

ETHNOGRAPHIC ECLECTICISM:

Ethnomethodology and the 'Postmodern'

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I INTRODUCTION

In this paper, I discuss some recent themes in the developing relationship between ethnography and 'design'. The occasion for these reflections is continued discussion not only about this relationship but perhaps more importantly the character of the ethnography in question. The ethnographic 'move' in design-related arenas is usually associated with Lucy Suchman's work,¹ in which, firstly, a complaint of a kind was voiced and, secondly, a remedy for that complaint was suggested. The complaint in question had to do with (broadly) cognitivist or rationalist conceptions of human mentality and behaviour. In a critique which ranged from artificial intelligence to photocopier repair, the gist of her argument was that the 'planful' conception was not sufficient to explain human conduct in whatever context.² The remedy was the careful, detailed examination of human conduct in context through the analytic lens of ethnomethodology. That is, it entailed the explication of 'situated action'. Over twenty years, this view – original in the design-related context, though appreciably less so in the context of debates within sociology and other social and human sciences – came to enact a powerful vision of how studies might be conducted such that they, in some way, support the work of design. In sum, Suchman and others were arguing for a radical reinvention of the description-analysis-prescription-procedures that, at a very general level, can be said to characterise a design process which recognises that 'engineering' might not be the only relevant paradigm.

Suchman's moves were, nevertheless, controversial, and remain so, for two reasons. Firstly, the ethnographic stance appears to some to form part of a more general sociological project which progressively rejects anything that looks like a scientific epistemology (in terms of concepts such as 'truth') and thus created difficulties for cognitive scientists, engineers, and so on – and this is particularly true of 'postmodern' moves which emphasise 'standpoint' and reflexive engagement. Secondly, the specific form of ethnography that has become known as 'ethnomethodologically informed ethnography'³ seems to, on the face of it, offer a

1 Suchman: *Plans and Situated Actions*; Suchman: *Human-Machine Reconfigurations*.

2 In some views, her argument was that plans were not necessary to human conduct in any context – an entirely different conception and one which is refuted by the existence of the railway timetable.

3 See Hughes et al.: "From Ethnographic Record to System Design".

much more radical rejection: not only of scientific and engineering conceptions⁴ but also sociological and anthropological approaches as well, in that it seems to reject explanation and theory of any kind *tout court*, for the ethnomethodologists appear to prefer explication to explanation; to prefer the rendering of subject experience in plausible ways rather than the analysis of processes that might affect that experience.

This has in turn arguably led to two oppositional and defensive tendencies – the reinsertion of a ‘scientific’ approach to the study of human behaviour into CSCW via cognitive science, social psychology, and even versions of conversation analysis,⁵ and the reinsertion of professional sociological/anthropological thinking via an analytic approach borrowed from a more general ‘postmodern’ argument. Both of these tendencies, I will suggest, reinsert disciplinary privilege into the description and analysis part of the relationship described above. Both have also been subject to a critique from those with an interest in work practice. In the case of the former, Schmidt observes:

For example, some CSCW researchers claim that the central role that ethnographic studies of actual practices holds in CSCW is in fact a source of ‘weakness’ of CSCW, and they advocate a ‘stronger orientation’ to what is claimed to be ‘a large body of well-validated principles about human behavior in group and organizational contexts’ that, correspondingly, employs ‘data collection and analysis methods that emphasize parsimony and identification of generalizeable features of human behavior’. The aim of this [...] is to develop ‘universal principles of CSCW design’.⁶

He goes on,

[they] seem to take for granted, without reflection or argument, that there is one and only one legitimate form of scientific generalization, namely that of identifying abstract universal principles (e.g., ‘laws’). Such an assumption is [...] evidently false, as it would outlaw scientific insights of great value in a range of research fields.⁷

4 There is no room to enter into debates within ethnomethodology concerning ‘science’. Suffice it to say that Conversation Analysis and its cognates are heavily influenced by Harvey Sacks’ claims to a ‘natural, observational science’. The ‘studies of work’ tradition makes no such claims.

5 See Schmidt: “CSCW Divided”, for cogent reflections on the consequences of this struggle for CSCW.

6 Schmidt: “CSCW Divided”.

7 Schmidt: “CSCW Divided”.

The objection, I will suggest, is in large part to an excess of mechanical rigour – to a belief that human behavior can be reduced to causal rules in the way of the natural world.

At the same time, a number of changes in the world of ‘design’ appear to have led to the re-emergence of a sociological/anthropological critique of technology. One factor has been the emergence of design paradigms which owe less, and perhaps nothing, to the scientific and engineering conceptions referred to above. This may include matters which relate specifically to computer system design, such as the emergence of ‘agile’ or ‘extreme’ programming techniques. Another might be the emergence of an approach to technological design which owes much more to the notion of ‘creativity’. As Crabtree et al. note,

We focus particularly on new approaches to and understandings of ethnography that have emerged as the computer has moved out of the workplace. These seek to implement a different order of ethnographic study to that which has largely been employed in design to date.⁸

They go on to say,

As Bell et al. [...] put it, the role differs from the one usually assigned to ethnography in HCI. It is characterized by ‘ethnographers turning their attention to ‘consumer culture’ and ‘cultural practices’ [...]’ to provide designers with ‘critical readings of the social context of use’ and to ‘generate innovative suggestions for and approaches to design problems’. New ethnographic approaches draw upon ‘humanities-based disciplines such as anthropology, literary, cultural and media studies’ to think about technologies as cultural artefacts.⁹

Further, and most relevantly for my purposes, they argue:

A particular issue is the way in which detailed analyses of the methodical ways in which people organize action and interaction in situ [...] get *replaced* by the kinds of broad generalizations of setting, action, and the cultural character of artifacts that characterized pre-Suchman investigations [...]. Replacing this kind of detailed empirical study with generic cultural interpretations runs the real risk that attention will be diverted away from what people do and how they organize action and interaction in diverse contexts of everyday life. In turn, this may well have a detrimental impact on the practical relation-

8 Crabtree et al.: “Ethnography Considered Harmful”, p. 879.

9 Crabtree et al.: “Ethnography Considered Harmful”, p. 879.

ship between ethnography and design that has developed over the last two decades.¹⁰

Here, then, I would suggest the argument is against an excess of speculation or imagination in the design process, and favours one which emphasises the 'vernacular voice' at the expense of textuality and other postmodern renderings.¹¹ At this point, I will say nothing about the justification or otherwise for this insistence on 'work practice' as contrasted with other approaches but want instead to make an observation about ethnomethodological work. In contrast to the tendencies remarked on above, ethnomethodology is a fundamentally modest perspective. It does not claim to be scientific¹² but adopts a standard of plausibility based on the evident fact of the known-in-common nature of our social world; it does not start from the position that professional sociologists or anthropologists (or indeed anyone else who claims an understanding of social life derived from their discipline) have privileged theoretical knowledge that provide better explanations than those of the person in the street; it does not deny that people may have different views concerning 'how the world works', and it does not assume any close relationship between the business of description and that of prescription (design). What is suggested, in the context of CSCW, is that it is worth doing good, careful empirical work which seeks to make sense of the way people – in whatever context they find themselves in – go about the perfectly ordinary and practical business of doing whatever it is that they are doing. It does not deny the possibility that there are other ways of describing the social world – as 'male dominated'; as 'socially constructed', or what have you – but it does constantly return to the issue of whether such beliefs about the nature of our political, moral, social and interactional lives are theoretically privileged, or rather reflections of the way we ordinary human beings (for that is what we are, regardless of our professional disciplinary status) decide – for the same political, moral and interactional reasons as everyone else – to describe the world in our chosen way. Most importantly, it does not claim to 'replace' anything at all.

So, at this point we have a number of candidate claims concerning method, and apparently differing claims about the role of ethnography in particular. It is this latter discussion that the paper will deal with from now on. As suggested above, we have what looks like a very modest proposal to look in detail at the work of members as against something which on the face of it is altogether more 'critical' and wide ranging. Astute readers will notice two things at this point; that there has been no mention of the consequences of this view for method, and that no attempt has been made to clarify what we might be talking about when we talk about 'design'. There are good reasons for this. Firstly, and as I will try to

10 Crabtree et al.: "Ethnography Considered Harmful", p. 880.

11 See Williams: "Sociology and the Vernacular Voice".

12 See Bittner: "Objectivity and Realism in Sociology".

show, the remedy that we seek – to balance description and prescription – is variously conceived of as consisting in a method or a methodology. Where the more ‘scientific’ approaches to empirical investigation take the view that ethnography is a flawed method, the sociologically-informed take the view that the debate is inherently methodological. That is, where differences occur between ethnomethodologists and others in respect of ethnographic practice, there are underlying agreements about the fact that ethnography is constituted in its analytic practices, not the mere fact of ‘going out and looking’. The other, and neglected, element, has to do with what we might be talking about when we talk about design and designers. It is here, I think, where some of the faultlines of the dispute become clearer, and it is worth examining some of them.

Embedded in the claims made by both Schmidt and by Crabtree¹³ et al. is the idea that the fundamental purpose of ethnographic work in relation to design is to be useful in some way. It should, in some sense, contribute directly to the design process. In contrast, Paul Dourish,¹⁴ for instance, has been read as suggesting a drawing back from this tight coupling between ethnography and ‘design’ largely in keeping with the critical claims we associate with sociology and anthropology. Now, one of the critical issues that needs to be carefully unpacked is the relationship between an ethnography (the ‘hanging around’; the grasping for ‘understanding’, for ‘interpretation’ or for explanation, depending on where one stands) and the record that is produced of that activity (in respect of ‘data’, argument, published papers, and so on) and the purposes attached to both of these things. After all, the debate we are trying to make sense of depends on what we think is going on when someone does an ethnography for whatever purpose. Dourish has indicated to me¹⁵ that the thrust of his argument is twofold: that the reporting of ethnographic results is impoverished if those results are to be considered only in terms of ‘implications for design’, and that it is a mistake to conceive of the ethnographic function as consisting in servicing a ‘design brief’. To do so, in his view, shared with Bell, is to concede too much to the mechanical, ‘engineering’ version of design we mention above. Both of these seem to me to be entirely sensible judgments, but equally they also describe a vanishingly small set of ethnographies, at least where ‘strong and successful’ coupling is concerned. In fact, ethnomethodological practice, I would suggest, does not and should not fit this model at all. Indeed, the proposal that ethnographies should, at least in the first instance, be ‘innocent’ and that ethnographies can both service design and furnish a critique at the same time has been around for a long time now.¹⁶

13 See Schmidt: “CSCW Divided”, Crabtree et al.: “Ethnography Considered Harmful”, p. 880.

14 Dourish: *Implications for Design*.

15 E-mail exchange.

16 See Hughes et al.: “From Ethnographic Record to System Design”; Randall et al.: “System Design: the Fourth Dimension”.

In any event, we might argue that in this body of work, an invitation is being issued to designers to look at things anew (a critical role) instead of the serving up of data in some putative 'service' role. Again, there is little to disagree with about the idea that some kind of critique might be embedded in our descriptions but we might not always agree about what kind of critique is most valuable, or for that matter why. Some clarification is perhaps needed here. The retreat from realism that we see in the 'postmodern' literature on ethnography¹⁷ has been accompanied by a variety of arguments concerning the relationship of different forms of ethnography to design, some of which make more sense than others and all of which tend to be conflated. My aim in exploring these issues is to try to show that 'being useful' does not entail viewing the 'critical' as against a 'service' role as mutually exclusive.

Firstly, let us be clear that the basic lens through which postmodernists tend to view the world – one which stresses the contingent, varied, and fragmented voices that one might find through a certain kind of analysis, and which contrasts with the over-unified and realist conception of culture which historically preceded it – is broadly shared by ethnomethodologists. There is sometimes an implicit confusion about 'realism' and ethnomethodology. In fact, certain similarities between the ethnomethodological and 'postmodern' project can be discerned. I do not want to exaggerate this, because they are clearly very different. Nevertheless, the postmodern complaint was precisely about the ways in which it was now impossible to believe in the old certainties established during the course of the 'Enlightenment' project. Where, in some postmodern work at least, this was taken as a license to produce alternative, and challenging, versions of 'how the world works', and to engage in a more 'reflexive' or critical approach, the very same foundation (but drawing on Wittgenstein and Schütz) was seen by ethnomethodologists from Garfinkel onwards precisely as grounds for making very modest, and commonsense, claims.

Secondly, the difference between the 'postmodern' and the ethnomethodological does not, and cannot lie, in the fact of 'critique'. Ethnomethodology has evidently been involved, implicitly and explicitly, with a critique of, for instance, cognitive science and philosophy¹⁸ and of management science.¹⁹ The difference, I will suggest, lies in the kind of critique offered.

Thirdly, the issue of the move towards 'non-work' settings is chimerical in relation to whether ethnomethodological work is possible.²⁰ As has been pointed

17 See e.g. Clifford: *Predicament of Culture*; Clifford/Marcus: *Writing Culture*; and more specifically in the context of 'design' Dourish: *Implications for Design*; Bell/Dourish: "Back to the Shed".

18 See Coulter/Sharrock: *Brain, Mind, and Human Behavior*.

19 See Bittner: "Objectivity and Realism in Sociology"; Harper et al.: *Organisational Change and Retail Finance*.

20 I do not mean to suggest that any of the protagonists to this debate think otherwise. Other commentators do, however, imagine some difference.

out on innumerable occasions,²¹ the ethnomethodological concern with 'work' has nothing to do with the sociological concern for 'paid work'. 'Work', for ethnomethodologists simply refers to the way in which people attend to whatever it is they are doing as they actively engage in doing it ('accomplish' it, in the ethnomethodological jargon). As Crabtree et al. suggest:

Ethnomethodological ethnographies have over the years showed that organized activities, or social facts in ethnomethodology's language, are at all times accomplished. It takes 'work' on the part of human beings to do them, and in the course of that 'work' participants (not ethnographers or other social or cultural analysts and interpreters) display the socially organized character of a setting.²²

That is, nothing about ethnomethodological studies limits their value to organizational and work settings in the sociological sense. It would follow that, on its own, therefore, an argument concerning 'moving out of the workplace' and the expansion of enquiry into new public and private spaces is not relevant. Having said that, choices concerning the relevance of various settings to design work most certainly are important and do not rely on an ethnomethodological view. Ethnomethodologists, as indeed is true of any other perspectival preference, can do exemplary work in relation to a given setting but do not demonstrate a necessary relevance to design in doing so. I believe this to be non-trivial and believe, equally, that in some cases debates concerning the relative merits of one preference over another may actually be less important than debates concerning what setting matters in relation to design.

Fourthly, it is by no means clear that a strong and necessary relationship between ethnography of whatever kind and design has ever been established in the workplace, and certainly not that such a relationship will exist in public and private spaces not associated with paid work. It is by no means clear that the processes of design will remain the same across the various different domains that are increasingly subject to the gaze of 'ubiquitous computing' and other interests. It is further not clear that what turns out to be – in practice – the main way in which ethnomethodologists orient to the 'lived experience' of members, and by this I mean a detailed analysis of the processual character of interaction, is necessarily relevant or necessary at all to certain kinds of design problem. In addition, the problem of design cannot be considered in the absence of a consideration of designers. Protagonists in the above debate seem to align on the basis of whether 'designers' are or are not in need of some critical remedy. What is striking about such an argument is that it takes place in a biographical and characteriological vacuum. We have no evidence of any kind about the ability of designers, whoever

21 See for instance, Randall et al.: "What about Sea Urchins?"; Crabtree et al.: "Ethnography Considered Harmful."

22 Crabtree et al.: "Ethnography Considered Harmful", p. 885.

they might be, to engage 'reflexively' in their work and it seems reasonable to suggest that this might depend in part on the kind of work they are doing and the cultural milieu they inhabit. If the reader will forgive a personal remark, I have known some people engaged in the design process who are stupid, sluggish and unimaginative. I have known others who are imaginative, brilliant and creative. Exactly the same is true of ethnomethodologists I have known.

The above points will be illustrated by reference to two recent studies, both of which the author has been substantially involved in. The first concerns the professional work of ontology-building, in this case the work of bio-informaticians as they go about the construction of a 'cell type' ontology.²³

2 CASES

2.1 THE CELL TYPE ONTOLOGY

The study formed part of a more general interest in the processes on ontology building, but focussed in this instance on a collaboration involving a small number of people over a period of months, but where some of the work took place in a geographically distributed and more or less, 'individual' way whilst other parts of the work were done face-to-face and synchronously. The study involved the videotaping of four days of collaborative work and the reconstruction through talk ('interviews') of the other aspects. Now, the main feature of this study turns out to be a contrast between the kinds of methodology typically advanced as appropriate for ontology building and the practical ways in which the work was in fact done. That is, and in line with what has been said above, there is an evident critique of existing methodologies in the work. One of the things that became increasingly clear was why it was that there was a strong preference for face-to-face work in respect of some features of the work, while other parts could safely and easily be left to more individualistic and separate strategies. A number of elements were apparent. Firstly, it was clear that the scope, size and ambition of the ontology in question had to evolve out of a set of concerns which might be glossed as 'political', but included concerns such as who in the wider community was involved in parallel work, assessments of what they were doing, and of their expertise and philosophical preferences, along with decisions about possible liaison. Hence, early in the meeting we hear,

'I want to take the OBO CTO ... which is a 'hand crafted' taxonomy ... it's a multiple hierarchy ... what X has described as a tangle ... what tends to happen when you build ontologies by hand is that you make mistakes ... what we have discovered is that one in ten of the classes has a missing or erroneous subsumption relationship on it and the

23 See Randall et al.: "What about Sea Urchins?", for a fuller account.

process of normalisation is supposed to give you reusable modules ... more maintainable lumps of hierarchy ... and highly axiomatised ontologies ... with more stuff in them ... so that you can get more computational inferences ... essentially it does all the work.'

The relevance of this lies in an implicit claim – that what exists is a 'taxonomy', not an ontology – and a more explicit statement concerning what will have to be done to turn it into an ontology. What exists is a 'tangle' which contains several different hierarchies. What exists is only a 'taxonomy', and thus does not contain enough in the way of subsumption hierarchies to do what ontologies in this view are here to do: derive computational inferences. The solution proffered is 'normalisation'. Following this, an assessment of the current state of play is made, and in part this entails arriving at some judgment about what community of people is interested in this ontology and what commitments members of that community might already have made:

R: 'from what I understand is that the OBO people have commissioned a reworking of the CTO ... and I'm perfectly happy for this to be a contribution ... but that is not something I will manage ... because the whole process would just drive me up the wall ...'

H: 'If I could comment here ... we've been using the CTO ... we started to look at the hierarchy but the fact that lots of things are not defined, they know there are lots of missing "is a" relationships' that need addressing ... They had a discussion about rebuilding the whole thing again from scratch ... start again ...'

D: 'who is 'they' here?'

H: 'active are ... [a list of names] ... the CTO doesn't have like a paid person to look after it ... originally it was [other names] and now it's just sitting there in no man's land ...'

D: 'but that no man's land is located over in Houston ...'

H: No not particularly, though most of those people are over in the US ... X is in Harvard ... right now, Y is in Boston too ... I don't know where Z is ...'

Following on from this, the group engages in the business of trying to find a 'good way' of going about the building process, which turns out to be much more difficult than first envisaged:

R.: 'The general flow of activity ... we're going to have a general look at the CTO ... we need to have a look at the axes of classification ...'

having identified the axes of classification we need to identify a primary axis of classification ... all the other axes are then pulled out into supporting ontologies ... a lot of these already exist in things like PATO [the phenotype ontology]... so the phenotype ontology, one of the axes is ploidy ... and so we need to and have a look at how ploidy is described in PATO, and then we will be able to take the actual cells at the end of the leaves ... And then for instance you can recreate the intermediate class ... but it's complete and it's dynamic and it's ... lovely.'

It is already obvious that interrogating other ontologies will form a significant part of this work, and also that the 'leaves', that is those cells that will eventually be at the bottom of the subsumption hierarchy, are not particularly important at this point. In other words, 'completeness' is not an immediate aim. Equally, a large part of the early business was to do with establishing what the right 'plan' for assembling the new version of the ontology might be. Again, very early on we see a candidate method put forward:

R: 'What I'm hoping to do in identifying the primary axis is do this somewhat formally, using Ontoclean. What Ontoclean does ... it's a way of evaluating subsumption relationships and checking that you've said the right things in the right way. It talks about unity, rigidity and identity ... unity is all about whether you're talking about parts and wholes cos one of the common mistakes is to talk about part-whole relationships as "is a" relationships'. Famously, ocean is a kind of water where water is part of ocean ... identity is all about necessity and sufficiency which I hope that, being OWL people, you're all reasonably familiar with. Rigidity is talking about things which are inherent to the ... ummm ... ah ... ah... what properties are held by an entity for the duration of its existence or only part of its existence. And what we want to do or what we should do is identify the primary axis to be a rigid property ... and helps us make a nice safe tree ...'

In effect, this means finding ways of describing cells that are always true for those types of cell (bearing in mind that cells can change over time). The attempt to delineate the function of the meeting was an important part of shaping the ontology itself through achieving a consensus about its scope and ambition. It seems obvious that such an ontology would be used by biologists, but in fact different ontologies can cover the same broad domain but model that domain very differently. For brief mention, anatomists and medics might have interests in such an ontology. Different communities of user can be and are envisaged. At the same time, decisions about how much work is to be accomplished have to be made:

L: '... was tempted to just look at cells in vivo, so basically we should just take that partition out ...'

L: 'The only reason I was interested is because it's non-canonical ... most of the ontologies in OBO are canonical ... so if you wanted to produce the full thing you essentially would make a cross product so you would take all of the terms in the one ...'

H: 'so you'd compose it out of other things but in this case the things that are being composed are part of the same ontology ... so experimentally modified cells are something I'm particularly interested in but I don't think they belong here ... this part of the CTO is really problematic and we shouldn't go there ...'

L: 'I agree with R., it's probably out of scope for this ...'

The discussions, which last a full day, result in the decision that no cell property is sufficiently rigid to form a primary axis, and therefore a new approach to the building of the ontology will be necessary. This approach, by default, will be to create a list of cells under the heading, 'cell', ascribe properties to them, and assume that if that is done correctly the reasoner will sort the cells into a hierarchy:

H: 'I think we may have got to the point where we cannot find a primitive axis ...'

R: 'well, in that case we go for the ultra normalisation ... of doing it all by restriction ... so my current proposal is that we just have cell and we list all the actual cells underneath ...'

L: 'so if we just have cell, are we making the assumption that everything in the CTO will hang under cell ... so cell functions or processes would not be a type of cell, so we should have more than one upper level ... we need classes as well as cells ...'

D: 'we need types of function ...'

L: 'we need a process hierarchy'

R: 'which, funnily enough, we have in GO ... so are we happy that we just have cell and do it all by restriction?'

L: 'well, not happy, but we haven't found any property that we can treat as rigid ...'

Roughly speaking, after this point, a 'toy' ontology is built which consists of 25 candidate cells which can be used to derive the main cell properties that will be

used in the construction of a fuller ontology. Whatever decisions they make, the work has to be doable. It remains the case that, although completeness is mentioned as an issue, the group is happy to adopt a satisfying attitude, such that what they do will be 'good enough' for their purposes. They nevertheless encounter a series of problems, including the use of terms from other ontologies, because ontologies have specific purposes and terms may be defined in different ways or may be incomplete.

At the point where the issue becomes one of populating the ontology in accordance with the various properties that have been defined, the work can effectively be done apart. In this group, there was some subdivision into pairs initially, but the bulk of the work was done subsequently – work that involves populating the ontology with 400 cell types.

By the time the group reconvenes, the serious business of developing a 'shareable' subsumption hierarchy in the ontology is underway. At this stage, of course, the process entails the identification and correction of mistakes, and what is interesting about this is the way in which different expertises are deployed. Corrective work is done by those who know:

A: 'Pericyte ... you've got it wrong ... I've just been looking it up on the web ... it's been used here as an example of a single smooth muscle cell on a blood vessel ... that is out of date, it's now known to be a primitive cell form, undifferentiated ... I found two references to this just now ... it can differentiate into, one, a macrophage, a fibroblast or a single smooth muscle cell ...'

A: 'So it develops into'

A: 'It develops into ... I can give you the reference for this ...'

R: 'how have we got it axiomatically described?'

M: 'yeah, it's 'located in' blood vessels, 'participates in' angiogenesis, and 'participates in' blood vessel [...] and 'participates in' organisation of an anatomical structure'

R: 'so we're saying all this is wrong ...'

The important feature of this, in our view, is that this corrective work is very much a product of the social distribution of expertise. Even the most expert of cell biologists may fail to recognise issues which are outside of their immediate area of interest. Just as importantly, the artefacts that are deployed in order to find relevant information are numerous:

S: 'R, I've just put the list up on the screen ... I just extracted all the terms ... there's a thousand here ... is it useful just to scroll down it?'

[on screen, S. navigates through]

R: 'as we go through the screens, can someone have OBOedit open?'

D: 'yes, but how do you do search in OBOedit ...'

S: 'you use term filter ...'

M: 'so, we've got the list ...' [appears on screen with IDs]

H: 'have you got obsolete terms in there as well ...'

M: 'yes ...'

H: 'better to invert them, cos the high numbers are likely to be leaf nodes ...'

R: 'good point ...'

L: 'course, now we're going to have terms where we have no idea what they mean ...'

H: 'Wikipedia man ...trophectodermal cell'

S: 'No, there are no definitions for trophectodermal cell

... so not that one ...'

[they proceed down the list. M reads aloud]

R: 'we can record these in the spreadsheet , H.'

H: 'and the Wiki ...'

R: 'don't forget the implicit categories ...'

Here, in the space of less than a minute, we see the use of a number of different artefacts. They are used synchronously, or in rapid succession, and more than once there has to be an exchange of information about how best to use them.

The process of ontology building is not well-understood, and distributed ontology building even less so. As one respondent put it, "the typical answer to the question, 'how do you build a good ontology' is, 'the way we did it'". The study seems to indicate that some key elements here have been largely unrecognised. First, understanding what work is done face-to-face and in groups, and what work is more easily left to individuals or sub-groups to complete is important if we are to understand the work that makes an ontology 'shareable'. Second, the data suggests that there is very substantially more work done on defining

scope and ambition than might have previously been recognised, and that there are good reasons for thinking that this is done economically through a synchronous collaborative process. Third, a very significant part of the work involves the need to 'test' decisions. This involves the resolution of ambiguities and the correction of errors and seems to be dependent on the use and deployment of a wide range of resources in rapid succession. More generally, and if we construe our purpose here as saying something about the way in which an ontology-building process takes place in a design-relevant way, it is hard to see how it could be done satisfactorily in any other way other than close attention to the way in which things are ordered, and why. Hopefully, even a cursory look at this kind of data is enough to convince us that formal, top down model-building of the kind originally criticised by ethnomethodologists and others is not adequate to the task of representing the work involved and, indeed, can give us a very distorted view of what is going on. Now, part of the point here is the impossibility of any precision about the relationship between ethnography and design. For the ethnographer, there are at least two quite distinct things going on in this setting, each of which might be treated as 'relevant' to design. Firstly, that there is a great deal of work entailed in establishing 'why we are here'. The scope, ambition and size of the ontology in question turns out to be constitutive of the work of ontology-building, and to depend on questions that can be termed, 'political'. Now, it is often thought that ethnomethodology is incapable of dealing with the 'political'. Conversely, ethnomethodologists often criticize those who do as providing mere glosses on political matters. What I have tried to show in this brief examination of the ontology building process is that participants show a very clear understanding of the politics that is relevant to their purposes: who needs to be involved; which sections of the community have to be managed and how, and whose interests are in play.

Part of the larger study that informs this work has also revealed the very considerable problems people have in using existing 'collaborative' technologies for this knowledge intensive work. That is, when dealing with the rapid deployment of different artefacts in order to resolve terminological problems, as we have seen above, the problem does not lie simply in the fact of different artefacts, but in the way that their use is 'visible' to others, so that sense can be collaboratively made of the judgments being arrived at. The point here, much as with the 'politics', it is not the fact of collaboration that is at issue here but how in detail it is accomplished – what work, if you will, seems to necessitate this elaboration of issues. It may be that, in order for this work to be effectively done, a significant degree of face-to-face collaboration is necessary. Again, there is an obvious level of critique here, at least by implication – that top-down modeling of ontology building processes is an inadequate gloss, that 'one size fits all' approaches to collaborative technology support might not do, and so on. It may be that someone will find a brilliant design solution to problems of this kind, or that technologies will improve incrementally, or that people will find ways of using them such that

they are made 'fit for purpose'. In either event, it is entirely reasonable to suggest that ethnographic work helps us understand what kind of problem might turn out to be relevant.

2.2 SENSECAM

The second study I want to discuss was done in conjunction with Microsoft Research and the BBC in the UK, and concerns the use of a 'passive memory capture device' called *SenseCam*. It is, therefore, precisely one of those studies which takes place away from the world of 'paid work' and primarily concerns the deployment of a technology in private and public spaces. The study shared many features of an ethnomethodological enquiry, insofar as it was 'unmotivated' in respect of theory and concept, and aimed to derive a picture of 'use'. Nevertheless, it too contained elements that can only be described as 'critical'. Here, we argued that the explosion in the sheer amount of memorial material – photos, videos, text, etc. – available to people to use might well associate with some change in what we called 'reconstruction narratives'. That is, the rise of user generated content in a range of media might form part of the decline of the 'authoritative' account and a rise in polyvocalism²⁴ as the digitisation of narrative collapses the distinction between 'producer' and 'consumer', and hence democratises authority and expertise (in some views).

SenseCam is, in effect, a wearable camera which takes photographs automatically, at a rate depending on the amount of movement sensed in its environment. Our work²⁵ showed how the use of *SenseCam* might in many cases be unexpected, and seemed to occasion a number of reflections which are to do with creativity, playfulness, the tension between the ordinary and the 'strange', and our emotional lives. Thus, where people were evidently engaged in 'remembering' work, they were not using the device to remedy failures of memory in the way that was originally envisaged. In a number of separate studies, we discovered that the technology sometimes prompted particular kinds of reflection and narrative production – was being used as a vehicle for 'discovery', reflection and celebration²⁶ – in others it was scarcely used at all. This occasioned a series of reflections on our part about narrative, memory and practice in order to better understand the different ways in which families oriented to the device in question. All of these concepts are, in my view, implicated in our understanding of the way in which digital media are typically deployed. That is, we seek to understand how a 'webs of significance' are constructed and maintained through both ongoing and ordinary concerns, as manifested in the day-to-day lives of perfectly ordinary people, and in their occasioned reflections on issues of wider significance.

24 See Shirky: *Here Comes Everybody*.

25 Harper et al.: "The Past is a Different Place".

26 See Harper et al.: "The Past is a Different Place".

Technological changes, it has been suggested, result in significant changes to narrative structure, to the role of the narrator (and specifically, rights over story-telling), and the prospect of new and multi-media forms. Our argument here had to do with the way in which narrative, in practice, can be understood as a set of choices around whether, when and how to construct a story. This may seem obvious, but some rather difficult arguments about 'identity' and 'reality' are seemingly implicated in this simple observation. To put it another way, the critique we were working through was both a critique of some general philosophical, sociology and psychological assumptions and a more specific, but related, one concerning the role of technology. We discussed, in various papers²⁷ how narrative has come to mean an indeterminacy of 'meaning' associated with the death of the author and the associated decline of authorial authority. These considerations have been applied equally forcefully in postmodern arguments about 'identity', in which the implication of a single entity which constitutes continuity and coherence amongst the parts of a human life is seen to be as fictional. It is replaced, that is, by views of 'identity' as contingent, polysemic and as a discursive construction. Narratives in turn are seen to have no fixed and essential character or content. This has obvious implications for the way in which we think of memory, for it too may find its expression in a variety of ways.

There is, again, no space here to discuss the extensive literature on 'memory' to be found in the disciplines of psychology and sociology. What we observed is, roughly, that there are two main fissures in the debate about memory. Firstly, there are issues around whether memory should be conceived of as an individual 'mental' phenomenon or a social accomplishment, and secondly whether the interesting issues that associate with the concept are those to do with its fallibility, or those to do with the occasions of its display.²⁸ Sociologically, the emphasis has been on 'collective' memory and the way it functions, for instance, in rite and ritual. Notions of 'family' memory²⁹, 'collective' memory; 'habit' memory³⁰; public representation³¹ etc. seem to emphasise broadly two themes. The first is the site of expression – where acts of memory are to be found – and how this relates to notions of tradition and modernity. The second has to do with performative elements – how memories are enacted in conversation, stories, photographs, and so on.³² It was our view that these 'performative' aspects had and have clear implica-

27 See e.g. Lindley et al.: "Narrative, Memory and Practice".

28 See e.g. Neisser/Hyman: *Memory Observed*; Schacter: *The Seven Sins of Memory*, for a fairly thorough account of these issues.

29 See Halbwachs: *On Collective Memory*.

30 See Connerton: *How Societies Remember*.

31 See Brundage, William Fitzhugh: *Where These Memories Grow: History, Memory, and Southern Identity*, Chapel Hill 2000; cited in Climo/Cattell: *Social Memory and History*, p. 4.

32 See Middleton/Edwards: *Collective Remembering*; Middleton/Brown: *The Social Psychology of Experience*.

tions for the future role of user generated content in relation to professional narrative concerns. At the same time, they are equally important to our understanding of how a new technology might work, however, for they implicate moral and emotional rights and responsibilities.³³

In sum, we might expect that, where memories are evoked by a technology, this will happen in and through a 'site of expression', that they will be organized and produced performatively, and selected for individual and collective significance. Our interest here, then, was to do with the prosaic business of when and how family and other groups might go about the work of constructing narratives. To put it another way, we were interested in how 'small stories' as they are told by ordinary people come to be the memorial and narrative artefacts that are placed before us. These themes are interesting in ways that any ethnomethodologist would be familiar with; they might lead us to consider the topical relevances that occasion the introduction of stories, how stories are organized on the basis of recipient design (built for those who are to hear them), how stories are organized to be understood correctly by those for whom they are designed, and how any given story is related to other stories in the same narrative structure. Note, however, that this work is also drawing on themes that are sometimes held to be 'postmodern' especially in respect of arguments about textuality, narrative, and so on. To be clear, we engaged critically with such arguments but they were nevertheless a source for our own reflections.

2.3 ROUTINE

Our methods were forced on us by time and distance constraints, and largely consisted of post-hoc interviews. What we found had to do with various matters that included the importance of routine in family life:

Mum: 'they weren't anything like as interested as I expected them to be. Of course some of the images were rather blurry and they lost interest rather quickly.'

D: 'was it boring ... did you find it boring ...'

Son: 'mmmm ...'

2nd Mum: 'yes, our children lost interest quickly as well, and I think one of the reasons is that ... if you wear them all the time ... life is quite boring really ... there's a lot of images of me washing dishes and putting things on the line ... to be expected to look through all the different images to find that little bit ...'

33 See Misztal: Theories of Social Remembering.

This seemed, in our work, to matter particularly in relation to young children and to the parents of young children. As a 12-year-old in one family put it:

'We didn't actually do much this week. Although it was the school holiday, we didn't go anywhere ... I just hung around with my friends ...'

The recovery of the mundane could, however, be an occasion for reflection on moral consequence:

Mum: '... we talked about the number of times we seemed to be sat round the kitchen table ...'

2nd Mum: 'we noticed that too ...'

D: 'Really?'

Mum: 'yeah, we often put it to one side rather than have it hanging around our necks, and that's what we found ...'

2nd Mum: '... and driving ... phenomenal ... driving is really boring ... it prompted us to get our bikes out, actually ...'

Mum: 'the thing that I noticed was how little time I spent playing with my children ... looking at them, walking away from them, back to them, but how little time I actually spent playing with them ... depressing ...'

2nd Mum: '... and you see how just getting out of the house takes twenty five minutes ... and you have to get them to school.'

That is, the various guilts, emotions and suppressed feelings concerning the mundane, even boring, nature of the daily routine can be thrown into sharp relief. Sometimes, these reflections seemed to prompt decision-making.

2.4 PLAYFULNESS

It is not especially surprising to discover that it was young couples without children who engaged with this technology in a largely playful way. We had, for instance, provided *SenseCams* to two young couples who were friends. One couple, on an afternoon walk, recorded and kept images of each other while playing on a seesaw, and attempted some time-lapse photography of themselves walking towards a tree (see images 1-4).

*Images 1-2**Images 3-4*

As the young woman in question said:

'we went for a walk round the reservoir on Friday ... we hung it on a tree and walked really slowly towards it and it was like it skipped bits it was like we'd jumped forwards ... Mick's dead arty anyway so we were like walk dead slowly and see what happens ... on that walk as well, this makes me sound about 10, we found two trees that had fallen over and like a see-saw, so we sat on that and were jumping up and down, so Mick's are looking up the tree and mine are looking down.'

They also arranged to meet up during the week to position *SenseCams* around a room to gain different views of themselves having an evening of games. In these cases, for the most part, the overriding impression was one of playfulness and indeed, by their own admission, the evening became progressively more chaotic, fuelled by the consumption of alcohol:



Image 5

In findings that resonate with previous work by Harper et al.,³⁴ the candid nature of *SenseCam* was appreciated by these young couples. This pleasure seemed sometimes to be simply artistic, reflecting the unusual pictures that *SenseCam* can capture:

Male: 'I prefer stuff like that, not like...'

Female: '...posed'

Male: '...yeah not posed, but not like perfect either, I just think you can find angles and perspective that you can't find ... I think there's something a bit more creative about it ... you get some really unexpected results.'

In other cases, enjoyment was derived from how the photos could be interpreted, especially when they seemed to say something about relationships with one another:

Female: 'did you not watch how you interact with each other as well, cos I found that interesting, like M., not consciously, but holds my hand when we're driving sometimes, and it kinds of made me go, oh how cute, you know just little things that you don't really notice.'

2.5 THE SENSE OF A FAMILY

All of these features can, in some sense, be described in terms of the way in which people orient to family life, and the (again not especially surprising) discovery that they may do so in a variety of ways:

Mum: 'I tended to hang behind ... I looked at Jonas [the grandson] when he was playing with the crabs ... Peter started to use his phone

34 See Harper et al.: "The Past is a Different Place".

and I got curious about how long he was on the mobile phone and he's was on the phone for at least 45 minutes, maybe an hour'

Gran: 'and I noticed the day before how often he was texting ...'

Grandad: 'I was saying the other day that, if you had storage capacity ... and it was wireless so you could download it automatically and you could record your grandkids ... do you remember when you had just one camera ...'

Gran: '... yeah, do you remember the Brownie camera, when you could take just eight photos.'

Grandad: 'And, you know, when you're sitting around telling a story, you know, do you remember when, and you could just get it out ... the laptop ...'

Mum: 'you see, when you run through it ... you see him fiddling with his Transformers ... he's at it all the time ... never stops ... and I'm just driving to Sainsbury's ... and then he puts them in a line and takes a picture of them, cos he remembered the SenseCam was there ...'

Mum: 'and then we're in the queue, waiting and waiting ... the 'enter' button in the Chip and Pin was stuck ...'

Mum: 'you see, he packed the bags for me ...'

Mum: 'I think we've got a clip of him doing his Easter egg hunt on Sunday ...'

[searches]

Gran: 'hmmm ...'

Grandad: 'ahhh ...'

Gran: 'he made a little Easter tree as well ... he got some twigs together and put em in a vase, then he just opened his eggs up ... it were lovely ...'

Grandad: '... ohhh, its brilliant that bit ...'

What is evoked here is more than the sense of routine that was mentioned above, and more than the playfulness also described. This is evidently emotional work – work which locates and fixes the roles of family members and their evolving his-

tory and work which, equally, locates the different roles of grandparents, parents and children in the emotional life of a family.

There are a number of ways in which we can interrogate this work. It relies on post-hoc reconstructions of activity, rather than the activities themselves – there are evidently ‘recordings’ of activity, but they are scarcely the recordings we normally associate with ethnomethodological work. It relies on ‘interviews’, albeit interviews of the most informal and friendly kind, and thus is open to the suggestion that participants are designing their responses in relation to what they believe the interviewer wants to know. The ‘narrative reconstructions’ we mentioned above are evidently ‘performances’. They are equally, however, occasioned and produced in quite specific ways. These retrospective viewings can be seen as vehicles for producing stories which pertain to the significance of family life. We have also seen how memories are interactionally produced out of a number of occasioned purposes, notably as a resource for sharing family experiences at ‘get togethers’. The central point in this respect is that narrative construction and delivery is a skilled and collaborative activity. A capacity for narrative is commonplace, and people are generally capable of producing ‘small stories’, of using narratives in their everyday conversations to deliver news, update people, amuse them, instruct them and so forth.

The performances we observed as people recounted their experiences to us, normally in the presence of other participants, reflected the identities they constructed for themselves at these moments. Our data seems to show quite clearly that who one is in respect of family life or social network powerfully affects not only the use one will, or will not, put *SenseCam* to but also the way in which one will subsequently talk about it. If *SenseCam* is little more than an encumbrance which intrudes upon the day, as it is for children, it is because their identities as children are produced out of a vivid sense of the world as either mundane or exciting. It sometimes, for busy parents, occasions reflection of a moral kind precisely because the family is a moral unit, one where questions such as, ‘what kind of parent am I?’, ‘how boring or otherwise is my day and can I make it better?’ and so on go into the production of an identity as busy parent. For young adults, without the encumbrance of children, it is a vehicle for celebrating relationships, more often than not in playful ways. Arguably, such relationships involve appreciably less concern for reflection on the mundane. For older people, specially those with grandchildren, reflections take a different form. They are, if you will, celebratory in a different way, reflecting grandparents’ position as custodians of family history. The use or otherwise of *SenseCam*, in other words, was intimately connected with identity questions, with the record being interrogated for what it showed about the kinds of people that the individuals making the recording and those they associated with were like. This can be glossed, as suggested, as associating with the ‘stages’ of family life in question, and the roles individuals play within it. And what does this mean for design? Evidently, the idea that there is a tight coupling here would be fatuous. Nevertheless, there are real implications.

We might argue that the data shows that naïve conceptions of ‘memory’ do not furnish the design of an artifact which supports them, but that people are eminently capable of appropriating the technology for purposes that suit them. We can argue that the data pointed to various ways in which the technology could be developed: to, for instance, further develop collaborative or performative aspects, to allow for more immediate exchange of ‘viewpoint’, even to suggest – as we did – that there might be certain work-related arenas where ‘memory work’ might be suitably supported by such a technology (we had in mind areas where attention to what is going around a ‘work’ area is necessarily difficult and thus aids to recall might be needed.)

3 CONCLUSION

I suggested above that there are a number of arguments concerning the relationship between ethnography and design, and the putative preference for one form over another, that we should take very seriously. As with so many aspects of intellectual life, the debate can be cast as one about the relative merits of rigour and imagination. I started by defending ethnomethodological practices on the basis that they provide careful, rigorous and modest accounts of the way in which people go about their business, whatever that might be. In rejecting, as a matter of choice, any form of analytic privilege, it seeks in its entirety to produce plausible accounts of a known-in-common world where people orient in practical ways to the settings they find themselves in and actively construct. The point was also made that ethnomethodology clearly entails a critique of some kind of existing theoretical or conceptual stances but does not seek to replace them. Indeed, as is evident in the second piece of research mentioned above, it can explicitly trade on them. The question in respect of the so-called postmodern turn, however, is whether those studies also seek to replace anything and I can see no clear evidence that they do. If what I have described as ‘professionally privileged’ accounts do in fact exclude the possibility of other ways of looking at data, or taking it seriously, then there would indeed be grounds for complaint but it is hard to see why they would need to do that.³⁵

Another element that has been extensively debated by the various protagonists in the debate is the ‘relationship to design’. We saw how at least two different conceptions of ‘use’ are in play – one which sees some coupling between data and design, and one which advocates a step back from that presumption (but which nowhere that I can identify suggests one cannot usefully couple these two things) in order to reflect more critically on the process and thereby serve another purpose. This is the point, of course, because the argument is precisely an

35 I should state here that I have no interest in engaging in debates about the merits or otherwise of any particular piece of published empirical work or any piece of work which represents the way in which design decisions were arrived at.

argument about purpose. Dourish³⁶ has suggested that the links between ethnography and design need not be represented in such a way as to needlessly couple ethnography and design. One obvious feature of ethnographic practice, as opposed to the way it is represented in published work, in reports, or what have you, is that any given ethnography can do any of these things. Data can be made to serve any number of purposes. One issue is evidently, as stated above, that no strong relationship between ethnography of whatever kind and design has ever been established in the workplace or elsewhere. How strongly a relationship, however, is going to depend on the quality of the ethnographic work done (under whatever auspices) and, just as importantly, the work of establishing relevance. This latter issue is, I believe, extremely important, and does not directly arrive out of any perspectival stance. The ethnomethodological commitments outlined above do not produce for us relevant settings, or relevant choices about how to deal with aspects of those settings because they must somehow be related to the design questions that someone, somewhere, has (and there is no particular reason to suppose that design questions are only asked by 'designers' except in the narrow, and tautological, sense). These questions could well be the product of a critical imagination, and may well be important, but they are simply not the same questions that ethnomethodologists would ask. They can nevertheless be questions that ethnomethodologists can trade on, and I have tried to show that the results can be fruitful. Once we have made choices about the kinds of questions we are interested in, and where we might find answers to them, of course, then we can approach them in ways recommended by ethnomethodologists if we choose. In design terms, in other words, what to look at it is just as important as how to look at it.

The second issue is, as Dourish indicates, one which implicates a political or moral universe. There is no question that engaging in something like the 'propositional character of interaction' – a commonly used trope in ethnomethodological work – is not going to provide us with answers to questions of that kind and they may well be questions worth asking. What I have tried to show above, however, is that a commitment to a plausible rendering of 'lived experience' can at least incorporate the way in which people typically orient to politics, morality, emotional life, sentiment, and so on. What we do with that subsequently largely depends on our own commitments. The issue of what is a necessary or sufficient stance to take in relation to all the different design problems and policies one might encounter, including the different ways in which the protagonists construe their tasks, is not one that is solved by casting the debate in terms of the 'replacement' of one stance by another. Ethnomethodology is not, and never was, a suitable vehicle for solving moral, political or social problems (whether other stances are more successful is a matter for one's own enthusiasms), and arguably 'postmodern' stances are not appropriate for providing the kind of detailed, sometimes

36 Dourish: "Implications for Design".

processual, data that ethnomethodologists wish to provide, nor for the kind of analytic work that ethnomethodologists wish to do. This also implicates the vexed question of 'what the designer does or does not know'. Crabtree et al. are insistent that:

We do not dispute the need for critical reflection in design or any other technical practice as that notion is ordinarily understood [...] but then we would argue that designers and users are already possessed of that faculty. As Garfinkel [...] argued long ago, members – be they 'designers' or 'users' – are not in real life the cultural dopes represented in theoretical models of society and the value schemes they represent and reflect. We would also argue, as any member of the HCI community will recognize, that social responsibility has long been a core concern of designers and is a recurrent theme at CHI.³⁷

Dourish, apparently in contrast, suggests:

I will argue two primary points. First, that the focus on implications for design is misplaced, misconstruing the nature of the ethnographic enterprise; and second, that, ironically, in so doing, it misses where ethnographic inquiry can provide major insight and benefit for HCI researchers.

Sometimes, after all, the most effective outcome of a study might be to recommend what should not be built rather than to recommend what should. More to the point, an analysis of the cultural and social organization of some particular setting or occasion is often best articulated independently of specific systems, technologies, or design briefs.³⁸

Of course, my point is that the contrast is illusory. I do not want to engage in a discussion about whether 'misrepresentation' is what is going on or what is 'best' might turn out to be, but it is clear that in this view design can somehow be remedied by an ethnographic engagement independent of specific design decisions or evaluations. And so it might. The ethnomethodological critique (for that is what it is) of the notion that people are 'cultural dopes' or 'puppets' is not, however, particularly relevant here. Garfinkel's argument traded on a specific, and entirely justified, criticism of a sociological approach which sees people as subject to various social influences or causes. In contrast, he wanted to point to the ways in which people are demonstrably interactionally competent, and so they are. The concept of 'member' means precisely that a set of skills and com-

37 Crabtree et al: "Ethnography Considered Harmful", p. 884.

38 Dourish: "Implications for Design", p. 879.

petences are displayed in the production of the setting in question. It does not problematise the idea that knowledge, expertise and even political awareness may be socially distributed, or distributed in such a way that it draws the disapproving look of the cultural anthropologist. In sum, my aim in exploring these issues is to try to show that 'being useful' does not entail 'critical' as against 'service' positions but can easily entail both. Indeed, many years ago we wrote a paper arguing exactly that.³⁹ Where I entirely agree with Crabtree et al.⁴⁰ that 'replacing' careful empirical work with cultural generalisations is not desirable, it is not apparent to me that this is happening. The careful analysis of 'situated' activity is a modest endeavour which is a useful place to start in relation to design work. It is not the only place to start, nor necessarily the best, nor is it obvious that that it will answer all the many and complex questions we choose to ask when we talk about 'design'. Of course, a singular difficulty in any such discussion would be the difficulty (impossibility) of demonstrating the superiority of any approach to ethnography on empirical grounds. The reasons for this are obvious. We would need an agreement over what a 'good' design outcome was, and an agreement about the connections between the various steps that led to it and how to prove it (i.e. that designers were, in fact, paying any attention to ethnographic results and that this could be demonstrated). We have none of those things. We suggested in 1992 that,

we describe the dialogues involved in rendering these observations 'informative' for systems design, and the mutual translations implied in attempting to reconcile sociological with software engineering questions about supporting the work. We conclude by specifying some features of cooperative work which an engineering approach is in danger of overlooking.⁴¹

That there is a continuing need to engage in these dialogues and to critically engage with over-mechanical conceptions of design seems to be, even so, something that all parties can agree on.

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