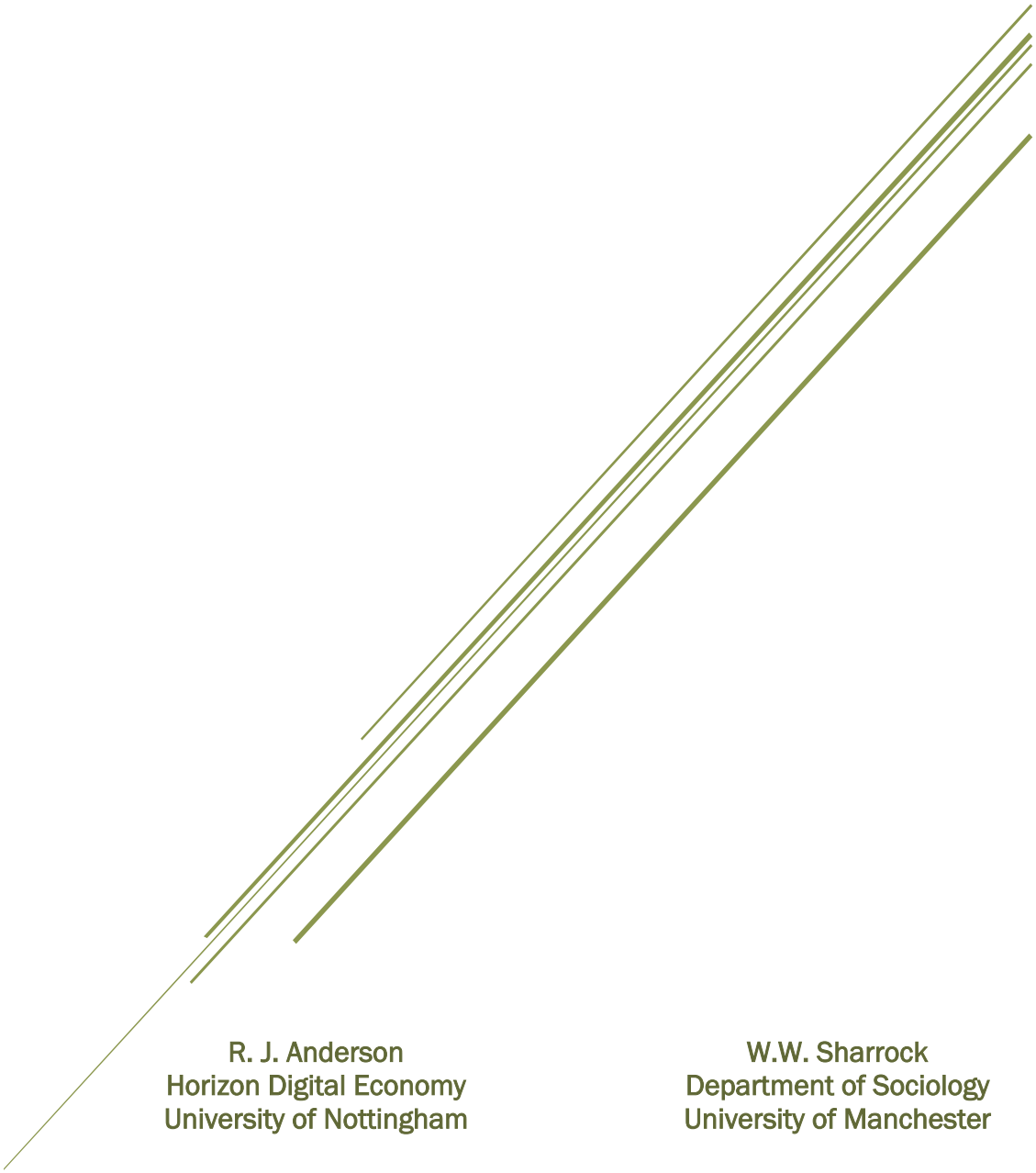
HAS ETHNOMETHODOLOGY RUN ITS COURSE?



R. J. Anderson
Horizon Digital Economy
University of Nottingham

W.W. Sharrock
Department of Sociology
University of Manchester

Draft 7.1

© RJ Anderson & WW Sharrock 2017

PROLOGUE

This has been a dispiriting piece to write. Its subject is the shortcomings of current investigations in Ethnomethodology (EM). We are not pleased with the things we have to say. Nor are we pleased we feel we have to say them. And yet, for some time now, we have sensed EM is in the doldrums, analytically losing impetus and excitement. During that time, we have often talked about what would be needed for it to raise its game. We now feel ready to share those views with friends and colleagues, though we have no illusions they will be either popular or shared by all. Our purpose is neither to pillory hard working and thoughtful colleagues nor to hand out plaudits and re-assurance. We want to stimulate a debate within EM about how to re-orient its energies and revitalise its work; a debate during which we hope even those who are far from being convinced by the case we make here, will nonetheless see value in our proposals.

Before we start though, experience tells us there are a couple of things we need to be clear about and a couple of responses we need to head off. First and most importantly, we are not saying EM is fundamentally and irredeemably flawed. We do not think its foundations are based on logical, conceptual or methodological fallacies, inconsistencies or downright wrongheadedness. That is not the thesis we are making. What we are saying is simply this. Looking across the majority of the work we see published today, it seems EM has lost its way. What was once its driving force, its mission, its *raison d'être*, has been replaced in part by the amalgam of traits, the complex, we describe. What we see now is not the EM we found when we first encountered it and certainly not the EM we imagined it might become.

Second, note the quantification. We are talking about the burden of the vast majority of studies published, not each and every one. Of course there is good work being done and of course there are studies and programmes of investigation which have escaped the complex we identify. But (and it is a big 'but'), these are harder and harder to find. Measured by volume of publications, EM is undoubtedly booming. The trouble is the ratio of the insightful, perceptive and distinctive to the routine, unimaginative and bland is getting larger and larger. What worries us is precisely this decline. We would be foolish indeed if we thought Gresham's Law did not apply as much to academic research as it does (or did!) to financial markets and its operation would not result in the same outcomes.

Finally, this discussion is couched as an essay (or what in the technology labs they used to call a 'think piece'); a conversation with colleagues and friends. Other than for direct quotations and instances which are somewhat off the EM beaten track, we have deliberately refrained from freighting the text with the impedimenta of scholarly citation. Those to whom we are talking know well enough what we are talking about.

HAS ETHNOMETHODOLOGY RUN ITS COURSE?

CONTEXT.

Has EM run its course? In the end, we are inclined to think not, but the question is worth asking and deserves an answer. That answer, of course, depends on what is meant by 'run its course'. We assume EM is an established, viable form sociological investigation and analysis[1] and there remain aspects of social life to which it has not, as yet, turned its attention. So, if the question means 'Has EM exhausted all the investigative avenues it might follow and run out of things to study?', then the answer is obviously 'No!'. Things are not quite so cut and dried, though, if we take the question to be asking whether EM has ceased to be a developing discipline which functions as an "asymmetric" and "incommensurable" alternative to conventional forms of sociology. In different ways, that question has been asked several times over the last few years. As long ago as 1991, Mel Pollner was fulminating against EM's loss of radical reflexivity and its assimilation into the body of conventional sociology. Somewhat later, having sampled the presentations at the IIEMCA conference in 2013, Roy Turner was moved to ask (we paraphrase in the argot of the modern young) "Why are you guys still doing this stuff?". For Turner, EM's distinctive position on the investigation of social life had been amply validated by the first generation of research. As far as he could tell, work carried out today is simply repeating that validation. Finally, an example much closer to home. During a session at the 2015 Mind and Society meetings in Manchester, several discussants questioned using the degree of cumulativity evidenced in studies as an appropriate benchmark by which to measure progress in EM. Somewhat exasperated, David Francis retorted, "Well, if it is not cumulativity, how does it progress?".

In their own ways, these three questions point to a complex of traits which, taken together, seem to imply (if we can be allowed to jumble our metaphors a little) EM has is becalmed or even stagnant. In our opinion, this complex represents a challenge to EM's continued existence as a developing alternative sociology. Unless it is taken seriously and addressed, the danger is not the phenomena EM has its sights on will no longer be studied but the distinctiveness of those studies will likely atrophy, so much so that gradually its claim to be asymmetric and incommensurable with conventional sociology will no longer be sustainable. When or if that happens, EM really will have run its course.

This discussion will try to dissect the complex we identify a little and sketch some of the unforeseen and unfortunate consequences arising from it. In the last section, we offer suggestions for how these consequences might be rectified. We are convinced what is needed is a reorientation in EM's attitude to its own work. The hope is this reorientation might provoke just the kind of reinvigoration the Pollners. Turners and Francises of this world are looking for.

[1] Our topic is EM as a mode of investigation exemplified by Garfinkel's studies. We are not including Conversation Analysis (CA) in this designation. We know CA had its origins in EM, but it has long since set off down its own path.

1. The Ironies of Garfinkel's Heuristic

EM's investigative approach rests on a particular device, an informal analogy which Garfinkel runs between social life and production processes.[2] What sustains the analogy is the notion of method and the invocation of an 'inexact'[3] isomorphism between the organisation and deployment of methods to construct social and material 'things'. On the analogy, social order is to be conceived as the product of members' practices or methods. The analogy is informal simply because the mapping is not one to one.[4] The story behind the introduction of the analogy is a familiar one. Garfinkel returned to it several times.

Based on his reading of Kaufmann, Gurswitch and Schutz, Garfinkel developed EM through a quite distinctive set of sociological idealisations. What defines sociology is its concern for the 'orderliness' of social life. Sociology's remit is to describe and explain that order. Using Alfred Schutz as his guide, Garfinkel suggested all forms of sociology presume (a) the activities which social actors undertake are based upon their interpretative understandings of the social worlds which they inhabit; and (b) central to these understandings is an assumption that their social world *is* an objective reality (and hence not a dream, figment of their imagination, cleverly constructed invention, or some other such confabulation). Based upon these idealisations, sociology seeks to describe how social activities produce the social facts which comprise society's evident social order. To do this, it follows the same rigorous strategy as the natural and mathematical sciences. It constitutes a world-for-investigation in terms of its idealisations. To re-use a quotation we have used many times before.....

> *...(t)he 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).*

In other words, when undertaking empirical investigations, the sociologist conceives what people do in their daily lives, at home, at school, in church, at work, visiting the cinema with friends, by treating these activities *as if* they are responses to the sociological challenge of producing the social facts of social order.[5] He called this "the praxeological rule". When he turned to what was then the dominant mode of sociology to see how this rule might be applied, Garfinkel came to the conclusion that despite its impressiveness as an intellectual construction, the whole edifice of the Theory of Social Action developed by Talcott Parsons and his colleagues rested on a key unexamined presupposition, namely that social actors share a common culture which provides the repertoire of understandings on which social order is based. Garfinkel did not deny there is such sharing but simply observed, as exemplified by the Theory of Social Action, sociology had not described *how* the relevant elements of the repertoire are aligned on any occasion. So, unless we stipulate social actors are

---

2 Our choice of the term 'analogy' is deliberate. We mean it in the logical sense of a translation between forms. The usage is an analytic one not a loose comparison such as a metaphor deployed for rhetorical purposes.

3 See Kenneth Manders (2008) 'The Euclidean Diagram'. The Philosophy of Mathematical Practice, edited by P. Mancosu, OUP Oxford. 2008

4 This informality mandates we use the existential copula with care in relation to it.

5 In this idealisation, Garfinkel follows Husserl is defining understanding and perception as Brentano intentional. Just as in ordinary life, under the sociological gaze, seeing and understanding are as much 'seeing as' and 'understanding as'. The world for investigation is, therefore, constituted by the *sociological attitude*, the sociological finite province of meaning which the investigator adopts. It is not for nothing that Garfinkel's annotated thesis outline was titled *Seeing Sociologically*.

'programmed' to see things the same way (i.e. are what he calls "cultural dopes"), or the alignment happens by the intervention of some hidden hand (the Almighty? The logic of evolutionary biology?), or by magic or accident, given the standard idealisation for constituting the social world for investigation, we have to assume social actors respond to the challenge of doing the work of producing social order by aligning their understandings using sets of standard and shared practices or methods. These methods facilitate the production process of social order and are a key taken for granted assumption at the heart of sociology's theorising.[6] EM's remit is to investigate these methods.

Having identified a lacuna at the core of sociology, Garfinkel had to find a way of investigating the phenomena which that lacuna represented; a way, that is, of conceiving the effort or work by which social actors overcome the interactive challenge and bring about alignment in their repertoire of shared understandings which would allow such work to be made observable and amenable to sociological investigation. In other words, he had to develop a methodology to operationalise his analogy. Kaufmann had made it clear such a methodology would have to encompass classes of phenomena to be investigated and investigative protocols by which these phenomena can be identified and characterised (or, in Garfinkel's own phrase, how the animal is to be "extracted from the foliage"). This was done by means of a further idealisation. Call the understanding individual actors have of the courses of action in which they are engaged, their "account" of that course of action. This is his rendering of the more familiar sociological term 'the definition of the situation'. Since, to solve Sociology's central problem of social order, social actors have to resolve the double contingency of aligning accounts (their potentially different understandings), assume they do this by using the standard methods we have already mentioned in order to make their understandings (or the accountability of action) available to each other. Assume also that this is done, not through some 'meta-activity' such as a running commentary or reference to a common instruction manual, but in and through the detail of the actions they undertake. On this view, accountability can be treated as displayed or observable within the particulars of social action and is the use of methods to do the work of producing social order. These idealisations comprise EM's 'analytic attitude' or 'gaze'. All that was left (!) was how to formulate studies to investigate these displays of accountability, a task on which Garfinkel spent the rest of his life.

First and Second Formulations

The first formulation is extensively set out in *Studies in Ethnomethodology*. Under it, social actors are treated as interpretive or sense assembling devices utilising repertoires of culturally defined definitions of the situation, sets of correlated appropriate or normative actions together with methods for displaying and discerning these definitions. The forming of social relations consists in overcoming the challenge of achieving a reciprocal alignment of definitions. The problem of shared understanding is, then, the problem of 'sense assembly'. Every social encounter can be treated as an occasion in which the challenge set by the open possibilities of choices over the definition of the situation (and hence appropriate courses of action) has to be overcome by the use of methods of sense assembly. For the purposes of sociological investigation, the

[6] For Aristotle, conceptions of 'being' or the 'first causes of things' underpinned or were taken for granted by all the other branches of philosophy. Their philosophical analysis was, therefore, First Philosophy. His writings on this topic were later gathered together as his Metaphysics. Using this a very loose comparison, we have dubbed EM a 'First Sociology' because it concerns itself with those social phenomena which sociology (necessarily) takes for granted.

understanding or 'sense' of the actions at issue are those assembled by the participants. That is, any judgements deployed in the analysis concerning the factuality, propriety, logicality, legality, rationality etc. of the actions should be those oriented to by the actors themselves within the course of that action. This is EM's policy of 'indifference'. The upshot of these policies is that settings are deemed to be self-organising.

To mount studies, Garfinkel 'proceduralised' (or 'misread') philosophical abstractions of the natural attitude and formal logic he found in Husserl and Gurswitch, as protocols for the design of investigations of the alignment of definitions of the situation via the sense assembly of appearances. In their different ways, the studies of patient records, jurors' deliberations, management of transgendered identity, the logic of psychiatric diagnosis and so on all attend to how the accountability of the phenomena of concern are systematically and collaboratively, that is methodically, produced as orderly phenomena.

The second formulation is contained in *Ethnomethodology's Program,* and with it the kaleidoscope is turned so that the elements of EM's gaze take up a somewhat different configuration. The nub of this move is a re-working of the analogy already described. As we saw in the quotation above, the analogy treats social actors as if they are living their social lives in order to produce solutions to sociology's theoretical problems. The social institutions which are created through the patterning of actions are therefore to be seen as solutions to sociological challenges, problems and puzzles. In his re-working, Garfinkel turns to Durkheim's famous aphorism about sociology's phenomenon being "the objective reality of social facts". The social facts Durkheim is pointing to are the same patterns of social action that Garfinkel had seized on in the first formulation.

The re-working is a 're-specification' of the aphorism using an extension of the earlier notion of method. For the sociologist, the task set for social actors is the production of social facts (the patterns of action). This is the job of work they are given. Extending the analogy from a general notion of production process to the organisation of industrial production, what social actors do to produce the social facts which are required to solve sociology's theoretical problems is now to be talked of as their collaborative 'shop floor work'. As anyone who has ever spent any time either working in or observing any kind of production shop will testify, to produce any designed artefact whilst satisfying its design specifications and the constraints of the production environment takes a lot of shop-floor know-how (how to prepare the materials, how to manage the line, how to get the machines to do what they are supposed to do, and how to do all these things at the defined cost, in the time laid down and with the least effort, etc.). None of this know-how is visible in the final product which rolls out of the door. *In just the same way,* none of the know-how required to produce patterns of social action is visible in the summaries, generalisations, schemes and so on which make up sociology's descriptions of its social facts. The know-how is necessary for the production of these facts (a pre-requisite even) but is taken for granted in their sociological use. On any social occasion, the participants to a course of action are to be treated as a 'local production team' or 'cohort' constructing the forms of social order on view as social facts. So, to use his own phrase once again, under the ethnomethodological gaze whatever sociology treats as a topic of order (patterns of kinship, educational attainment, power and influence, suicide, crime and so on) is to be seen as a phenomenon of order; that is, an object whose social construction is to be studied in its own right. On the first formulation, EM is a First Sociology because it studies those things which underpin sociological investigations. In this formulation, EM is a First Sociology

because it studies those things which are pre-requisites for the production of the social facts which are Sociology's phenomena.

Under the second formulation, studies can be framed simply by triangulating on topics investigated by 'formal' or conventional sociology and, as described above, re-specifying them in terms of the analogy of shop floor work. Garfinkel points out the initial studies reported as part of *Studies in Ethnomethodology* can be re-construed as studies of 'the shop floor work' of producing various patterns of social facts under the natural attitude as well as within other finite provinces of meaning. In a similar way, many investigations which responded to the insights of the early studies, for example those of medical practice, organisations, the law, education and the sciences, can be similarly re-interpreted. What the programme in the *Program* illustrates is how principles set out in *Studies* can be re-worked by centring on the relevant 'shop-floor methods' and the details by which they are employed (what he calls the "haecceties") which make the production work recognisably what it is taken to be. Associated with this reconfiguration are several recommendations for undertaking EM studies. These include suggestions like aiming for 'descriptive precision' or the possession of 'unique adequacy' of competence in the methods, searching for 'populations' defined in terms of those methods, identifying groupings (pairs, triplets and so on) of objects oriented to in using the methods as well as instances or cases where social actors explicate for themselves how the methods are worked through.

So where in all of this does the irony arise? It is in the following. Both the first and the second formulations offer a rich palette of choices for constituting EM studies. Various types of investigative approaches and analytic methods using the central heuristic are described and demonstrated in a panoply of fully worked out and sketched studies. The investigative and analytic attitude which Garfinkel set out was radical, inventive and highly generative. Unfortunately, over time we have seen a narrow concentration of method and analytical forms based on a combination of what is loosely called 'ethnomethodologiocally informed ethnography' and recorded (audio/video) data collection. Overwhelmingly, this combination is what investigators mean by EM. As a result, studies have clustered around domains and topics which suit such techniques and the materials which are provided by them. These studies have been mostly taken up with the common sense settings of everyday life or the worlds and sub-worlds of professional life. In both arenas, the focus of attention has generally been on forms of social interaction and the paraphernalia used in support of those interactions. What has been presented is a homogenous (almost clone-like) collection of 'analytic ethnographies' which describe 'doing the shop floor work of...' whatever setting is being studied and where analysis is largely a re-description of audio-video transcripts. What once was part of an insightful and radical methodological move has been turned into the gradgrind of routinised data collection and so has become limiting and constraining. Ostensibly wearing the goggles of EM's liberating re-specifications, study after study shows the same blinders and the same blandness. The possibilities offered by Garfinkel's proposals and summarised in his own presentations have largely been ignored and the original revolutionary purpose of Garfinkel's heuristic has been lost. Ironically, what we are left with is a degenerate, moribund metaphor.

One result of this homogenisation has been the displacement of strategic sociological and ethnomethodological interests for the framing of investigations by discursive topical interest, an unfortunate importing of an all too frequent feature of conventional sociological studies. In sociology, when a study is

introduced, either explicitly or implicitly, its rationale is cast in terms of its topic not what is to be made of that topic. Usually the investigator chooses a subject because he or she is interested in it for their own reasons and not because it demonstrates some particular sociological challenge of which the discipline was unaware or by which it was stymied. For example, at the risk of being provocative you could argue the issues of contemporary culture and hence the topics encompassed by Cultural Studies, pose sociology *qua* sociology no new or challenging problems. As a domain of studies and the methods used for them, Cultural Studies is much of a muchness with conventional sociologies of education, the family, power, industrial life and the rest. Pair it with any other and replace all the 'domain terms' with placeholders and you couldn't tell the difference.[7]

Far too many EM studies display this tendency. Not that they look like conventional sociologies (or, at any rate, the good ones don't). Rather, what is offered as making them interesting is the supposed interestingness of the topics (and in some cases the 'edginess' of the materials and/or the way they were collected) not how the investigation was constituted nor what the findings, conclusions and insights offered might imply for EM. Given that EM's central orientation and policies enabling a variety of modalities for setting up studies was taken to be one of the key features making it distinctive and exciting, the homogenisation of investigations and analyses together with the displacement of criteria for determining their point is a deeply ironic situation for the discipline to be in.

2. The Tenacity of Empiricism

The trait we examine in this section is how EM analyses tend to be grounded. It takes many different forms, but the one we will look at here has the appearance of a puzzling paradox and is evidence of the tenacious hold empiricism still has over our thinking about the relationships between phenomena and data. More often than not, it is displayed in the way the data and findings of a study are positioned.

The paradox turns on the relationship between analysts' accounts and actor's accounts. The way to address it, we suggest, is by applying 'the ethnomethodological attitude' to EM itself.[8] We will illustrate its features by reference to that most elusive of all sociological topics 'the actor's point of view'. Once we understand what the issue is, it ought to be possible, not to "re-specify" the actor's point of view, but to specify it rigorously in the first place and to do so in ways that enable us to give adequate EM descriptions of it.[9]

Here is a particularly stark case of what we are thinking of. It is taken from Wolff Michael Roth's (2015) discussion of 'the documentary method of interpretation' as an answer (or something) to the classical

7 The same goes for Gender Studies, but that may be a provocation too far.

8 The disappearance of this particular reflexive turn of thought was precisely what Pollner was inveighing against. For some the demand seemed too radical and highly likely to be subject to John Austin's "Law of Diminishing Fleas". We will show this is not so.

9 Two things need to be said here. First, this issue is not unique to EM. Nor is EM especially poor at providing adequate sociological descriptions. In fact, it has taken the issue more seriously and more robustly than most. Even so, swathes of current practice do seem unsatisfactory. Second, as we have already said, Garfinkel's attempt to specify the actor's point of view within the framework of the Parsonsian schema was the stimulus for the development of EM, and hence in one sense you could say that what has followed has been a working out of what that specification means and how to apply it. We are not trying to roll back that work but highlight some issues which seem to have been underplayed or overlooked in the subsequent propagation of the programme.

question: 'What is meaning?' (or rather. What are meanings? In this instance, what are the meanings of concepts used in mathematical education?)

> *In the classical approach — which has been handed down to us through generations since Plato.....there is a metaphysical realm where "ideas" exist. For example, the mathematical idea of a cube is an ideal object. As other ideal objects, it does not and cannot exist in the real world, which is full of imperfections. Thus even with the most exact machines existing today, the (perfect) cube that is the object of geometry cannot be engineered. As soon as we look a little closer, using other devices such as an (electronic) microscope, the very best cube that we could currently make would have rounded (rather than point-like) vertices. (Roth: 2013, p 173).*

Another, perhaps even more telling example is:

> *I insist on the difference between the lived experience of mathematically seeing and the accounts of experience of seeing in mathematics that societal actors — children, teachers, or lay and professional mathematicians — provide when asked about what they see. Almost all research, both quantitative and qualitative is concerned with accounts of experiences of mathematical seeing rather than with the living/lived work of mathematically seeing.....I insist that the real work of (doing, seeing) that makes mathematics an objective science is actually lived and the result of our living/lived sensuous bodies rather than that of the constructivist mind. (Roth: 2012, pp 227-8)*

Appended to the final sentence is the following footnote.

> *During the WISDOM conference [it was suggested].....that I did not understand (radical) constructivism. But all they were doing was reiterate the subjectivist idealist position that von Glasersfeld has laid out, a position that many philosophers have shown to be untenable in the face of* ***real data****.....(fn5 p 244, our emphasis)*

Finally, since it takes the familiar form of a commentary on 'data', this is the clincher. Once again, it is taken from Roth's discussion of the meaning of mathematical concepts in educational settings.

	63	(0.84)
→	64 C:	like what do you mean?
	65	(1.10)
	66 W:	´does it match. We said THAT this group ((*points*)) was ´squa::re (0.31) or cube (0.49) ((*looks at Connor, nods*)) does it match that?
	67	(0.41)
	68 X:	<<pp>yes.>
	69	(0.48)
	70 X:	or::.
	71 C:	((*gazes at Mrs. Winter*)) yes.
	72	(0.70)
	73 T:	<<p>o>kay. (.) ben you wanna add? ((*nods to Connor*)) (.) thanks connor.

> *As in the previous cases where Mrs Winter has offered up a question while gazing at Connor, there is a pause. And then, the return question again:*

> *"Like what do you mean?" (turn 64). Taking the next turn at talk, Mrs Winter utters what grammatically is a question though said with the falling intonation of a statement: "Does it match?" But before an answer could come, she continues with a statement referring to what they had said before, when they transactionally named the group in which Connor's object is now located "square, cube". She then offers up another question, "Does it match that?" (turn 66). Some unidentified student states an affirmation, and eventually Connor does too: "yes" (turn 71). Producing the affirmative interjection "okay", Mrs Winter's turn allows everyone to hear that the expected answer has been provided. That what was to be achieved has indeed occurred is also evident from the fact she asks, without further ado, whether Ben wanted to add and then, while nodding in the direction of Connor, thanks him, (Roth: 2015, p 172).*

We don't want to be thought to be picking on Wolff, though.[10] The way the trait appears in his discussion and analysis is widely found elsewhere in the ethnomethodological literature. It consists, first, in the explicit adoption of the conventional EM position which questions and rejects empiricist and realist conceptions of social reality and the associated modes of investigation promulgated to study them matched, second, to the adoption of a thorough going empiricism with regard to the 'data' cited and how they bear upon analytic claims being made. The first two snippets are really only indicative since appeal is made in them to the evidential authority of 'data' in physics and related disciplines or conventional sociology and philosophy. It is the third which really brings out the characteristic EM form, giving, as it does, a 'matter of fact' gloss on what happened in a classroom as that is 'captured' by a transcription of the interaction. The account Roth gives becomes the basis for his analysis of the lived-work of seeing mathematically. In all three examples, what data say, what they represent or (to use a particularly ethnomethodological turn of phrase) 'what these data are data of', is treated as thoroughly unproblematic. The reality of the cube captured by advanced microscopy, the nature of experience as described by (some) analytic philosophers and (some) sociologists and Roth's version of the classroom exchanges, all capture reality and can be used as petards to demolish the social constructivist account of how meaning, thought and (social) action are related.

Here, then, is the problem. How can an approach which takes the reflexivity — the socially constructed facticity — of social facts as its most favoured motif rest so heavily on an unreflexive account of 'the facts' it seeks to analyse? We think the answer lies in an often unacknowledged issue at the heart of EM, namely the relationship of data to the account being given of it.[11] What precisely is this relationship and on what exactly is it founded? Some will see these concerns as essentially epistemological or even, heaven forfend, metaphysical. Neither captures precisely what is troubling us. Nor does the suggestion that what we have noticed is a familiar and unsurprising side effect of the 'normalisation' of EM and its accommodation (even suburbanisation) within the professional discipline of sociology, though as we have said, the issues are closely related. Because mainstream sociology is empiricist at heart, or so that argument goes, to become

[10] To judge from much of what he says about his approach, Roth seems to have been particularly inspired by Anne Rawls' introduction to Garfinkel's *Ethnomethodology's Programme*. Whether this influence actually contributes his adoption of the trait is another perfectly proper question to ask, but not one we will take up.

[11] A brilliant but now somewhat forgotten paper *Data? Who needs it?* by Howard Schwartz, remains the most insightful discussion of the core issues.

publishable and teachable, EM has had to adopt the overtones of empiricist rhetoric. This tendency may well be going on but we don't think it is the whole problem nor even the key. Rather, we think the challenge is *methodological* and, as we say, its resolution lies in the use of EM's methodological insights on EM's own methodology.

DECONSTRUCTING THE PUZZLE

To keep things simple, we will focus on the final snippet we cited earlier. This is from a contribution to a conference and is addressed to an audience interested in new ways of understanding learning (and, presumably, teaching). It is a paper; written to be read as such. It has a message, a point, which Roth wants his readers to accept, namely that the findings of ethnomethodologically informed analyses of class room activity demonstrate the inadequacy of social constructivism as an account of learning. Its objective, we assume, is to be convincing about that inadequacy. The written-read document is designed to be the demonstration; that is, given its audience, it is designed to have that outcome. In line with Garfinkel's heuristic, EM has a favourite trope through which such interactional production work can be fixed. An action is formulated as 'doing (Φ) x-ically'; in this case, 'doing (demonstration/persuasion) ethnomethodologically'. The formulation allows attention to be given to what, as we saw in the first section, is routinely talked of as 'the work' constituting the doing. Roth himself uses exactly this formulation in positioning his contribution and so thereby underscores the provenance of his piece.

So, what is this work? What are these tasks? We can gain a little clarity if we stay with the notions of 'demonstration' and 'persuasion'. These are rhetorical terms; terms referring to the construction of arguments. Effectively built and deployed, arguments carry the plausibility of their conclusions and thus their conviction. We can think of them as conventionalised formulations for realising plausibility.[12] The work of using these forms comprises, to use EM's jargon once more, the accountable methods by which writers and readers accomplish a written/read demonstration of a convincing argument.

The propositional calculi of formal logic provide one bundle of possible methods. The one which Roth uses, regressive reduction, is widely used in EM. Its logical form looks like this.

Logical Structure	Roth's Argument
P - > ¬Q	If social constructivism is true then learning is not socially organised
Q	Learning has been shown to be socially organised
∴ ¬P	Therefore, social constructivism is false

The point about regressive reduction is its reliance on the strength of the evidence mustered to demonstrate Q. In other calculi, the truth of the conclusion follows as an entailment of the truth of antecedent propositions.

[12] We have talked about these issues before. This time, though, we are coming at it from a slightly different angle.

In regressive reduction, the proof rests on non-logical (but not illogical) grounds, namely the availability of data demonstrating Q.

Obviously EM takes logical forms to be idealised abstractions. In each and every case, demonstrable conformity to the form is a practical accomplishment; one of the tasks required for the co-production of the written/read argument. Those producing the argument orient to these tasks as do those perusing the argument in order to find their way around in it. These orientations are the conditions of the recipient design of the demonstration. Their plausibility structure depends on the assembly and marshalling of appropriately relevant data. Constructing such data *as* relevant *and* plausible is integral to the work of making the argument. And, of course, this work is often carried out with disarming effortlessness, skill and elegance. At other times, it can appear disjointed, bumbling and inept; or, indeed, anything in between.

The need for the work of evidential demonstration creates the possibility of our problem. Writing research reports is a practical activity and, as with all practical activity, those who do it develop routines, standard methods, tricks of the trade which facilitate carrying it out. These are the famous "seen but unnoticed" methods that make up common sense reasoning in any domain. Those familiar with the field recognise this practical know-how and, in their turn, rely upon it to understand what is happening and so are able to design their responses accordingly. Every craft has these routines, these conventional formats. Our claim is that EM has developed a standard presentational modality for closing the gap between the regressive reduction of its arguments and the evidence it offers to secure the plausibility of the cases being made. This modality is the empiricist cast it gives to talk about data. Because it has become standard, it passes without remark or even reflection. And that is the problem.

Modes of Plausibility Construction

Our claim is a practice has grown up in EM which has an odd consequence. To understand why this has happened, we need to turn to more esoteric but fundamental matters.

Although the introductions and expositions are innumerable, for the most part accounts of EM deal with matters of method rather than methodology. That is, they are concerned with the status of the phenomena which EM takes as its own and the extent to which they are distinctive. It would also be fair to say, again for the most part, that such summaries endeavour to show either that EM marks a significant but clearly incremental improvement over what sociology has done heretofore, or that it should wholly replace sociology as an account of social action, social life and social reality. Discussion of methodology, that is the logic of investigation itself, is far rarer. In the following sections, we will look at what has become the standard position supporting the notion of the transcript as evidence and what such evidence can be held to say, demonstrate or even prove. Its origins are in a central concern underpinning CA's approach to its data. However the routine and unreflective deployment of CA-like (and CA-lite) transcription protocols in ethnomethodological studies has been accompanied by a disregarding of that concern

Transcripts as Evidence

Early in his classic paper on telephone identification, Emanuel Schegloff offers this account of his interest in his (and by extension all EM's interest in its) data.

> *We bring to the materials.....an interest in detecting and describing the ordinary phenomena of which conversation and interaction are composed, and an interest in depicting the systematic organisation by reference to which those phenomena are produced. (1979, p.24)*

The key terms are, of course, "detecting and describing" and "the systematic organisation...." In CA the notion of systematicity implies 'structure and generalisability'. Whatever patterns are observed in any individual piece of data are examined for their generalisable structures. The key facilitating device for "detecting and describing" these generalisable structures is the turn taking machinery. Notice the status of the implied proposition about the existence of generalisable structure. It is not a discovery *from* data but a postulate *about* data. The analytic work of detecting and describing is, to repeat Garfinkel's phrase, a means of "extracting the animal from the foliage". One way of thinking about this might be to say (albeit very loosely) the turn taking machinery stands as a set of axioms derived from the postulate of systematic orderliness. The postulate 'There is systematic order in the plenum of ordinary talk!' motivates the whole enterprise and is its *sine qua non*. Findings stand to the axioms and postulate as demonstrations of what deploying them can come to. Working out how, in the context of actual data, the machinery and postulates might apply and presenting that working out within a definitive plausibility structure is what the recipient design of a written/read CA argument amounts to.

Key to this plausibility is the role allocated to data. Since most contemporary studies in EM rest on them, we will assume, as a simplifying move only, such data are fragments of transcripts, the most widely used protocols for which are variants on those created by Gail Jefferson. Jefferson's protocols render audio or video tape materials in terms of the turn taking machinery. That is, the transcription procedure assumes the operation of turns, the boundaries of turns and the sequentiality of turns to be the stuff of co-ordinated conversational action. By listing turns, the conversation-in-the-tape can be rendered as the conversation-in-the-transcript. Thus, given the postulates, the phenomenon to be analysed is the systematic turn-taking character of transcribed conversation.

Framing things in this way enables us to bring out two important facets. First, the phenomenon for CA is the structure of conversation; not talk within the therapeutic encounter, not talk as a means of reporting matters to the police, nor talking as a mode of teaching, or talking as work, or family matters, or any other of these other, distinct activities. The focus is talk *as* talk, not talk as doing something else. It is this phenomenon for which CA strives to provide a rigorous description; a description organised in terms of the systematics of the turn taking machinery. CA constitutes talk (its phenomenon) as the transcribed display of the systematics of the turn taking machinery. Second, for CA the way its descriptions are to be secured to its phenomena is through a very precise mapping device, what Sacks et al. call a "proof procedure".

> *But while understandings of others' talk are displayed to co-participants, they are available as well to professional analysts, who are thereby afforded a proof criterion (and a search procedure) for the analysis of what a turn's talk is occupied with. Since it is the parties' understandings of prior turn's talk that is relevant to their construction of next turns, it is* ***their*** *understandings that are wanted for analysis. The display of those understandings in the talk in subsequent turns affords a resource for the analysis of prior turns, and a proof procedure for professional analyses of*

> *prior turns, resources intrinsic to the data themselves. (Sacks et el.: 1974, p. 729 emphasis in original)*

We do not think this suggestion is a throwaway comment nor slip of the pen. They were very clear about what was being suggested and very circumspect about what its remit should be. In their hands the notion of a the turn taking machinery as an analytic description applies only to conversation, though they do, of course, entertain the suggestion it might have its complements in other domains. There is a second aspect to Sacks et al.'s notion of proof which is worth bringing out here — the requirement for correspondence between accounts. The use of correspondence here is not an epistemological one nor, thankfully, a psychological one. Rather it is methodological. Correspondence is needed to be able to choose between competing analytical accounts. Following from the basic postulate and its related axioms, the use of members' next turns in the data provides a carefully controlled way of making the choices required which respects the way the phenomena have been constituted in the first place.

This circumspection and the subtleties on which CA is based entirely disappear when its transcription protocols are uprooted and transplanted simply as a technique for recording data in EM studies. The vocabulary often used is important. Activity has been 'captured' on tape and 'presented' in the transcript. Capturing and presenting assume a constancy of the object captured and presented. The activity performed, the activity-on- the-tape and activity-in-the-transcript are all treated as 'the same' social action but without the equivalents of CA's postulate-axiom derivations. Instead, transcription protocols function as a technology for generating sequentially organised phenomena for analysis. The application of the protocols produces a sequential order to be analysed. The formulation of activity-in-the-transcript is taken as an unproblematic proxy for the streams of activity structures it presents.[13]

Whereas in CA, the mapping between descriptions and the constituted phenomena is to be achieved by a proof procedure, in EM connections between the transcripts as proxies for phenomena and statements about those phenomena use two broad strategies. The first is the colligation of cases, often deploying what today might be termed 'minimal bulk data'. Instances of what is held to be 'the same' phenomenon are drawn from data bases of instances. In this use, something like a locally imputed Law of Large Numbers neutralises the contingency of each case. The second strategy is the detailed description of single or a small number of cases where contextual details are provided to elaborate and secure the analysis. Here direct emphasis is on the analyst's competences, either as participant in the events being described (e.g. Roth) or as member of a relevant membership category associated with the setting, to be able to provide the understandings necessary to bring about the correspondences between activity, tape and transcript. This duality elides the differences between professional and members points of view and, moreover, institutes the primacy of common sense understandings over analytic ones. Members' understandings *analytically construed* are to be judged against members' understandings *commonsensically construed*. This

[13] Let's be careful here. We are not saying that those undertaking analysis of transcripts should be permanently oriented to the reflexive character of the transcript as a generative technology. You cannot use the frame and question the frame. Neither are we saying that any analyses so produced are valueless fictions or fables. We are simply casting an ethnomethodological eye on a social practice which just happens to be present in ethnomethodological and ethnomethodologically informed analyses. Weider and Pratt's (1990) discussion of their own analysis is an exemplar of the sensibility we are pushing for.

argumentational strategy requires members' understandings to speak for themselves simply because they are unproblematically 'available in the data'. And *that* is precisely what produces the puzzle we began with.

What is happening here is a kind of analytic double shuffle. The constitution of the member under the natural attitude is being inserted in place of the constitution of the member under EM's analytic attitude, often by means of the analyst's covert sliding in and out of both guises. The insertion allows the argumentational mapping to be achieved, but only if the way the mapping is presented is that the phenomena can be 'read off' from or 'found' in the data. And doing that is talking empiricist talk.[14]

3. The Mysterious Absence of Additionality

The third trait we wish to draw attention to is an orientation to what we will call *additionality* or, rather, the absence of such an orientation. Now, additionality has become an overused (and misused) term in the assessment of contemporary research, often being interpreted by means of measures of 'impact' which, for sociology at least, look either too stretched to be meaningful or too aspirational to be achievable from where the field is now. We want to set this use of additionality to one side and ask simply how, that is in what ways, we might expect individual studies to make a contribution to the developing discipline of EM. The 'd' pairing is important. At present, EM is a long way from delivering on the ambitious claims Garfinkel made for it. In fact, we would argue quite strongly it is still in its infancy as a working sociology. We would also argue equally strongly it should be conceived as a discipline; that is as a body of pre-suppositions, inferences and *modi operandi*, which while sharing much with sociology is nonetheless distinct from and alternate to it. What the components of the required discipline are to be is still being formulated. Additionality, at least in this discussion, means no more than a measureable (and we do *not* mean only quantifiable) contribution to developing EM as a discipline. In what follows, we sketch some of the objectives or outcomes demonstrating additionality which might be used as design criteria for the formation of studies and, hence, criteria for their assessment.

Forms of Additionality

It would hardly be an abrogation of EM's basic principles to suggest one reading the policies we outlined in section 1 could be as instructions or criteria for ethnomethodological good practice.[15] The purpose of this reading is to underscore a central feature of developing disciplines, probativeness; a feature which Garfinkel felt was not sufficiently emphasised in sociology and which was to be one of the ways EM marked its own distinctiveness. What is meant by this term is simply the achievement of epistemological, ontological and methodological sequentiality. Things get settled and having been settled, they allow other things then to be addressed. Of course this does not mean such matters are immune from revision at a later date with all the consequences which follow. But as bodies of practices, developing disciplines do not constantly rake over the same ground. They display some level of collectively agreed progress. In that respect, a case could be

[14] Lurking here as well is another familiar feature of empiricism, the method of decomposition and its associated countervailing principle, singular description. In contemporary EM transcripts, action is decomposed into parallel streams of talk, gesture, gaze, *materiel* deployment and so forth. Combined with contextual detail concatenated descriptions of these streams constitute performativity; the 'what' of what is going on.

[15] Or as Garfinkel might have put it: grounds for determining the efficacy, appropriateness, application……..

made for considering classic CA to be a 'normal science'. On the other hand, a similar case would be very hard to make for EM.

For EM to become probative, we think two shifts in attitude need to occur. First, priority focus should be placed on identifying and resolving outstanding strategic disciplinary Issues. To judge from the majority of 'meta' or introductory commentaries found in EM studies, you would think its major challenge is that it is barely understood by sociology and other adjacent disciplines. Rather than spelling out the relevance of the work being presented for the central tenets of EM, study after study positions itself by explaining to a supposed audience of non-EM investigators what EM is, why it should be taken seriously and why such readers should pay attention to the upcoming re-specification of whatever topic the study is about. Attention seems to have slipped away from the 'internal' issues that stand in need of resolution and the framing of studies to address them. We are not here thinking about the 'constructivist wars' or deprecations over 'professionalisation', ' normalisation' and the like. Rather it is the empirical, conceptual and methodological barriers and constraints EM is challenged by we are concerned with. Do we have a good sense of what they are? Is there agreement on the list and its rough priority ordering? Do we know what aspects of what social phenomena we aren't able to study and why? What things don't we know about the topics we have studied? What are the forms of connectivity/interfaces/hooks, plugs and ties between particular types of findings and particular orders of study? Do we have a good account of obvious forms of social life other than 'interactivity' (such as 'consociation' and 'association')? What are EM's equivalents of science's theories of measurement and associated measurement systems? What are the analytic connections between data types (such as gesture or the manipulation of objects and talk) as opposed to the transcription relationship of synchrony? In the absence of answers to these and similar questions, what we have is a proliferation of studies rather than their accumulation.

Second, there is the task of anomaly detection: This is the pre-emptive search for emergent issues. Again the focus is on the disciplinary structure. Anomalies arise from limitations to our addressing of current challenges and issues. They indicate points and places where our systems of concepts are fractured or in tension. Within EM, is there a good sense of what and where the major anomalies are likely to be found? Which groups or types of findings are proving difficult to align with others and why? Are studies being framed to investigate these features? For EM to become a probative discipline, effort must be directed both to identifying, clarifying and addressing EM's current internal challenges and to searching for and characterising the anomalies which are likely to arise as it pushes towards being a more and more explicitly articulated account of social life.

All these questions point to the need for strategic choice in the selection and formulation of investigations. In this section, we will describe some types of additionality which might inform such choice and so contribute to the development of EM as a probative discipline. As we have suggested, we will do so by framing them in terms of criteria for the assessment of studies.

Incrementatlity

This is the most obvious form of additionality. Here the interest is in determining just what 'more' a study is offering and how much more it consists in.

1. Scale: This is the minimal form of additionality and is its degree of aggregation. Does the study offer a single additional case of an already familiar phenomenon or does it offer comparative pairs or triplets (or more) of 'the same' phenomenon? For example, are we being offered (yet another) example of students learning to 'see as a scientist' or 'users' working with a computational device? Are we being offered cases of learning to read equations, understand physics formulae, interpret graphs and so on as part of assimilating 'scientific ontologies'. In the end, all studies of singletons and even multiples can do is contribute to the building of a corpus of *point investigations*.
2. Scope: Here we are concerned with extension of coverage or applicability of insights about one area or from one study to hitherto unexamined topics. This is the power of the study. Examples might be of directly basing a studies of 'hearing like a car mechanic' , 'feeling like a vet', or 'playing like an expert' on the insights offered by applications of Goodwin's notion of 'professional vision'.
3. [Research]Objects: By this term we mean the introduction and analysis of novel analytic phenomena. Examples of the kind of thing we are thinking of are Sormani's 'optimal move' in a formatted course of action, Baccus' notions of the 'embodied logic' of tools and the 'lore of materials',[16] Laurier's 'scaffolding of graphs' in a scientific explanation, Schneider and Wagner's '*document representif*' as a digital/material ensemble and Maynard's use of 'narratives' in the analysis of therapeutic diagnosis. Each of these provides a different cast to familiar phenomena though whether they are extensible beyond their original use is, of course, to be determined over time.
4. Domains: these are analytically construed finite provinces of meaning. Contemporary EM is focused on just a few: everyday life under the natural attitude, professions and their sub-worlds (the sciences, medicine, teaching, management), formal organisations and the like. Studies of the theatre and especially the production of dramatic and musical 'performance' (beyond that offered by Sudnow) would be a novel domain, as would those of dreams, religious experience and psychosis. Additionality is not obtained here by re-casting studies in terms of analytic insights derived from already well tilled domains (psychosis in terms of Quantum Physics, exchanges on social media as etiolated conversation), but by providing relevant, domain-derived and specified, precise analytic descriptions. Naturally, the choice over which new finite provinces of meaning might be investigated should turn on the likely contribution such studies might make to developing EM's sociology.

Connectivity

Connectivity is closely related to incrementality. Here, though, the key is the explicitness of the close coupling of *this* study to prior studies in ways that bind them together so, rather than their being disjoint, one can build on the other.

1. Reference: Once again, this is a minimal form. We do not mean a study should simply cite relevant other work but rather it should treat such work as a systematically exploited reference

[16] But not, alas, her identification of the 'treachery of objects', something all too familiar to regular craftsmen, bricoleurs and amateur DIY enthusiasts.

model. An obvious example is the way numerous studies of control rooms and similar settings use Suchman et al's analysis of San Jose's Airport Control Room as a systematic reference model for 'centres of coordination'. In that sense, when done well these secondary studies are modelled reproductions of Suchman's study. Interestingly, as far as we know, no-one has used the seminal study reported in *Plans and Situated Actions* as a reference model for their own investigation.

2. Coordination: Here additionality is in the contribution made by the relationships between findings. If alignment is the minimal form of coordination associated with reference, integration is the form of coordination which occurs when findings are systematically constructed and reconstructed as an unfolding accumulation of outcomes. One clear example of integrative accumulation is Lynch's series of studies of the practices associated with the use of scientific images. Another is Livingston's set of studies of tick-tack-toe, checkers, origami and chess. Of course, the *piece de resistance* of this form of additionality remains Sacks, Schegloff and Jefferson's Turn-Taking paper.

Method

In previous sections, we have spoken of reservations we have about forms of method widely used in the discipline. These reservations concerned shortfalls against both key criteria under this head.

1. Investigative Technique: To repeat our earlier concern. Currently heavy reliance is placed on a combination of 'ethnomethodologically informed ethnography' and recorded (audio/video) data collection. As a result, studies have concentrated on domains and topics which suit such techniques. In contrast, Sudnow's studies of the embodied character of playing jazz piano, draws upon first person experience to frame analytic descriptions as do Weider and Pratt's description of criteria for the recognisability of being a Native American.[17] Other exemplars might be the way Weider himself learnt magic in order to understand and be able to demonstrate the practices of prop arrangement and prestidigitation magicians rely on to manipulate appearances or Stacey Burns' reflection on the practice of law. Somewhat different is the kind of professional 'assimilation' envisaged by Button and Dourish's 'technomethodology. In each case, different and differently interesting questions can be asked simply because the investigations are not chained to the standard techniques.
2. Data Types: The use of audio and video recording has encouraged an almost exclusive emphasis on 'interactivity' as 'captured' or 'represented' by transcription protocols. As a result, the patterns of reasoning which make courses of action distinctive have been pushed into the background. Some studies have used collections of the flotsam and jetsam of daily life (such as occasioned maps, documents, household lists and notices) as indicative data In tandem with transcripts. And, of course, there have been some studies focused entirely on first person experience such as the Schwartz study we mentioned earlier. However, apart from Lindsey Churchill's early work, numbers, quantities and calculations have not been widely exploited as data for studies (other than in the limited ways to be seen in studies of science and the now

[17] This is not, let us hasten to say, anything remotely like the methodological monster of autoethnography.

standard critiques of sociology's reliance on statistical methods). Equally, Garfinkel's own sketches seem to be the only examples of the possible use of data such as traffic flows, or instructions for flat pack assembly. This narrow framing of what is to count as data begs the question whether studies are being framed in ways which systematically underemphasize (or even disattend to) the indigenous 'materials of life' deployed in settings being studied. From passing comments by Garfinkel and Lynch, we understand Schrecker (1980) described how the setup of an experimental bench could be used as data. Could these insights be extended to the diagrams, sketches and other whiteboard scribblings one sees everywhere in science labs? And how might they be 'captured' and 'collected' not as evidence of 'collective', 'collaborative' or 'distributed' cognition but as evidence of the structuring of practical scientific reasoning? The same questions could be asked about the lay-out of an operating theatre, an artist's studio or an engineering shop. In this respect, Greiffenhagen's study of the proof-relevant spatial organization of blackboard writing introduces a novel data type and offers pointers for other possible data types.

Additionality of method, then, is the expansion of EM boundaries with regard to its resolution of the problematic possibilities of investigation, where, when these limitations are overcome, distinctive phenomena of social organisation are made observable.

Generality

Incrementality, connectivity and method are in service of the search for additionality with regard to generality. This is *not* generalisation in the usual sense of universal or representative 'propositions'. Instead it refers to the emerging conceptual structure (or, to use a leading but helpful metaphor, the emerging conceptual anatomy) which a developing discipline acquires. It is this structure which is the discipline's centripetal force, holding it together by configuring and calibrating the compendium of studies, findings and results which have been generated. As the structure is extended and elaborated, emphasis shifts away from the additionality of studies toward the conceptual incrementality and connectivity being achieved through the refining of concepts and the sharpening of their relationships.

1. Conceptual Innovation: The initial conceptual repertoire used to frame studies was set out in Garfinkel's original formulations and the ground breaking studies which enacted them. It includes indexicality, reflexivity, recipient design, repecification, and some of Garfinkel's other "strange phrases". However, innovation beyond these has been extremely limited. Other than Lynch's introduction of 'aesthetics' as a device for constructing his account of the modes of choice which scientists use when arranging images and his proposal of 'epistopics' and 'ontography' as foci of analysis, it is hard to recall a novel concept which extends or elaborates EM's gaze rather than functioning as a distinctive empirical type for the organization of phenomena revealed by an investigation (as 'centres of coordination', 'lebenswelt pairs' and 'convict code' do). Garfinkel himself recommended 'misreading' philosophers and others as a mechanism for conceptual innovation. What, we wonder, might be gained by misreading Bergson on *la duree* or *attention a la vie* or Weik on sense making as commitment and manipulation. On the other hand, given the interest in modern technology, perhaps more or more sophisticated use might be made of Heidegger's concepts of the 'ready to hand',

'enframing' and 'standing reserve'. Most intriguingly of all, there is Freud on the psychopathology of everyday life. As with innovative research objects, not all of these will necessarily be immediately serviceable nor stand the test of time. But this is always a risk in a developing discipline.

2. Novel Forms of Idealisation: these are bundles of different chains or other collections of abstractions used to frame programmes of investigations. As heuristics, they often take the form of metaphoric homologies, classifications, orders of types and similar analytic structures. As we saw in section 1, the notion of 'work' is the predominant heuristic for studies of the detail of courses of action. Bittner's typology of the uses of 'organisation' is also often used to frame studies of other aspects of formal organisations. Although of late studies of science have been much taken with 'ontologies' and 'taxonomies' as research objects and as grounds for debate, as instances of EM conceptual structures both are hardly in evidence. This is even more so for abstractions such as models, simulations and other formal and semi-formal descriptions, except (once again) for CA's elaboration of the turn taking system for conversation.

Providing additionality with regard to an evolving conceptual structure is exceptionally difficult and so any success is to be applauded and its relative rarity to be expected. But being hard does not mean it should not be attempted or that lack of success is outright failure. There is much to be learned from the iterations of trial and error.

4. Re-appraisal and Re-orientation

For better or for ill, EM has often been cast (or cast itself) in the role disciplinary critic, seeing its mission (or part of its mission at any rate) as the correction of fallacies, errors and infelicities perpetrated by other disciplines — notably sociology, but also cognitive science, psychology and even, here and there, philosophy and economics. We think the time has come for EM to relax the imperative to churn out studies in order to enable reflection on the really quite numerous collection which has already been generated. And what their analytic import might be. In so doing, it could turn its critical acumen on itself and to be less forgiving about the health, vibrancy or robustness of its own work as a convincing demonstration of the possibility of what Harvey Sacks called a "naturally observable social science". Turning a critical eye and being less forgiving do not mean 'shock and awe' tactics, internecine battles and scorched earth strategies.[18] There is much to build on; but there is also much more building to be done. In this discussion, we have touched on just three elements, or traits as we called them, the ones we felt were most obvious and most concerning. But there are others, some stylistic and some institutional. No matter where a start is made, the purpose of re-appraisal and re-orientation is the provision of a strategic framework within which programmes of suitably marshalled tactical investigations and their related studies can be sequentially positioned to provide the optimal possibility of systematic disciplinary development. In the absence of such a framework and the orderly constellations of programmes of work which populate it, individual studies will remain as they are now, opportunistic point contributions.

[18] Even if these have tended to be EM's preferred modes.

Since we have identified three broad bundles of concern, it seems right that we should offer proposals for how they might be addressed within the compass of an emerging strategic frame. Each will necessarily have its own defining 'problematics' and each should be shaped to achieve maximum symmetry within the scheme as a whole, a symmetry which might be preserved by formulating the questions raised with the prefix "Given EM's central principles and policies...."

First and most obvious is the topography and topology of the research space EM inhabits. We have pointed out how skewed the distribution is, with an overwhelming emphasis on interactivity in a few domains. Other interests such as the construction of objectivity, the management of identity, the characterisation of experience and, most surprisingly, the place to be allocated to language as a constituent feature of all these things, seem to have been forgotten. Once we have a sense of the overall mapping of the space, we can ask about its topology, the logical principles which link studies and groups of studies together. Here leading questions will be about are the appropriate strategies of comparison or colligation across the findings offered by studies in contiguous or associated fields and the portfolios of techniques (mensuration, calibration, concatenation) we can call on. Whereas the topography of research is necessarily simply a catalogue of findings, its topology may well be organised as a taxonomy. Such a taxonomic exercise will reveal just where the gaps in the literature might be and where initial or constraining anomalies are to be found.

The second concerns method and in particular how to conceive the embeddedness of members' methods of enquiry, be they those of therapy, policing, sociology or whatever, in the activities which are entrain. Here an initial guiding question might pick up what are or should be the relationships between data, evidence and findings and how these relationships are to be secured. How does the deployment of the documentary method, the method of incongruity as well as devices such as *factum valet*, *ceteris paribus* and the *etcetera. clause* in a research setting relate to their use in the analytic description of that setting? And, by extension, what implications do the answers to those questions have for the alignment, coordination and integration of findings?

The third proposal centres on the organisation of the conceptual space. We have asked about conceptual innovation through mis-readings of other lines of investigation of the phenomenal world, but the most pressing question is clearly the form that EM's conceptual architecture currently takes and its likely resilience in the face of intensive effort to extend and elaborate it.

From EM's disciplinary perspective, these three seem to us to be the matters most deserving of sustained and thoughtful attention. However, there is another which since it concerns the sociological character of EM might well be included too. In section 1, we placed a lot of emphasis on triangulation with sociology as the central thread of continuity in Garfinkel's formulations of EM. The inconclusive flirting with hybrid disciplines notwithstanding, this remained a constant in his thinking. Many contemporary EM studies, on the other hand, are designed to investigate issues or even 'problems' more usually associated with disciplines other than sociology. This tide of 'applied' work, often in partnership those disciplines, seems to be flowing and is usually justified by the 'interestingness' or social importance of the problems being addressed and the pragmatic need for funding and demonstration of impact. All of which this collaborative work is felt to provide. As the volume of this work grows, the following matters become more and more strategically relevant. Again they are to be prefaced the same way as the first three. What are the methodological principles which facilitate

or prohibit EM's triangulation on the idealisations which form the equivalent for other disciplines of its own re-specifications of the praxeological rule in sociology? Do or can these principles allow a straightforward exporting of EM findings and descriptions into those disciplines? Or is the parallel with sociology a general one with EM consistently constituting itself as asymmetric to and incommensurable with any discipline whose topics its takes up? What do answers to these questions portend for the strategy of seriously adopting research policies such as multi-modality, inter-disciplinarity and hybridity?[19]

Quite rightly, most overviews of EM point out it steadfastly sets its face against plying (sociological) theories. Sometimes they even say (quite rightly) it must do so since its concerns are apodictic to those theories. The questions we have posed are not 'theoretical' in the sense meant by this steadfast rejection. Instead they are broadly methodological and EM is nothing if not rooted in methodological ruminations and extended reflections on the consequences of choices made on their basis. It was just such ruminations which originally provoked Garfinkel. All we are suggesting is that as part of the re-appraisal and re-orientation we are calling for, EM should revisit its own founding principles if it wishes to engage in collaborative, even interdisciplinary, investigations. We think it will need to, if for no other reason than to reassure itself they are still serviceable for that purpose.

[19] The qualification is an important one. Of course EM can *play* at adopting these policies. But, as in the rest of life, such subterfuge is dangerous. Continual failure to deliver will likely lead to reputational damage if not worse.

EPILOGUE

We started by saying writing this piece was dispiriting. We didn't say it made us feel the situation was hopeless. We feel EM is still to be fully explored and its value as a First Sociology fully documented and assayed. Moreover, we believe the best of what it does demonstrates this is so. The trouble is the best is in danger of being drowned out by the mediocre. To counter this, we have proposed EM undertakes a reorientation; that it focuses on its own challenges, seeks to address them and does so with a clear view as to what making progress will look like and how it might be organised. For ourselves, we hope to devote some of our future efforts to these ends. We know this will be a bracing and painstaking exercise. But then being bracing and painstaking is just what EM is best at.

REFERENCES

Garfinkel, H.	1967	*Studies in Ethnomethodology*. Prentice Hall. Englewood Cliffs.
Garfinkel, H.	2002	*Ethnomethodology's Program*. New York: Roman and Littlefield
Manders, K.	2008	'The Euclidean Diagram' in P. Macosu (ed) *The Philosophy of Mathematical Practice*. Oxford. OUP, pp 80 – 133.
Roth, W.M.	2012	'The work of seeing mathematically' in S. Makhopadhay & W.M. Roth (eds) *Alternative Forms of Knowing (in) Mathematics*. Rotterdam, Sense Publishers, pp 227 - 245
Roth, W.M.	2015	'On meaning and mental representation'. *New Directions in Mathematics and Science Education* vol 28. Rotterdam. Sense Publishers, pp169 -186
Sacks, H, Schegloff, E.A and Jefferson, G.	1974	'A Simplest Systematics for the Organisation of Turn-Taking for Conversation.' *Language, vol 50 pp* 696-735.
Schegloff, E.A.	1979	'Identification and recognition in telephone conversation openings' in *Everyday Language: Studies in Ethnomethodology*, by George Psathas (ed), 23 - 78. New York: Irvington.
Schrecker, F.	1980	'Doing a Chemistry Experiment; the practices of chemistry students in a student laboratory in quantitative analysis.' Unpublished paper, Dept of Sociology, UCLA.
Weider, D.L. and Pratt, S.	1990	'On the occasioned and situated character of members' questions and answers' in D. Carbaugh (ed) *Cultural Communication and Intercultural Contact.* New York, Psychology Press, pp 65 - 75