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Beware the Primrose Path

.....like a puffed and reckless libertine,
Himself the primrose path of dalliance treads.
(Hamlet)

INTRODUCTION

In tracing the history of Institutional Ethnography, we saw how Dorothy Smith's original proposal of a sociology for women gradually lost both its radical edge and its distinctive *point d'appui* as it was assimilated into the broad mass of conventional Sociology. An element in this process was the form of disciplinary liaisons it engaged in. Sociology added yet another investigative domain (the sociology of the marginal and dispossessed) to its repertoire whilst Institutional Ethnography gained conventional investigative techniques, academic respectability, students and career structures. This brief discussion looks at why just the same consequences might result from similar processes of accommodation between Ethnomethodology more broadly and Sociology and/or the wider social sciences. In so doing, it looks behind or beyond the lessons drawn from the history of Institutional Ethnography and the story of hybridity.

Section 1. Background

No discipline worth its salt can remain static, endlessly re-running the arguments which formed its original rationale. Equally, to survive and prosper no discipline can waste energy and resources hopelessly chasing every vision (or fantasy) proposed for its future. Time, funds, manpower and mental energy (especially mental energy!) are always in short supply. Choices over where the discipline should go next and how to get there must be set against the strategic objectives which it has set itself. In addition, the strategic benefits likely to accrue from innovation must at least match, if not outweigh, the practical costs incurred in the effort of acquiring them. It is the character of such balances and the need for them which make many current suggestions for closer collaboration between Ethnomethodology (EM) and some of the institutional bodies of conventional Sociology (as well as the swathe of related disciplines and professional practices to which EM applies the epithet Formal Analysis (FA)) not quite the straightforward matter some have presented it as. Historically, EM has always insisted on an arm's length relationship with FA, one which it enshrined in the principle of "ethnomethodological indifference". Moreover, it has continuously and vigorously asserted the only viable relationship it can have with FA disciplines is to treat them as sites within which to reveal the social organisation of practical reasoning and the practical activities which underpin it. One would have thought this attitude would militate against the expectation closer, more arm-in-arm, cooperation might be feasible and profitable both for EM as well as its putative partners.

This does not mean we do not recognise and share some part of the aspiration. EM was begot by Sociology (reluctantly, maybe) and there is considerable shared heritage. In addition, today many of those who identify with the EM community earn their corn contributing to the general teaching of Sociology either in dedicated departments or as in-house specialists in other places. There ought at least to be cordial working relationships between practitioners of both modes. However, we are not convinced either the mutual understandings or the conditions required for larger scale collaborations to be successful are in place. In fact, we are quite sure what might be thought of as the general Heads of Terms for such inter-disciplinary endeavours have never been contemplated, let alone worked out. The issue is not how individual researchers or research teams might work together and even prosper under ad hoc or long-term collegial working arrangements. It is how the disciplines *qua* disciplines can be first aligned and second coupled without either feeling violated or exploited.

The central questions from the EM side (and no doubt possible collaborative partners will not be wholly ignorant of EM's official stances) revolve around how the precept of

ethnomethodological indifference to FA disciplines, their interests, theories and problems is to be managed, rooted as it is in a fundamental judgement EM has made of its own incommensurability with FA *tout court*. Can conditions for mounting sets of practicable collaborations be arrived at which satisfy the requirements of both partners without turning either side into lowly service providers, colonial subjects, fashionable playthings or the whole transaction descending into one-way traffic?¹ None of these possibilities are outcomes to be risked by a serious discipline. Moreover, for those finding themselves caught up in them, they are hardly marks of success either.

Graham Button and colleagues [Button et al. 2022] as well as others have issued stern warnings about what they see as a creeping recidivist constructivism in many proto and embryonic collaborations, particularly but not only between Conversation Analysis and Linguistics and Social Psychology. The same drift is clearly visible in other, earlier prominent partnerships, for example between EM studies of work and HCI or the natural sciences. In all these cases, the programmes of collaboration appear to violate a central tenet of EM, namely the determination to prioritise and pay relentless attention to the local organisation of solutions to the problem of meaning in courses of action and the consequences which follow from that focus. The considerations to which we draw attention in what follows are related to this apparent violation. The warnings from others which we have just mentioned are motivated by worries about the intellectual ballast which keeps the ship of EM stable.² We are more concerned about the composition of the crew, the working relationships among them and the likelihood neither the relations nor the crew will last the voyage.

Section 2. The Principle of Indifference

The precept EM and FA are incommensurable is the single most important background issue whose implications will surely radiate through any set of arrangements put in place. Needs must we start with it, what it means and how it applies. Starting in this way gives the primary knot of issues to be untangled before we can move on to frame subsidiary questions. Even though the precept is widely cited as the centrepiece of EM position statements, its precise basis and its methodological consequences are often under appreciated. This is because when the notion is used as a way of defining EM, invariably it is not given the order of careful explication it demands. One pivotal

¹ In that respect, the last of these might be the most efficacious outcome for EM, assuming all the value in the arrangements accrued to it. However, such arrangements wouldn't last long.

² This is not an idle image. Just like the ship of Theseus, EM has undergone and is undergoing regular maintenance, remodelling, refurbishment and extension. Today, it is both the same and not the same as Garfinkel's original enterprise.

example of what we mean can be found in Larry Weider's [Weider 1993] discussion of interdisciplinary Conversation Analysis (CA). Although Weider accepts the notion of incommensurability has had a history of opaque use and conceptual abuse, he offers no worked through definition of what he takes it to mean or imply. Instead, he begins by insisting the determination of the paradigmatic status of CA and Experimental Social Psychology (the two disciplines with which he is concerned) is not a relevant matter for his argument. He then goes right on to assert:

....the exclusive claims of interest here are that these enterprises are incommensurable in the same way that work guided by incommensurable paradigms in the physical sciences is. [Weider 1993, p. 214],

The only clarification he offers is:

CA procedures and those of experimental social psychology present analysts working within these enterprises with different sets of entities. [ibid]

How this *ontological* construal of incommensurability maps on to EM and CA's relationship to FA disciplines such Social Psychology is left unexplicated.³

We know (and indeed are sympathetic with) what Weider was trying to do here. For some time, Garfinkel had clearly become exasperated with mainstream Sociology's unwillingness to see the significance of EM's findings for its disciplinary practise and its inability to accept EM's desire just to be different. What seemed to irritate him most was not ludicrous attempts to deny any sociological legitimacy to EM but the refusal to accept the scale of difference entailed in EM's mode of doing sociological investigations. This exasperation is clearly on view in many informal notes, texts of talks and meetings and elsewhere. (See for example, Hill and Crittendon [1968] and Garfinkel [1990]). This seems to have spurred him to want to make the point as forcefully as he could. During the late 1970s and early 1980s, Garfinkel began to talk of EM and FA as "incommensurable" [Garfinkel 1988; Garfinkel and Weider 1992; Garfinkel 2002]. The problem is that in linking EM's incommensurability (and hence its principle of indifference) to the debate over scientific paradigms stimulated by Kuhnian analysis [Kuhn 1962; Kuhn, 1972], even in the lightweight way they did, Weider and Garfinkel jointly and severally missed the opportunity to mark the difference between what the term meant for them and what it meant for Kuhn. Missing this

³ As we shall see, the fact Kuhn toyed with this and many other construals of the concept did not help matters. But saying that does not mitigate Weider's omission.

opportunity left the space open for commentators, interpreters and some would-be defenders of EM to 'join the dots' in ways which had little or nothing to do with what originally motivated EM's position. Not only were the waters muddied, unnecessary flotsam and jetsam were thrown in as well.

To see why, we first need to go back to basics and recapitulate the grounds of the original position. The place to start is with the rarely noticed and hardly ever remarked upon fact that many of Garfinkel's studies of 'investigative work' and 'data collection' explore whether, and if so how, an investigator could actually follow the standard requirements of Sociology's official methods as part of their actual practise of sociological investigation. The point he is seeking to elucidate is simply that those official methods commonly serve to obscure a persistent disparity between 'data' and 'phenomena'. The data do not capture 'the things themselves' but in large part consist of abstracted renderings of the 'materials' assembled by 'data collection'. Garfinkel is not condemning investigations for failing to meet the standard requirements of method but asking about the practicality of those methods since manuals of them do not acknowledge nor attend to the requirement they be applied, not as outlined in the abstractions, but as part and parcel of managing the necessary detail of practical sociological actions. This means sociological investigations cannot exclude practical (that is, everyday) reasoning. Indeed, they depend entirely upon it. The 'data' they deploy is created through the pervasive use of everyday understandings but such understandings are not specifically recoverable from the worked over materials presented to Sociology's research audiences.

Sometimes Garfinkel presents this as a matter of Sociology proceeding primarily by "shoving words around" or of dealing in "signed objects" (words, diagrams, records etc.) in contrast to investigating instances of situated action. At its deepest, the issue emerges with respect to Sociology's use of natural language. Here natural language is used as a means, a "resource" as it was described at the time, for talking about instances of situated actions rather than as a constituent of those actions and, therefore, as a topic of sociological investigation and analysis in its own right. It is this characterisation of the distinctive modes of investigation which gives whatever substance there is to the idea of an incommensurability of FA and EM. Treating FA and EM as topics for sociological investigation and analysis entails understanding that *both* FA and EM are empirically located within the domain of practical action and thus examinable in the same way as all other activities found there.

This entailment imposes three constraints on EM. Accepting them prevents EM from accommodating FA's approaches, topics, issues and problems, and vice versa. The constraints can be summarised quite succinctly.

1. EM rejects the extensively accepted (and often unwittingly incorporated) presupposition social order can only be found through the contrivance of a general scheme designed to satisfy the overarching requirements of institutionalised Sociology's aims and ambitions. EM declines this presupposition because it has set itself the task of identifying an order which is intrinsic to its phenomena, an order that is 'built in' to the performance or production of the intelligible actions which make up the evident order of practical doings.
2. Consequently, EM denies forms of Sociology *must* comprise two principal elements: theory and method. In contrast, it proposes this distinction embodies the adoption of a transcendental standpoint. EM sees no *necessity* for this distinction because, whatever its merits from the point of view of FA's aims and achievements, it functions to distract from the very things EM would call attention to. The distinction (unavoidably) must be treated as 'a given' by FA and as such cannot feature as topic of investigation in its own work.
3. The first two constraints lead to the third and EM's most radical proposal.
 - a. Investigators should give the same time and intensity to the examination of common, obvious features of social life as they give to things which Sociology has hitherto regarded as interesting, provocative and momentous.
 - b. Investigators cannot be analytically separated or effaced from the social world under investigation. Investigations are as much in play as practical actions as anything else and are located in the self-same social world of daily life as those actions which are under investigation. The ordinary, mundane stuff of daily life is or should be first class phenomena for sociological investigation and the sociological attitude to such phenomena should be one of reflexivity. The presuppositions of Sociology and other FA disciplines mean they can neither accept nor adopt either suggestion.

The implications of these constraints ripple through the way EM specifies its sociological world *for* investigation and how it constitutes its own social world *of* investigation. Out go the

ontologies of the social actor imbricated in macro-, meso- and micro-structures and in comes the social actor as the organiser of courses of practical reasoning. Out goes the metaphysics of functional relations and structural emergence and in comes the metaphysics of intersubjectivity and social order as collaborative production. And with these two, out go the operationalisation of transcendental method based in epistemologies of analytic and structural realism and in comes interpretive understanding and the calibration of the adequacy of method to the particularities of the finite province of meaning under view. With the observer now placed within the investigative frame of reference, these transitions make non-negotiable the adoption of a methodological relativism articulated as the indexicality of descriptions (or 'formulations', as EM often puts it) and the reflexivity of describing. This adoption is operationalised as an orientation to the problematic possibilities of description as an in-situ, omnirelevant praxeological problem for investigators and their subjects alike.

One result of this radical re-specification is that the flotilla of issues associated with Sociology's axiom of a three-way relationship between theory, method and the social world fade away. These are the "problems" of Realism, Objectivity, and Truth and the practical ways their contingencies have been grappled with under the stipulations of Sociology's topics and methods. To all of these, EM has quite rightly and understandably adopted its attitude of principled indifference. It can contribute nothing to the resolution of these "problems" and they have no bearing upon its practise. It has nothing to say about them and has no interest in them.

Section 3. The Distraction of Kuhn

It is here where the missed opportunity we referred to earlier enters the discussion. What drove Kuhn's original concern with theory succession in the physical sciences and what continued to drive it long after he had abandoned talk of "paradigms" and their organisation, was precisely the same three-way interrelationship between theory, method and the world which Sociology struggles with. Kuhn encountered them, though, under the rubrics of the physical sciences. The active elements might have different terms as well as different characteristics (measurement, for example, matters a great deal more in science), but the order of problems was the same. How is the validity and objectivity of description to be guaranteed as part of the practical detail of undertaking scientific investigations in the face of the helter-skelter of theoretical 'churn', 'competition' and 'development'? Nonchalantly invoking Kuhnian incommensurability as a descriptor of EM's principle of indifference hooks EM to these 'troubles' not because EM openly debated its encounter with them but simply because it was assumed they *had to be* pressing questions for EM

because Kuhn had asserted they were what was confronting the paragon natural science disciplines.

If Kuhn's concept of incommensurability is to be used as a designation for the EM/FA relationship, then that can only be as a technical concept in the philosophical arguments about the natural and social sciences. Its adoption depends upon the outcome of an examination of the appositeness of a direct comparison between EM's stance on FA as summarised above and Kuhn's account of theory succession consequent on his conjoining of the metaphysical consequences of distinct "thought communities" as described by Fleck [Fleck 1979] with notions of the logical irreconcilability certain orders of scientific postulates. Kuhn argued adopting revolutionary theories necessitated a collective Gestalt switch. Those adopting the revolutionary theory constituted the natural world in an entirely different way. Either side of the revolution, scientists lived "in different worlds". This metaphysical addition provided the backbone for Kuhn's much-discussed term "paradigm". The image Kuhn offers is of the history of science as punctuated evolution, the driving forces of which are socially (as opposed to logically) organised tensions over the interpretation of experimental results generated by the incommensurability of revolutionary theories. Unlike Feyerabend, Kuhn's attribution of incommensurability to theory change is not a formal analogy of the original mathematical definition of the term. It is more a rhetorical trope enhancing his argument for the complete divergence between certain domain theories and their metaphysical consequences. In an early working through of his account [Kuhn 1964], Kuhn explicitly associated the philosophical psychology underpinning paradigm change with the Sapir-Whorf hypothesis in Cognitive Anthropology.

As we all know, in the face of stern criticism Kuhn began to retreat from the forward positions he initially adopted. This was not a return to a well-defined use of "incommensurability" but part of a series of shifts to other, often more obscure, technical definitions, none of which those in EM wishing to lean on the association seem to have tracked. The major line Kuhn adopted reinforced the semantic character of theory succession by identifying incommensurability as non-translatability of terms rather than Gestalt shifts. His [1987] interim position, examines three very different types and orders of theory change: from Aristotelian to Classical Mechanics; from Volta's electrostatics to modern theories of electricity; and Plank's introduction of the notion of the "quantum". From these cases, he identifies the following significant properties of major theory changes.

1. The shift is not piecemeal but happens as a rapid condensing out of agreement on the new view. This may take some time to be realised, but when it is, it happens quickly.
2. The central change is in the character of "metaphor-like juxtapositions" (p. 21) which provide entirely different pictures of that portion of the observable natural and physical worlds the theory addresses.
3. Such changes are evident in the re-configuring of the constituents of operational taxonomies, both in terms of the re-arrangement of members and the development of new taxonomic structures.

Towards the end of his life, Kuhn [2002] talked of "lexicons" and the way changes of terms often merely re-ordered and re-clustered properties by using of different definitions. The wholesale disjuncture of the earlier conception had turned into a gradual 'speciation' of distinguishable concepts. Once introduced, the meanings of key terms morph and are then institutionalised as the bundle of referenced properties stabilises. There are two important features of such lexicons. First, they are projectable. Their application can be extended to new contexts, settings and bodies of observations. Second, as they are acquired and deployed, sets of expectations solidify around them. These provide a normativity to their use which secures their acceptance. The result of these amendments, however, leaves Kuhn in an uncertain position.

Some of the kinds that populate the worlds of the two communities are then irreconcilably different and the difference is no longer between descriptions but between the populations so described. Is it, in these circumstances, inappropriate to say that the members of these two communities live in different worlds? [Kuhn, 1993, p. 319]

For Ian Hacking [1993], the terms Kuhn picked out are 'scientific kinds' gathered in disciplinary taxonomies. Either side of a revolutionary theory change, there are different orderings across some of the categories. Such re-arrangements are not of the order the lowly slime mould has been subject to, pushed back and forth between plant and animal kingdoms, but consist in new (such as quark and pulsar) and reconstructed (such as quantum and black hole) conceptions structured under the innovative theoretical frameworks. What the novel taxonomy provides is a new way of describing the 'old' observational data as well as new locales in which to look for new data. As a result, new ways of using old instruments are found as well as entirely new instruments designed for the new theoretical landscape. Hacking's rendering of Kuhn's notion of lexicon is restricted to natural science and natural scientific terms. In his comments on Hacking, Kuhn [Kuhn 1993] is clearly unhappy with this constraint and wants to extend the scope to natural kinds not

simply scientific ones. His recently published posthumous work [Kuhn 2022] presents his final arguments for this position.

Section 4. Conclusion

Why does all this matter now? Certainly not because EM has responded to the Lazarene re-emergence of incommensurability as a topic in discussions of inter-disciplinary relations by restating the methodological logic of its own position and underscoring what its implications were. It has simply hewed to the “messaging” it has adopted from the outset: the principle of indifference. This ignores the fact the expectation these issues and their Kuhnian interpretations are relevant remains integral to any investigative discipline. Such expectations are embedded in sociological, social science and other FA disciplinary considerations of the relationships among their theoretical constellations. These days such expectation is articulated as the need to reconcile features of different ‘modes of discourse’ and ‘standpoints’ by translating their ‘lexicons’, integrating their ‘taxonomies’ and concatenating their ‘results’. Only by reconciliation (or so it is felt) can the possibilities of “multi-method” and “interdisciplinary” investigation be realised. Here, just as one straightforward example of the expectation and its aspiration, is a comment about the coming together of Socio-Linguistics and CA (as well as a motley of other disciplines).

The most important premise of interactional linguistic research is that linguistic categories and structures are designed for service in the organization of social interaction and must be described and explained accordingly. For this, descriptions of linguistic structure are combined with CA-informed analyses of sequential organization. Where relevant for an account of particular actions or action sequences, interactional linguistic analyses should also be combined with multimodal analysis, for example, gaze, facial expression, gesture, body posture, etc. [Couper-Kuhlen and Selting 2018, p. 15]

It seems the extent to which any discipline can support reconciliation of this kind has become a signal criterion of its worth.

We’ll sum up by putting this outlook in the terms in which we think these issues will be encountered by those seeking accommodation and collaboration with FA disciplines. What they amount to is a budget of problems to be faced when setting out the practicalities of how alignment, contribution, integration and synthesis will be achieved. Such challenges are no more than the latter-day version of just those matters EM set its face against right at the outset. EM itself can provide no resources for the collaboration-seeking adventurers to call on. As a result, it seems to us the most likely consequence will be the subordination of EM’s conceptual apparatus to that of

whatever FA discipline is being flirted with. The outcome of that can only be unhappiness. Hence the moral of our title.

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