11 Ethnomethodology: a First Sociology?

Introduction

Ethnomethodology has been a lively research endeavour for well over half a century, so it is hardly surprising over time new initiatives, emphases and outlooks have emerged. Equally, since Harold Garfinkel remained at its heart for most of that period, it is also unsurprising his view of how Ethnomethodology should be defined shifted and developed and, given the erratic sequencing of his publications, that his readers should have difficulty pinning down precisely what those changes might have been. Lately, then, Ethnomethodology's direction of travel has come under increasing scrutiny, with a number of experienced members of the field questioning whether the path currently being followed is not only more conservative than originally envisaged, but actually involves a reneging on the foundational principles. We think these suggestions are misplaced and, using the notion of Ethnomethodology as a First Sociology, in this final chapter we summarise why. We will then use the same conception to position the studies presented in previous chapters. Finally, albeit very briefly, we will address some residual issues entangled in but not central to the debate over Ethnomethodology's current state. These issues mostly turn on what many identify as the unambitious character of the studies currently being carried out.

A First Sociology

For some little while, we have been arguing the most reasonable way of approaching Ethnomethodology is to conceive it as one Sociology among others. There is no given way of investigating the social world. Diverse assumptions can be integrated into coherent pre-investigative postulates and operationalised in studies, which is exactly what Ethnomethodology has done by developing its own distinctive set of principles. What it has chosen to study are features of the social world other forms of sociological research have hitherto largely passed by. This is not a defect on their part though. Given their standpoints and their investigative methods, not only are these topics unavailable to them, they are irrelevant too. Ethnomethodology and conventional Sociology can sit alongside one another without the need for sibling rivalry. Each has its own programmes and its own preferred procedures.

Such tolerance does not catch everything about the relationship though. Whilst they should not be conceived as competitive, nonetheless there is an ordering. It is not temporal (Ethnomethodology was certainly not first on the sociological scene), explanatory (sociological phenomena do not 'reduce' to Ethnomethodology's) nor conceptual (Sociology's concepts do not pre-suppose Ethnomethodology's). It is *ontogenic*. Ethnomethodology concerns itself with the primordial social facts on which Sociology's research depends. In that sense, as a *First Sociology*, Ethnomethodology occupies a position analogous to that Husserl intended Phenomenology should stand in vis-à-vis the natural sciences and Philosophy. It discloses what Sociology presupposes.

The question which preoccupied Husserl was what made the natural sciences possible? What does the scientific understanding of the world rest on? He perceived that to answer this question we have to step back beyond the practices and findings of science to the point in analytic reflection at which scientific conceptual schemas are introduced. Rather than starting by giving the scientific representation of the world logical priority over our pre-theoretical understandings, Husserl sought to begin from the historical priority of those pre-theoretical understanding in relation to scientific ones – the latter arose in a world already experienced through the terms of the former. Understanding how those scientific understandings could arise in an environment experienced in pre-theoretical terms was a way of more clearly understanding the relations between pre-theoretical and scientific understandings. Marvin Farber summarises this proposal:

> For Husserl, the 'final measure' of all theory is that which is 'originally' given in simple seeing. The term 'original' applies to that which can be experienced in direct observation; the 'originally given' is something that is 'naively' meant and possibly given as existent. That which can be 'grasped' by simple looking is prior to all theory, including 'the theory of knowledge'.
> (Farber 1943: 203)

Aron Gurswitch particularised the analysis to the case of Formal Logic.

> While the technical logician is engaged in constructive work . . . the philosopher of logic raises questions as to the very sense of the constructive . . . procedure. *The perceptual world as it presents itself in pre-predicative experience appears* in our analysis *as one of the fundamental presuppositions of logic*.
> (Gurswitch 1966: 353; emphasis in original)

In much the same way, Ethnomethodology's questions concern how the social world is understood independent of and prior to its construal within sociological schemas. These understandings comprise the common ground on which those sociological schemas stand; a common ground which existed before Sociology and continues to exist independent of it. In that sense, Ethnomethodology's concern is with the understandings which make sociality possible and hence

available for sociological investigation and analysis. Husserl's conclusion was that although the sciences saw themselves as breaking with the 'naivety' of pre-scientific thinking, their conceptual apparatuses actually incorporated much of it. In a similar way, vernacular modes of discriminating social institutions, for example, have been taken directly into Sociology's own conceptual structure in categories such as its conventional divisions between sociologies of the family, work, religion, education, science, and so on. This dependence on vernacular concepts allows non-sociologists to understand, in the most superficial but nonetheless reasonably correct ways, just what those domains encompass. It is also vital when, as much of the profession professes to want to do, the discipline sets itself an ambition to have relevance for the formation of policy. Without a reliance on vernacular understandings, those who have no training in Sociology's technical apparatus could struggle to understand both what is being said to them and ~~what is~~ its significance.

The articulation of a First Sociology is what motivates Ethnomethodology's interest in the investigation of social affairs exclusively in the terms these are understood by those engaged in them. The purpose is not to measure members' understandings against the ones a sociologist might offer, thereby calibrating 'robustness', 'objectivity', 'factuality', or 'generality' against the standards Sociology adheres to, and certainly not to seek ways of replacing such understandings with those drawn from Sociology. This does not mean issues of assessment cannot be a matter for investigation, but only when framed in the terms the participants in a setting use when seeking to determine how robust their understanding is and whether it will yield what is expected. How do they see a case as an 'instance' of some category and how do they tell if their expectations about that category are fulfilled in any actual case? Finally, how do they determine the dependability of the information on which they make such judgements? Undoubtedly, these are questions sociologists ask about their own social data, but what motivates their framing and what would count as satisfactory answers are entirely different to those of ~~ordinary~~ members of society.

The determination to step back beyond sociological frames means the settings of social action have to be conceived in ways which allow participants to interpret activities and events and share their understandings without recourse to Sociology's technical apparatus. In particular, 'what things are' and 'what they mean' must be recoverable from the activities themselves, such recovery being part and parcel of engagement in ~~and~~ flow of action. This has an initial and very important implication. Since those engaged in the activities under view determine what is going on and what it means within the flow of their activity, the distinction between being an investigator sitting apart from the action, and being a member immersed in it, collapses. Gaining a working understanding of social life does not require abstracted Cartesian reflection nor an 'objective method'. Neither does it depend upon the design of special interventions to gather material. Looking at what is available to those in the setting is sufficient to determine how they came to the understandings they patently did.

First Sociology and the current state of Ethnomethodology

As we set out in Chapter 1, Garfinkel's early thinking derived from an attempt to align, if not integrate, Alfred Schutz's (1967) social philosophy with Talcott Parsons' general theory of action (Parson and Shills 1951). His conclusion was that Parsons' schema could not accommodate a line of development based on Schutz's principles. The reason, as Schutz himself had observed to Parsons (Grathoff 1978), was because trying to do so would require 'one more radical step', namely the bracketing of the presumptions of the schema itself. This would be necessary if a way of theoretically grounding the interactions between socio-logical investigators and their informants was to be found. Since such interactions were necessary to the gathering of materials and the interpretation of data on which analysis depended, for rigour to be maintained, such grounding within the investigation's theoretical premises was required.

Ethnomethodology was forged by taking that 'one more radical step' and with the step, a First Sociology became possible. The body of work which has built up since then has been in service of identifying, working out and working through the range of topics and analytic avenues made available by that radical move.

In recent discussions, though, the epithet often used to mark the current state of the discipline is a supposed 'loss of radicalism' consequent upon a redirection in, or even retreat from, the original impetus. This loss is said to be taking place across three different fronts:

1   From roughly 1967 to the millennium, Garfinkel fundamentally changed his view of Ethnomethodology. This change involved a retreat from the direct challenge to fundamental views on the nature of social life not just in the profession of Sociology and associated disciplines, but in society in general. Instead, what emerged was a point of view which simply acceded to conven-tional outlooks in social science and society.
2   Ethnomethodology has tended to adopt a less combative stance towards the rest of the Sociology profession. Instead, many working in the field actively seek collaboration and partnership. This has led to the suggestion Ethnomethodology increasingly accepts professional Sociology's image of what it is to be an academic discipline even though part of Garfinkel's original concern with regard to sociological schemes was not their substance but the way they were framed in terms of general academic pre-suppositions. Garfinkel's radicalism has, then, been interpreted by some as a resistance to academisation. Although the issue is couched in terms of a concern over the image projected, for us it seems more motivated by the belief that increasing accommodation will necessarily moderate the challenge being made and so undercut one of Ethnomethodology's foun-dational principles. Certainly it is true Ethnomethodology's traditionally obdurate stance is felt to be unhelpful at best for those seeking cooperation and close working.

3   Although not central to the first two concerns, associated with them is a worry that the studies being carried out these days have little of the 'edge', the novelty and energy seen in the very early work. As a result, the vast body of contemporary work is felt to be unexciting and conveying the sense of ~~the~~ discipline ~~being~~ becalmed.

What gives these observations especial force is that they are not just the disparaging allegations of those lacking sympathy for the field, but come from highly respected members of Ethnomethodology's own community. If these people are worried, shouldn't we all be?

Taking the long view

In this section, we will run over the above list. Unfortunately, we do not have the space to do much more than offer a limited recital and rehearsal of the issues involved. We will start with the last.

A sense of lassitude

This issue is the least intellectually consequential though, as we will see, because it implies a reservation about how lively and hence attractive the discipline might seem, it is the one which ought to prompt the community to immediate remedial action. As sociologists, none of us should be surprised to see an enterprise born of a determination to make a radical break with the status quo begin to show signs of 'routinisation'. With continual explication and demonstration, what once was surprising and exciting becomes less so. Familiarity breeds not so much contempt as contentment. There is another aspect though. While conventional Sociology hardly regards it as *kosher*, ~~nonetheless~~ Ethnomethodology has become generally accepted. It is taught and taught about (though whether it is well understood is another matter entirely). This gradual institutionalisation has meant in orders of magnitude more students have been exposed to it, considerable numbers of whom, having taken up research and other professional careers, have sought to align their work with it. Routinisation is associated with a loss of challenge and innovativeness. The explosion in the researcher base has meant what innovation there might be gets drowned out in the volume of work being produced. Contemporary academic culture with its emphasis on the publication treadmill only makes the situation worse. The studies are bland, to be sure, but their blandness is not testimony to the loss of the gene for radical investigation, simply a correlate of increased mass.

A methodological disjunct

It was a prominent ethnomethodologist, the late ~~Mel~~ Pollner, who first publicly questioned Ethnomethodology's apparent shift from confrontation to absorption within Sociology. Caustically, he referred to it as 'settling down in the suburbs'

(Pollner 1991: 370). Although he saw it had many dimensions, for him what was critical was the tendency to conflate the roles of ethnomethodologist-as-analyst with ethnomethodologist-as-member.[1] Pollner thought this tendency had been given momentum in the period between the publication of *Studies in Ethnomethodology* (Garfinkel 1967) and *Ethnomethodology's Program* (Garfinkel 2002). For Pollner, it constituted no less than a re-conceiving what it was 'to do' Ethnomethodology. Originally, the adoption of the Schutzian distinction between the scientific and natural 'attitudes' and the pre-suppositions on which they are based meant ethnomethodological investigation was predicated on setting aside the assumptions underpinning ordinary life. To use phenomenological terminology, the ethnomethodological attitude 'bracketed' the pre-suppositions of normal social life.

For Pollner, this bracketing is the core of Ethnomethodology's radical stance. It carries a conception of social life and social reality orthogonal to that of ordinary members of society. In Chapter 3, we briefly mentioned one of the implications he felt could follow. Someone who grasped the import of this bracketing is likely to be confronted what he called an 'ontologically fatal insight' (Pollner 1987: 88). Lynch suggests this often takes the form of

> an insight sometimes arrived at in a moment of heady delight, but often as a horrifying realization – that the world we take for granted as an independent environment of action is not what it seems; instead, it is a product of our own constitutive practices and 'it could be otherwise'.
>
> (Lynch 2013: 449)

Because the heart of Sociology's analytic practice retains the natural attitude, such 'insight' poses a profound challenge. Under the natural attitude, the social world is taken to be an external, constraining reality which shapes our experience. Under the ethnomethodological attitude, social reality is re-framed as the construction, the conjoint output, of our interpretive actions. Ethnomethodology's phenomena are the 'methods' by which that reality is constructed.

For Pollner, Garfinkel's promotion of the research agenda known as 'the studies of work', and especially the studies of science, marks where the erosion of the distinction between analytic and natural attitudes becomes really obvious. In outline, his argument goes like this. The strategy of refusing to adopt the pre-suppositions of ordinary life was in service of making them visible and analysable. In the studies of work (and this is especially true in the studies of the sciences), investigators are charged with turning themselves into competent members of the local, often highly specialised, communities being studied. *Prima facie*, this is a significant demand. It means researchers have to acquire and then be able to enact the distinctive competences possessed by those communities. The measure of success is the degree of approximation between the understandings the researcher possesses and can convey and those of the community members. Calibration between researcher and member is the only means of determining the quality of the findings. Rather than demanding a distancing

of investigator and the investigated, the studies of work require the investigator to be submerged as a fully practising member into the local community. The distinction between analyst and member central to Ethnomethodology disappears entirely.

We can see how Pollner can come to this conclusion from the way the studies of work are talked about in *Ethnomethodology's Program*. It is also very easy to find in that book whole stretches of text which appear to be given over to making a decisive break with Sociology as a discipline and brutally re-fashioning Ethnomethodology as studies of the 'classical accounting' other disciplines give of their own work. However, we suggest that if the later work is viewed in terms of exploring new and different possibilities for a First Sociology, the perception of a shift fades and the claims about a severing of relations with Sociology become much less substantial.

To begin with, radical re-direction or not, the moves being discussed were in train well before *Studies in Ethnomethodology* was published. They involved not so much a revision of theoretical principles as an adjustment of practice. This can be traced in the published sources, though the following personal anecdote offers equally strong evidence for our view:

> Late in his life, Garfinkel made regular telephone calls to Jeff Coulter. When Jeff told Wes [Sharrock] about these, Wes said, 'I've spent years trying to work out the logic in Garfinkel's move from the highly theoretical – commonly deductive – reasoning in his earlier work to the insistence on studies, but I've never been able to pin it down. Can you ask him about that?'
>
> A few days later, Jeff rang Wes. His opening line was: 'Saul Mendlovitz. That's your answer.'
>
> 'What are you saying?'
>
> 'One day Mendlovitz said to him "Harold, you've got to stop this theory shit."'
>
> 'And that was it?'
>
> 'That was it.'

Mendlovitz worked with Garfinkel on 'the juror study' and we take it his advice was offered around that time. In essence, he was suggesting what was needed was more than simple identification of the theoretical possibilities in Ethnomethodology's transformative position. Those possibilities had to be demonstrated through empirical investigations – that is, in actualising a First Sociology. This was the reason Mendlovitz advised Garfinkel to stop theorising and undertake studies, advice Garfinkel accepted and followed. Doing so involved moving from a pre-occupation with Sociology's modes of creating theoretical schemas to a focus on its methodology and data collection. Both in the *Studies* and after, Garfinkel seems to have had much less interest in the content of findings and what they might imply for Sociology than in how the studies were actually being carried out. The widely used soubriquet ~~of~~ 'the coding study' carries the point well. Ostensibly, this was a study aimed at using the records of a psychiatric clinic to demonstrate how it operates. What was actually presented was a study of the ways ~~in which~~ members of the research team combed through, interpreted and shaped

up the material in the records to provide the necessary condensed rendition of the clinic's activities which the research objective required.

The form the rendition took did not seem to be of much concern to Garfinkel either. What did arouse Garfinkel's interest was the device of using the lens of strict conformity with the standard principles of methodological practice to bring out the work required to mount a sociological investigation. As is well known, the study showed much of the research activity undertaken was not specified by the standard procedures even though it was indispensable to collecting the data needed for the study. The insights offered by 'the coding study', then, are about what happens in sociological investigations *prior to* the sociologising getting underway. Attention is directed to what in Husserl's terms we would call the 'original' activities which facilitated the undertaking of sociological investigation.

If the transition to an interest in studies is a turn, it is one which, to use the oft-cited phrase, transforms Sociology from a resource for ethnomethodological studies into a topic for them. In Sociology, theoretical and methodological issues are largely conceptualised in terms of the relationship between theory and data. By framing his stance as a First Sociology, Garfinkel's studies (as well as those of others) threw the problematic relationship of data and phenomena into relief. That this relationship is problematic, with a slippage between the 'intended' and the 'actual' object of investigation was not, however, to be taken as a *discovery*. It was something every practising sociologist was aware of and the topic of endless advice sharing and conversations at professional gatherings. Every researcher has come up against the same impasse Garfinkel observed in the coding study. Although the object of investigation was the clinic, what was being examined were the residua of the clinic's organisational activities captured in the records, together with the common sense understandings held by the research team of how psychiatric clinics might work. Equally, every practising researcher is well aware of the inscrutability of the transformation process by which materials collected in someone else's investigation are turned into the data presented in published report. Both materials and means of their transformation are necessarily filtered from the sociological findings. Although every researcher was familiar with both these features of sociological research, until Garfinkel's intervention they were not topics for sociological investigation in their own right.

What Garfinkel did not do, though, was frame his investigations as a search for remedies to salve Sociology's theoretical and methodological ambitions. Rather, he sought to avoid the one and ignore the other. Instead of treating social life as a plenum of intrinsically unorganised activity only rendered orderly by the shaping given it by sociological schemes of empirically real categories, he uses Schutz's description of the natural attitude to postulate 'There is order in the plenum!' (Garfinkel 2002: 94).[2] This allowed him to treat the materials on which sociological investigation and analysis depend as the embodiment of understandings used by ordinary members of society in living their everyday lives. To conceive of activities as social action is to conceive of them as organised responses which members of society exhibit to their experience of the social environment.

In so far as Sociology posits social action as its fundamental phenomenon, that assumption is only possible because there is a prior organisation to the ways social affairs are carried on. The order produced by the understandings members use must be *conceptually prior* to the order produced by Sociology. But not only that. The work of finding an order in the social world cannot be exclusive to Sociology's methods of systematic data collection. If there is an order to the social world, such order must be available to and findable by ordinary members of society going about their ordinary everyday affairs. Social order is not simply or only the result of systematic sociological enquiry. It is to be found everywhere from the most fragmentary to the most extended routines in everyone's daily lives. In Harvey Sacks' memorable phrase: there is 'order at all points' (1984: 22). Describing how that order is produced is the remit of a First Sociology.

This line of thinking forced two notable changes in Garfinkel's investigative procedure. First, rather than continuing to seek alignment between collected materials and a preconceived phenomenon, it was possible to take fragments of material and, from their close inspection, ask what phenomena they could instantiate. A key part of investigation, therefore, became identifying what was to be analysed. Second, the examination of materials could be directed to determining the internal coherence of specific runs or stretches of activity, a procedure which suits the treatment of the in-course-organisation of lines of social action. Taken together, these two imply there is no need for the investigator to be equipped with any specialised repertoire of practices to identify the features of social order and so there is no need to differentiate between the professional sociologist and ordinary members of the society with regard to the organisation of the setting of action. The twin suppositions (a) analysts are extensively members, and (b) members are practical analysts, do not entirely eradicate the difference between members and analysts, but they do reduce it to differences in the types of interest characteristic of the two. Analysts have an interest in seeing how the order of ongoing social life relates to scholarly or theoretical themes – an interest which is irrelevant for those intent on getting on with their lives. The idea of erasing the distinction between 'the sociologist' and 'the member' of society was effected as part of a turn to undertaking studies to demonstrate Ethnomethodology's phenomena and not as a ~~consequence of them~~. As a consequence, what Pollner and others have highlighted are not really markers of any ~~sudden~~ change in direction.

What is being cast as fundamental change is better seen as adaptive modification and adjustment in the light of changing circumstances in the development of Ethnomethodology as a First Sociology. Much the same can be said in regard to another of the supposed markers of fundamental change: the introduction of the requirement for 'unique adequacy'. The earliest studies were predicated on the assumption that investigations of such ordinary things as answering the telephone, playing tick-tack-toe and engaging in talk, required the investigator to have enough knowledge and skill to recognise what was happening. There was no need to emphasise this simply because these competences were part of

most people's cultural resources. Most investigators were also members of the particular society from which the materials for study are taken.

That straightforward presumption no longer holds when attention is focused on Mathematics and the rest of the natural sciences. Here, investigation requires the acquisition of competences very different to those usually held by sociological investigators. In addition, acquiring them is no easy matter. Investigators are expected to do precisely the same when mounting these studies as they do when describing 'the missing interactional what' characterising talk, children's games and telephone conversations which members of society rely when doing those things. However, grasping the 'missing interactional what' of quantum tunnelling experiments or jazz improvisation, and seeing 'directly' and 'originally' as the scientist or jazz musician does, are not everyday competences. What is demanded is greater not because the investigative rationale has changed but simply because the domain has. Being an ethnomethodologist, a sociologist, or an ordinary member of society is no preparation for playing jazz piano sufficiently well to pass muster among other musicians nor for being adept enough to aid in carrying out a chemistry experiment.

As Lynch (forthcoming) points out, the key to unique adequacy is the difference between speaking *of* the work being carried out in the setting and speaking *about* it. This is the distinction carried by Husserl's 'directly' and 'originally'. The aim is to move the investigator over the line demarcating a well-informed commentator from a member of the community under investigation. The concern about unique adequacy amounts to a fear that the investigator will be reduced to simply repeating what the subjects of the investigation would say. We see it as more a measure to ensure that whatever the investigator does say, at least they have acquired enough of the necessary competence to be able to speak authoritatively on behalf of those whose activity it actually is.

Looking at the history of Ethnomethodology as an unfolding development of a First Sociology leads to the conclusion that the idea of a complete rupture between earlier and later versions of Ethnomethodology is overdrawn. First, what happened was more a re-shaping which occurred prior to the publication of the *Studies*. Second, it took the form of sustaining continuity through the adaptation and adjustment to extant approaches in order to apply them to a new set of investigative domains.

An intellectual caesura

But what of the other suggestion? Does *Ethnomethodology's Program* mark a complete break with Sociology as a discipline? Well, it is true Garfinkel repeatedly insists that Ethnomethodology is independent and incommensurable. At the same time, though, ethnomethodological work (including Garfinkel's own) is routinely positioned by using standard sociological themes as foils. The situation, it seems, must be more nuanced than the melodramatic claims might allow.

To begin with, taking Schutz's radical step did not supply answers to the questions which Parsons had posed. Rather, it raised many questions Parsons'

schema did not and could not ask. To do so, Parsons' scheme would have had to have been so thoroughly revised it would have lost its integrity and identity. The coining of the term 'ethnomethodological indifference' refers to this disjunction and is, in effect, a policy of self-denial. Ethnomethodology sets aside the questions asked by Sociology because, given its own pre-suppositions, it cannot answer them. The relationship between Sociology and Ethnomethodology is often presented as a face-off rooted in the latter's 'critique' of the former. This formulation is only acceptable if 'critique' is taken to mean the examination of the conceptual foundations of an intellectual enterprise in order to see whether, when suitably developed, new and different possibilities can be derived from it. Ethnomethodology is not a *correction of* or *replacement for* Sociology's theories and findings, but a systematic *critique* of them – a critique which led to a First Sociology. It is not a better way of doing what Sociology wants to do, but a way of doing sociological research which professional Sociology (both at the time and later) most definitely does not want to do.

What this begs, of course, is the question: 'Can Sociology do what it says it wants to do?' Once again, things often get muddled here. Proponents and critics alike usually present Ethnomethodology as mounting an attack on Sociology by denying ~~that~~ it can. It is also true Garfinkel often appears to be traducing what he claimed were Sociology's shortcomings. Given its placement in the midst of a discussion of sociological reasoning, a classic example of this attitude appears to be the paragraph heading 'The unsatisfied programmatic distinction between and substitutability of objective for indexical expressions' (Garfinkel 1967: 4). Surely this and the discussion it headed are tantamount to an attack by claiming Sociology cannot do what it says it wants to do? Sociology wants to construct formal theory but such formalisation requires precisely the substitution of forms of expressions identified in the heading. Since the distinction between the two cannot be made, the substitution is impossible.

This is another misconstrual. Rather than identifying a hitherto unrecognised problem, Garfinkel is, once again, pointing to a challenge which was, and is, well known among those trying to develop formal theory in Sociology and elsewhere. Innumerable remedies have been offered, not by ethnomethodologists, but by sociologists and the practitioners of other disciplines deeply committed to the ideal of formalisation. As with the slippage between data and phenomenon, the irredeemably indexical character of expressions is a familiar practical, ~~originary~~ fact of research life. It is not that sociologists have no way of managing these problems and working around them, but that the work-arounds investigators use are work-arounds, and not in-principle, theoretically justified solutions.

The idea that Ethnomethodology was constructed to be an existential threat to Sociology is a myth. The myth rests on the premise that what Ethnomethodology says about Sociology came as a complete surprise. This is nonsense. Sociology has long been troubled by the deficiencies in the premises of its modes of theorising, its procedures for operationalisation of investigations and the methods it uses to collect data, especially when these are compared to the rubrics and standards used in other disciplines, particularly the natural and physical sciences. And yet addressing these

deficiencies has remained a far lower priority than undertaking studies themselves and developing theoretical categories on the basis of the findings. This is what Ethnomethodology and Sociology are at odds over: the significance to be attributed to the familiar features just outlined. For Ethnomethodology, they are central and pressing matters, crying out to be addressed and resolved. For Sociology, they are expectable, not to say routine, limiting facts of research life. Ethnomethodology's own methodological prescriptions do not provide remedies Sociology might use. Rather, they are designed to displace the very assumptions which give rise to the deficiencies in the first place.

If Ethnomethodology is not an attack on Sociology, what is their relationship? A weak sense of the term 'foil' used earlier makes Sociology out to be an easily invoked straw man against which some ethnomethodological study or argument can be positioned– a rhetoric of 'They say this . . . but we say that.' And, to be fair, a number of studies (many of which we had in mind earlier) do suffer from this. A much stronger conception, though, would link the ideas of 'foil' and 'hybrid studies'. Given what we have said about Garfinkel's re-thinking of the foundations of Sociology, the consequences of the move of methodology to centre stage alongside (and perhaps in place of) theory, and the way both fed through to the interest in the operationalising of investigations, a strong case could be made for proposing rather than a rupturing of the relationship between (professional) Sociology and Ethnomethodology, the initial realisation of Ethnomethodology as a First Sociology was actually as a hybrid 'ethno-sociology'. The introduction of the studies of work simply extended the range of opportunities and possible disciplines with which hybrid relationships might be sought. Where once the source of investigative topics was derived from Sociology but not cast according to Sociology's conventions, now topics can be drawn from Mathematics, Chemistry, Physics, Philosophy and practically anywhere else. With the adoption of the idea of hybrid disciplines, the notion of using a discipline as a 'foil' comes to mean a detailed explication and triangulation of Ethnomethodology's 'radicalising' interest in the common sense constitution of some set of relevant topics against the interest generally shown towards them within the discipline being studied. Whereas once this was done solely with regard to Sociology, now it is to be carried out with a range of disciplines. As with Sociology, what this means is a drawing out of why the very matters which usually are taken to be of little scientific interest to the discipline concerned are of central investigative concern to Ethnomethodology as a First Sociology.

We are left, though, with the question of the strategic advantage to Ethnomethodology of a stress on hybrid studies. To be sure, more topics are made available thereby, but will the successful undertaking of these studies make a significant, long-lasting difference to Ethnomethodology itself? Where, to coin the phrase, is the ethnomethodological beef? At this point, it is difficult to say, in part because there are so few, if any, fully authenticated examples. Take what many regard as the leading case. Is the pay-off supposed to be that ethnomethodological studies of Mathematics can provide mathematicians with novel ways to pose and solve problems they grapple with? Or, is it expected that

after sufficient joint work, ethnomethodological studies of theorem proving and the like will offer modes of mathematical reasoning which can be integrated into the portfolio of practices mathematicians invoke? From what is on offer so far, both seem unlikely. Far more likely is that the relationship will turn out to be civil toleration between two cooperating disciplines which, without any intention of fusing their disciplinary standpoints, are seeking ways of working together for mutual benefit.

If this is (all) the turn to hybrid studies amounts to, it is important but not radical reformulation.[3] Moreover, it also implies the myth of a necessary and endemic antagonistic relationship between Ethnomethodology and Sociology will have to be re-thought. Just as with Mathematics, we can imagine, for example, the findings of ethno-sociological studies of surveys might help improve question formulation and response rates. This would hardly require either party to review the integrity of their investigative frameworks and theoretical pre-suppositions. Studying surveys is not done by using the survey method, nor does ~~administrating~~ surveys need to involve Conversation Analysis or third person phenomenology.

One recent initiative, however, does not seem to fit so easily with this ecumenism, nor within the general rubric of a First Sociology. Although the work carried out in 'epistemics' seems to fit very closely with what was and is done in Ethnomethodology and Conversation Analysis under the heading of 'the social distribution of knowledge', it has been received with some considerable suspicion.[4] The source of this suspicion is a conviction that the wish to forge epistemics is actually a desire to bring Conversation Analysis into line with the requirements of proof and analysis found in Linguistics. This would require both the relaxing of conversation analysis's foundational requirement for the traceability of the phenomenon being analysed in the specifics of the materials being reviewed and the acceptance of Linguistics formalisations as the premises for its descriptions. If so, both would seem to involve stepping away from the stipulation to focus on the primordial set out by Husserl, Schutz and Garfinkel, which is why so many of its critics accuse it of 'constructivism'. At the moment, the best one can say about epistemics is that its results do not appear to be offering any order of improvement on traditional conversation analytic descriptions of talk. Indeed, on occasion, given the moves mentioned above, the policies invoked occasionally seem to exhibit the familiar sociological substitution effect we described in Chapter 3.

To summarise. The conclusion we draw from the debates over the supposed retrenchment in Ethnomethodology is a sanguine one. On close inspection, points of rupture and discontinuity of principles appear to be more adaptation and evolution of an emerging First Sociology than anything else and, as a consequence, the idea of necessary antagonism with Sociology is more myth than reality.[5] Seeking cross-disciplinary connections, be they be in search of hybrid disciplines or with somewhat more modest ambitions, does not seem to us to be either a new radical innovation nor a slighting of the autonomy of Ethnomethodology's own foundational position. In these cooperative ventures, neither partner needs to feel under threat and so any mutual learning which does eventuate should not be sniffed at.

materially to the identification of what makes Ethnomethodology distinctive. Instead, our claim is this. As a First Sociology, the investigative space which Ethnomethodology occupies is distinctive enough, interesting enough and, as yet, unexamined enough to warrant its systematic exploration. That others find the issues we attend to trivial is no reason for us to be reticent about making our materials and analyses available. If the phenomena we present are not gripping enough, nothing we can say about them will make any difference.

Looking forward

The burden of this chapter might be summarised as 'Beware the fallacy of the immediate!' Ethnomethodology has been around for a long time and while there have been shifts in emphasis, vocabulary and interest, there are also strong skeins of consistent reasoning which preserve its radicalism. By treating it as a First Sociology, we emphasise Ethnomethodology's primordial, self-contained structure; one which facilitates a line of investigation which does not compete with more conventional sociologies. Given this separation and insulation, it is hard to see what detailed topical continuities there might be between them nor how they might be integrated. In our work, we have sought little more than loose coupling and indicative identification.

Even if we don't see Ethnomethodology as entering a death spiral, that does not mean we think there are few problems and issues to be addressed. The most important (and this is certainly associated with the sense of ennui many feel) is the problem of capitalising on the achievements made thus far. The body of studies faithful to working out the original impulse have clearly demonstrated the fertility of the approach and its character as a distinct form of sociological investigation. Unsurprisingly, as the momentum of the work began to pick up, differing clusters of emphases and interests have emerged. The most important of these was Conversation Analysis, although video analysis and ethnography should not be overlooked. The evident success of Conversation Analysis, though, may have owed much to the nature of the phenomenon being studied. The early identification of the centrality of turn taking within the analytic object, 'a single conversation', certainly allowed the development of a programme of focused studies wherein each could see itself codifying distinct aspects of a unified and generalised organisational structure. How cumulative this programme actually is remains less clear. Whatever the case, it is plain the studies undertaken are exemplifications of the close analysis of ordinary phenomena.

Determining progress and cumulativity in the enormous range of studies framed as following Garfinkel's own work is a far greater problem. Taken en masse, it is hard to say exactly what their overall cumulative force is – apart, that is, from repeated identification of the viability and fertility of the standpoint. Whilst Garfinkel himself may have insisted on the primacy of studies and the absolute necessity of undertaking them for oneself, it may be time to review what has been achieved in the accumulation we have and to ask what order of success

this achievement really displays. Once we know how far we have come, we ought to be able to decide where we need to go next and how we might get there. With this stocktaking in hand, our hope is the studies we present here and the approach we have used in this book will prove useful pointers to one of the paths which deserves to be followed.

Notes

1 In the final pages of *Mundane Reason* (1987), Pollner points to the dilemma he sees Ethnomethodology facing. Either it can extend its own 'radical' agenda to itself, thereby threatening any claims to empirical realism, or it can exempt itself from its own tenets and adopt the objectivist stance of the rest of Sociology. By 1991, he became convinced it had chosen the latter.

2 In classical and medieval philosophy, the plenum was the chaotic universe of fundamental matter which filled the cosmos. Time and space were not real but simply 'figments of the mind' used to order our experience. Enlightenment thinking, especially following Newton, asserted fundamental properties such as time and space were real and ontologically coeval with matter. Kant's metaphysics of 'empirical realism' tried to give a philosophical grounding to this view and became the consensus. Kant's philosophy was the backdrop against which the sociologies of the nineteenth century were developed.

3 Indeed, 'interdisciplinary studies' would do just as well as 'hybrid studies' and would have the added advantage of drawing attention to the extent that such studies are commonplace across Sociology and other disciplines.

4 The debate is ongoing. See Heritage (2012a, 2012b) and the papers in *Discourse Studies, vol. 18, no. 5 (2016)*, especially Macbeth et al. 2016, and Macbeth and Wong 2016.

5 We want to place emphasis on the word 'necessary' here. We think the undoubted antagonism is unnecessary. It could be that Pollner himself may have put his finger on its cause when he suggested the bracketing of a mode of enquiry such as Sociology's frame of reference could provoke a questioning where the sociological investigator comes to doubt, what as an ordinary member of society, it means to be a social actor and to live in the social world. This is just the kind of reality disjuncture examined by Pollner (1987) though not one of his examples. To take just one instance, a dispassionate review of some of the responses to Garfinkel and Sacks in *The Purdue Symposium* (Hill and Crittenden 1968) might well conclude this is precisely what is happening.

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