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FIELDWORK AND ETHNOGRAPHY: A PERSPECTIVE FROM CSCW

DAVE RANDALL
Manchester Metropolitan University

RICHARD HARPER
Microsoft Research Cambridge

MARK ROUNCEFIELD
Lancaster University

PREAMBLE

What value does 'ethnography' have in the design of organizational and technological change? We ask this question in light of the fact that ethnography, whatever it might mean or entail, has been a key component of systems and organization design research for some time and has become—seemingly unproblematically—almost the *sine qua non* of contemporary practice in Computer Supported Collaborative Work (CSCW), the area in which we have plied our trade. Indeed, one can plausibly claim that CSCW was the first (and conceivably remains the only) interdisciplinary perspective in which some version of fieldwork, namely of an ethnographic kind, has become the default mechanism for intervening in design.

On the face of it, however, the dominance of 'ethnography' as this default fieldwork approach in CSCW sits rather uneasily beside the contested nature of ethnography, and particularly the examination of the reflexive relationship between fieldworker, subject and field, that preoccupies contemporary anthropology and sociology. If, for CSCW, ethnography is almost a taken for granted approach, in these other disciplines, almost nothing about 'it' (or even that it is an 'it') is taken for granted (1). Associated with this indifference, we suggest, are both benefits and costs. The benefits have to do with the way in which a 'body of practices' have quickly grown up and been maintained in CSCW. These were adapted to a set of interdisciplinary research problems that, during the 1990s especially, were core issues for CSCW (See Anderson, 1994). As it happens—though as we shall see this is significant—these were not and indeed are not the same as those that confront anthropology or sociology or other non-design related disciplines. Whatever the concerns of these disciplines however, we would argue that CSCW did quite well out of this adaptation, applying various fieldwork-based techniques to solve particular design questions. Nevertheless, and as we say, it came at a cost; one that CSCW practitioners are only now paying for. The costs are to do with the suitability of that 'body of practices' for engaging in a set of new and arguably more complex research problems: whereas, in the 1990's the issues that confronted CSCW were related to understanding 'plans' as descriptors and aides in organisational action (in the work of Suchman and especially Schmidt, 1997), the role of 'local knowledge' in large scale organizational processes (in our work and numerous others), and so on, now CSCW researchers are perplexed by such things as the 'moral order' of home settings (See Harper, Ed, 2003), the social

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organization of information production and use in public spaces (See O'Hara, et al, 2003), and the intersection of novel technologies in each.

Now without wanting to explain why there has been this shift in topic or why the body of practices developed in the 1990's does not provide all that is necessary to orient to these problems, we raise this issue now since it seems to us that an exploration of how CSCW might solve this current dilemma can be a pretext for saying something of relevance to the community that will read this paper. Whether our view is right or wrong, it seems to us that an answer to CSCW's current dilemmas might be produced by considering the distinction between disciplinary assumptions about *method*, substantive disciplinary *concerns*, and disciplinary *sensibilities.* More particularly, we contend that to undertake ethnographic fieldwork for the home or for public spaces (and other new or in some ways perplexing domains) and attending to the potentialities of new technologies requires a particular open-mindedness about method, a thoughtful selection of concerns, and an artful refinement of disciplinary, particularly design-oriented sensibilities. These cannot be taken lock, stock and barrel from other disciplines (say anthropology or, within sociology, ethnomethodology), it seems to us, since they have to be 'worked out', 'worked at' and 'tested' through trial and error. This does not mean that they have to be created from scratch, either. It seems to us that they can be built on what has been learnt about adapting ethnographic fieldwork practices to design concerns over the past decade or more of CSCW research, and indeed from what has been learnt about the use of fieldwork techniques in other design oriented disciplines too, such as human factors and human computer interaction. The problems that confront CSCW are new, we know; and their solution will be new, also, we believe. But it is our view that their solution will partly come out of what has been crafted in the trials and errors and testings of fieldwork in numerous disciplines, including CSCW, in the past. These can be combined with some new thinking, some new trials, some new testings. The result, we hope, will be a new body or practices, ones central to the concerns of CSCW and possibly–but only possibly–for other design oriented disciplines as they start to deal with new issues at the start of the twenty-first century.

ASSUMPTIONS AND ISSUES

In CSCW, the ethnographic turn from the late eighties onwards was (and still largely is) predicated on a–more or less–ethnomethodological version of what ethnography entailed or might be. This approach claimed (its practitioners still do, as we shall see) to be unique in its concern for understanding the subtleties of work as it occurs *in situ*. This claim was made despite the fact that some version of fieldwork was then and is now common in other design oriented disciplines; indeed, it is a concern that has at times preoccupied Scandinavian and German 'work science'; French 'cognitive ergonomics', human factors, computer machine interaction, and even, most recently, the industrial design communities.

We want to start our discussions by asking, then, what assumptions are contained in any of the versions of fieldwork that typically get practiced in design? It's not that we want to say that any approach is right or wrong, or that this approach is ethnographic and that is not; we want to start by saying, hold on, just what do we mean when we say fieldwork in design? Is it all of a muchness? Or is there a difference when it comes to design between, say ethnographic styles or other styles? Certainly, we know there is a difference in ethnographic research when practiced in anthropology and let us say, ethnography when practiced by archaeologists just as there is a difference between these and ethnomethodological ethnography–but that is not our concern. The issue is what is special when 'it' (whatever it is) is applied or linked to design?

We propose to get there by exploring, albeit briefly, different texts reporting on what fieldwork might entail. The texts in question are *Cognitive Work Analysis* (Vicente, 1999), *Contextual Inquiry* (Beyer and Holzblatt, 1998) and *Designing Collaborative Systems* (Crabtree, 2003). These are all concerned, in one way or another, with design and fieldwork. They represent what we will call (no doubt unfairly) a human factors (with a cognitive science slant) viewpoint; a practical Human Computer Interaction (HCI) approach; and an ethnomethodological one, respectively. Unfortunately, they don't tell us all that we need to think about as we confront the new problems for CSCW. For this we will turn to some one primarily concerned with fieldwork in anthropology, which of course means ethnography. Given what we have said about the extent of fieldwork and design it might seem odd to look here, but if the reader can bear with us till the end of our paper then perhaps they will see the merits of doing so. In any event, Marcus's *Ethnography through Thick and Thin* (1998) offers what one might call a post-'postmodern' view, but addresses a question that is fundamental to any and all fieldwork practices: how the analytic foci of those practices determine the nature of the enterprise itself. When consideration of this is combined with the insights and learnings we can take from the other more design oriented books, we think that we will then have enough to propose some solutions to what fieldwork in design might be, most especially of the ethnographic kind and, perhaps, some directions for how we might solve some of CSCW's current concerns.

A SET OF COMPETING VIEWS

As we noted from the first, fieldwork for design has existed outside of the sociological and anthropological traditions for some time. Vicente's book *Cognitive Work Analysis* represents a fairly typical view of how it might work from within human factors and related disciplines. The book reports methods intended to help specify socio-technical systems that go well beyond current systems. Designing for the future, he claims, requires understanding of the constraints that shape an environment. These constraints are both human and technical. To get to these requires a mixture of data gathering techniques, one of which is the task of fieldwork. Fieldwork is useful, according to Vicente, since it provides evidence about how workers actually do their work and the skills that are necessary in that real work. Fieldwork data may be contrasted with data produced by studies that tend to over-idealise work, especially those which use laboratory data-gathering tools alone. These Vicente calls 'normative'. But in addition to the use of the fieldwork 'technique', Vicente also thinks one needs evidence from studies of cognition, ergonomic models of body movement, and much else beside.

Now, though we don't want to go in to too much detail about the human factors approach here, it is worth delving in to it a little bit if only to provide some evidence for later discussions. It can also provide us a chance to draw the readers attention to the fact that some of the claims made in anthropology and sociology about the need for ethnography (and the nature of that 'need') are pretty common in other disciplines too, even when they don't use the term ethnography themselves, preferring the more neutral and all encompassing term, fieldwork, as does Vicente.

More particularly, Vicente claims that fieldwork of his kind (and we are wanting to claim it could be others' kinds too) is intended to uncover five characteristics or issues. They are: first, that it should describe and explain how individuals deal with what Vicente calls context variability, with everyday contingency. It should describe how work is social and socially organised, even in those situations where work might seem at first glance solitary. It should show how workers have developed tools that enable them to reduce the mental work that a task might demand, tools that reduce the strains on what

Vicente calls cognitive processing capacities (this is beginning to get away from the social and more towards the cognitive sciences). These tools may take many forms: lists and memoranda, wall charts, displays and so on. It should demonstrate how historical factors have influenced the current organization of the work (irrespective of whether those influences are good or bad). Finally, fieldwork should present the techniques that workers devise to get work done despite the contingencies that arise and also within severe time constraints.

These claims are not controversial–they would be recognizable to many ethnomethodologists, for example (leaving aside the issue of a quite distinctive disciplinary language) as 'stuff worth looking at'. The point Vicente is making is that these analytic interests enable certain kinds of design related work to be done–conceptualizing the information and skills needed to deal with contingency, for example, along with more generic sets of skill that enable the 'right' work to be done: these are concerns when judging what a newly designed system might do. Taken as a whole, knowledge of these sorts of skills, tasks and strategies enable the specification of the competencies that workers need, and this enables a system to be designed that ensures that these competencies are still sufficient to make the work interesting and rich enough to ensure worker motivation.

We should point out that there is much here we find problematic but we do, nevertheless, want to highlight three particular features. These have to do with the unique claims of this approach, rather than the gross similarities with other approaches (some of which we have alluded to). To begin with, it clearly invokes fieldwork as a corrective to other methods, and vice versa. As we shall see, this is something of a leitmotif in all the texts we deal with, irrespective of the particular view they have on fieldwork. Second he views fieldwork data as necessary for design but is not sufficient for design. The next two books we deal with have different views on this: Beyer and Holzblatt saying that 'it' (fieldwork data) is adequate given that it is rendered for design from the outset; Crabtree because 'it' is the 'right stuff' since it is ethnomethodological. Third, Vicente's argument is specifically applied to what, for convenience, one can call 'command and control' systems. That is, to tightly coupled, time- and safety critical work. As we shall see, there has been a tendency to focus on these in CSCW too, especially when the discipline first developed, but now it is trying to deal with other less tightly focused domains and this is causing it problems. The limits one might see in Vicente's work, then, might be reflected in the limits of CSCW's historical expertise with this move to other places. Fourth, for Vicente, fieldwork is pretty much unproblematically a matter of recording data. It therefore requires no particular skill or expertise beyond a training in the 'method' (which often has to do with such matters as how to point a video camera and index the resulting data). The serious analytic work is done within the modeling process.

It should be clear then Vicente is not deluded as to the nature of work practices and seeks to avoid over idealising them. He is arguing for an understanding of contingency, nuanced skills and complexity which is broadly similar to the kinds of understanding we might associate with various positions in sociology, for example (about which we shall say something shortly), though with some obviously significant, not to say stark, differences in respect of the role of modeling. What is curious to us, however, is that–given all this–his approach seems to imply that data collection in the field hinges (merely) on fieldwork method, for which one imagines only a little training is required. Yet it seems to us that this is to avoid questions (bigger ones in our mind) about what one might look for. After all is it certain or so clear what is, say, a work skill or a control task might be? Can a fieldworker simply go and gather data about them? How are these to be distinguished, described and clarified? All of these questions, it seems to us, need to be dealt with before the modeling starts. Of course such concerns will sound familiar to those in the anthropological tradition where the question of what one sees and

how it is bound up with how one sees (ones' subjectivity if you will) has long been a bug bear. We shall come back to this.

In any event, another concern we want to raise has to do with how Vicente holds that fieldwork data is in need of a remedy. Getting the raw stuff is easy, he implies, but he then claims it requires correction: in particular, it needs abstraction. At this point, of course, the 'ethnographic practitioners' from anthropology might be getting queasy, thinking that such modeling would mean losing the sense of verisimilitude, of situatedness, that makes their kind of fieldwork 'real'. We don't think this kind of contrast is quite as clear cut as it seems. As with some other issues, we shall come back to this but suffice it to say that, the need to model, to abstract, raises an issue of comparison that we consider deeply relevant to the problems of design we discuss later. For us, when it comes to design, comparison is at least as important as verisimilitude; for others, less interested in design, it is this verisimilitude that matters above all else (though as it happens this does not mean that these others are agreed as to what this verisimilitude might be or how to attain it). This does not obviate treating the issue of what kind of comparative analysis might turn out to be relevant to design seriously. Vicente thinks modeling and abstraction as a way forward; we think comparison of various kinds and of various current contexts can offer a way forward too, perhaps even a better way to see what the future might hold when one is thinking about how to design for it.

In any case, another concern we want to note has to do what one might call problems of method. If we have noted that for Vicente, fieldwork is merely descriptive and hence requires relatively little, if any, disciplinary training, we ought to note too that in contrast, design is based on a kind of imaginative exercise, worked through but extending well beyond the 'here and now' concerns evoked by fieldwork data. In other words, *contra* the claim by some disciplines that engineering–like disciplines such as human factors are empiricist, positivistic and even 'scientistic', Vicente is arguing the exact reverse: he offers a way to transcend the data, if you like (though quite how much imagination comes in to play as a result is another issue, one that we will comeback to). We do not want to take sides here, but it is certainly worth noting. Besides, it is our view that much of this debate (about empiricism of one sort or another), which sometimes invades discussion about fieldwork and design, is miscast.

Be that as it may, let us now turn to Beyer and Holzblatt's *Contextual Inquiry.* This is now the most popular textbook used to teach the requirements capture for interactive systems design in North America. It is intended to be used as a reference guide, a book of rules-of-thumb and maxims of conduct, for students and practitioners in industrial settings. In part its success has to do with how it lays out some of the practical tools and techniques that need to be used in any design process in ways that can be easily understood, in part because in the process it strips out any reference to how these rules of thumb have emerged through the prior (and canonical) research traditions to be found in anthropology, sociology, HCI and our own patch, CSCW.

Whatever their disciplinary roots, Beyer and Holzblatt treat the business of data collection as a matter of method, where fieldwork data is a remedy for other, less accomplished methods. Unlike Vicente, though, Contextual Inquiry has a view about labour or rather about how many people supply it. In anthropology, researchers can and all too often do squabble over which of them has rights to report particular places, as if ethnography was essentially a monopolistic, sole trader enterprise. In Contextual Inquiry, by contrast, it is a team business. Indeed, much play is made of multi-person data gathering mechanisms, all of which are oriented to producing evidence that can be agreed by everyone. Affinity analyses, for example, entail placing Post-It notes on whiteboards to represent different skills and elements in a work process, and by having more than one person interview participants within any

process, the consistency with which these Post-It notes are arranged can be used as a tool to force reconsideration and examination until there is some consistency of view. Affinity analyses are not about highlighting dissimilarity and specificity, but commonality and unity of interpretation.

Though collaborative agreement about the significance of data is a goal in Contextual Inquiry, the fieldwork as a whole is not data driven but design driven. Contextual Inquiry treats fieldwork as only one part, the first, in a three-step set of actions: fieldwork, design and implementation. And as a consequence of this, what fieldwork entails, how it is organised and documented, is always construed in reference to the larger purpose at hand: design. There is no sense in which fieldwork can be done by, let us say, one individual or team and then handed over to the next team who undertake subsequent tasks. Though there is a clear distinction between the skills used at different points in a Contextual Inquiry—interface design in particular having an especial role—the process of undertaking fieldwork is structured from the outset around the needs of design and the practical process of implementation.

Space precludes saying much more about Contextual Inquiry at the moment. Suffice to say that we can delineate some notable similarities and differences between this approach and the Vicente's. Firstly, both concur in viewing the merits of fieldwork in its superiority as a method. It is contrasted with the failure of other 'methods', interviewing being a notable example. In the latter, the tacit nature of some knowledge remains just that, tacit, and hence the data resulting from this method will be impoverished. A difference between them is that for Contextual Inquiry fieldwork is not merely a matter of data collection but of a structured orientation to design questions. A further difference is that in Contextual Inquiry data analysis is a team-based activity in which uncertainty is progressively eliminated by a move towards consensus. Put another way, data needs to be 'worked up' in and through a process of collaborative design reflection.

We want to say something about what we can usefully appropriate and what we find more problematic. We would like to use the word 'sensibility' here to give a clue as to our view on this. We are unimpressed with any argument that proposes that say, ethnographic fieldwork as superior by dint of its status as a method. In our view, no single form of methodical form of data collection, and hence the data thereby produced, can, in and of itself, be regarded as superior to another, except in respect of particular purposes. The purposes in question are, in our business CSCW, related to design. Another related issue we want to note has to do how there are some subtle but important differences between Vicente and Beyer and Holzblatt on this issue. The former sets out circumstances where he feels fieldwork data are inadequate to the design of the future–at least in some critical system cases; Beyer and Holzblatt assert the superiority of fieldwork data over other data for design of the future, at very least in terms of future interface design. We want to suggest that both might be right *and* wrong, depending on what kind of design problem is at hand. At the same time, Beyer and Holzblatt hit on something very important (if slightly controversial) in their insistence that data must orient in some way to design. What to look for, how to look for it, and how to assess its significance are, from the outset, design-related matters. Contrast this with Vicente's approach to fieldwork: that it's just a matter of collecting data and that, given some basic training almost anyone could do it. Part of the difference, of course relates to the fact that the fieldworker in Contextual Inquiry is part of a team though it is more than that. Through an iterative process, this team reflects on the data and offers suggestions as to how to go and look again. Seeing leads to understanding and in turn to demands for seeing differently. That is, what might be relevant, what is worth looking for, what might be documented and shared, is not clear beforehand but must emerge. It seems to us that this sensitivity to the problem of data is right: the problem is not one of how to capture it, it is *what* to capture (2).

As suggested above, however, the key here is the different treatment of the future. For Vicente, future design–design beyond minor iterations–cannot be based solely on knowledge of current practice, no matter how sophisticated. For Beyer and Holzblatt it can, as long as the data is subjected to a rigorous and consensual treatment by an assemblage of experts in a team. Now, it seems to us that whatever one might think about the relationship between how things are done now and how they might be done tomorrow, the real issue is one of imagination: how to imagine what the future might be. Here, there is a substantive difference. For Vicente the future can be imagined without creative thought, paradoxically without imagination: simply through the rendering of the constraints, and other factors in graphical forms. He outlines a tool kit for presenting systems in ever reducing hierarchical orders so that the mind's eye can see what might be. Process produces imagination. In contrast, it seems to us that Beyer and Holzblatt offer rules of thumbs and lists of topics to look at as if they were sufficient to ensure that the fieldworker can imaginatively see what might be, even as they look at what is. If one offers a technique for mechanical imagination post-fieldwork then the other offers tools that ensure that imagination is deployed in a manageable way in the field.

We are skeptical of both, though for different reasons. As to Contextual Inquiry, one of the problems we see here is that when Beyer and Holstblatt offer lists of such things as artifacts and 'work arounds' and so forth as topics to be looked at in the field they don't explore how these topics have arisen as being the-kinds-of-things-worth-looking-at in the first place. Now of course, they might not mention these histories because theirs is a textbook and cannot encompass scholarly reflections of prior research. But it seems to us that in eschewing any such discussion their book gives the impression that one only needs a list to be imaginative. In contrast, we think that being imaginative is cultivated through becoming familiar with the subtle ways in which prior studies have uncovered and explored issues; oftentimes through what one refer to as the sensibility of the particular discipline in question: from ethnomethodology, for example, or through anthropology, or indeed CSCW. Whatever these sensibilities might be (we shall come back to them), what we want to suggest now is that 'success in the field' depends, in large part, on understanding prior studies: their pitfalls as well as their successes. Knowing this does not guarantee imagination, as if scholarship and creativity were the same thing. It does however, it seems to us, provide the basis–and so we can now point to the sensibility we are sketching–that allows creative looking in *ever more complex or differentiated domains*. Our own (sometimes bitter) experience of undertaking, managing and assessing fieldwork prompts this view. We will discuss some of these experiences below.

Thus we find ourselves sympathetic to Vicente's claim that looking at the present is not sufficient to imagine a future. But oddly enough we don't think that one only needs to bring in materials from other sciences and make graphical, abstracting schemas that let the mind's eye wonder–more lookings at more places is, we think, also a fairly good way of getting to see what might be. Bearing in mind other ways of thinking about socio-technical systems can bear fruit, we nevertheless think that the future can also be seen around us if only we had the wherewithal, historical, comparative, to see it. Let us put this another way: it seems to us that one of these books allows a designer's sensibility to suffuse fieldwork, but somehow makes that sensibility appear mechanical and this, in our view, renders fieldwork devoid of what is sometimes called the ethnographic imagination. The other book, in seeking to produce formal apparatus that can foster design imagination, makes the process of design mechanical. Neither, we believe, is satisfactory, certainly for what fieldwork-for-design might do, all the more so as CSCW, as our concern here, moves to address new domains and technologies.

The next book we want to consider, *Designing Collaborative Systems*, we deal with because of its explicitly ethnomethodological orientation, as we have noted one common in the CSCW literature.

Crabtree restates, as does Suchman before him, the distinctive quality of ethnomethodological work. He explains that the ethnomethodological programme of inquiries was developed as a way of rejecting and moving on from some of the more commonplace approaches to studies of work within sociology in the 1960s. These tended to look at work not with an interest in how it is done so much as with a focus on how traditional sociological topics, gender, power, and so forth, were manifest in that work. These concerns may result, Crabtree reports, in neglect of many other aspects of the work. More strongly, he argues, if one lets the data speak for itself, rather than expecting to see certain kinds of things, then it may be the case that those expected concerns would be reduced in significance. When he says data speaks for itself, he means that members' 'understandings' speak for themselves. By this is meant how people in any situation see that situation is how that situation should be reported and described. Such members' understandings can be captured through finely detailed studies of the ways in which those understandings are accomplished.

Now, what is being highlighted here is that Crabtree is claiming that ethnomethodological studies of work are empirical in a specific way, one that is a contrastive with other sociological approaches. Put simply, ethnomethodological studies produce evidence that traditional sociologists miss. The problem we have with this, though, is that what may be an empirical 'remedy' to other sociological methods may not *always* be corrective for design; indeed, the idea of offering a corrective may be misleading. Crabtree's view is that the empirical materials his approach produces is the kind of material that designers might want and need *because it is ethnomethodological material.* In other words, the close attention to the processual-character-of-egologically-organized-work (as it would be put in the correct argot) is what designers need, and it is what they do not get from other traditions. The task of the ethnomethodological fieldworker is to produce this data, and subsequently participate in a process (of whatever kind) that one might call, 'design'. In this sense, the fieldwork produces 'descriptive' evidence rather than analytic; stuff which can, as it were, be handed over. None of this precludes the changing of hats so as to participate in the design process. When the ethnographer becomes a designer, Crabtree suggests that they act like a 'bricoleur', shifting around the various elements that are used to construct the context in question. He goes further and suggests that the way this bricolage may be undertaken can be oriented towards various ideas of patterning and invokes Alexander's work on this topic (Alexander, et al: 1977).

Space precludes saying a great deal more on *Designing Collaborative Systems*. For our purposes, what arises from Crabtree's book are two main things: first, the assumption that relevant data is simply there waiting to be captured, and it takes the form of organized, sequenced activity. This may be contrasted with Beyer and Holzblatt's view that what one might look for, how one might render the world in ways pertinent to design, is itself a matter for ongoing and team-based decisions about how to *orient* to design. In contrast, for Crabtreee, detailed data pertaining to the processual character of work is not only necessary, it would seem to be sufficient (in no need of any remedy) for design purposes.

Close attention to the processual character of work and its real world contingencies is something that all the writers we have looked at so far have in common—despite their evident differences. One would go further and state, unequivocally, that it is ethnomethodology that has engendered the focus, detail, and interest in the processual character of work we find now in all CSCW-related work. Only Vicente, oddly, starts with the assumption that data gathered in this way might not necessarily be relevant, appropriate, adequate or pertinent. We would like to claim that it might be, or it might not be, depending on what kinds of design question are at stake. As we noted above, we want to suggest that far too little attention is paid to what the design questions might be. These decisions–when to design, where to design, how to design– it seems to us, cannot be made *post hoc*. Or rather we ought to say they

should not be made only then. It seems to us that they should be dealt with before, during and as well as after the process of fieldwork.

This leads us back to imagination or sensibility, the ability to see the field in ways that lets possibilities arise, that makes fieldwork more than a mere mechanical looking. One of the concerns we had with Beyer and Holzblatt was the lack of discussion and reference to prior research, and the possible implication that followed that the fieldworker only needed lists of things to look for and things to see to ensure that fieldwork is productive. Crabtree, by way of contrast, spends some time referring to other studies of work, elucidating as he does some of the insights that one expects of ethnomethodologists. But we get little insight in to what *designers* might need to see that is of concern neither to ethnomethodologists or sociologists. It seems to us that this has all sorts of consequences. Not only might it restrict what might be noted through contrast, metaphor, distorting illumination and occlusion; it even restricts the frame of reference that might drive a design oriented fieldwork. For Crabtree, anywhere is a good a place to start as anywhere else since it is not design considerations that determine what the fieldworker should capture, it is members' understandings. Thus, one cannot start with the hope that a different future might be made possible by, say, the deployment of some new technology if that technology is not within the gestalt of the members.

SOME OF OUR STUDIES

None of this should be read as being especially critical of the books we have discussed for they all, in their different ways, make broadly sensible remarks about a specific set of fieldwork and design relationships even if we have reservations about some of their assumptions and particularities. But a larger problem we have is that some part of the research we have undertaken does not really fit with the specified set of relationships described in the books above, and we feel that less and less of it will do so in the future. What we have in mind is, for instance, the issue of the 'coupling' of ethnographic work with specific design issues, although the possible set of problems is actually much greater than that, as we shall see. Our concern lies in the degree to which any and all of these approaches either helps or does not help with a new and more complex set of design-related problems. We turn now to some studies we have conducted to illustrate some of these complexities. We do so not because we think they are exemplary or canonical, but because we know them best and can be self-critical about them. Our primary concern is to try to disentangle some of these issues of method, analysis and disciplinary concerns and, from this, examine why we consider none of the above approaches to be entirely adequate for dealing with the next generation of design problems, at least not on their own.

We begin with reflections on one of the earliest ethnographic studies conducted in CSCW, that of an Air Traffic Control room (see e.g. Harper et al, 1991; Bentley, et al, 1992; Hughes et al, 1992; Hughes et al, 1994). It is not entirely coincidental that this study was rigorously ethnomethodological. It focused heavily on the processual character of work. But at the same time this slant reflected another fact, not merely our own prejudice for ethnomethodological inquiries (of that time). The 'stuff we looked' was of a tightly-bounded domain, and this was well suited to the kinds of approach we wanted to deploy (as it happens very much the kind of domain that Vicente is interested in). The work was also tightly coupled with a specific design problem: the replication of paper-based information under 'glass'. Its main conclusions had to do with the cooperative nature of ATC work and the very public exchange of information that takes place in ATC settings that ensures that the work is done effectively and safely. The research made a series of fairly general recommendations concerning the kind of information that needed to be conveyed, the constraints (at that time) and affordances of 'glass' as

opposed to paper, the importance of collaborative error-finding rather than error omission, the actual nature of the skills deployed, and so on (for an overview see Sellen and Harper, 2002: 110-18).

Nevertheless, we should point to some features of this work that were left largely unremarked at that time. Firstly, it traded on a very considerable amount of background knowledge on the nature of ATC work that was derived from interviewing and general chit chat away from the work site; in other words without abiding by the imperative to see work *in situ*. Hence the 'data' included something of the character of 'interviews' which some of the authors above feel is ill-advised. Secondly, part of its analytic work involved the evocation of a 'culture': some typical 'doings' that suggest some limited generalizing about 'the-ways-things-are-done-around-here'. These ways of doing could be compared as different 'cultures'. Here the cultures we had in mind related differences between military and civilian ATC, cultures which existed (as it were) in side-by-side rooms (quite literally: the two sides of ATC occupying the same building near London Airport). There are two points we are alluding to here: first, the ambiguity of data collection methods: in our view, it's not the method that might matter, it's the data. It was the data we were after in the ATC project, irrespective of the method. Second, we are wanting to allude to how we saw and sifted through and indeed oriented ourselves to what data might be relevant: in this case through comparison of very similar things.

The second study we want to mention is research we conducted into retail finance services (see Harper et al, 2000). Here, we want to point to the very different set of problems entailed. Firstly, the retail finance institutions we looked at were large, complex organizations (and there was more than one of them) and secondly, the range of technologies in question were many and varied–they included, for instance, database technologies, video-conferencing, e-mail applications and expert systems. Some points we might make about this work include that we were, for the most part, feeling our way as regards what to look for in the context, since there were, to begin with, no clearly defined design problems for us to look at and no existing studies of the kind we intended to do, of socio-technical systems design for large, complex organizations (though needless to say there were many other studies: in socio-technics, for example, as well as in management studies that we could learn from). The kinds of conclusions that came out of work had to do with such things as the peculiar time-criticality of customer-facing work in retail finance and how the design of existing database technology and proposed video conferencing systems paid little or no account of it; and the role of 'local knowledges' commonly used in branch-based organisations and how these 'knowledges' might progressively erode in more large scale centers to the detriment of service provision and worker engagement. These research outcomes were driven by comparison of different institutions and different contexts within the institutions, and reflected our desire to say something that was to a degree more generic than we had attempted in the ATC study.

If the ATC and the banking study were different in the scope and degree of comparison they entailed, there were differences in the 'methods' too. Some of our fieldwork was done over a long period of time entailed attention to the processual character of the work, some to trying to understand the kinds of 'knowledge' that workers seemed to have, and some to the more general kinds of organizational problems they have to orient to. Some of it involved brief visits, 'interviews' with individuals, etc., whilst other parts involved very close attention to interactional processes. Yet very few if any of the 'problems' we encountered (leaving aside the trivial) were problems of method. They were much more often problems of analysis and how to say something useful and insightful in a context were there was no close coupling between fieldwork and design. There was, nevertheless, an expectation that we would say *something* on this topic. In ATC the relationship between user and system cried out for exploration; in retail finance it was altogether much more attenuated. We found

our way by evolving as analytic instruments a series of tropes, many of which were borrowed from ethnomethodological thinking. We used these to conceptualise problems of what 'working' in retail finance might mean and entail in relation to a range of technologies, some of which we listed above. There is no space to discuss the use of these tropes in any detail, but they included such matters as the 'ecology of the workplace'; the 'egological' organization of work activity, 'the 'social' organization of work, the nature of 'skill' and 'knowledge', and so on. These turned out to evolve some of those we had developed in the ATC project, where the egological organisation of controller's work had been a useful device, for example (3). Two things strike us about the tropes we deployed in the bank study. Firstly, some of them bear a striking resemblance to concepts deployed by others under very different intellectual auspices (as we have seen, above in the work of Vicente and Beyer and Holzblatt). Secondly, these tropes have turned out, in varying degrees, to have a recurrent value in that they continued to make some sense across a variety of domains even now: the lesson is not that they are always relevant however, it is how some tropes have more power than others to do work, analytical work. We shall come back to this.

The next example of our research is of a study conducted in a non-work setting (at least in the conventional sense); in this case, in to a so-called 'smart home' (see Harper, 2003, especially Randall pp227-246). This study entailed looking at family life in an existing (but constantly evolving) smart home. The location, in a sense, was not a 'home' to anyone insofar as families were there for relatively brief periods of time. But the point we want to highlight was that, as before, the brief was quite vague and our research ranged over the usefulness of various control systems for domestic life (lighting, heating, security) and the adoption or otherwise of trans-domestic technologies, such as for internet shopping and health monitoring. Our methods in this context involved relatively brief visits (typically two, lasting on average a day each) where something that looked like 'in situ' interviewing (of the 'show me' type) was undertaken at the beginning and end of each family's stay in the house, accompanied by ongoing video recording of family activity in the house (with two cameras in each downstairs room). We adopted these methods not out of any conviction concerning whether they were 'right' or 'wrong' but because they seemed to us to be 'do-able' in the light of the kinds of design-related problems we were dealing with. Some part of what we were doing clearly involved the evaluation of specific technologies with a view to further design (such as with some of the control systems), but other aspects were more speculative (such as related to medical monitoring).

That is, there was what we can only describe as a 'variable' coupling between the ethnographic work we were doing and design activity. Analytically, one of the things we found most interesting was the extent and limitations of the tropes we had found useful in our prior analysis of work settings. Some could be deployed, though not mechanically, in a domestic setting; but in many respects the domestic domain and the technologies we were dealing with were recalcitrant to our previously developed tropes and techniques. It turned out that the domestic space was step too far for the expertise we had developed in CSCW studies of work (hitherto the primary concern of the field). The sorts of new matters that we had to define new ways of dealing with (and by that we mean dealing with them analytically and not in terms of method: we could get data about them) were such things as the 'rhythms' of family life, rhythms which were as much about symbolic as practical matters, ephemeral in some ways yet robustly oriented to in others; and relatedly the 'moral order' of the home, where patterns of appropriate behaviour were bound to age, status, time, activity, economy and accountability. At the same time, the technologies that could effect these and other aspects of home life were—are—diverse, with different affordances and consequences on the nature of behaviour in question. All this and more perplexed us when it came to design consideration, not in the sense of

coming up with design solutions, but in the sense of how we could get them to fit in to a design 'space': an arena for thinking sensitively about design possibilities.

BACK TO THE GENERAL ARGUMENT

We could, of course, make reference to other examples of our own work or indeed to any amount of exemplary work carried out by others. It would add little of any consequence to our argument. Our purpose in raising these studies is twofold: to show that these studies do not quite fit the picture of fieldwork and ethnography and its relation to design offered in the three books mentioned above. Somehow our experiences were much less tidy. Second, to point toward what we earlier called the corpus of reasoning on fieldwork and design constitutive of the body politic of CSCW. What we have seen here is that this body politic has a history, a past; but also this body politic needs to evolve and shift as the discipline (CSCW) moves to new design concerns. This leads us on to the last book we want to deal with, Marcus's *Ethnography through Thick and Thin.*

It seems to us ironic, to say the least, that Marcus, who has nothing whatsoever to say about design, encapsulates what we think is the problem of how fieldwork is to meet with design. He is an anthropologist who has been at the forefront of thinking about the nature of ethnography, the way in which ethnographic materials are presented or conveyed, and what 'usages' ethnography can be put to for some time (4). The key link between this book and the others we have considered is, for us, how it asserts the view that fieldwork, and in his case he means ethnography, needs to be understood as *always being driven by particular analytic foci*.

He gets to this issue indirectly. He gets there by exploring another question: how 'ethnographies' in anthropology are typically the product of solitary individuals, sole researchers on voyages of observation. He suggests that these voyages, like Sinbad's, produce stories, and that this begs all sorts of questions about fact versus fiction, about narrative structure and the problem of evidence, because they *are* stories. These 'limited views' from 'the particular', as Geertz would have it (Geertz, 1999), are, in Marcus' post-postmodern view, less and less authoritative. Now, we do not want to explore anthropology's agonies here, but this loss of authority that Marcus highlights relates not only to the individual's voice but also to anthropology's chosen topics. Marcus is adamant that anthropology's future lies in interdisciplinary concerns and that interdisciplinarity carries with it its own tropes which might be well removed from the story telling praxis of traditional anthropology. The importance of this, in our view, cannot be overstressed. It moves the argument about what ethnography might be away from a concern with reflexivity and narrative, for example, towards other matters. It explicitly relates to what we above distinguished as disciplinary methods, concerns and sensibilities.

Before we list what these might be, let us note that Marcus proposes as a way forward. He outlines a so-called 'multi-sited' approach to ethnography. This represents, he thinks, a return to comparative ethnography, but in a different way:

> *'… comparison emerges from putting questions to an emergent object of study whose contours, sites and relationships are not known beforehand, but are themselves a contribution of making an account which has different, complexly connected real-world sites of investigation …. In the form of juxtapositions of phenomena that have conventionally appeared to be 'worlds apart'* (pp86).

In our understanding, 'multi-sitedness' is explicitly not a matter of visiting more than one place (although it may well include that). It is, rather, about the problem of making analytic *connections*. Now, we have to be careful about what we make of this here since, as we have stated, Marcus is not concerned with design and, in point of fact, much of what he describes as 'interdisciplinary' doesn't appear to us to be very interdisciplinary at all. Indeed, his notion of interdisciplinarity is world's removed from the kinds of interest we have, but his arguments provoke us to ask what contribution 'multi-sitedness' (or new forms of comparison) can make. The most important feature of this argument, it seems to us, is that the problems of interdisciplinary engagement are not problems of method. Multi-sitedness implies an eclectic approach to 'method', and thus there are not and cannot (in any simplistic way) be about remedying the failure of other 'methods'. Nor are they problems of substantive disciplinary specific concerns because the contemporary crisis in those concerns is precisely what leads him to interdisciplinarity. The 'multi-sited' view of interdisciplinarity, then, leads us to reflect on problems of *empirical* relevance, of *conceptual* orientation, and of the role of *comparison*.

The questions that are entailed in considering these things include the following sorts of queries: what kind of site constitutes an appropriate site for investigation?; to what extent can detailed and local results typically resulting from ethnographic enquiry produce results which are generalizable?; and for what purpose are these generalizations being sought? These questions entail some engagement with the relationship between evidence and purpose, an analogue, though hardly ever put this way, of fieldwork and design: in this case the fieldwork being the 'data', the design the 'theory'. It seems to us that there is often a failure to answer these questions amongst those who are newly arrived in the ethnography/fieldwork and design domain, and even in CSCW, where there can be no excuse for being naive, there is too sometimes a neglect of them. Within CSCW, even for those who claim some scholarship, another problem, one that concerns us, is that as CSCW tries to address new domains of inquiry the just what of relationship between fieldwork and design is different. Hence, the pattern of that relationship we have somehow come to map out and learn does not now help us as we would wish.

CONCLUSIONS

We have been arguing across a range of topics, theoretical, conceptual, empirical, and historical; we have also been alluding to what disciplinary perspectives and sensibilities might be, especially in a context where different disciplines are competing with one another, and one discipline in particular (CSCW) has reached a point where it might need to adjust itself.

Many of these concerns are grand and the solutions to them equally likely to be grand, but not all of the matters we have dealt with turn out to be about very grand problems. We have wanted to imply that much of what passes for a discussion of fieldwork in design, for example, turns out to be musings about how to how to collect and use fieldwork data. These musings have been raised in a context of a largely restricted set of domain and technology problems, where the main issue has turned out to be which method needs to be remedied by which other method. These musings, as we call them, are not illegitimate concerns. We do not wish to suggest otherwise, though do wonder whether they should be the main substance of what is discussed at conferences like EPIC. Nevertheless, from the perspective of CSCW, we consider the historical relationship between ethnography and design to have been a broadly happy and useful one, albeit serendipitous. We are, however, also wanting to state, explicitly, that this success cannot be relied on as CSCW tries to encompass new topics, domains,

technologies and concerns. These are serious matters, and talk of data gathering are somewhat distracting, we think. There are major problems inherent in fieldwork for design as we see it evolving and these are more than methods related.

To begin with, we think the importance of design as an analytic focus of what CSCW has been and what it will become needs to be made greater, more central, more of a focus, if you will. We should at least begin to set out a stall here. The special quality of ethnography in design we assert and hope for does not lie in the superiority of one 'method' over another, nor in the privileging of the local and contingent over the formal, the ideal for example, nor in focusing on the tacit as against the explicit; and so on: all the stuff that seemed to perplex the first three authors we considered. In our view it lies in the deployment of a specific 'ethnographic sensibility' and its relationship to a complex assemblage of activities one can call design and, related to that, a 'design sensibility'. The marriage of these 'sensibilities' ought not to treat data as freestanding and separate from design, as if fieldwork material were an object that stands for the skills, artefacts, processes and culture in question like a painting; nor should it treat design as a straightforward fitting of solutions to problems. It should not do so for this somewhat paradoxical reason: *the relationship between fieldwork and design cannot be determinate*.

We say paradoxical because we have been claiming throughout that there is a relationship between the two; now we are saying that it is not fixed. We do not think this unreasonable to claim this: the more important issue is that one recognizes that there is a relationship, complex, changing, fraught, and attenuated though it may be. Once this is recognized, the fact that the range of possible design-related questions that fieldwork data might contribute to is increasing should be then thought of as natural consequence of this relationship; even a measure of success. But this increase will make things more difficult, not less, even if we can pat ourselves on the back for getting in to this position.

We have tried to show the reasons for and the character of this increasing complexity, albeit briefly, by alluding to some of the problems we have had to deal with in our own studies. These experiences point to a growing range of relevant issues. Fieldwork practices have to develop that deal with, for instance, areas where there might be no clear idea what the design problem might be. It might be that fieldwork is funded to address the potentiality of future technologies that have, as yet, no clear application domain in view. Besides, it might even be that social mobilities are such that the location at which any technology might be used are so dynamic that it is difficult to know where to look in the first place.

Very little of the discussions of fieldwork and design have, it seems to us, begun to grapple with these sorts of problems, which are, in our view, practical and conceptual, analytical and empirical. The books we have reviewed have dealt with some of the issues, but not adequately. Fieldwork itself, for example, may have to take place in organisational contexts where design methodologies might be increasingly complex, and design teams may increase in size; fieldwork may take place in conjunction with various change management or project management methodologies which also might be changing. Design possibilities may range from incremental development of known applications to radical re-design. No set of recommendations concerning the relationship between fieldwork and design as yet has dealt with all these different possibilities. The point here is that no standard or orthodox approach from anthropology or sociology or more likely, even ethnomethodology with its focus on members' methods, or even from CSCW itself, could help us determine such things as which members are the relevant ones for design, or what particular work processes are important. Our feeling, to complicate matters but based on our own experience, is that organizations and their representatives sometimes have only fairly vague ideas about what the purpose of any given research

relationship may be. Even more problematically, functional divisions within the organization might mean that assumptions about purpose may vary from department to department, or role to role: whose member's understandings count really is sometimes entirely unclear, almost unfathomable.

Thus we are back to the beginning of our arguments, to the idea that the purposes of fieldwork for design depend on the interdisciplinary audiences to which they are addressed if by that is meant not just academics and scientists but the people, the users, who end up with designed technologies. Collaborative endeavour is indicated, but we are not a great deal closer to defining what that collaboration might entail than we were at the outset. Certainly, the answer does not lie in what one might call disciplinary-specific concerns for method, nor the great themes that constitute a disciplinary 'body politic'. We think that, when it comes to CSCW, these disciplinary themes—given by anthropology, sociology, psychology and ethnomethodology—were merely a starting point a long time ago and that now, some twenty years after the term Computer Supported Collaborative Work was first coined, what counts as evidence, what is relevant, how it is to be assessed, what are the ways in which the evidence can be used imaginatively and so on, are all part of a new body politic, the one of CSCW itself. In this view, anthropologists can't do fieldwork for design if by the term 'anthropologist' they mean professionals concerned with the body politic of anthropology (and of course the same applies to any other set of disciplinary interests). But beyond this, it also means that the themes that have merged in CSCW are ones that have a history and, presumably, a future: the trouble, though, and as we say, is tat what worked in the past might not work in the future.

As it happens, and leaving aside the future for the moment, we think that it is possible to list many of these themes or tropes that have worked to date. We have already mentioned some. Of course, space precludes a detailed rehearsal, and in any case we hope it is clear from our discussion of aspects of our own research history, we would be absolutely against any suggestion that these tropes can be used in a 'cookbook' fashion. Rather, we have been at pains to point to the way in which disciplinary 'sensibilities'–which in our case where historically ethnomethodological but are now distinctly CSCW–may allow us to produce a set of broad, 'sensitising' or 'illuminating' concepts, starting points that can serve as reminders that some kinds of thing are often to be found whilst not diverting us from our equally powerful interest in what is uniquely 'situated' about what we are studying. In short, they can provide us with a 'way of looking'.

The kinds of things we have in mind, and we put them in a language that Marcus would be familiar with, though we could equally well express them in other terms, are as follows:

a. Follow the plan: Investigate the documentation, the procedures and the rules as formally stated and how people orient to them. Appreciate the 'information life cycle'- its birth, life, and death.
b. Follow the job/trade/business relationship: Understand what various 'stakeholders' expect, anticipate and want from new technological and organizational forms.
c. Follow the skills: Understand in detail what skill is actually being deployed rather than assume that any externally imposed description (e.g., the organizational chart; theories of flexible specialisation) is adequate.
d. Follow the knowledges: Find out what people actually need to know in order to do the things they are trying to do, and understand the ways in which these knowledges might be 'local' or 'contextual' and intertwinned.

e. Follow the use: The particular things that people do with technology, and the ways in which they do it.
f. Follow the ecology: establish the ways in which people elegantly integrate their purposes with a material environment which they in part organise.
g. Follow the 'troubles': Investigate the 'false starts', 'glitches', 'diversions', 'distractions', 'interruptions', and 'digressions' which are aspects of all activities and not noise to be eliminated from the data.

These topics and tropes are not–they cannot be–exhaustive. That is not their purpose. They will evolve (or more properly, co-evolve) as they are compared, juxtaposed, re-emphasised and viewed in ways that echo Vicente's juggling of abstractions (without endorsing assumptions about their formal properties) across the huge potential range of domains we could feasibly be interested in, and in the context of the varying relationships between ethnographic investigation and design problem we indicate above. New tropes are needed to figure out, to make 'workable' fieldw ork for the design of smart home applications, for various mobile devices, for activities supported in novel ways in public space, ad so forth. These new tropes won't abandon the old ones. Nor will they abandon the robust conviction that the material captured and juggled thereby reflects a persistent concern for 'situatedness', because part of their use will always be to explore what is unique about the phenomenon under investigation and what design requires in terms of 'patterns', 'typicalities', 'generalisations' (or whatever one might want to call them).

Whatever the nature of this mix of the already cultivated and new tropes, it is our view that they will allow imaginative explorations of the future and thus empirically grounded comparisons of what could be with what currently is. Capturing what might be for these exercises will entail thoughtful comparisons of the present and the future. It won't be a mechanical thoughtfulness; it won't be an empiricist one; but it will be a grounded thoughtfulness almost certainly undertaken by teams of individuals where the link between fieldwork and design is diverse and often changeable. We have sued this paper to delineate why we think these (and other) features we be common.

In a roundabout way this brings us back to the original remit of anthropology: Otherness. What we have been wanting to say is that Otherness of and in the future can be foreseen through observation of, comparisons within, and studies of the arrangement of, artifacts, processes, technologies and human endeavour in any and all of the places around us, even those places we have not looked at before. Fieldwork for design, a certain kind of fieldwork oriented to, married with, suffused by design, will do this. Wherever this fieldwork leads one to look, we think the shadows and ghostly intimations of the future will be found if only the fieldworker has the requisite sensibility to see them. This sensibility owes much to disciplinary origins, to disciplinary history and auspices. But to be successful as a sensibility it needs to be more than that. It requires a genuine engagement across the various fault lines we have mentioned and needs to involve the respectful combination of a *design imagination* and an *ethnographic imagination*. This mixture, we think, will produce a fieldwork for design that can work in the future. It has produced fieldwork for design in the past for CSCW, and some success can be seen here. The question we have posed in this paper is how this sensibility can evolve so as to maintain the link between fieldwork and design despite different domains, topics, auspices and technologies. We know how the past has got us here; we can see that getting to the future will entail some trouble, some effort. Our case is that effort will be required because it that future is unknown. Yet it seems to us that surely that is what should excite us. We know it's going to be different: it's going

to be different in terms of what we look at and what we can design for. This paper has set out some arguments for how we can bind the looking in to the future with the design of that very same future.

NOTES

[1] For a thoughtful review of the same from the perspective of anthropology, see Geertz, C. (1999) *Available Light*, especially 'The state of the Art', chapter V, pp89-144.

[2] There are similarities between the naivety about data gathering of Vicente and the early claims of the ethnomethodological ethnographers in CSCW. See Hughes et al, 1994.

[3] Though some of the tropes from ATC seemed to have little provenance elsewhere though were much celebrated n the literature. One such was the idea that a division of labour could create a cocooning effect in ATC.

[4] Perhaps most famously for his book, *Writing Cultures*, written with J. Clifford, in 1986.

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