CRITIQUE, COMPLACENCY, CUMULATIVITY AND COMPARATORS

NOTES ON 'RADICAL ETHNOMETHODOLOGY': MANCHESTER 22-23 JUNE 2016

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1.1 In so far as there is a broadly common feeling, the overwhelming impression left by the 2 days in Manchester was that most of those attending both days do not share Mike Lynch's concerns about the current state of ethno/CA. There are issues, and in this note I will try and catch some of them, but in general terms the sense that ethno/CA is in trouble was not shared. It took a while to get to the nub of it, in part because of the disparate (uncoordinated) nature of the presentations and discussions, but in the end it was possible to identify 2 main things Mike was worried about.

   a. The emergence and popularity (or at least visibility) of Epistemics in CA as a concrete instantiation of an underlying trend of 'retreat to constructivism'. This was the upfront worry;

   b. With a number of notable exceptions, across the board the quality of studies in ethno/CA have become humdrum. Each is 'another one' demonstrating pretty much what is already known. The discipline is in a rut with little indication of any kind of progress or cumulativity. This emerged as a strong theme only on the second day.

These two issues ran interference on each other, particularly the former on the latter.

1.2 Mike had chosen to position his worries in terms of the relationship of 'radical' ethno/CA to sociology, thereby implying, first, that there had been a departure from radicalness (constructivism in Epistemics being the leading instance) and, second, that a return to ethno/CA's radical 're-specifying' of sociology is the way to counteract it. The trouble was that what was radical in ethno/CA's 're-specifying' was never actually made clear, or at least clear enough for everyone to indicate whether they agreed with that interpretation and hence whether they endorsed the idea of 'degeneration', 'departure' or 'slippage'.

1.3 What did happen, though, was the voicing of a number of somewhat different themes in ethno/CA's relationship to sociology, at least as that issue raised itself for many at Manchester. The first was whether sociology as an institution was felt to be the 'anchor' or 'oriented to' discipline. The

responses were spread right across the board. Second, there was the question whether the operational home of the individual was in a sociology department and hence whether the local professional demands of teaching and professional visibility were derived not from sociology but some other discipline. Third, there was the continuing perception that sociology was 'institutionally antipathetic' to ethno/CA , whereas those whose homes were now in other disciplines felt they were viewed with interest and engagement by their colleagues. Quite strongly attached to the 'home' issue was the vexed nature of career planning and progression. The standard model (call it ethno/CA in a diverse sociology dept) does not seem to be an obtainable career path for many. As a consequence, adaptations, adjustments and accommodations have to be made to get publications and promotions. Finally, and this came and went in various guises, was the 'difficult', 'antagonistic', 'critical' relationship itself. There is no denying that responsibility for this (if responsibility is the right term) should be allocated to both sides, but if sociology is a necessary foil, is it essential that the playing off the foil has to have the tone it often has? There was a diversity of views on this.

1.4   In subsequent paragraphs, I will try and sort out my own views on some of these issues. Some, in particular the 'institutional' ones of positions and careers require more thought than can be given here. However, in my view, they are just as important to the long term vitality of radical ethno/CA (if there is such a thing today) as matters of intellectual legacy and continuity.

1.5   Finally, These notes are not to be taken as a set of proxy 'minutes' of the meeting, but rather my (almost immediate) reactions to what I saw and heard.

2. 1   I will begin with critique and criticism if only because I introduced that distinction at the meeting. In so doing I want to tease apart a range of what you might call 'analytic attitudes', many of which shade into one another, and offer some initial reflections on them. In these reflections, I want to discount for personal or writing style. Some people are assertive, not to say opinionated and abrasive. Others are more measured but no less effective in their persuasiveness. I don't think style is the issue here. Rather it is the modality of analysis that is deployed.

2. 2   Let's start with the paradigmatic interest ethno takes in 'the work' of other disciplines. If you look at the studies of physics, mathematics, biology, computer science and so on, what you see is an *investigative* interest in the methodic practices by means of which that work is done. That is, the methods by which each discipline's 'objects' (e.g. experiments, findings, data, proofs and so on) are constructed and deployed are subjected to an attitude of analytic indifference. What those findings say, mean, purport, imply is for those engaged in the discipline to determine. It is how that determination is co-constructed that is the general phenomenon being examined. It is no part of this investigative attitude to find the work to be convincing, reliable, correct nor that "the Emperor has no clothes" either.

It is not a difficult or big step to extend that interest to the social sciences and, indeed, to sociology or even ethno/CA itself. There are exemplars in Garfinkel's coding study, studies of education and

elsewhere. Here again, the attitude of analytic indifference holds. It is no part of the investigative attitude to make judgements on the social science quality of the social science work done.

2. 3 A somewhat different analytic interest might be called 'critique'. Here the concern is to work systematically through the presuppositions, postulates, arguments, evidence and conclusions of a corpus of work to draw out the effectiveness of the connections between these elements and hence the coherence and systematicity that the work has. This is a *clarificatory* interest. The analysis may well be informed by a reflection on the technical work of making the connections, the construction of the internal logic of coherence, but its import is very much a judgement on that coherence; about the claims made and the support for them. The inclusion or otherwise of findings, conclusions, insights and so on from ethno/CA in the corpus being attended to is not, of itself, a guiding concern. Although there may be strong affiliations between paradigmatic ethno/CA analyses and certain forms of critique, they are not identical. Leading examples of critique incorporating ethno orientations are Cicourel's *Method and Measurement* and Bittner's *The Concept of Organisation*.

2. 4 With critical commentary we are talking about a *corrective* interest. Now the interest is in making the case for replacing or substituting a favoured set of interpretations or a preferred set of concepts for those adopted by the corpus being examined. The case is to be made by pointing to the demonstrable advantages of favored ethno/CA approaches or arguments deployed by authorities (explicitly or implicitly) seen to represent general ethno/CA orientations and concerns for the objectives set by the approach under review for itself. To be effective, of course, critique has to be rooted in a clear and dispassionate positioning of the presuppositions and objectives of the work un review. A great deal of ethno/CA analysis of 'conventional sociology' is critical commentary in this mode. As such, it does not and cannot lay claim to the rubric of 'ethnomethodological indifference'. It is, though, in the classical tradition of 'stress testing' theories and arguments to which sociology (and other disciplines) hold. As Wes Sharrock put it, sociologists are just as unhappy about most of sociology as anybody else might be. The point, though, is that criticism is an auxiliary or correlated not essential element of ethno/CA. The leading example must be Garfinkel's review of Parsons in *Parson's* Primer and his Thesis. Another obvious well known example is Schegloff on Speech Act Theory .

2. 5 The final category is critical commentary which *in extremis* spills over into the *oppositional* strategy of premise denial. Now it is the pre-suppositions of the corpus which is under direct attack. Much the weakest of ethno/CA's criticisms of sociology and other social sciences fall into this category are simple the working through of premise denial, where the rebuttal consists in simply denying the cogency of the presuppositions in question simply because they are asymmetric with those of ethno/CA. What might be called the epistemic moral high ground is laid claim to whereby those under consideration can be condemned simply for their failure to adopt the tenets held by the critic. What distinguishes the contributions in this category is the shrillness and abrasiveness of the tone. It would be invidious to nominate such exercises, everyone has their own favourite list of 'calm and collected' to 'whirling dervish' examples. The persistence of this modality, I would argue, has something to do

with one of the factors underlying the humdrum character of ethno/CA work, namely the lack of clear and generally accepted critical and quality standards (in which, of course, ethno/CA is not alone).

2. 6 Reflecting on the discussions at Manchester, it seems plain that whether we wish it or no, the available narrative about ethno/CA's relationship with sociology has been cast as criticism and opposition not critique and certainly not investigation. Moreover, that narrative is viewed uneasily by a significant portion of our community. Criticism and opposition (in the ways defined above) does not define their attitude to sociology or anything else. If, as has been claimed, ethno/CA needs a foil, a stance, a position to triangulate or set itself against, then it would seem one challenge we face is to find a widely supported modality under which that can be done.

3.1 Not so long ago, when I was musing on the ennui which many (including myself) seemed to feel about much recent work in ethno/CA, Martyn Hammersley suggested, in a kindly way but nonetheless pointedly, perhaps it was because I had been "doing it for too long". What once had been startling, original and incisive had, he thought rather naturally, become routine with re-visiting, re-working, re-application and, yes, repetition. This did not mean that those studies and arguments were not just as startling, original and incisive to those who come to them now through the work of contemporaries as they did once to me when I read the originals. And, to be fair, there is probably something in this generational interpretation. Equally, it would be just as fair to say, as Stu Card once did in a different context, those prospectors who are first on the scene are the mostly likely to find the easily mined gold. Late comers often have to spend a lot of time and energy "bouncing the rubble". This is not to deny or undervalue the insights of Garfinkel, Sacks, Schegloff, Pollner and others, but it is to say that as the studies, expositions, commentaries and introductions have piled up, gold has become a rarer and rarer commodity. These two, what you might call 'natural cycle' versions of the roots of the sentiments of many of the older generation contrast with the version Graham Button asked us to consider; complacency. The challenge Graham put to us was to ask ourselves if we were being self-critical enough. Had we become far too easily satisfied with the routine, the obvious,……….the easy In regard to our own work? Of course, there is one sense in which Graham cannot lose with this challenge. No matter how self-flagellating we are, we could always demand more from ourselves, put the bar higher on the rigorousness, depth, innovativeness of what we do. But, I take it, that is not what Graham meant. Rather, I suspect he was urging us to take personally Mel Pollner's allegation that ethno/CA had joined the ranks of the contented middle aged, middle class suburb dwellers. These days, do we prize comfort over excitement?

3.2 While none of these versions captures everything, there is, of course something in all of them. But if there is a generational divide on the matter, then I think it behoves those of us for whom the ennui signifies what might be an inflexion point in the trajectory of the work to do something about it, if only for ourselves. If we think things are flat, stale and routine and hence degenerative, then it is up to us to say what we think needs to be done to change them and then try to do it. Rather like the

disbelieving stares that David Lewis was often greeted with, no amount of tetchy grumbling will convince anyone we are right.

3.3 Unfortunately, there were no clear proposals on offer for how to do this. However, as Wes Sharrock pointed out, while , in the early years of ethno/CA, there was a reluctance to set out clear programmatic statements, nonetheless many of the studies undertaken were strategic in orientation. They addressed, or tried to address, obvious and obviously different sets of questions and problems. For Wes, it was the loss of strategic direction that was the underlying problem. I think one way we could build on this suggestion is to pull together a simple schedule of 'strategic' or 'break-through' studies of the kind Wes had in mind and to summarise what was distinctive their intent. We could then ask ourselves (in response to Graham) what areas/topics/agendas were missing and what kinds of studies might be put together to address them. I know many believe the work only proceeds through the studies, but for some of us at least, the direction of travel of the studies we are currently undertaking is in a holding pattern if not actually going backwards. So, what I have in mind is more of an audit than programmatics.

3.4 To get the ball rolling (the usual starter for 10), let me suggest some possible contender questions.

   a. We have lots (too many?) studies of talk-in-interaction, but very few studies of other forms of coordinated interaction. Where are the studies of traffic management?, mingling?, queuing? It is true we have a small number of initial forays, but where are the detailed, worked out analyses?

   b. We have some (actually surprisingly few) studies of non-conversational aspects of 'the natural attitude'. Should we not have more studies of routine, daily life *as* routine, daily life? Mention was made of occasioned maps. What other topics might we want to look at?

   c. On the other hand, we have lots of studies of the turn-taking aspects of technical talk in the accomplishment of technical activities, but where are the studies of technical talk *as* technical talk? (Isn't this Shill's complaint applied to 'disciplinary' talk?)

   d. We have a heap of studies of sociality as interpersonal (face to face and its analogs) social interaction, but where are the studies of other forms of consociation? Where are the studies of collective and group sociality (the 'travelling wave' study, such as it is, is only a gesture towards this)?

   e. The studies we have are almost overwhelmingly studies of realities defined under the natural, scientific or technical attitudes. Where are the studies of other realities which make up the multiple realities of the social world. Where are the studies of fantasy, theatre, religion, making music together, playing sport and so on? We have some but, again, surprisingly few.

   f. Drawing a bow at a venture, I would suggest that the preponderance of non-CA work involves studies of practical reasoning as forms of intersubjective cognition. What we lack are studies of activities as embodied praxis — social activity as the practical management of

our bodies and other objects (and I don't mean the coordination of gesture and talk). Taking a leaf out of Dusan Bjelic's study, who has done anything with Merleau-Ponty?

g. Finally, and this might be strategically the most important, where are the anomalies? Where are the phenomena that resist being treated under the praxeological rule?

3.5   Clearly the list just given is not exhaustive and the elements are not mutually exclusive. They are suggestions for where to start not where to end. The point is to gather the studies together and to structure them in terms of their strategic character. Once we have a populated mapping of the kind an audit like this might give us, we could go on to ask other equally important questions about just what from the corpus of 'iconic scriptures' is actually relevant for what we want/need/propose to do next. Picking the bones out of Garfinkel, Sacks, Schegloff, Weider *et. al*. might be an important marker to ourselves and to others that we were indeed 'moving on'.

4.1. One topic that came and went several times in the discussions was the idea of 'cumulativity'. For the most part, we associate this with the 'discovering sciences' and the idea of making progress by "standing of the shoulders of giants". What is cumulated are bodies of secured knowledge through programmes of sequenced investigations. Whatever else one might want to say about CA, it is clear it has achieved an order of cumulativity in this sense. Whether that makes it a 'discovering science' is, of course, moot. As currently constituted, ethno can have no pretence to cumulativity of this kind.

4.2. There are many reasons for this. One is the character of the enterprise. Like the rest of sociology, it is individualistic. People do the studies they want to do because they want to do them. There is no sense of a collectively organised endeavour. No-one has issued the grand challenges to sociology or ethno that you find in the natural sciences. Second, there is a history of re-starts, re-beginnings and re-directions. Garfinkel shifted direction at least twice (arguably more often) and was often quite severe on initiatives (his own as well as others) which did not realise his ambitions (which themselves seemed to change quite often). Third, and this is where criticism, complacency and cumulativity tumble together, the focus of much of the work, but not all, has not been on driving internal progress but on determining external alignment. The foil used to position work has not been the current state of a topic within (and now I am talking about ethno in particular) but without, in sociology or sibling discipline (like psychology, economics or management). Other than in a handful of cases, the concern with 'epistemics' being one, our analytic criticism/critique has not been directed towards ourselves and the positioning of our investigations has not been in terms of building on, complementing or replacing what we have got. You could call this 'complacency' if you wanted. I prefer to think of it as a lack of collective standards of appraisal. Taste, personal preference and the desire to offer collegial support should count for a lot in our assessment of the quality of work, but not everything. This *is* the positive that I would take from the discussion of Epistemics, CA's current state and ethno's flaccid state. The lines of internal critique which were introduced, the dangers of constructivism and empiricism, could well form part of a set of critical tenets we should use to appraise our own and the work of others. Other things should matter too; things like distinctiveness and rigour of argument

rather than rehashing of established positions and novelty of evidence rather than recycling of standard examples.

4.3. One important thing that immediately comes to the fore when 'evidence' is mentioned is data; exactly what should and should not count as data for any study and whether, as Howie Schwartz once asked, we always need data. To my mind, the problem of cumulativity is tangled up with the fetishisation of data and especially audio and video-taped recordings as data. Mike Lynch alluded to this fetishisation as a form of 'empiricism', which in some cases it undoubtedly is. However, my concern here is to point to the way it results in a narrowing of horizons and hence a tunnelling of investigative questions. If we can/will only study what we can tape and transcribe, how can we expect the cumulativity of the discipline to match the variety of the social worlds we experience?

4.4. There is another 'standards' issue here too. The citing of snippets of transcript or video is not, of itself evidence, and explication (this is what x said, this is what y meant) is not analysis. Far too often, the studies and reports we find published claiming ethno/CA legitimation are little more than edited reportage of transcript snippets.

5.1 The overriding question at Manchester was the 'radical' status of ethno/CA vis a vis sociology. Much time was spent puzzling over what 'radical' meant in this context, or rather what was 'radical' about ethno/CA and its stance towards sociology. For myself, I think the answer is simple. Ethno/Ca breaks with sociology 'at the roots' — that is, apart from taking its guiding question(s) from sociology, it is an entirely different enterprise. For some, this throws up the tricky question of which discipline it should therefore affiliate with whereas I would suggest that the comparisons, and hence allegiances if felt necessary, should be drawn on an issue by issue or trait by trait basis. For some purposes, e.g. the explicitness of presuppositions, ethno is like philosophy. For others, say the primacy of meaning as the driving analytic phenomenon, it is more like cultural anthropology or history. In other respects, I am thinking of rigour here, it is more like economics, in that it tries to work through its presuppositions and postulates in as consistent a manner as possible. There is one comparator that is not often mentioned but which I think might actually be helpful and that is biology, and particularly the natural history end of biology. The comparison here has nothing to do with content or method but with the use of analytic frame works. Species differentiation through natural selection is the guiding framework in biology, all the way from those interested in the human genome and its equivalents, to those who have taxonomic interests, those interested in ecological models and those who just want to discover what is there. For field naturalists, species differentiation through competition for resources seems to play the same role that the turn taking system or the classic studies do in ethno/CA. It provides a generalised gloss of how, in principle, things of a certain kind will work. What field naturalists have to do is fit the particularities of the cases they are studying (orchid hybridisation in the Derbyshire Dales, Emperor Penguin chick rearing in Antarctica) to the general principles and show how those principles work *in this case*. Just what are the competitive forces at work and how is the budget of energy costs and benefits managed? Where do the competitive

advantages lie? Cumulativity (to return to that) lies in the accumulation of a body of structural forms (formal structures or mechanisms, if you will) through which the fitting can be demonstrated. In turn, such analyses inevitably throw up cases which are not easy to fit within the principles, where the budget lines are quite opaque and where simply not enough is known to be able to float an answer. These anomalies challenge the principles and the field naturalist's ability to use them. In ethno/CA we have principles which seem to function is a very similar way. What we lack is the sophisticated mechanisms and the continuous challenge of anomaly. In the end, this may be what Graham was really getting at with his challenge of complacency. We'd rather go for the easy cases where the application of the principles is obvious than try to chase down the anomaly.

RJA
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