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The Possibility of Hybridity

INTRODUCTION

Garfinkel talks in his *nachlass* of there being two Ethnomethodologies. In fact, his comments are about two Ethnomethodologies of the natural sciences, but the point is a general one. One is the kind which most of us have done and continue to do. Garfinkel calls this “analytic ethnography”. The other is a form which few if any of us can do and which he calls “hybrid* Ethnomethodology” (hybrid*EM). For Garfinkel, the distinction turns on whether the findings of an investigation are couched as “inside-with” descriptions which ensures their results can be “taken seriously” by practitioners in the domain under investigation. By “taken seriously”, he means incorporated either in formulations of how it sees the world of its endeavors or in how it acts on that world within the praxis of the relevant domain. What “inside-with” designates is more complicated and has to do with overcoming the observer/observed, insider/outsider dualities and, with them, the counterposing of category related accounts. Providing inside-with descriptions requires the observer/investigator to be both competent practitioner in the setting and hence able engage with it in the same ways practitioners do and, at the same time, able to reflect sociologically on the taken for granted ethno-methods which practitioners deploy. Inside-with descriptions are the joint product of both points of view.

In this commentary, we ask about the reality of the analytic ethnography/hybrid* EM distinction. By “reality”, we mean two things. First, does the distinction catch an empirically real distinction? Has it been realised and displayed in actual cases? We will suggest there are scant grounds to suggest it has and some significant ones to suppose it hasn’t. Despite what Garfinkel

says in the draft just mentioned and elsewhere, the evidence for the existence of these two types of investigation is just not there.¹ This does not mean the term does not mark a possible distinction, only that the distinction has not been demonstrated through actual cases. The second sense of “reality” refers to conceptual coherence, a property which is required for it to be a distinction which can possibly be deployed. We will propose it could be a possible distinction, but pursuing it is likely to take EM a long way from where it is now and demand much more of it than it seems willing to give.

We begin from what is admittedly a debateable premise. The subtitle to *Ethnomethodology's Program* [Garfinkel 2002] is not a piece of intradisciplinary marketing, but a serious claim about Ethnomethodology's (EM) central achievement. EM has provided demonstrations of Durkheim's aphorism “the objective reality of social facts is sociology's most fundamental phenomenon”.² If EM has shown how to access and describe the objective reality of Durkheimian social facts, then it is because Garfinkel got lucky. In 1947, when he began work on what would eventually lead to his thesis and the beginnings of EM, there was no available way to get from where he was then to Durkheim as he wanted to interpret him during the period after 1970, when the work presented in *Ethnomethodology's Program* was largely carried out. He had to get to that position via Parsons, something he didn't know when he joined Harvard to pursue his PhD. In fact, he could only have got there via Parsons—so proving the Irishman's dictum both true and false. In demonstrating the lacuna at the heart of Parson's solution to the problem of order, Garfinkel found EM's ‘method’. Having found that method, he undertook the programme of investigations which became Ethnomethodology.

Garfinkel's luck ran out when, using the wholly new conception of EM as a ‘hybridising’ discipline, he tried to extend its reach to cover the natural sciences. He couldn't bring off the same ‘coup’ regarding theory and description in the natural sciences, and Physics in particular, he had with Sociology. As a result, his own study of Galileo's demonstration of the Law of Free Fall was unsuccessful. As we suggested in our discussion of that experiment, this was because Garfinkel had failed to appreciate both the extent to which Physics after Galileo is itself a hybrid and its reliance on modalities of mathematisation. At its core is the development of mathematical theorems and

¹ We are aware of the implication of this conclusion for how we should read Garfinkel's later writings. We will not dwell on it here.

² This is Anne Rawls' paraphrase of one of Durkheim's propositions. We won't ask which, though it probably does matter.

related proofs. What Physics does is demonstrate those theorems in experimental set-ups. In developing its theorems and proofs, Physics applies mathematics to physical phenomena. The mathematics drives the demonstrations. If EM wants to intervene in the praxis of Physics in the ways that Garfinkel describes, it has got to show how its findings impact the mathematics—the theorems and proofs. It has got to reach through the data to the mathematically formulated phenomena. This he failed to do.

Section 1. Foundations

To appreciate what is at issue in the distinction Garfinkel makes, we need to remind ourselves of how it all began: his Doctoral Thesis. These days, if this is mentioned at all, it is usually identified as a critique of Parsons' Structural Functionalism. Like most soundbites, this summary is both partly right and mostly wrong; somewhat helpful and extraordinarily misleading. The thesis certainly does have the identification of an omission (or oversight) in Parsons' standard account at its heart. Moreover, it does make it clear the gap in Parsons' argument leads to some irresolvable problems with its viability as the general sociological theory of the nature of social order.³ But what is often missed is that these problems only become apparent and can only be grappled with if one commits oneself to a single-minded pursuit of Parsons' goal, to the frame of reference he sets out and to its premises. Garfinkel does this and then *pushes them to the limit*. What the thesis says is that there is nothing out of the way in Parsons' premise as a *premise* for a theory of social order. The phenomenology of our experience of social life is orderly.⁴ There is nothing wrong with the 'what' of Parsons' account. The difficulties lie in using the theory to describe the *how*. These only become visible if the account is treated as a detailed prescription for the workings of social order in any and all circumstances rather than an albeit detailed theoretical gloss of its general character. This is hardly critique in the normal sociological sense of "problematization". It is, rather, an exercise in rigorous adherence to the terms of a conceptual structure; an exercise which revealed a previously unremarked and monumental lacuna in the argument.

³ The thing to bear in mind at this point is that in North America during the early 1950s, there really was only one general theory which anyone could take seriously. In his *Theory of Action*, Parsons had hammered critical bits and pieces of 19th and early 20th century social thinking together as a possible sociological solution to the problem of order. In *The Social System*, he went on to integrate that solution into the tenets of system theory. No other theory at the time (or after) matched the scope, systematicity and precision which Parsons aimed for and achieved.

⁴ As Harvey Sacks once said: "There is order at all points".

Furthermore, EM did not leap, Athena-like, from Garfinkel's brow in the midst of his thesis. The work he completed left him with the conviction radical adjustments were needed in the premises of standard professional Sociology. What those adjustments should be and what their implications might entail had yet to be worked out. As he scoped and investigated the issues, he became convinced no amendments to Structural Functionalism nor even its reconstitution would suffice. A wholly new and radical methodology was required.⁵ Creating that methodology was his lifelong project.

PARSONS' LACUNA

Garfinkel arrived at Harvard already convinced Sociology's central problem was 'the problem of meaning'. By this he meant not a technical challenge in the framing of investigative protocols but how to describe the intersubjective coordination of definitions of the situation fundamental to sociality. His thesis title, *The Perception of the Other*, homes in on this problem by addressing Parsons' phenomenology. The idea Parsons had a phenomenology might seem a bit surprising but, in fact, he had two. Following the premises of his theory, Parsons treats the experience of the social actor from the point of view of the total social system and its homeostasis. The social actor is Parson's *homunculi* fashioned within his frame of reference. How actors experience the social is the product of patterns of management processes responding to systemically experienced tensions. These responses ensure adjustments in normative structures while related value orientations act as feedback mechanisms sustaining continued motivated compliance and hence the orderly experience of social life. Components of the cultural system shift and re-balance, thereby allowing order to be preserved. Putting it briefly, the phenomenology of the social actor's experience of social orderliness is a functional consequence of the phenomenology of the system's experience of its own inconsistency. These inconsistencies generate threats to its coherence and hence orderliness. As with any dynamic process, there is always a degree of 'play' in the mechanisms maintaining the process. Such 'play' appears as differences in commitments, interests, relevances and the like and are expressed as differential orders of motivated compliance.

Garfinkel's crucial insight concerned how time features in these two phenomenologies. At the level of the social actor, time is marked as the flow of routine courses of action. The "tensions" to which variation in motivation gives rise, are encountered, managed, and resolved on the

⁵ Note 'methodology here does not mean investigative technique but the whole apparatus required to construct and then investigate specified small worlds. See Garfinkel's discussion of Felix Kaufmann [Garfinkel 2006; Garfinkel 1967] and [Anderson and Sharrock 2019].

temporal scale of the flow of intersubjective action. At the level of the system, adjustments take place in what Garfinkel calls a "fat moment" of cultural evolution and social change.⁶ The gap in Parsons' account is the lack of specified, theorised structures for managing the integration of the temporal orders.

To bring out the import of this gap, Garfinkel constructed a slightly different localised small world. It provides a frame of reference for 'objects' such as actors, roles, identities and so forth defined in terms of Parsons' theoretical schema. This small world has just two dimensions; time as an experienced succession of courses of action and normative orientation as degree of motivated compliance. From the beginning, Parsons had insisted the characteristic of social action was that it was meaningful. This required a solution to the interpretive double contingency of definitions of the situation held by actors within the small world frame of reference of social action. To bridge the gap in Parsons' theory, Garfinkel introduces themes drawn from Husserl's phenomenology as interpreted by Alfred Schutz and Aron Gurwitsch. An understanding or interpretation of action is possible because of the intersubjective sharing of finite provinces of meaning. Shared interpretation is provided by agreement on the 'relevance' of definitions, values and norms associated with a finite province of meaning. Parsons' conception of motivated compliance is treated as an expression of this agreement. Garfinkel operationalises this conception by defining compliance as ranging to the limits of complete (programmed) compliance (the default position in Parsons' theory with the actors as "cultural dopes") and complete absence of compliance (where at least one actor is 'fully anomic'). His question is: 'What happens to the theory's empirical fit if you push the frame of reference to the limits by using a combination of full compliance and full anomie?'

To investigate this question, Garfinkel formalised his frame of reference and then derived and proved a theorem. The theorem proposed that in the absence of shared motivated compliance to the normativity of a finite province of meaning, fully compliant actors will be unable to find the other's actions meaningful. The double contingency and hence Sociology's problem of meaning, cannot be resolved. Instead, the compliant actor will see the actions of the other as "specifically

⁶ 'Moment' here is a technical term. It refers to the smallest possible unit of time in the frame of reference's operations. In the same way, the "social actor" is the primary unit of action in the system in much the same way as the particle is the primary unity of mass in Classical Physics. The frame of reference constructs a small world of defined primitive objects.

senseless". Using a later terminology, under such circumstances the compliant actor will fail to sense assemble the joint activity.

The issue for Garfinkel was not whether he could derive and prove his theorem from the principles on which he had built his small world but whether he could show how the conditions set out by the theorem can be reproduced in actual courses of social action by using a setup which approximates as closely possible to his envisaged scenario. If his theorem holds up under circumstances even loosely akin to naturally occurring behaviour, Garfinkel will have demonstrated the import of the lacuna in Parsons' scheme. The theory is incomplete.

The demonstration Garfinkel ran was the first of what later became known as his "breaching experiments". The setup involved playing an interview of a boorish applicant to medical school to an experimental subject. Having formed an opinion of the student, the subject was given further information which subverted the evidence provided in the interview on which the opinion was based. The aim was to promote "maximum incongruity" between the two accounts of what was happening. The rationale was to push the subject to re-organise their account of the interview beyond the point where further adaptation and re-organisation is reasonably possible. At that point, it was expected the subject would claim not to be able to make sense of what was going on in the interview. However, that didn't happen. In every case, the subjects used all sorts of modifying, ameliorating and explanatory stratagems to try to remedy the apparent "senselessness" of what they were being told. The experiment did not demonstrate the theorem.

For Garfinkel, this did not imply Parsons' theory was complete. Neither did it suggest we should jettison the problem of meaning and its relation to social order as the central analytic topic for Sociology. Rather, the whole methodology underpinning the conceptualisation and investigation of the social should be re-built. Relying on Whitehead's metaphysics, Parsons had maintained a distinction between concrete activity and theorised social action. The latter was the former construed under the auspices of sociological theory. To find order in the concrete, we had to theorise it. Its orderliness was revealed in our theoretical descriptions. Garfinkel took his 'demonstration' to show there was an endogenous orderliness in the concrete which was not captured by Parsons' theory. Note, and this is important, he did not conclude we should not 'theorise' sociologically about it. It was how a particular theory and its categories were shaped and used in investigations which was at issue not the use of theories and categories as such. Throughout the rest of his career, wrangling theoretical categories and types was central to his thinking about how to investigate the social, including the social character of the natural sciences.

FORMULATING THE METHODOLOGY

After Harvard, Garfinkel continued this interest in how the coordination of understandings is achieved. Using Schutz's conceptualisation of the rationalities of common sense and science, he explored the presuppositions of intersubjectivity under the heading of "trust" in relation to both the reality of how things appear as well as the repeatability of action. The concern with "rationalities" in the organisation of the resolution of meaning and the achievement of reciprocal understanding was explored in studies of decision making by jurors as well as by those charged to code organisational records.

A key shift occurred in the way he framed his studies when he began to use concepts from Phenomenological Psychology in the same way he had used Schutz and Parsons. Drawing first upon the analyses of Aron Gurwitsch and later of Maurice Merleau-Ponty, he began to explore the processes of configuring or registering the details of context as a means of coordinated definition of the situation. Using optical illusions such as the famous 'Duck/Rabbit' drawings, Phenomenological Psychology had emphasised the different configurations of what Gurwitsch called "fields of consciousness". Pointing to how the zonation of attention to the detail of the field was shaped by interests and relevances, Gurwitsch analysed how fields of consciousness were reconfigured during courses of action.

Merleau-Ponty brought a different emphasis. For him, it was 'sensation' which was the central concept for the analysis of experience and how particularly sensations were experienced by embodied beings immersed in a world of sensation. *The Phenomenology of Perception* [Merleau-Ponty 1962] plays a major role in Garfinkel's thinking at this point, as can be appreciated from the repeated references to Merleau-Ponty's organising conception, 'the phenomenal field', as well as the broadening of analytic scope to sensorily engaged embodied perception. Using an image which Garfinkel borrowed, Merleau-Ponty states his problem as the description of how we extract the animal from the foliage by means of sensorily engaged embodied perception when constituting a phenomenal field.⁷

An important text for Merleau-Ponty and one which must have been important for Garfinkel too (although he nowhere refers to it) is Husserl's *The Origin of Geometry* [Husserl 1970]. 'Origin' here is not simply intended historically. It refers to the 'foundation' of Geometry in spatial experience. Husserl calls his 'depth analysis' of the sensorial foundations of Geometry in

⁷ This image refers to the 'task' set in a widely known children's puzzle of the time.

our management of objects in space an "archaeology" of Geometry's "sense origin". This term was picked up and re-purposed by Merleau-Ponty's close friend, Foucault. For Husserl, Geometry and all the empirical sciences which rely on it, are premised on our immersion in spatiality. Merleau-Ponty suggests Husserl came to 'limits of Phenomenology' as he had conceived it with this analysis because central to his approach was an essentially intellectualised conception. Husserl turned to Geometry as an intrapersonal problem with the perceiving subject, the Ego, "riding in the car" of the body (as he put it in the fragment subtitled *The Originary Ark* [Husserl 1981]). This notion of the subjective being of perception separated or distinct from the objective being in the world creates a zone or boundary between them along which Geometry originated. For Merleau-Ponty, it was necessary to step over that boundary and embrace a position where, in that sense world, subject and object were an 'intertwined chiasm'. Garfinkel balked at this.

In the investigations included in *Studies*, Garfinkel adopted an approach of 'misreading' the categories and frameworks proposed by the philosophies just summarised and applied those 'misreadings' to various social settings. In doing so, he was able to point how the detailed management of the flow of action involved the use of procedures which, in a joint paper with Harvey Sacks [Garfinkel and Sacks 1970], he would identify as "the formal structures of practical action". These consist of context free formats for the sequencing of action available to be used in context dependent circumstances. What he called the 'recognisability' and 'accountability' of these formal structures provided for the constitution of our experience of social order. The class of formal structures are 'social objects' used to sense assemble such 'social facts' as the operation of turn taking in conversation, queues and traffic, the deployment of systems of extensible categories and related actions (normal crimes, routine troubles, etcetera clauses). This sense assembly was the achievement of the identification of actions and actors as well as the provision of locally relevant descriptions via generally applicable formats (storytelling and glosses).

At the time of the publication of *Studies*, the position adopted by EM might be summarised as follows.

1. Professional Sociology studies the constitution and dynamics of social order. It does so by applying theoretically deduced propositions ('theorems') concerning resolutions of 'the problem of meaning' to 'concrete' courses of action.
2. Investigations of the application of Sociology's central tenets pushed them to their limits and so revealed:

- a. The dominant theory is incomplete as an account of the production of social order through the resolution of the problem of meaning. There is 'a gap in the texts'.
 - b. Examination of 'concrete' courses of action revealed the existence of a domain of endogenous analytic practise (ethno-methods) in daily life which had hitherto been overlooked. These provide for the accountability of actions within the course of their flow and hence solve the problem of meaning. It is these methods which provide the solution to the problem of social order.
3. Professional Sociology relies on members of society and their cultural objects (either as informants or as exhibits) to provide its entrée to concrete social action. As a consequence, it relies on the use by social actors of ethno-methods for the successful deployment of its investigative protocols. Professional sociological descriptions are built on and from contextually generated and locally ordered sociological methods. This result is important for professional Sociology's ambitions to be rigorous and probative.
 4. The implication of the findings described in 3 is that the practise of professional Sociology must be treated from within the same analytic frame of reference as the rest of social life. Its practices and settings are as available for EM analyses as any other.
 5. EM motivates its own studies by 'misreading' professional Sociology's descriptions of social order and treats them as providing protocols for the investigation the achievement of social order in some designated setting (including within Sociology itself) and then pointing to 'a gap in the texts' which can only be closed by the endogenous ethno-methods deployed in the setting.

In the above positioning, EM is defined as an 'alternate sociology'. This is key. There is an interdependency or possible interdependency between EM and professional Sociology. EM draws its topics from professional Sociology. Its disclosure of the grounding of Sociology's methods of investigation and analysis was made possible because EM investigators were professional sociologists *and* culturally competent members. They could "see" how the orderliness of social life was made accountable both in the theory and in the concrete. Analyses juxtaposed these points of view. This interdependency was extended to other disciplines when EM turned to settings where similar 'sociological' accounts of phenomena are used as part of analytically dominant modes of theorising (Management Science and executive decision making, Medicine and diagnostic

routines, Jurisprudence and legal trials etc.). In what became Garfinkel's master descriptive trope, these studies were revealing the "shopfloor work" of the setting; the actual production of the phenomenon of social order for which the theory provided an abstract design.

Section 2. Hybrid Ethnomethodology

For reasons which are not altogether clear, from the late 1970s onwards Garfinkel began to take more and more interest in the natural sciences.⁸ This interest engendered a number of projects. A study of introductory lectures to Chemistry was mounted in collaboration with David Sudnow. This study is included in *Ethnomethodology's Program*. There was a study of the discovery of the optical pulsar with Mike Lynch and Eric Livingston [Garfinkel et al. 1981] which was separately published. Alongside these, Garfinkel undertook some informal prospecting by 'hanging around' science labs as well as talking to scientists and historians of science. In addition, for a while he took a lively interest in Kuhn's analysis of theory and theory change in the sciences.⁹

THE PROPOSAL

The 1988 summary released by Lynch [Garfinkel 2022] identifies three broad theses arising from the exercises just mentioned.

1. The natural sciences are 'discovering' disciplines. This is because they share two complementary characteristics:
 - a. They can 'lose their phenomenon'. In other words, changes or differences in the constitutions of the phenomenal field can cause investigators to fail to be able to extract the animal from the foliage. As scientists sometimes have it: nature is not always cooperative.
 - b. They are "probative". The possibility of losing the phenomenon implies the possibility of not being able to recover the phenomenon and extract it from its foliage. However, it is a (social) fact scientists routinely do agree

⁸ We could hazard any number of guesses. See [Greiffenhagen and Sharrock 2019] for some suggestions. Hopefully, as more of the unpublished materials are examined, the considerations motivating Garfinkel will become apparent. Garfinkel rationalises it simply as the pursuit of the bases of Galilean Science, which it is. But this could have been undertaken in numerous ways.

⁹ We offer some comments on his interest here in Essay 12..

on the identity-for-all-practical-purposes (or not) of results and hence the demonstration of the theorems under test. In that sense, questions can be resolved and things settled (at least for now).

2. Because of the relationship between 1a. and 1b. above, contingencies matter. This makes identification of the range and import of contextually relevant contingencies as they are available and displayed in the praxis of the science EM's paramount investigative task in a study of a natural science. It was this aspect where Garfinkel felt his exercises had been inadequate. Hanging around the labs and asking "coathanger" questions of scientists made no essential difference to his and his colleagues' ability to learn, see and follow the science-work in hand in the ways the scientists did.

The aim of our research was via discussions with bench scientists to ask for and get from them an explicit explanation of the coat hangers. Remember, we didn't know and we wouldn't know to see for ourselves what we were asking the scientists do describe for us, in detail. Given the foregoing particulars, and in their light, we pose the question: In any actual case of discovering work in a natural science, just what can be settled by "saying so?" [Garfinkel 2022, p. 39]

In Garfinkel's view, two modes of EM study of the natural sciences we mentioned at the beginning of this discussion are possible. The first is exemplified by Lynch's work on Micro-biology; the second by Livingston's work on Mathematics. What marks them apart is the 'seriousness*' with which their findings can be taken by the scientists they study. This difference arises because Lynch is not competent in the Micro-biology he describes, whereas Livingston is competent in Mathematics. Crudely, Lynch gives an outside-in description and Livingston gives what Garfinkel calls an "inside-with" one.¹⁰ What escapes Lynch is the interior configuration of the phenomenal field of the practical science of Micro-biology as that is oriented to and worked with by the scientists he studies. This phenomenal field is the shopfloor organisation of scientific contingencies.

Taking an investigation of the practise of Physics as an example, this summary and the detailed discussion on which it is based seems to imply the following positioning analogous to that for early EM provided just now.

¹⁰ We return to this term in depth in later.

1. Professional Physics studies the constitution and dynamics of material order. It does so by utilising theoretically deduced propositions ('theorems') about the interaction of forces and particles as descriptions of 'concrete' examples of the actions of objects.
2. If EM investigations of the application of Physics' central tenets mount investigations which test them to their limits:
 - a. The dominant theories will be shown to be incomplete as an account of the production of material order. There will be 'a gap in the texts'.
 - b. Examination of 'concrete' cases of studies of objects and their actions will reveal the existence of a domain of analytic practise (endogenous physics-methods) which has hitherto been overlooked. These endogenous physics-methods provide how Physics provides for the continuous coordination of matter in motion. These methods thus provide a solution to the problem of material order.
3. Since professional Physics uses the exhibited properties of material objects as its entrée to the study of concrete material, it relies on the yet to be identified endogenous physics-methods for the successful deployment of its investigative protocols. Professional Physics accounts are built on and from endogenous physics-methods. This result should be of signal importance to professional Physics' achievement of rigour and probativeness.
4. The implication of producing the findings described is a requirement to place the practise of professional Physics within the same analytic frame of reference as the rest of the material order.
5. EM could motivate studies endogenous physics-methods by 'misreading' professional Physics' descriptions of material order as providing protocols for the investigation of the production of material order in some designated context. It could then point to an inevitable 'gap in the texts' which can only be closed by revealing the endogenous-physics-methods being deployed there.

Any adequate EM study formed under this positioning should be characterised by a requirement and two conditions. The investigators must be competent in the Physics which they study. This is the requirement. The conditions are:

1. The findings will be rendered as "inside-with" EM/Physics descriptions.

2. These descriptions will be construed as instructed action so they can be incorporated in the praxis of the relevant Physics.

EM's strategy of pushing professional Sociology to its limits was achieved by asking *how* the problem of meaning was resolved in the theory, since resolution of the problem of meaning as a solution to the double contingency was the condition of social order in Parsons' theory. This solution allowed for the coordination of social action. EM studies pointed out such coordination was achieved *within* the flow of action. Pushing professional Physics to its limits can be achieved by using the same strategy. The challenge is to find Physics' version of the problem of meaning.¹¹

Section 3. The Scarcity of Existence Proofs

The core objectives of hybrid*-EM are practitioner competence, inside-with descriptions and transferred findings as instructed action. In our discussion of the inclined plane experiment, we pointed out Garfinkel was unsuccessful on all three fronts. In our view no other ethnomethodological investigation of the sciences or any other domain has been successful either. To understand why, we have to understand what would be required to deliver the set of objectives as an integrated and coherent course of practical investigative action. Because these objectives are an integrated triplet, such analysis will involve a great deal of back and forth between them. But, as the whole rationale of hybrid*-EM seems to turn on the transfer of findings, it might be as well to start with that.¹²

INSTRUCTED ACTION

'Instructed Action' is a term of art in EM and is used in three closely related ways.¹³

1. As a general analytic category for moves in a joint course of action, be it co-present interaction or action at a distance. An actor designs an action (asking a question,

¹¹ It is important to recognise Garfinkel does not mean simply describing what Physicists do on a day-to-day basis in their labs to ensure their experiments work and that such working conforms to standard disciplinary expectations. These practical embodied and other skills are relevant *in* the practise of Physics but not *as* the Physics itself. Identifying their use in Classical Mechanics is talking about Physics not talking Physics and hence takes the form of analytic ethnography. See [Lieberman 2007, p.4] for this distinction.

¹² We will talk indiscriminately of findings and results. We recognise they are not necessarily the same. However, since EM does not place any weight on their difference, neither will we.

¹³ See [Lynch and Lindwall 2024] for an array of studies of various ways the term might be employed analytically.

writing a memorandum, presenting a purchase at a till) such that the action produced either initiates an action sequence or responds appropriately to the Other's prior action and so frames what the Other's next action should be. Conceiving of the flow of activity as sequences of appropriately structured instructed action provides a mechanism for the coordination of definitions of the situation and configuration of phenomenal fields.

2. As a ploy for designing investigations. An investigator chooses to 'misread' a sociological, philosophical or other theoretical account of some phenomenon as instructed action for its investigation. This 'misreading' generates the necessary "gap in the texts" on which EM analyses rely.
3. As a design feature for the findings of hybrid* EM. To be transferred into the praxis of a discipline, findings have to be (co-)constructed to be followable by recipients as instructed action for how to deploy the recommendations contained in the findings.

Notice the different role reflexivity plays in these uses. In 1. and 2., the means of sequencing actions is reflexive on the in-situ shared local history and context. Thus, it is a purely *analytic* conception and hence grounded in the principles of the disciplinary outlook of EM. In the third usage, it is a deployment issue and shaped by the requirements of achieving practical success. Its grounding, therefore, reaches beyond EM to the recipient professional practise. This is where the audience question (which, as we will see, is raised by Meyer's [Meyer 1991] concept of inside-with description) appears. If recommendations based on the contents of inside-with descriptions are to be transferred as instructed action, a key question in framing of any hybrid*-EM project cannot just be how much competence in the setting the investigator can or should acquire but also how much competence in the sociology those in the setting are expected to have acquired. As a corollary, if the double fitting of competences is required, how is the calibration to be carried out to prevent misfires, mismatches and misunderstandings? In addition, if EM is not so much a 'discovering discipline' as a 'transferring discipline', what project demands must be satisfied for it to carry out its function successfully? Although EM has touted this possibility, it has spent no time or effort thinking through what the requirement imposes on the project team.

Garfinkel addresses what hybrid* EM might entail in the following way.

Ethnomethodology is not critical of formal analytic investigations. But neither is it the case that EM.....has no concern with a remedial expertise and has nothing to promise or deliver. Ethnomethodology is

applied Ethnomethodology. However, its remedial transactions are distinctive to EM expertise.

That expertise is offered for phenomena whose local, endogenous production is troubled in ordered phenomenal detail of structures. EM does not offer a remedial expertise that is transcendental to these phenomena. [Garfinkel 2002, p. 114]

This statement is important for more than its relaxing of the stipulation EM should be “indifferent” to FA claims, theories, accounts and so on. It seems to be intimating EM can or might offer correctives to the particular forms of FA reasoning relevant to the specific circumstances in which they have been studied. A little later in the same discussion, during a laudatory account of Robillard and Pack’s work on paediatrics, we find the following.

Their program was notable for working out and demonstrating the condition of EM adequacy that the analyst’s Ethnomethodological findings be taken seriously in the FA discipline that was studied. By being taken seriously I mean that the work site practitioners *will demand of EM findings just as they demand of FA findings* that they satisfy the work-site-specific, discipline-specific corpus status of FA investigations and that EM findings be incorporated in FA work at hand or reasons be given for not doing so. [ibid p. 127. Italics in original]

It is here Garfinkel lays out what we termed the “condition” for hybrid studies. The adequacy of an applied and remedial EM stands on its ability to ensure the deployment of its results in its host/partner domain where, remember, that domain may be any scientific, professional or other routine working practice and not just professional Sociology. All such disciplines have previously been categorised as Formal Analysis (FA). Accepting this is a condensed sketch account, nonetheless two crucial aspects seem to have been lightly skipped over. The first is the conceptual coherence of holding to ethnomethodological indifference whilst pursuing hybrid studies as just defined, since the former is simply the operationalisation of the radical asymmetry claimed for the incommensurability between EM and FA. The second is a more operational matter; the defining of criteria for designating particular sets of activities as “troubled” and who makes that judgement. There is a further aspect. The operational challenge of satisfying the requirements of seriousness and the engagement with FA disciplines in order to transfer results.¹⁴

¹⁴ More recent work in domains such as HCI (see, for example, Crabtree [Crabtree 2004] and Hartswood et al [Hartswood et al. 2002]) for early statements (but note Pollner’s [Pollner 2012a] disquiet) indicate such partnerships require a great deal more than “throwing a few recommendations over the fence”.

Let's take the question of logical coherence first. If EM involves a conceptual disjuncture with any and all FA disciplines of the order often stipulated by Garfinkel, Weider and others, how can it be logically possible to incorporate the results of EM investigations of FA practices into the accounts which the FA being studied gives of its own procedures *whilst at the same time* insisting on maintaining conceptual integrity and coherence of both EM and the FA discipline? To propose this requirement without clear demonstration of a logical fit with claims about EM's relationship to FA, risks not just the appearance but the reality of conceptual incoherence and inconsistency. Search as we might in *Program* and elsewhere have not found an argument showing why this risk does not arise.

Does this absence matter? Well, it depends on just how seriously we are to take "seriousness". Presuming EM's promulgations about itself speak to how we should view the essential character of authentic EM work, i.e., those features which the discipline should aspire to and try to exhibit, then surely there is an obligation to provide analytic re-assurance concerning the necessary logical continuity between the condition being laid down for adequacy of investigations and the way the discipline is defined? This cannot be done simply by citing lists of studies as exemplars of authentic EM. Any itemising cannot demonstrate logical consistency and coherence between the two propositional stances. Neither would it identify which adjustments might have to be made in the protocols of EM and FA's own descriptive methods for consistency and coherence to be attained and maintained. At the very least, if studies do show the required consistency and coherence, there must be some detailing of how the triangulation between the format, content and potential deployment of EM results was achieved in the conceptual and practical contexts within which the target FA is operating.

What makes the notion of "troubles" problematic is both how they are identified and what kind of "trouble" they are. The question of identification turns upon the skills of description and assessment we require of EM researchers engaged in hybrid studies as well as how we expect they will normally have acquired them. If the domain is an academic or professional discipline (Astrophysics or Neurosurgery, for example), do we expect at least entry level Graduate competence in both the explicit and tacit knowledge of the target discipline melded with Graduate level understanding of EM and its methods in order for the investigator to "see" the identifying configuration of details of disciplinary reasoning? And if so, how do we imagine that melding might be achieved? Putting it at its simplest (and its grandest!) just to make the point, take the contrast between EM's favourite category of "normal natural troubles" such as those which arise any accountable social production process (the sorts of things Livingston points to as the necessary

correlated troubles involved in proving or untold numbers of studies of science labs have revealed about experimental practice). Now compare these to mathematicians' compilations of key problems in Mathematics such as Hilbert's famous programme or the lists of fundamental problems like as the measurement problem in post-Quantum Mechanics. Whilst any competent EM investigation should be able to churn out observations on the 'bench science' and its normal, natural troubles, it is hard to imagine how EM might make a direct contribution to solving the kinds of "troubles" scientists think they are confronted by. The one area where there might be interplay is pedagogy. Even here, though, to make a contribution would require reformulating EM's descriptions of the informal logic of practical scientific and mathematical reasoning in ways which would allow translation into the formal rules for constituting well-posed problems or the disciplinary norms for generating robust findings. Although he has returned to this issue a number of times, Livingston, for one very leading example [Livingston 1999, 2015], acknowledges he has not found a way to fashion the necessary scaffolding to allow the desired bridging to take place. This does not mean it can't be done, simply that it hasn't be done. The fact as able as exemplary an interlocutor as Livingston has not managed to make much progress should give us considerable pause for thought.¹⁵

Whichever way the issue of conceptual coherence is addressed, a second challenge remains. This is we will call the requirement to "organise for delivery". What is involved here is way downstream from abstract determinations of what incommensurability might be and its application to EM and FA or strategies for misreading theories and philosophies. Ensuring particular project outcomes are implemented involves matters rarely if ever discussed in Sociology. By "delivery" we mean the transferring a suitably packaged set of research results to be incorporated into their own work by those who take them. Engineering disciplines have a whole raft of procedures, advisories and rules of thumb for managing successful research and development projects to ensure delivery

¹⁵ We have not been able to access the Robillard and Pack materials, so we cannot comment on those. However, some researchers cited by Garfinkel such as Stacey Lee Burns [Burns 1996, 1997, 2023] do not comment on the problem of defining "troubles" nor on the uptake of their results in regard to them. Things are not quite so straightforward with others such as Suchman [Suchman, et al 1999, Suchman 2000] where it certainly is true some of her findings were acted upon. However, the extent to which those findings derived from the utilisation of EM methods rather than fieldwork ethnography or Scandinavian Socio-Technical Systems techniques of user centred system design is not clear. To help calibrate these cases, it would be useful to have Lynch's reflections on his own experience of researching in a number of 'applied' fields.

to time, to budget and to specification.¹⁶ By laying down the stipulation he does, Garfinkel commits us to the successful transfer of deliverables to time, budget and specification.

What sorts of things are we talking about when we speak of procedures, advisories and rules of thumb? They are the praxeological disciplines of project management. Project management is not research proposal forward thinking; the designing of research activities in the hope of obtaining sufficient results to justify a project in ways which approximate to how the initial proposal justified itself. It is results implementation backwards thinking, very much cast in the pluperfect tense. What will we have to have accomplished in order to be able to transfer the results we are certain we will have secured by then? Projects are scientific, organisational and social structures. All three elements require management to ensure successful hand off of deliverables. Hand off can take many forms: adopted implementation plans for policy recommendations; enacted business plans for re-organised activities; deployed prototype technologies, and many, many more. One aspect of all this is the infamous Error 33 problem (See [Kay 2004]). At Xerox PARC, Error 33 was the term used by the designers of the Alto to describe a strategy of saving time, money and other resources in the development of an innovative system by incorporating onto a project's critical path as-yet-unproven or perhaps even as-yet-undesigned (so called "slideware") technologies from other research groups. Obviously, if you can't control a technology's development, you can't be certain of its availability nor its capabilities. In effect, what Error 33 does is predicate a project's success on the expectation/hope of someone else's research progress. By making the transfer of EM findings into the *practise* of FA a condition of adequacy for research findings, Garfinkel seems to be awfully close to requiring EM to risk Error 33 in the delivery phase of its projects. If EM wishes to satisfy Garfinkel's condition, it seems the arts and sciences of serious project management are going to have to be part of its stock in trade. That they are not may well be an in-principle reason why there are no existential proofs of hybridity.¹⁷

Although Garfinkel does insist on the necessity for his objective, at the same time he offers an escape clause in the final words of the passage we cited: "...or reasons be given for not doing so". As ever, though, we are given no indication of what are and what are not acceptable exculpatory reasons disregarding the objective. Adopting the least line of resistance through a strategy of studied disattention to all these issues might make life easier but it comes with its own

¹⁶ None of which involve simply publishing papers, the method favoured by Sociology and most of the social sciences for transferring results as deliverables.

¹⁷ Thanks to Graham Button for pointing this out.

risks. If problems are not raised and arguments are not resolved, rigour may well be traded off against practicality. The result could only be intellectual drift and disciplinary amorphousness. Eventually, EM will have become whatever anyone wants it to be.

INSIDE-WITH DESCRIPTION

Near the beginning of the description of the inclined plane experiment, Garfinkel tells us:

To display the demonstration's [Galileo's] properties of social order, to make them examinable, to make them instructably observable bench work, and to make the bench work adequately articulate, requires the work-site disciplinary expertise of both physics and sociology.

The tasks of adequate and evident display by collaborators are endogenously interior to the display. Therein their tasks being embodiedly oriented within the display and as of the display, "inside-with" the display, are to describe Galileo's achievements of physics, and for physics and western science, in coherent details of sociological phenomena of social order. These phenomena are Durkheim's neglected phenomena of order. [Garfinkel 2002, pp., 290-1 italics in original]

Attached to the term "inside-with" is the following footnote.

"Inside-with" is a phrase that Lois Meyer coined in her Ph.D. dissertation. The use of "inside-with" by EM authors should be used to criticise Merleau-Ponty's "intertwining" and "chiasm" as well as recent variants on these metaphors. [ibid, p.271, fn.12].

What is odd about the footnote is the injunction Garfinkel propounds in regard to the phrase "inside-with". Whatever its meaning, it should be used to criticise Merleau-Ponty's "ontology of Flesh" as developed in *The Visible and the Invisible* [Merleau-Ponty 1968]. This appears to be a flat contradiction to EM's standard line regarding the rationality, reasonability, logic, coherence, consistency and all other epistemic virtues of theoretical and philosophical discussions. As we have already said, this standard line is the stance of ethnomethodological indifference. EM's interests, objects and findings do not bear upon these matters and so cannot throw any light, illuminative or critical, on them. Why then, does he want EM to use Meyer's term as critique of Merleau-Ponty in precisely this way?

We can get some insight if we look at the context in which the concept of “inside-with” describing was developed.¹⁸ In the Introduction to her thesis, Lois Meyer [Meyer 1991] set herself three tasks:

1. To provide an analysis of the teaching and learning of a school “as it occurred *in situ*, the local effort of a specific group of people at a specific school site” (p.9).
2. The descriptions provided would have “to look and feel familiar to those who do that work locally, inside the school”. This recognisability was a criterion of adequacy.
3. To provide an account of the practices of “The Language Circle” as a method of teaching and learning language in which those practices could be taken as objects for instruction by “outsiders” who did not participate in that particular local scene.

One of Meyer’s audiences is defined as those who are interested in but unfamiliar with the context she is reporting on. This means she cannot trade on insider assumptions to satisfy her third objective but must render her account in more generally recognisable terms. To be successful, the first two objectives must satisfy precisely the inverse condition; contextually shaped and located descriptions. To resolve the tension, Meyer decided on the adoption of an investigative stance where the praxis of investigation would be “inside-with”. Her explanation of what she took it to entail is worth quoting at length.

In stark contrast to a subjectivized, psychologized internal probe, the term “inside” in this study refers to Heidegger’s existential and phenomenal sense of “inside” or Being-in-the-world, where “the world is always the one that I share with Others” [Heidegger 1962:155], ...“those among whom one is too” [154]. Dreyfus [1990] points out that Heidegger’s Being-in-the-world implies involvement and caring; it has a meaning much more like “being in love” than “being in Houston.” The Language Circle is composed of a company of teachers and children who are care-fully involved with each other in its teaching-learning work; each participant is “inside” the Circle’s work, together with the rest of the company. In this way, participants’ knowledge is “inside” knowledge precisely because it is witnessable by, available to, and dependent upon, others who are also at work inside the Circle. Bringing newcomers inside the Language Circle means teaching them “our work,” not monitoring their internal thoughts. The social, phenomenal meaning of “inside,” that is, the “inside” which Language Circle participants experience and know as their work world shared and

¹⁸ We are grateful to Lois Meyer for providing sections of her thesis and details of how she approached the study it contains.

lived together with others and in each others' company, is crucial to this study. To underscore this meaning, the use of the term "inside" in the title of this study and in the text itself should be understood to mean "inside with" that is, "inside with other participants in the Language Circle."
[Meyer 1991, p. 11]

Inside-with descriptions, then, speak from the experience of the community who share that experience and in terms which convey that experience as the lived world of daily, ordinary life as a community member of The Language Circle. But it is not just a description of the community and for the community. It is also *about* the community for those who are not members. (In addition to those just mentioned, she also had her PhD Committee to satisfy.) As a result, because of the plethora of audiences for whom the account is to be provided (that is, the one account given 'here and now' in the context of the thesis being read), the text has to be recipient-designed so it can be sense assembled by *all* these audiences as a coherent, plausible, rationally acceptable account in terms of whatever relevances they have. As she puts it, the text has to be report, novel and parable depending on who is reading it. What allows for this is the possibility 'the same detail' might be given different configurations depending on the frame of reference adopted for any reading. To put it another way: the aim is for the lines on the paper to assemble themselves into the duck or the rabbit in and through the reading. But achieving that is the exercise of artistry in itself.

Note the stance brings with it an investigative stance, one in which it is possible for the investigator to be a member of the community whilst adopting the analytic attitude of investigation and yet not shaping description in terms of a disciplinary gaze. Foucault's concept of the (professional) gaze has had quite an airing in discussions of ethnographic reporting in the social sciences. Using it, commentators have argued the investigator fillets social activities of all but sociologically relevant meaning. What activities mean for the participants, what they amount to and demand of them are downplayed under the theoretically constructed objective purview. The inside-with description wants to bring meaning back in without sacrificing technical clarity, precision and interests. This means reporting investigations by talking sociologically and locally. However, what it takes to do that is as yet unsolved. It is interesting very much the same tensions are evident in Dorothy Smith's later work.¹⁹ It is no part of our interest here to determine if Lois Meyer resolved these tensions nor how effective her descriptions as descriptions for her various audiences were. All we can say is that so far EM has failed to do so.

¹⁹ See Part III.

Perhaps EM's failure derives from a 'misreading' of Merleau-Ponty's descriptions of embodied perception? Could that be the reason Garfinkel was unable to deliver the detailed analysis of the transition from pre- to post-Galilean phenomenal fields he was seeking? The discussion in *The Visible and The Invisible* suggests the progress which Merleau-Ponty had made using Husserl's analyses to extend beyond the *Psychology of Perception* was insufficient to ground the apodictic character of embodied perception. He had, as his title says, reached the limits of Phenomenology. A new ontology was required to supersede that on which earlier studies were based. Is the stance of 'inside-with' description consistent with either Merleau-Ponty's new ontology or, as in the case of the Inclined Plane Experiment, the conjunction of the configurational orders of practical detail in EM and Physics? In other words, does the difficulty arise because EM is committed to 'inside-with' descriptions or because 'inside-with' descriptions in the contexts studied so far are impossible? Could it also be, and this is perhaps the most likely, that inside-with descriptions would not fit any Merleau-Ponty ontology EM might wish to employ?

We are now at the heart of the inside-with problem which hybrid* EM sets itself. Garfinkel's proposal/claim that EM has substantiated Durkheim's aphorism to the effect that sociology's fundamental phenomena are social facts has been taken in two ways. The first takes statements like "The unemployment rate is 8%" and co-classes them with other statements like "light is a stream of photons and a wave of radiant energy". The co-classing is brought off by proposing both are "produced" by methods of investigation involving the imposition of disciplinary and or professional categories on phenomena. Call this the "professional gaze" stance. What is involved here is a substitution of 'sociological' for 'social' in 'social fact'. Substitution is often complemented by an "as-told-to" stance of sociological analyses. Under that feature, sociologists can only learn about different ways of life from those who are engaged in them. They are social facts 'as told to' the investigator (in whatever way the telling takes place). In both stances, once the sociologist encounters a 'social fact' in a setting, it is re-construed as a "sociological fact". This is precisely what happens in the majority EM "studies" as well. The detail of experiments, diagnoses, taught classes, traffic flow, police work and conversations etc. are re-construed as the shopfloor work of the setting.

An alternative way such statements might be taken in an EM investigation would be to reveal the constitution of the "natural attitude" which characterises the setting as that setting. The objectivity which is substantiated is the 'objectivity' which is presumed, taken for granted, treated as the case, found to be the case or whichever other way you want to describe "bracketed under the natural attitude". What this kind of EM then has to struggle with settling on a construal of that

attitude in order to suspend and analyse it. But to suspend that attitude, you have to hold it. Hence the competency requirement.

Under the first interpretation, neither Sociology nor EM can claim 'inside-with' descriptions. The best one can achieve is analytic ethnography. Under the second, you have to find a way to do the 'inside-with' describing by conjoining the sociological and the local account. We can now see why Garfinkel has to resist Merleau-Ponty's fusion of accountability using the duality of interleaving and chiasm. That would dissolve the disciplinary identity of the account for the audiences likely to receive it. He has to maintain a conjoined approach. But neither he nor anyone else has managed it. The question is whether this means strict adherence to hybridity's tenets makes EM impossible?

COMPETENCY

Competency in professional practice is not like perfect pitch. It is not a question of either having it or not having it. There are degrees of competency and degrees of understanding. Discussion of competency in hybrid*-EM never makes clear exactly what level of competency is being looking for nor what degree of understanding is required. However, given the centrality of the requirement to configure the phenomenal field as a practitioner does, we have to assume it is some combination of being able to 'see' as a practitioner does and to 'do' what a practitioner can do. The question is: has anyone in EM acquired that level of competence? And if not, why not?

Work by Robillard and Pack is heavily referenced by Garfinkel but unfortunately details of their projects and findings are unavailable. So we will set them to one side. David Sudnow's often referenced *Ways of the Hand* [Sudnow 1978] is a remarkable phenomenological analysis of his experience of struggling to learn how to play jazz piano. It is about his acquisition of the competence to hear, see and do things which others did "thoughtlessly". It is not a manual on the methods of its performance. The commercial "method" which he developed is hardly a piece of instructed action in jazz piano but simply a set of useful recommendations (for instance, start by learning just a few tunes) and some tutorial exercises for how to become more accomplished. According to at least one person who used it [Valeo 1987], it worked remarkably well. Stacey Burns [1997;1996] did practice Law but her studies of instances of law practice in action are precisely the kind of analytic ethnography Garfinkel wished to move beyond.

This leaves Eric Livingston's studies of Mathematics and Physics.²⁰ It is interesting that while Livingston's studies were Garfinkel's touchstone for hybridity, Livingston himself never talks about them that way. He does express the hope that one day they might be. (See, for example, the Appendix to his [1986] and the Epilogue to his [2008]). Discussions in *The Disciplinarity of Mathematical Practice* [Livingston 2015] and elsewhere, repeatedly observe that he is a novice prover²¹ and struggles with the gap between what he can do and what mathematicians seem to be able to do effortlessly. It is of significance, we suggest, that Livingston prefers to refer to his studies, both of modes of mathematics and other modes of reasoning, as "ethnographies".

Regarding the competency requirement, then, only Sudnow and Burns seem unequivocally to have attained the level required, but nowhere does either offer an inside-with account of its performance as instructed action.²² The published cases of hybrid*-EM cited by Garfinkel and others either fail to satisfy the requirement or fail to attain the objectives. In other words, there are no existence proofs of hybrid*-EM. But, as we have said, this does not mean hybridity is impossible only that, perhaps for the reasons summarised earlier, it has not so far been done.

Section 4. Summary

Resolution of the problem of meaning as a solution to the double contingency provided the intersubjective coordination required for social order in Parsons' theory. This solution allowed for the coordination of courses of action. Garfinkel's strategy of pushing Parsons' sociology to its limits was achieved by asking how the problem of meaning was resolved in the theory. Further EM studies pointed out such coordination was achieved *within* the flow of action. The problem is resolved both within the theory and in the concrete. The result of this 'discovery' is that the investigator\observer should be placed within the analytic frame of reference because of the dependency of the theory on the resolution of the solution in the concrete. That is EM's fundamental premise.

²⁰ Dusan Bjelic [2023] has also studied Galileo's work and was the stimulus for Livingston's own studies of pendula. It is not clear, though, that Bjelic is aiming for anything other than analytic ethnography.

²¹ Note in many of his studies he acknowledges the contribution his brother, Charles Livingston who is a professional mathematician, has made to his work.

²² Larry Weider was, apparently, a highly proficient conjuror. However, we have no analytic accounts by him of the lived work of conjuring.

This premise was actionable in Garfinkel's early studies and those of other members of the EM research community because what they were examining were the 'gaps in the texts' between social scientific (predominantly sociological) accounts of social phenomena and the descriptions available to investigators in the accounts and actions of social actors themselves. As social scientists and members or possible members of the cultural communities being studied, investigators could meld the sociological and the social into the required "inside-with" descriptions. Only a few (for example, Dorothy Smith) tried to transfer their results into the practice of social science as recommendations for its improvement. As we saw in our discussion of Institutional Ethnography, that did not work out as she intended. Retrospectively, then, it would not be unfair to talk of the first phase of EM work as creating a quasi-hybrid*-Sociology/EM

When EM turned to the natural sciences, the dual competency feature and with it the capacity to deliver 'inside-with' descriptions was at risk and had to be managed by ensuring the acquisition of the required domain competency as part of or in advance of the investigation. Neither the inclined plane experiment nor any of the available studies achieved this or laid claim to achieving this. On the other hand, many did learn how to find their way around their settings sufficiently well to be able to provide plausible analytic-ethnographic descriptions. Such analytic-ethnography was not, however, the hybrid analysis stipulated to be the goal of hybrid*-EM.

Nothing we have said in this discussion proves hybrid*-EM is impossible. All we have claimed is that the challenges are formidable and require significant conceptual, methodological, and practical work to shape and hone EM's investigative *modus operandi* so it can achieve the triplet of objectives Garfinkel set for it. Hard questions about ethnomethodological indifference and incommensurability must be addressed. Tricky questions about the place of the investigator as actor/observer in the investigative frame of reference need be resolved. Perhaps most challenging of all is the likely requirement to create a raft of mechanisms and associated techniques for framing EM results and findings as instructed action so that they can be transferred into the domain of study. All of these will take time and effort. The very things the EM community has shown little inclination to spend on them.

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