sociology and engage in wholly spurious comparisons of ethnomethodology with 'conventional sociology'.
- 2) they focus their attacks upon peripheral and emblematic figures but refrain from careful review of the core writings in the field.
- 3) they commonly misunderstand and misinterpret those writings they do examine and consequently treat ethnomethodology as if it were a series of isolated and sloganised assertions without attempting to appreciate the reasoning that underlies the assertions

which they have isolated.

- 4) in criticising ethnomethodology, they cast their arguments in terms of categories employed by ^{other} sociological ^{approaches} when it is precisely those systems of categories that are rejected by ethnomethodology. Criticism, thereby, dissolves into a ritualised re-assertion of difference with ethnomethodology being denounced for failing to be whatever brand of analysis the critics subscribe to.
- 5) the critics seem to have a powerful interest in attending to ethnomethodology as an argument about epistemological and ontological concerns, whereas it seems to us that matters of procedure are just as salient, if not more so.

Of course one of the major difficulties we share with the critics is in the delineation of what ethnomethodology is. The writings which make, or claim to make, contributions to the field are highly numerous, diversified and often mutually contradictory. Anyone seeking to formulate ethnomethodology's position is in danger of being charged with selective citation and consequent misrepresentation. On the other hand, anyone conversant with the literature ought to be able to see that the work of Garfinkel, Sacks and, to a lesser extent, Cicourel, represent the central core of ethnomethodology to date, and are much more definitive than other writings. To draw, as critics often do, upon lesser, and, in some cases, utterly marginal contributions in preference to detailed discussion of the main texts, is to pick the easiest targets. One of the things which renders lesser works attractive to critics is that they provide ready and concise characterisations of ethnomethodological positions which may be neatly extracted and cited following

only a superficial reading whereas the work of Garfinkel, Sacks and Cicourel do not provide such conveniences. The examination of these latter texts cannot proceed without involvement in their specifics; something which critics do not seem amenable to. We shall not attempt to overcome the problem of divergence in interpretation of what ethnomethodology is, nor try to state, here, any kinds of definitive positions which can be construed as those which ethnomethodologists do, or must, take. We will offer what seems to us to be reasonable interpretations of central positions. It is of less concern to us to say what ethnomethodology is or must be than to show that it does not have to be understood in the terms that its critics stipulate,

A reciprocal difficulty arises with respect to our characterisation of 'the critics' because they too comprise a large and mainly disunited collection and because we cannot hope, in such brief space, to inspect the whole array of critical argument. We have selected for particular attention, three relatively recent accounts which we feel encapsulate, in their different ways, many of the standard arguments.¹ Furthermore, they do seem to display as careful a reading of the literature as [do] most of the alternatives. We do not regard these as particularly effective essays in criticism but that is the point. So far as we are aware, there are no more successful contributions which might have been preferred to them. This much by way of introduction; it is now time to turn to the charges listed above.

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1. S.J. Mennell Ethnomethodology and the New Methodenstreit, in New Directions in Sociology (ed D.C. Thorn) Davis + Charles 1976.
A. Tudor. Misunderstanding Everyday Life. Sociological Review vol 24 no 3 1976.
D. Gleeson and M. Erben. Meaning in Context: notes towards a critique of ethnomethodology. British Journal of Sociology vol 27 no 4 1976.

The Fiction of Closed Ranks

In any disagreement it is always possible to claim that there is very little difference between the opposing positions and this tactic has often been used against ethnomethodology. It has been argued that ethnomethodology adds little to the stock of ideas already available to sociology. This argument reaches its nadir in the assertion that ethnomethodology is merely another name for the techniques of participant observation.² But to argue that kind of case effectively, there is a need to be careful not merely to clarify what it is that ethnomethodology is supposed to be saying, but also to specify accurately the other sociological assertions with which ethnomethodology is being compared. In much of the critical literature, the characterisation of 'sociology' is just as inadequate as that of ethnomethodology. It is not so much that specific sociological theses are misrepresented as that they are rarely specified. Instead, there are frequent ~~and~~ vague appeals to an indefinite 'sociology'. Some critics seek to put ethnomethodology in its place by a sanctimonious invocation of the 'standard' sociological position as though that were accepted by all except recalcitrant adherents of ethnomethodology. But the 'standard position', like the Unicorn, is ^{something of} a mythical beast.

Ethnomethodology may seem to have initiated this practice of talking of the 'standard position' and 'sociology' by its proclivity to indulge in tirades against 'conventional sociology' - a label which insensitively lumps together quite diverse and disputing schools. However, ethnomethodologists have never supposed that sociology incarnates

2. B. Gidlow. [~~Ethnomethodology - A new name for old practices.~~
~~British Journal of Sociology~~ vol 23 no 4] (1972)

some united and coherent enterprise for, after all, part of the motivation for undertaking the creation of ethnomethodology in the first place was an awareness that sociology is a disunited and disorderly pursuit. Nonetheless, ethnomethodologists have, naturally enough, sought to identify those features common to, or prevailing among, the many schools of sociological thought, for it is surely more economical to decide how to do something after one has examined the avenues that have been explored and found wanting: there being little point in replicating avoidable errors that one's predecessors have made? But it is not merely the case that sociology is diverse and riven by disagreement. The parties to the disagreement are themselves often aspiring to make radical breaks with each other on the grounds that the discipline is in such a parlous condition that some drastic course of action is required. From the point of view of the individual sociologists, it is often the case that the rest of sociology is in an abominable plight. Ethnomethodology is not, therefore, unique in proclaiming that 'conventional sociology' is in a bad way. It shares, with many of those that criticise it, the disposition to reject the bulk of sociological work as being of very little interest and restricted achievement.

It may very well be the case that, as Mennell suggests,

"ethnomethodologists reject deductive explanation of the 'covering law' kind in social science....."

(Mennell p 139)

but the argument about the hypothetico-deductive model concerns its association with the logical positivist philosophy of science, and the application of that to 'sociology'. It simply will not do to come to the defence of 'sociology' by championing the hypothetico deductive model, since those who recommend its

adoption are only too apt to point out that sociology does not have, nor appear likely soon to obtain, anything approaching a viable hypothetico-deductive theory.³ Neither can claims on behalf of sociological orthodoxies be shored up by indications that the hypothetico-deductive model provides a general and desirable goal for sociology since what few efforts^{that} have been made in the direction of creating sociological embodiments of hypothetico-deductive theories have met with a less than enthusiastic reception.⁴ The appeal to the hypothetico-deductive case serves only to remind us of the very point ethnomethodology makes: namely, that sociology is in need of some remedial effort and that the available sociological schemas are far from achieving the goals which might be set for them. Even here we must be careful not to do an injustice to those who employ such schemas, for often they too are aware of their manifold and inherent limitations. Parsons, for example, has always been careful to point out the inadequacies structural functionalism relative to the goals

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3. It is in order here to note that the rejection of the hypothetico-deductive model, where that is done in order to reject the logical positivist philosophy of science, need not consist in arguments about the impossibility of hypothetico-deductive theories in sociology. It need only involve showing, as many of the counter arguments do, that the model is not the only legitimate one for proceeding in science. Consequently the prescription of hypothetico-deductive theorising as a solution to sociology's ills does not represent the only course of treatment. The kinds of things that individual ethnomethodologists are doing may be just as legitimate as the strategies proposed by Homans and other proponents of logical positivism. It follows that there need be no embarrassment in the discovery that Schutz accepted the deductive mode of explanation as outlined by Nagel. Good theories should be explicit and systematic. Who would want them otherwise? However, that some piece of work does not display an explicit and systematic theory does not mean, as advocates of hypothetico-deductivism sometimes seem to think, that it is not scientific. In so far as logical positivism offers hypothetico-deductivism as the criterion of science, it is inept.
 4. As in the case of Homan's work on an exchange model.

he envisages for it. It seems that everyone agrees that all is not well with sociology.

Mennell's invocation 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..."

(Mennell p 156)

and Tudor's yearnings for a "synthesis" of

".....sociological perspectives which emphasise the creative role of the actor with those which emphasise his status as a more or less passive recipient of larger social forces."

(Tudor p 501)

exemplify the kind of appeals to which we are objecting. What theories are they that could make this combination, synthesis or linkage ? Candidates for such a role might be functionalism, Marxism or symbolic interactionism, but none of these can seriously be regarded as constituting a hypothetico-deductive theory or of successfully displaying the 'combination' or 'synthesis' required.⁵ Both Mennell and Tudor display an optimism about the power of combination or synthesis, assuming that such positions are evidently superior since they encompass both extremes, whereas it might equally blandly be assumed that the blending of two (or more) partial or distorting views will lead, not to clarity, but to even greater distortion. In any case, what is required is an account which looks like a real synthesis rather than a wish for one. ~~[But]~~ Until criticism becomes more specific, we feel that the argument ought to be cast in terms of promises. Much of what is called 'conventional sociology' may well promise

5. Or at least their proponents do not consciously accept them as such.

better results in terms of the ends sought by Mennell and Tudor for it is meant to pursue those ends. Ethnomethodology, on the other hand, is held by the critics to reject them: it is not surprising, therefore, that it is unlikely to attain ~~them~~^{those kinds of results}. Further, the fact that many sociologists are dedicated to those ends does ensure their realisation. There are strong grounds for doubting if sociology will ever achieve the results required by critics like Mennell and Tudor. That is why ethnomethodology ventures a different approach. It is at best a strategy for further enquiry having the same provisional character as the sociologies with which it contends.⁶ The argument should not be over ^{a comparison of} the programme of ethnomethodology with the achievements of conventional sociology but of the viability, acceptability and promise of the two programmes. To find out who is right, if everyone is not wrong, we must wait and see what the results are.

The Presentation of the Position

One of the more remarkable things about the critical commentaries is the ease with which they seem able to refute the positions that ethnomethodology is deemed to hold. Anyone who comes to them without prior acquaintance with ethnomethodology would, no doubt, feel justified in asking how anyone could hold the positions ascribed to ethnomethodology. It is often true, of course, that no one actually does hold these views. The presentation of ethnomethodology is so unsystematic

6. It is precisely this provisional character of sociology that Garfinkel is attending to in his discussion of "the unsatisfied programmatic distinction between and substitutability of objective for indexical expressions". (Garfinkel 1967 p 4)

bowdlerised and disjointed as to be virtually unrecognisable to those who think of themselves as ethnomethodologists. The very term itself is held to encompass such a rag-bag of activities that Tudor is forced to coin the ferocious neologism "everyday-lifers" to cope with them while Mennell admits in his first foot-note that

"To speak of ethnomethodologists as I do in this paper necessitates oversimplification. They have many difference among themselves; some writers often identified with the school (eg Douglas) would not or would no longer apply the label to themselves."

(Mennell fn1 p 157)

and yet, despite this, he persists in treating them all as one. There is a pervasive superficiality apparent in the supposition that any kind of summary of the field will be more than adequate.

"They see actors as continually groping towards a definition of the situation at hand, and situations as being subject to continuous redefinition. In particular, all conversations contain numerous expressions that 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 situations at hand."

(Mennell p 146.)

It is not even a question of whether or not this is a correct characterisation. It seems to us that the assertion is so badly expressed that it is closer to a travesty than an accurate description. We think that we can see what Mennell is trying to say and even if we were to concede that the insights of ethnomethodology were something akin to the ideas formulated by Mennell, we should jibe at their expression in such a manner. Furthermore, if we were to agree that, say, actors were seen as "continually groping ...etc.", we should like to know, firstly, how in seeing that we differed from functionalists, Marxists, action theorists, and symbolic interactionists; and secondly,

how that view affects sociological practice. Even if ethnomethodology^{ists} did see actors as "continually groping.... etc" that would be a most marginal element in their thought. As it is, of course, they seem to us to stress the 'routine' and 'unproblematical' character of defining the situation at hand.

At least Mennell does attempt some statement of the position. Tudor and Gleeson and Erben contrive to avoid any explicit formulation of what it is that requires their criticism. Tudor manages this by lumping together all those who have "a concern with a sociology of everyday life" (Tudor p 420) and then refusing to deal with whole groups within this amorphous set without any justificatory argument, except to say that they do not make "general criticisms and alternative proposals" (p 480). What is excluded is, thereby, reduced to the status of the marginal, the irrelevant and the easily dismissible. The residue contains such diverse brands of sociology as that of Berger and Luckmann, Glaser and Strauss and the^early work of Garfinkel. A melange which makes the notion of any common position ludicrous. Gleeson and Erben do not even get as far as specifying what they mean by ethnomethodology. Consequently, we found it impossible to deduce from their article precisely what tenets, proposals and propositions were under inspection and criticism from their curious position of an admitted and yet "implicit" Marxian sociology.

There is, then, a persistent unwillingness to grant ethnomethodology a parity of status with the perspective, school or stance from within which criticism is being offered. This refusal, naturally, presents ethnomethodology as an inconsistent, incoherent and illogical jumble of recipes,

programmatics and findings. In fact, it is a refusal to deal with the topic in any serious way at all. We would argue that ethnomethodology does have a systematic and articulated character but that the critics fail to present this [in any way] whatsoever. An inspection of the relevant literature, Garfinkel's papers, Sacks' lectures and Cicourel's early work, and the extensions, elaborations and clarifications that have been published since, should provide ample material for undertaking this task. While it is not our concern here to detail the relationships between Garfinkel's work on practical decision making and indexicality and the kind of analyses engaged in by others in the field, any serious attempt at criticism would have to work through this relationship and its ramifications in order to come to terms with the central thrust of the position. Glib summaries of the uniqueness of social situations, intentionality, contextuality are simply not enough. Garfinkel, Sacks and Cicourel would readily admit the triviality of claiming that situations are distinct, meanings contextual and actions intentional. Their work suggests that the interesting and difficult thing is to work out what implications the explicit recognition of such commonplaces would have for a rigorous sociology. In addition, the fact that there are many 'schools' of ethnomethodology must be taken into account. An examination in detail of the various research proposals now included in ethnomethodology, would require a spelling out of the particularities of each, for there are very great differences between them. Any worthwhile criticism would both explicate these differences and assess them in terms of the central corpus of work rather than dismissing them as "professionalism" or "alienated labour reflecting on itself" (Gleeson and Erben p 475).

explicitly

The failure to provide anything like a systematic presentation of the field is often conflated with the presumption that the sketch of an intellectual biography is enough to frame a body of sociological practices. Re-iterating that the term ethnomethodology has been in use since the mid-fifties when Garfinkel was working on Jurors does not tell us anything about the interest that people now have in the field, nor about the reasons why researchers in the field adopt the methods they do. Nor is it enough to record that Garfinkel follows the logician Bar-Hillel in using the term indexicality and that Schutz and Parsons were formative influences on his thought. It is necessary to discriminate the range of influence and to demonstrate how it can be discerned in his work. To undertake this task would be to follow Garfinkel's own lead, for as he says in his Preface,

"These articles originated from my studies of the writings of Talcott Parsons, Alfred Schutz, Aron Gurwitsch, and Edmund Husserl. For twenty years their writings have provided me with inexhaustible directives into the world of everyday activities. Parsons' work, particularly remains awesome for the penetrating depth and unfailing precision of its practical sociological reasoning on the constituent tasks of the problem of social order and its solutions."

(Garfinkel. 1967 p ix)

and prevent absurdities such as Gleeson and Erben's allegation that

"Ethnomethodologists paint, for example, Parsons Merton, Durkheim etc as intellectual dopes who are both naive and methodologically clueless on the grounds that they fail to examine the complex glosses concerning the constituent features of how interaction occurs."

(Gleeson and Erben p 475)

The refusal to embark on this kind of examination is precisely

why all that are available to date in the critical reviews are shoddy summaries and wholesale characterisations. It is on the basis of just these summaries and characterisations that the *enfeebled* strawmen erected by the critics ^{are found to} collapse at the slightest touch.

If, for the most part, it remains opaque as to exactly what the critics feel ethnomethodology to be, there can be no doubt that its eminence *gris* is the shadowy figure of Alfred Schutz. In both Mennell's and Tudor's papers, it is assumed that the major contribution of Schutz to ethnomethodology is an epistemological one, and in particular, the proposals contained in the papers on theorising and method. Now, while we would not wish to deny the importance of Schutz to ethnomethodology, we would not ~~wish~~ ^{want} to overrate or misrepresent his influence. A close reading of Garfinkel's work and comments would lead to the conclusion that his relationship to Schutz's brand of phenomenology is complex, and that more recent work retains an even more tenuous and complicated connection. What would ^{also} be apparent is that ethnomethodology is certainly nothing like the sociology that Schutz envisaged. We doubt if it is the case that the majority of ethnomethodologists would find most of Schutz's theoretical prescriptions as acceptable as Mennell asserts :

".....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."

(Mennell p 149)

Since he offers no documentation we cannot examine the sources, but for us, and we suspect for others, Schutz distinction between primary and secondary constructs is a largely useless one.

If Schutz's influence is not found mainly at the level of theory, where does it lie? Yet again we have to recommend an inspection of the literature which would show that Schutz's essential contribution to ethnomethodology was to provide it with its phenomenon. Any kind of close analysis of original papers⁷ would show that it is the crucial notion of the 'natural attitude to the life-world' that has been taken up together with a concomitant exploration of its 'intersubjectivity'. The connection between Schutz and Garfinkel is probably best understood by recognising that Garfinkel sought to take up Schutz's topic in relation to the condition, problems and technology of American sociology in the post war period. Schutz's own efforts were largely based on Weber's ^{work} ~~conception~~. They have, therefore, very different conceptions of what would be involved in taking up the emphasis on the pre-given life world. Thus, Garfinkel's oft cited experiments with students display a concern with the nature of the common-sense world as it is experienced by social actors, but his concern is mediated by a reading of Parsons. In particular, they are an attempt to show how systems of norms and values feature in the taken-for-granted world. This is, then, no simple minded adoption of Schutz's phenomenological position. Rather, the influence of Schutz must be seen in terms of Garfinkel's reading of both Parsons and Wittgenstein, and his attempt to derive a sociological procedure by welding all three together. They are, then, subordinated to the task

7. Eg those in D. Sudnow (1972), R. Turner (1974) and of course Garfinkel himself (1967).

The influences

of doing sociology and the taking up of issues and problems they may not even have conceived of. Any serious teasing out of these strands could hardly come to the conclusion that

" Ethnomethodology asserts the randomness of social activity which makes sense at the level of intentionality."

(Gleeson and Erben p 476)

for no-one would suggest that Parsons, Schutz and Wittgenstein treat the social world as random and, indeed, ^{the result of} Garfinkel's reading of them, ^{is to} stress ~~its~~ its orderly and methodical character. If ethnomethodology is to be measured against the traditions with which its critics see it as affiliated, then it must also be seen that, insofar as they do adopt such traditions, they do so in a critical and selective manner. Analysis of ethnomethodology's lineage can only be ^{carried out} ~~done~~ by detailed exposition, most crucially of the ways in which the derivative elements interact. It is not enough simply to repeat generalisations and imputations which in themselves are misleading or distorting. If the influence of Parsons, Schutz or Wittgenstein is held to contribute to the failure of ethnomethodology, then this baleful influence must be documented in the work of the practitioners. That will require the critic to grapple not only with ethnomethodology but with structural functionalism, phenomenology ^{or} ~~and~~ analytic philosophy as well.

Misunderstanding Just About Everything

Where the writings of someone like Garfinkel do receive careful inspection, they are, alas, all too commonly misconstrued. Furthermore, as we have pointed out, attempts to dismiss the points that ethnomethodology is making are directed against the critic's own creations, namely positions that they have invented for ethnomethodology to hold. Because of

the limitations of space we cannot attend to all such misreadings and misconceptions. Instead, by way of illustration we will deal with one, Mennell's treatment of indexicality. Mennell ^{Contrasts} ~~summarises~~ ^{and 'Conventional' Sociology} Garfinkel as follows:

"Garfinkel.....illustrates his view of indexicality with an excerpt from a highly elliptical conversation between a husband and wife, together with the 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 the purposes of sociology."

(Mennell p 146 emphasis in original)

Here again ethnomethodology is held to be saying nothing new: Garfinkel is merely demonstrating a sociological commonplace, namely that people share understandings. If, however, one reads Garfinkel's text carefully one can see that he himself is far from claiming any originality in showing that understandings are shared. It would be a poor sociologist indeed who did not know that one of the main supports of much sociological reasoning is the assumption that social order is generated through shared understanding. What Garfinkel is doing is counterposing two alternative conceptions of shared understanding: one of which we think is original to him; the other is conventional enough. The standard notion of shared understanding is that of overlapping sets of knowledge, whilst Garfinkel's conception of it is an operation. Sharing understanding is showing and finding understanding, not holding something in common. Garfinkel attempts to enforce the 'matching sets' conception but

finds that it does not seem to work. If there are some shared understandings which generate activities, and if the theory holds that the activities arise from understandings, then it should be possible, indeed is desirable to spell out the understandings that are shared. The purpose of Garfinkel's experiment is to have his student's articulate those understandings; a task which they come to realise is impossible. If some sociologists argue that social stability is generated through tacit understandings, and if their theory requires them to specify the corpus of knowledge which constitutes those shared understandings, then the implication of Garfinkel's experiment is that the task cannot be brought to a successful conclusion. Consequently, those theories current in sociology which seek to generate activities through specification of a normative order will be in difficulty. Their conception is not a realisable one unless they have recourse to supplementary elements not included in their theories. It is this which underpins Garfinkel's argument concerning cultural dopes in sociology. In following Schutz's policy of treating theorising rigorously he characterises the theorists' accounts as featuring not actors or persons but dummies, entities having only those properties endowed by the theorist. Such cultural dopes in contemporary sociology are creatures allowed the capacity to follow rules and enact roles, but denied by the theory other human capacities for decision making, invention, imagination and the like. If the implications of Garfinkel's experiments are correct then the theorists cannot specify the exact nature of the rules and shared understandings which their theories suppose concert action. As a result they find themselves invoking the 'etc principle' that their theories be understood to ~~refer to~~ ^{refer to} things ~~that~~ ^{they} do not

Such things
mention because ~~they~~ cannot be spoken of in particular and in detail. Hence, although the theorists do not endow their creations with those capacities explicitly, they invoke them tacitly. If it is assumed, therefore, that the explicit incorporation of elements into a theory is essential, then the available theories of rule following are inadequate. Garfinkel may well be wrong in this claim, but he is certainly not assenting to a sociological commonplace.

It would be foolish to deny that many of the things that ethnomethodology says are commonplace. But much of ethnomethodology's reasoning involves the attempt to try to see clearly what is involved in accomodating such commonplaces within systematic sociological thinking. It is these implications that are important, for ethnomethodology could argue that the analytic consequences of 'shared understanding' have never been appreciated in sociology.

Commonsense: Its Place and Role

Arguments against ethnomethodology are regularly cast in epistemological and ontological terms with the result, we would suggest, that the role of 'commonsense' is repeatedly misunderstood. These arguments often involve ethnomethododology being charges with a) denying the possibility of objectivity and b) elevating commonsense to an inviolable status. But for it to do both would surely be inconsistent? We shall argue that in fact it does neither, and that the misunderstanding arises from the confusion of ethnomethodology's procedural recommendations with epistemological theses.

A fairly characteristic manifestation of this misunderstanding can be seen in Mennell's complaint that many ethnomethodologists maintain that

" Many of the 'facts' that sociologists tend to accept as data have been created in everyday situations for practical purposes. They are not facts but 'accomplishments'."

(Mennell p 142-3)

By replacing facts with 'mere' accomplishments, on this argument, ethnomethodology is not ^{درد} ~~only~~ undermining objectivity but replacing it with something else. Mennell, as is usually the case, wants to generalise the point into a discussion of sociology itself. We cannot engage with this additional complexity here. We would like to suggest though, that it makes a considerable difference if matters are restated only very slightly: ethnomethodologists treat facts as accomplishments. This does not entail bringing their factual status into doubt, particularly as it is ethnomethodology's pre-eminent policy to suspend judgement about the factual status of assertions or claims made by those it studies. It recommends, instead, that we study the ways that people accomplish for themselves and for others the factual character of things: in plain terms, how do people convince one another that things are as they are ? The policy of suspending judgement applies to 'commonsense' as well. Ethnomethodology neither concurs with nor endorses those ideas or beliefs which constitute 'commonsense'. It cannot do so for it does not accept that 'commonsense' comprises a definitive set of propositions held in common. It views all social phenomena as 'courses of treatment'; a stance which requires it to look upon them as consisting in the ways that people deal with them. / The ethnomethodological emphasis on 'commonsense' is

^ This applies to 'commonsense' as much as to anything else.

a stress upon the observable fact (which above all else Garfinkel's experiments were designed to show) that actors in any social situation will insist that some things be treated as transparently obvious, intelligible, indubitable and beyond argument and question. Though, of course, exactly what things are so treated may vary from person to person. As far as ethnomethodology is concerned it is the insistence on the indubitable nature of some matters that is the phenomenon of note and the important thing is to investigate the ramifications of that insistence for the production of orderly activities. It is ~~not~~, therefore, ^{not} logically possible for ethnomethodology to ratify any of the diverse and contending things that might be asserted as fact and to raise 'commonsense' to an inviolable status, even if it wished to. For it is not possible to hold both a proposition and its contrary.

Consequently, a methodological strategy which identifies the 'reality' of a phenomenon with the procedures used 'to make it real' (that is, identifies 'the world' as 'the world perceived') must not be confused with any relativistic ontological assertions about that reality. A procedural suspension of judgement cannot be even a tacit agreement.

Atheoretical Categories ?

In this final section we wish to deal, as briefly as possible, with by far the commonest kind of criticism launched at ethnomethodology, namely the allegation that it ignores the kinds of analyses felt to be characteristic of sociology in general.

" Because they focus on the micro-negotiation of interactive realities, they neglect the traditional concerns with 'emergent' phenomena such as stratification, power and institutional structure. (Tudor p 499)

Gleeson and Erben make it clear that they have "misgivings"

"concerning the failure of ethnomethodology to make explicit critical comments concerning those features of interaction which may exercise oppressive influence on the meanings of members."

(Gleeson and Erben p 474)

and come to the conclusion that it

"fails to engender discussion of a sympathetic commitment to the alienated condition of members' present states of theorising."

(Gleeson and Erben p 482)

Now what are we to make of these charges ? Since neither argue to their case we are left in some quandry as to why ethnomethodology should have to take an interest in these kinds of phenomena in the first place. More sense could be made of them if there was general agreement in sociology that, firstly, these were the core concerns; and secondly that there were specific ways of treating with them. But as we have pointed out, sociology does not exhibit any agreements on general issues or methods.⁷ The assumption of a unified body of theoretical categories and methodological practices is a fiction. But even if that fiction were fact the charge would still be insubstantial for it pre-supposes that ethnomethodology wishes to usurp the whole of sociology. ~~(But)~~ This is not the case. Rather, one of the consequences of treating sociology as a practical activity will be to imply that for-all-practical-purposes it works. That it has the character it does renders it amenable to study; that is, the investigation of how sociologists accomplish the factual status of the things that they do.⁸ If ethnomethodology has

7. One of the continuities, in fact, of sociological debate has been the ontological status of these 'emergent' properties.

8. Cf Blum's remarks pp334-6 in his paper 'The Corpus of Knowledge as Normative Order' in McKinney and Tiryakian (1970)

We have not
we have
Gleeson &
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no pretensions to sociology's place, then there are no good reasons why it should take up all of sociology's supposed 'traditional' concerns.

Because ethnomethodology has no imperialistic ambitions it follows that criticism must be addressed to its own concerns, the pre-given world of everyday life. Here, it is for the critic to show that analytic devices alleged to be 'traditional' have utility with regard to the data that ethnomethodology studies. Criticism cannot consist solely in bemoaning the fact that other researchers do not share the critic's interests or outlook. That would be bigotry. It must be the careful and detailed exposition of the failings of any procedure or body of thought relative to the goals it sets itself. This is not, of course, a retreat into simplistic relativism, that structural sociologies cannot criticise interactional ones. But it is to suggest that the responsibility lies with the critic to demonstrate, both in general and in particular, how the traditional categories of sociology are of direct relevance to ethnomethodology's data and interests. The hackneyed re-assertion of differences will not suffice, and even less impressive are acrobatics like the following:

"Marxism has been as keenly aware as ethnomethodology that to put down to a pure externality all the occurrences of mind is a reduction which ensures a logically absurd position. But to deny pure externality does not also imply the pure denial of an externality."

(Gleeson and Erben p 480)

What, of course, is significant is the fact that, as far as we know, no-one has managed this kind of criticism. The categories of power, institutional structures and stratification come trailing clouds of theory; theories within which their usage is made clear and sensible but which are inappropriate to

to ethnomethodology's research interests. To apply the categories would be to invoke the theoretical perspectives which make sense of them. So, the claim that ethnomethodology neglects traditional categories, in fact, turns out to be a claim that it does not work within the theoretical perspectives held to be the corpus of 'conventional sociology'; and what kind of criticism is that? Consequently, for Gleeson and Erben to suggest

"there is a need to explore the nature of educational contradictions such as truancy; deprivation; failure or delinquency as indications of structural alienation rather than mere 'mangled' glosses of socially structured meanings or taken-for-granted realities. The nature of such 'routine' and 'normal' problems are (sic) of course socially constructed, but what is not clear are (~~the~~) the conditions of continuance and domination through which the routine and normal become reproduced."

(p 478 emphasis in original)

is nothing but rhetoric. Naturally, if what is at issue here is the perennial question of the proper topics and procedures for sociological enquiry, then of course, it is always possible to contrast two approaches by listing the various things they consider as legitimate topics for investigation, the procedures they recommend and so forth. But, then to complain that one approach does not attend to the same things as the other, or does not attend to them in the same ways, is hardly to offer serious criticism at all. It is purely the emphasis of differences since what divides the two approaches is not particular topics of enquiry but their thematising pre-occupations and their conceptions of what ought to be done. Criticism has degenerated to sophistry. This is where we started. By emphasising the differences the critics wish to suggest that theirs is the only possible theoretical apparatus for sociology. The fiction of

closed ranks justifies the misplaced invocation of atheoretical
justice
categories and ~~just~~ once more fails to be done.

We are well aware that in this Note we have not taken up what many will think to be the significant issues raised in the three papers we address. We would not wish to deny that these issues are important and must be examined. However, we feel there is little purpose in discussing these kinds of issues until the elementary mistakes, misconceptions and confusions we have examined here have been dealt with. This Note has been an attempt to clear the ground so that sensible debate might begin.