

THE RELATIONSHIP BETWEEN ETHNOMETHODOLOGY AND PHENOMENOLOGY

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Introduction

The orthodox view of ethnomethodology holds that Garfinkel's foundational work was an attempt to forge a synthesis between the phenomenology of Alfred Schutz and the sociology of Talcott Parsons. This, we think, is a mistake. Garfinkel was always at great pains to indicate that his interest in Schutz was *methodological* not theoretical and was directed towards the use of Schutz's work to ground "study polities" enabling a distinctive examination of the range of sociological problems circumscribed and defined by Parsons. In this paper we will elaborate and defend this view. We will insist that a great deal more sense can be made of ethnomethodology when its interests are construed in methodological ways. As part of this general discussion, we will address two minor issues. One is the tendency of discussions of these matters to blur the distinctions between Schutz and the rest of phenomenology. The other is the presumption that a Schutzian or any other phenomenological sociology must take a particular form and set of interests. We will show that Garfinkel's work demonstrates that this need not be so.

The following remarks encapsulate Garfinkel's interests in phenomenology.

I would like to distinguish a pedagogic interest in what phenomenologists' writings might have to do with issues of professional sociology from another interest that Jim Heap's remarks have helped me to identify as 'following the animal' . . . I can't see why we should be concerned with the pedagogic interest . . . 'Following the animal' means making phenomenologists' materials available as resources with which organisational studies can get done. It is my opinion that phenomenologists' writings should be made available as technical preparation for sociologists' work in its identifying and local detail. (Garfinkel 1977 p 14)¹

The pedagogic interest is a concern with ancestry, correspondences, influence and derivations. It involves placing a premium upon making theoretical sense of collections of writings. In contrast to what is, of course, a perfectly acceptable interest is the one which Garfinkel recommends, a methodological use. In this desire to make all issues bear upon methodological matters can, we think, be seen ethnomethodology's distinctive character. Most commentaries and expositions fail to appreciate it and instead cast their discussions in epistemological or metaphysical terms.² The consequence is that assessments are made which fix ethnomethodology in ways that are entirely alien to it. Naturally enough,

this becomes most prominent in the lists of central constructs which are formulated for ethnomethodology as well as the topics such as power, the external social structure, inequality, the distribution of wealth, and so on, which it is said to ignore. Most of all, though, it can be seen in the way that the influences of Parsons and Schutz are discussed and appraised.

In one of his letters to Alfred Schutz, Parsons complains that he cannot see what difference Schutz's views might make to the findings of *The Structure of Social Action*.³ It would be our claim that while Parsons may be right with regard to the *theoretical* goals which he set for that work, Garfinkel demonstrates what the order of methodological difference might have been made. 'Following the animal' with Schutz involves a distinctive reconstruction of Parsons's sociological project. To show how this is so, we need to review Garfinkel's initial stance towards the role and nature of sociological theory.

Garfinkel approaches theory with what might be termed 'the investigator's problem.' Given we wish to make studies of social life, how are we to go about them? In the light of all of the philosophical reflection on theory and method, we simply cannot assume that we have direct and unmediated access to the facts of social life independent of some theoretical terminology and conceptual schemes. Garfinkel's distinctive move, one which is more familiar to phenomenologically inclined analysts than others, is to argue that if we wish to carry out studies in sociology we will, perforce, have to *choose* between theoretical systems. Different systems locate what the facts of social life are and where to find them differently. We cannot appeal to independent criteria, for example the facts, in order to make our judgement. The theoretical scheme tells us what will and will not be a *relevant* criterion. Criteria, then, are internal to investigative and theoretical systems. Each competing system is an exemplification of its own standards of assessment. One common response, the epistemological, would be to worry about the nature, scope and grounding of such judgemental criteria. While not insensitive to these matters, Garfinkel sets them aside. Instead he raises the methodological question. What difference would opting for different theoretical schemes make to our studies? He asks what the consequence would be of treating theoretical schemes as heuristics, as facilitating devices for investigations. Choices could then be made on pragmatic, practical investigative grounds, what is made available for study, rather than epistemological ones, which is the correct one or the true theory? This methodological turn is not an attempt to resolve epistemological problems but the expression of a realisation that there is no end to such problems and that if studies are to be made, we will need to draw epistemological lines with investigative relevances in mind. It is clear, then, that adopting this methodological stance is not some determined effort to find the secure, philosophically grounded foundations for sociology. It evinces, rather, a guiding interest in finding out what order of difference making choices between philosophical predispositions and

theoretical schemas will make to the professional sociological practices that can be implemented.

In order to see how Garfinkel explores the consequences of just one of these choices, consider the following statement.

The seen, but unnoticed backgrounds of everyday activities are made visible and described from a perspective in which persons live out the lives they do, have the children they do, think the thoughts, enter the relationships they do, all in order to permit the sociologist to solve his theoretical problems. (Garfinkel 1967 p 37).

At first sight this statement appears bizarre. It seems to have inverted the classic conception of the relationship between theory and observation in sociology, much so that for some it may be difficult to see in it any serious, sensible sociological method. But, as we have just outlined, Garfinkel conceives sociology as a collection of alternative and at times competing ways of depicting socially organised activities. Each of the rival sociologies constitutes the social world that it is inspecting according to its own theoretical elections. What sociological investigations can be seen as, then, are exercises in "conceptual play".

By conceptual play is meant that the investigator undertakes the solution to a problem by altering imaginatively the features of the problematic situation and then following through the consequences of this alteration without suspending a respect for the basic rules of his discipline (Garfinkel 1956 p 188)⁴

The way that this conceptual play has usually operated in sociology is through the treatment of the social organisation of activities as conformity to a set of logically derived imperatives, be they biological or psychological drives, social needs, functional needs for the equilibration of the system of activities, or the unfolding of the immanence of history. Garfinkel proposes we should view these clusters of imperatives as ways of mapping social activities, or rules of sociological method, and not as the essences of the activities themselves. Two implications follow from this. The choice of principles is an open one. We do not have to draw our maps in any one way. Second, there are clear limitations for the ways in which our theorising or description should proceed, namely "the basic rules of (the) discipline". Garfinkel is not making a mockery of serious, scientific, rigorous endeavours in sociology, far from it. He takes the notion of a rigorous or formal sociology very seriously indeed.

Let us unpack this just a little. The construal of social activities according to a derived logic is not a free floating metaphysics which presumes the existence of or necessity for particular forms of social organisation. The need for clarity, precision, exemplification and progress should not allow theoretical rigour to be suspended. Rigour, here, has a use that does not quite square with that often to be found in sociology and is much more akin to that of the phenomenologist. In sociology it is often taken to mean methodicalness and precision in measurement. For Garfinkel, it is the reduction of theoretical categories and objects with which the investigator engages in conceptual play to those fundamentals which are essential for the activity under study. In conceiving rigour in this way, Garfinkel is following the moves made familiar by Husserl and other phenomenologists who sought an eidetic reduction

of categories to their essences.⁵ But the point that Garfinkel is making is a sociological and not a philosophical one. If one is to seek for some essential characterisation of social activities which is to be arrived at in a rigorous manner, it will not do to be satisfied with depictions of these activities as instances of highly generalised glosses or formulations.⁶ To treat office life as an instance of game playing, or visits to the doctor as participation in ceremonies, misses what it is that makes office life and visiting the doctor essentially and recognisably what they are. It is this very particular notion of rigour that makes for the apparent iconoclasm Garfinkel's sociological proposals and hence why it is a mistake to see ethnomethodology as primarily a critique of 'positivism'.⁷ By this criterion of rigour, pretty much of all sociology has to be rejected. Even the most sophisticated, self-conscious and formally constructed of sociological theories, that of Parsons, is found to be predicated on theoretical assumptions that can themselves be brought into doubt and to employ categories which can be further reduced. The point that is being made is not that in laying out the predications and further reducing Parsons's categories, one arrives at a refutation of Parsons. Rather, in doing this, it becomes apparent precisely which steps have to be taken to make the theory work as the basis for sociological descriptions and investigations. Once we are aware of the steps involved in making some theory work, we are in a position to ask if these steps really are necessary ones. What would happen if we suspended *those* steps and the framework from which they derive and adopted another set entirely? It is this concern with what difference to sociological practice such a proposal might make which explains Garfinkel's use of Schutz and it is to that we will now turn.

Following the usual phenomenological strategy, Garfinkel starts out by adopting the distinction between the natural and other attitudes on daily life, one of which is the attitude of science. Under the scientific attitude, no proposition may be deemed to be indubitable-in-principle. Instead, in formulating its research strategies, science conceives of the world as a set of 'problematic possibilities'. Scientific theories constitute the world under varying conditions and choices. This is in direct contrast to the natural attitude within which the 'real world' is ontologically stable and given. Since, under the scientific attitude, the world is given within the theory, scientific investigations interrogate the world, the data of experience given by the province of meaning incorporated in the theory, in many differing ways. Phenomenologists take theorising in philosophy to mean the reduction of theoretical categories to presuppositionless experience. Garfinkel translates this into sociology by proposing that Parsons's theory, for example, can be reduced to its own fundamental categories and these are revealed when one reflects upon the ways that the theory is constituted as a scientific theory of action. Parsons, following Weber and others, sees the rationality of social action by defining it in means/ends terms and locating it within the system of analytic categories.⁸ The radical step for Parsons would be to suspend the systematic nature of

purposive rational action. It is Schutz's recommendation in his letters to Parsons that this step be taken in order to see that the rationality espoused by *The Structure of Social Action* is just one of the ways that rational action can be rationally described. The conclusion that Garfinkel draws is that if we were to take such a step then, at the two basic levels of the description social action, the characterisation of activities and the premisses or grounds of conduct underpinning the performance of the activities, we would be able to adopt as a theoretical election an entirely different conception of the social nature of action. Garfinkel discerns three topics for investigation within the premisses of conduct: (a) the items that are to be invariants of the system of social action; (b) the treatment of the possibility of failures in mutual understanding; and (c) the processual nature of the activities themselves. In talking of invariants of the system, Garfinkel is asking what properties remain constant no matter what activities are performed. By introducing the possibility of failure in mutual understanding, Garfinkel is asking for reflection on the ways that its continuance as a presupposition of the natural attitude is ensured. Questions concerning the processual aspects of activities introduce an interest in the sequential unfolding of activities over time. By contrasting Parsons and Schutz with regard to the ways that they conceive of what was important at these two analytic levels, Garfinkel is able to indicate what the difference between the two sociological elections amounts to. For Parsons, the prime concern is to show how the system of action is maintained as an integrated system. Schutz is more concerned with the conditions under which individual activities maintain the ground of their own possibility. At the level of premisses of conduct, Parsons equips his actors so that their activities operate as part of the system thereby maintaining it, hence the importance in his theory of the concepts of role, norms, values, and socialisation. For Schutz, at this level the problem is to specify the conditions under which an activity could proceed so that a particular finite province of meaning can be achieved for the organisation of activity while at the same time allowing variation in the regulations governing the course of such activity. How is a particular activity recognised as that activity in the face of the variety of problematic possibilities without chaos or failure in mutual understanding? What are the conditions for such routinization of meaning? Hence, the importance to Schutz of the concepts of relevance, motivation and interpretation.⁹ If one elects to suspend Parsons's primary concern with the systemic nature of activities and 'following' Schutz deal only with the activities themselves, what are the methodological consequences for sociology? That is Garfinkel's question. In the paper which we quoted from earlier, he gives the sketch of an answer. Such a methodology would mean the explicit adoption of a "praxeological rule"¹⁰ for sociological investigations.

Accounts by sociologists of the conditions under which a phenomenon occurs may be mapped point for point into the terms of strategies that persons follow whereby, whether knowingly or not, they achieve the pay-off represented in the value of the variable under study. The praxeological rule states that any and all properties whatso-

ever of a social system that a sociologist might elect to study and account for are to be treated as technical values which the personnel of the system achieve by their modes of play (Garfinkel 1956 p 191).

That is to say, whatever properties we might wish to describe can be viewed as the 'accomplishments' of the actors engaged in the activities under study. What we need to do now is set out what is involved in making this methodological election.

One contrast between Parsons and Schutz is the starting point of theorising. Another is the nature of theorising itself. For Parsons the purpose of scientific investigation is the pursuit of a correspondence between the organisation of theoretically constituted objects and the referents for those objects in the world. The correspondence will provide a symmetry between the logic of the theory and the logic of the world. In Parsons's theory, this logic is a systemic one. A direct corollary of this epistemology is the need to find criteria for the assessment of the fit between the world and the theory. For this reason the problem of description arises at a very early stage. There have to be ways of telling if the theoretically derived description matches the world. We have to be able to tell if the DNA molecule 'really is' a double helix or whether quarks are particles or forces. This problem appears in the social sciences as the attempt to make a correspondence between the world as that is experienced in daily life and social scientific descriptions of actions, actors and situations. The argument generally offered is that since theories are built up by reflection on the world as it appears to us, it is only by adopting grounded procedures and the standards they incorporate that we can be sure that the logic of our theories matches the logic of the world. The procedures adopted are, of course, those of science and their standards those of scientific rationality. It is criterial to the whole exercise that these standards and procedures be assumed to need no justification. They cannot themselves be subject to scrutiny. They are beyond the scope of any one theoretical system.

Schutz starts with a very different view of theorising.¹¹ For the phenomenologist, there is no disjuncture between experience and reality and hence there is no point in seeking a correspondence between them. Reality is reality as it is experienced. There is nothing behind the experiences we have which has a greater reality than they do. The theoretical task is the building up of theories by reflection on our experience and reducing our conceptualizations to the essences of reality as it is experienced. We arrive at such apodictic evidence by means of the scientific and philosophical use of the phenomenological epoché. The grounding of such theories must be observations of experience. Hence, all categories have to be reduced to those which can be located in the fundamental level of experience. This is the reason that phenomenologists are so interested in the constitution of appearances. Since the fundamental category of social life is experience, it is with the apperceptive mass of continuous experience that Schutz has to begin. The social world is constituted in experience as continuous activity which is then

accounted for as rational, or just, interpretable and so forth. The phenomenon for investigation is *how* such experience is found to be rational, just, interpretable, logical or justifiable. How is it that we produce the rationality, logic and justice of social activities? Instead of treating these features as inherent in activities, Schutz takes one step backwards and asks how rationality, logicity, justice, etc., are accomplished. By suspending the givenness of these characteristics, Schutz makes them observable and so investigable. This is the attitude which lies at the heart of Garfinkel's methodology. The very things that Parsons assumes Garfinkel wishes to investigate. In his suggestion that sociology could take that one more step and follow Schutz, Garfinkel has laid down a criterion for rigour which much of sociology fails to conform to.

We hope that the contrast between the two styles of theorising can now be appreciated. Parsons seeks to find a way of moving from appearances to reality by providing activities with a systematic orderliness. Hence a major problem for his approach is the specification of a set of externally validated criteria for the assessment of the fit between the theory and reality. Such a problem does not arise for Schutz. Whatever orderliness an activity displays has to be treated as an accomplishment of the activity itself. Furthermore, there can be no question of imposing any one set of standards. There is no reason why common sense should conform to scientific rationality. What we know 'rationally' under the scientific attitude does not have to match what we know 'rationally' under the natural one. Each constitutes the world with different motivational relevances in mind.¹² Since for Schutz experience is the fundamental category, then a science of social action should begin its theorising with the activities that actors engage in. It is this that Garfinkel adopts as a methodological recommendation. He proposes to treat all activities as observable accomplishments. Rather than start from the assumptions used by Parsons, Garfinkel seeks to provide sociological accounts of the phenomenon Parsons sought to study, namely the social organisation of activities, but by treating them as sequences of activities. The methodological implication is that we can now ask how actors accomplish the rational, natural, obvious character of their activities *in the course of doing them*.¹³ It is this sociology that can be formulated as treating the world as providing data for the resolution of sociologists's theoretical problems. Such problems consist in making decisions on how to treat activities as the outcome of actors' methodological procedures for the management and accomplishment of the accountability of their actions.

Probably the most famous and certainly the most often read of Garfinkel's 'studies' which illustrate this methodology is that of "Agnes". Time and again in his discussion, Garfinkel points out that from the sociological point of view, any and all of our inferences, deductions, opinions and findings about Agnes's 'femininity' are based upon what Agnes says or does. Her identity as a 'woman' is assembled by us on the basis of her actions. These actions embody 'methodological practices' for

the accomplishment of her femininity.

Agnes's methodological practices are our source of authority for the finding, and the *recommended study policy*, that normally sexed persons are cultural events in societies whose character as visible order of practical activities consist of members' recognition and production practices. (Garfinkel 1967, p. 181, emphasis added).

A few pages later on he says:

For Agnes the stable routines of everyday life were 'disengagable' attainments assured by unremitting courses of improvisation. (ibid. p. 184).

It is crucial to notice here that Garfinkel's move is methodological, not philosophical, and certainly not a detective one. For the purposes of doing sociology and not for the purposes of finding out about the veracity or otherwise of Agnes's claims about events in her life or the essences of human nature, Garfinkel is choosing to treat activities as observable, methodic productions. The point is not to determine whether "all along" Agnes was "really" hoodwinking the doctors. Given Garfinkel's methodological stance, that *cannot* be the question. It cannot be the topic of enquiry because any characterisation or account of Agnes's sexuality is based upon resources which Agnes herself provides, namely her activities. The "facts" do not stand to one side of the histories, stories, revelations and descriptions that Agnes provides. From Garfinkel's methodological point of view, they are produced in and through these accounts. The canons of sexuality that Garfinkel is interested in are not derived from outside of the accounts of her own activities that Agnes herself gives. They are not located in some over-arching set of norms held in common, nor in a culturally defined framework of meaning. The accounts themselves are held to be *self-explicating* by means of the practices used in their production. Further, these practices are observable in the activities themselves. The serious implication of Garfinkel's adoption of Schutz is that as a methodological principle, he can suspend the use of any fixed, permanent and therefore unsatisfiable criteria for factuality, legality, rationality, or femininity. That is to say, he suspends the search for criteria that will provide a decontextualised account of what such characterisations are to mean. This leaves him free for the sociological pursuit of what, for him, is an awesome contingency, that factuality, rationality, legality and femininity can be treated as members' accomplishments. They can be treated as products of members' practices and hence viewed in terms of the praxeological rule.

Our argument thus far has been that Garfinkel derives from Schutz and elsewhere, a series of 'study policies' for the investigation of sociological topics. Because of the indebtedness to Schutz and other phenomenologists, it is usually assumed that such policies must have the features which characterise phenomenological investigations. However, given the importance of the praxeological rule and the use of the strategy of pragmatism associated with it, this does not follow at all. By directing attention to the nature of stable systems of action, what are termed 'organisations of action', which are then treated as the accomplishments of social actors engaged in such systems of action, Garfinkel could be said

to be mounting a distinctive sociological project which far from being interested in the introspection of consciousness, experience and motivation, takes up the observable characteristics of the methods members use to produce those stable systems; his is a *third person* phenomenology and not a first person one. The concern with consciousness, motivations and knowledge is with regard to their observable, social character. It follows that if ethnomethodology is premised upon the methodological turn which we have outlined, it cannot be expected to engage in the sort of phenomenological sociology envisaged by most of its critics. The question might then arise as to whether it was phenomenological in any serious sense at all since it seems to be so different from investigations more usually associated with that rubric. To us it seems that the squabble over the extension or restriction of the label 'phenomenological sociology' is of no relevance since it is quite plain that Schutz's own sociological writings are consonant with both the aims and the methodological turn adopted by ethnomethodology.

We do not here have the space necessary to give the extended examination of Schutz's writings that the full documentation of our argument would require. However, enough evidence can be assembled from a limited corpus. We will consider just two, the reflections on method and the implications to be drawn from the discussion of 'making music together.'¹⁴ From this brief review, we will show that Garfinkel's sociological project is not a departure from Schutz's phenomenology but an extension of it. Whether it constitutes the 'proper' phenomenological sociology will not concern us.

In all of Schutz's methodological writings the same theme is to be discerned. He wishes us to notice the supervenience of sociological concepts and constructs upon commonsense ones. It seemed to Schutz that this supervenience had hardly been acknowledged, let alone explored. The dependency of scientific theorising derives from the qualitative difference between human action and other forms of activity. The grounds of human action are its meaningful character; subjective interpretation is a necessary postulate. Scientific understanding and theorising about such action is 'second order' theorising invoking scientific relevances, frames of meaning and stocks of knowledge, and deals with objects which are the products and displays of commonsense theorising, utilising commonsense stocks of knowledge and so on. Along with this postulate of subjective interpretation, Schutz places two other general postulates: those of adequacy and logical consistency. The postulate of adequacy requires that no motivation should be attributed to actors as the grounds of their action other than those which would be rationally understandable by those actors and their consociates. The postulate of logical consistency requires definitional clarity and distinctiveness for the framework of concepts employed.

For Schutz, the supervenience of sociology upon commonsense implies a symmetry of method. Both sorts of theorising invoke course of

action and personal types. The difference is that commonsense typologies derive from the biographically given stock of knowledge possessed by an individual, whereas those of sociology are constituted with the specific relevances of science and by adopting the scientific attitude. In Schutz's view, this is carried out in sociology by the elaboration of 'limited models' of actors, what he terms "homunculi" who are activated, motivated and guided by the relevances of the sociologist. The ends sought, the paths chosen, the aspirations held are *all* defined by the sociologist. As Schutz himself says, these homunculi are not to be confused with real human beings, living ordinary or even extraordinary lives.

The homunculus was not born, nor does he grow up, and he will not die. He has no hopes and no fears; he does not know anxiety as the chief motive of all his deeds. He is not free in the sense that his acting could transgress the limits his creator, the social scientist, has predetermined. He cannot, therefore, have other conflicts of interest and motives than those the social scientist has imputed to him. He cannot err, if to err is not his typical destiny. He cannot choose except among the alternatives the social scientist has put before him as standing to his choice. (Schutz 1963 p 340)

The objective of these reflections, of course, was to indicate that Weber's aim of causal and meaningful adequacy could be realised for sociological explanations. The ideal typical method provides a means for satisfying both requirements and is an extension of that used by Weber in his analyses of radical protestantism, bureaucracy and elsewhere. Schutz's method, then, is sociological in character, in the Weberian sense, and not merely an amorphous mulling over and musing upon the nature of human consciousness and experience. What is clearly involved is a limited, monothetic constitution of action because the field of daily life and normal social intercourse simply is not amenable to scientific theorising in any other way.

The outline for this latter argument, as Fred Kersten has shown,¹⁵ is to be found in Schutz's work *Making Music Together*.¹⁶ The phenomenological method entails the systematic reduction of levels of experience to their simplest concepts. Thus the multi-dimensional character of the visual world is shown to rest upon a sub-stratum of monothetically realisable concepts, identity and difference. It is the synthesis of these which provides for the visual world's character. Schutz suggests that the world of social life may be pluridimensional in character and so not amenable to monothetic constitution, but can only be constituted polythetically. Our experience of social life may have more in common with the auditory basis of making music than it does with the visual basis of seeing objects. Making music together involves 'tuning in', a process which cannot be characterised by separating out the parts or voices which individual instruments contribute. One tunes into the music as a pluridimensional whole, not just to the rhythm, the notes, the key, the tone and so on individually. Decomposing the experience into these elements cannot capture that experience. The pre-given character of the social world is similarly pluridimensional. Hence, it too cannot be captured by the monothetic constitution required of social theorising. This does not mean that sociology is impossible, simply that it cannot aim to theorise

social life in a straightforward manner. It cannot simply focus on the experience of the daily world, in Schutz's eyes at least, because that experience will elude theoretical reflection. What it can do is just the sort of 'limited model' building that we identified earlier, not as an attempt to capture the experience of social life but as a *method* for offering meaningful and causal descriptions and explanations in sociology. The phenomenological grounding in Schutz is a *method*, not a set of prescribed topics.

In drawing attention to the supervenience of sociological theorising upon common sense, Schutz provided Garfinkel with his topic. The object of study was to be the stable, socially organised character of common-sense theorising. The praxeological rule allows him to view the products of such theorising, the stable features of social life, its organisation, as accomplishments. Each and every activity can be treated as displaying its own methodic practices which achieve as *technical values* its recognisability, rationality, sensibleness, understandability, in short its 'accountability'. Many commentators have sought to grasp this sociological character by raising for discussion the concepts of 'indexicality' and 'reflexivity'. However, in doing so, they have tended to pull ethnomethodology back into the quagmire of epistemology from which it was attempting to escape through its adoption of the methodological strategy we have outlined. For us, this 'methodological turn' is much more clearly expressed in two other concepts related to 'accountability', namely an occasioned corpus of knowledge and 'self-explicating settings'.

The central idea of ethnomethodology as we have presented it is that activities can be viewed as displaying their own accountability. This may be topicalised by asking (if we may be allowed to formulate it in distinctively Garfinkelian ways) as "Just how, just where, just in what, in this setting, with these actors, upon this occasion is this accountability done in and through the activities to be discerned there?" How is the accountability of ordinary phenomena — conversation, breakfast on a workday morning, teaching class, listening to a concert — achieved as just those things in the course of doing those things? The occasioned corpus of knowledge is nothing other than what actors do know, here and now, and how they know it. It is not a background set of expectations which we all possess, but the *in situ* knowledge of this occasion which participants have. The self-explicating character of settings draws attention to the ways that such an occasioned corpus of knowledge is constituted and made available in unique ways on each and every occasion. What is made available uniquely is the character of the occasion as just another conversation, workday breakfast, class being taught. Such accountability is achieved from within the activity by members' methods for accomplishing the self-explication of settings and the occasionality of a corpus of knowledge.

Ethnomethodology's studies of courts and classrooms, homes and therapy sessions, conversations and phone-in programmes all address

these features as matters for investigation.¹⁷ What is it about the character of the setting which makes it *recognisably* a class, a court hearing, the telling of a joke? What do members know about how settings and activities are organised and how do they display that knowledge in a co-ordinated fashion in the midst of carrying on the activities of teaching, giving evidence, and telling stories? The attention to the particularities of occasions involves a non-formulaic conception of settings and the materials which they make available for analysis, a tendency which is negated when these interests are cast in terms of the irredeemable 'reflexivity' and essential 'indexicality' of activities in general.

We have been endeavouring to bring out the sociological character of ethnomethodology. If we have been at all successful, it ought now to be clear why the charge that ethnomethodology fails to be phenomenological enough misses the mark. We can round off this part of the discussion by taking up in passing two other related matters, namely the lack of a phenomenological character to 'ethnomethodological indifference' and the relative laxity which is adopted towards data. 'Ethnomethodological indifference' refers to the disregard that studies have towards matters which are not circumscribed within the definitional framework of relevances. This framework is, as we have seen, the self-organising nature of settings. Such indifference is motivated by considerations of consistency, distinctiveness and the determination to follow through *these* relevances to see what they might result in. It involves a methodological election, one that is in direct line with Schutz's proposal concerning the formulation of homunculi. In our view it displays sociological conception. The wish to engage in a first person phenomenological conception is a countervailing preference, not a vitiating argument.

With regard to the question of data, one thing that ethnomethodology has derived from phenomenology is a concern to give primacy to the phenomenon in the data and not to subordinate it to the secondary role of illustrating, filling out, locating theory. This primacy can be seen in the attention which all studies pay to the particularities of data as 'this data' and by addressing the famous question "what is this data data of?"¹⁸ Ethnomethodology has an analytic interest in data not an inferential one. It does not wish to step from this 'sample' to all of the data that could possibly be collected. It is only where inference is involved that controls on validity and verification need to be invoked. It is for this reason that progress in Conversation Analysis has been so rapid. The data of talk as talk is easily preserved in transcripts. The provision of the transcript provides the method of study. This is not to say, of course, that questions about the nature of ethnomethodological arguments, the role of data, the use to be made of 'bulk data', the prescription of methods, and many, many more, are not topics of genuine debate, nor that we all have to become adherents of the 'fetish of the transcript'. However, these questions should all be raised from within an understanding of

ethnomethodology's own standards for investigation. These might be summarised as follows:¹⁹

- (1) studies should use methods which are uniquely suited to the topic in question;
- (2) methods should seek to capture and preserve the essential and identifying characteristics of the phenomenon under study.

These two provide yet another exemplification of ethnomethodology's methodological stance. The phenomenon in all instances is the self-explicating character of activities. But the slogan 'Look for self-explication!' is empty. It can only be put into practice through the examination of the detail of the data to hand. The primacy of data once more becomes apparent. The determination to preserve the essential and identifying characteristics of activities as those activities means that sociological investigative methods will have to be supervenient upon members' methods for identifying and displaying the accountability of such activities. The requirement that methods should capture the uniqueness of the phenomenon cannot, therefore, be treated as a pious hope, an investigative ideal. It is definitive of the exercise itself. Such 'unique adequacy' is a stipulation not a request.

The upshot of these two guiding principles is the dissolution of the hallowed distinction between sociological researchers and their subjects. For the purpose of this sociological analysis, both are treated as enquirers into the setting. Second, and equally important, the methods for preserving and describing activities are made available within the activities themselves but not as a matter of theoretical reflection. They are displayed in practical action. Finally, it is a strong implication that no one method can be generalised beyond its own domain of enquiry. Each topic requires its own method of analysis.

We have tried to show in this paper that many of the arguments concerning the origins of ethnomethodology in phenomenology have been misguided. In order to point up how distinctive Garfinkel's methodological turn is, we will end by comparing briefly the sociological concerns we have outlined with those displayed in a study which according to one commentator, at least, shows just what a phenomenological sociology could be like and just how impoverished ethnomethodology is. The study is Kai Erikson's *Everything in Its Path*, the commentary is by Mary F Rogers.²⁰ At the outset, let us make it quite plain that we do not wish to quarrel with Rogers' right to prefer Erikson's approach to that of ethnomethodology, nor would we want to decry Erikson's work. Our interest is simply in showing that once it has been properly understood, it should be abundantly clear that ethnomethodology could not hope, let alone try, to fulfil the goals which Erikson set for his study. It is engaged upon an entirely different sociological project.

In talking about Erikson's book, Rogers says:

In order to understand the experience people had during the flood and its aftermath,

Erikson had to turn to Appalachian social history, the types of biographies associated with, and the life styles that resulted (Rogers 1983, p. 158).

With these in mind, Erikson was able to capture and express how people in a town which had been devastated, coped with that experience. Capturing and expressing the nature of the individual's experience is not ethnomethodology's topic, that is the identifying orderliness which an organisation of activities displays. In turning away from the aftermath of the flood to social history, biographies and the like, Erikson turns away from whatever orderliness the activity of 'coping with disaster' might have shown. In choosing to focus upon 'communality', 'common experience' and 'culture', *Erikson has to take for granted* that these are features of the social life of the town he is studying. He cannot treat them as investigable: he cannot make their accomplishment his topic. But this is precisely what ethnomethodology does. It treats 'culture', 'common experience' and the like as technical values accomplished by members' activities. Erikson's concern is with the impact of the flood, with how people experienced it, what it meant to them, how they reacted to and coped with it. He could not suspend the reality of 'Appalachian culture', 'the common experience of the flood', and 'the feeling of communality' to see how they were accomplished. They are things which, quite rightly given his interests, he has to rely on. The same line could be taken over method. Erikson consults news clips, local papers, histories, and folk memories. For ethnomethodology the interest would be in what it is that makes 'this account' an identifiable news clip, a newspaper report, 'another instance of this story' or whatever identifying orderliness may be attributed to it. The fact that all such accounts were 'about' the disaster or bore on it in some way would be of no interest. It would be treated with ethnomethodological indifference.

The choice between Erikson's interests and those of Garfinkel is an open one. Any individual's preferences are their own to make and neither here nor there. All we would ask for is a sensitivity to the distinctiveness of ethnomethodology's concerns and consideration of it on its own terms and not as an inadequate version of something else. Far too often this has not been the case, with the result that criticism has been wide of the mark. Nothing we have said should be taken to mean that we think ethnomethodology is immune from criticism, nor that we feel there are no serious and troublesome matters to be discussed. What it does mean is that we feel such discussion will be sharper edged and more productive if it is based upon a clearer understanding of the aims and aspirations ethnomethodology sets for itself and the reasons for the adoptions of those goals than has often been seen heretofore.

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4. H. Garfinkel, 'Some sociological concepts and methods for psychiatrists'. *Psychiatric Papers*. Summer, 1956 vol. 6. pp. 181-195.
5. E.g. E. Husserl, *The Paris Lectures*. The Hague. Martinus Nijhoff 1979. See also A. Schutz, 'Husserl's Influence on Me', *Annals of Phenomenological Sociology*, vol. III 1977, pp. 41-44.
6. H. Garfinkel and H. Sacks 'On the formal structures of practical action', in E. Tiryakian and J. McKinney (eds.), *Theoretical Sociology*. New York. Appleton-Century-Crofts. 1970. pp. 337-366.
7. A misunderstanding of A. V. Cicourel's *Method and Measurement*. New York. Free Press 1964, seems to have occasioned this. A review of contributions such as L. Churchill 'Ethnomethodology and Measurement'. *Social Forces* vol. 50. 1971. pp. 182-191 should rectify matters.
8. T. Parsons, *The Structure of Social Actions*, Vols. I & II. New York. Free Press, 1939.
9. The easiest access to Schutz is probably through his *Collected Papers*, vols. I, II & III, The Hague, Martinus Nijhoff, 1962, 1964, 1966 together with *Reflections on the Problem of Relevance*, The Hague, Martinus Nijhoff, 1970. Parsons has outlined his theoretical thinking in numerous places, e.g. *The Social System*, London, Routledge, 1951 and 'On building social systems theory'. *Daedalus* vol. 99. 1970, pp. 826-881.
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12. A. Schutz and T. Luckman, *The Structures of the Life World*. London, Heineman. p. 208.
13. It is this which gives the firm foundation to that much misused phrase 'the social construction of reality'. As our colleague David Melling pointed out in discussion, this might be better rendered 'the social construction of factuality'.
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15. F. Kersten. 'Alfred Schutz on Social Theory'. *Annals of Phenomenological Sociology* vol. I 1976 pp. 57-66.
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18. This question was first addressed in detail in a series of seminal lectures by the late Harvey Sacks. These lectures are available in mimeo.
19. In a yet to be published collection of papers and studies, Garfinkel sets out these maxims and their implications.
20. Mary F. Rogers, *Ethnomethodology, Phenomenology and Experience*, London. Cambridge University Press. 1983.