

Part 1

Theoretical and Analytic Issues

The State of Play

Progress is marked less by a perfection of consensus than by a refinement of debate.

Clifford Geertz (1973)

Much of what is sailing under the name of sociology is a swindle.

(Reputedly said by Max Weber)

Given that the central theme of this book is the application of ethnographic stances to design problems, it might appear that the quintessentially sociological issues that trouble social scientists in standard texts on ethnography and, indeed, elsewhere are not worthy of consideration here. For the most part, we are in agreement with this sentiment insofar as we are impatient, sometimes downright rude, about this tendency towards '*nombrilisme*'² in sociology. Having said that, Kenneth Burke (1935) pointed many years ago to 'occupational psychosis' (borrowing from Dewey): that tendency to see and understand everything through the occupational or intellectual concerns of one's own chosen professional and disciplinary viewpoint, and there may be some truth in the idea that some of the problems involved in interdisciplinary working stem from occupational psychosis. If so, it would seem to have a direct relationship with problems of 'communication' and 'understanding'. It may well turn out to be what occludes the 'sensibility' of which we speak. In any event, our interests in this chapter lie in the origins of this sociological sensibility and its relationship, in outline, to design issues.

We are, of course, merely setting out a stall as to what ethnography, as a fieldwork method, might offer CSCW. Fieldwork of one kind or another has been around for almost a century now, and has been a key component of

²This is the French for 'navel-gazing'. Because we do, in fact, discuss some of these inward-looking issues of reflexivity and other sociological issues here, we thought a certain pretension would set the right tone.

design-related research for longer than we imagine; certainly some kind of fieldwork has been endemic in Scandinavian work science, German industrial sociology, Tavistock school social psychology in the United Kingdom, the francophone 'cognitive ergonomics' tradition,³ and so on at least since the 1960s and in some instances earlier. Not least, of course, cognitive science has always had some tradition of fieldwork and this has been consistently present, if not always to the fore, in areas such as human factors research and its cognate, HCI. If so, we need to ask ourselves what our version of fieldwork (as typically practiced in CSCW) has to offer that was not already there. It would be bizarre, to say the least, in a context where new technology (often of a radical kind) is our main interest, if we were to reinvent the wheel. This point exemplifies our main concern, which is the analytic auspices of the fieldwork we do, a form of fieldwork that we, and others down the years, have called 'ethnography'. These auspices pertain not to sociology but to design.

Whatever our view, ethnography remains contested. This is firstly on the basis of its theoretical auspices, where proponents of ethnomethodologically informed ethnography grapple with those who prefer distributed cognition, who in turn attempt to establish the value of their perspective as against, say, activity theory and vice versa. In the meantime, various other theoretical commitments are inserted while no one is looking, such as actor-network theory (Callon, 1986a; Latour, 1987; Law and Hassard, 1999) and structuration theory (Giddens, 1984), arguably long after they have reached the limits of their fashionability in sociology. The functions of ethnography, or what it can be expected to achieve in practice, are equally contested. Not only has it been hyped to such a degree at times that one (nonsensically) might almost consider it a fully fledged replacement for requirements analysis, it has also been promoted as a solution to all the moral ills of the design process and a means for 'critical' reflection on the theory and politics of design. It has been, conversely, critically received because it takes too long, offers only snapshots (and of 'current practice' at that), and it seems so bereft of theory that we are in desperate need of some general theoretical concepts which will allow us to make sense of more than one immediate context. Above all, in the light of what has already been stated, its relationship to more formal, idealised, or 'businesslike' models has also been puzzled over. In sum, why it has becoming increasingly promoted in systems design in the past decade or so requires some elaboration.

³Vicente (1999) notes that although the use of fieldwork is sometimes thought to be new, in fact it is old within the ergonomics community, and indeed the descriptive approach has a long history in what is called francophone 'cognitive ergonomics', where phenomenological descriptions have long been a stock in trade. (For an overview in English see De Keyser (1991).)

2.1 Disciplinary Assumptions, Fieldwork, and Ethnography

Here then, we want to sketch out the range of perspectives on fieldwork and the set of historical transformations that have led to the version of it which we call ethnography. We do so by caricaturing two stances at extreme ends of a continuum. One we call antianthropology (or antisociology), and this would be the stance that would typically concern fieldwork rather than ethnography. On this side, we have a position which holds that fieldwork is merely the observation of people in their natural contexts, and requires no particular analytical acumen or expertise. It is, for the most part, just a case of watching and gathering data and thus can be done by anyone. This same position is somewhat more concerned with the tools that enable observational materials to be transformed into theoretical or design insight by, for instance, providing models, and with the tools that enable teams of individuals with different skills – interface designers being one group for example, work process people another – to agree on design and systems implementation choices to be driven by fieldwork data. In this position, then, fieldwork is easy and requires little training; transforming data into design is hard and does require tools, theoretical expertise, and practice.

The other end of the scale is exemplified in the anthropological (and to a lesser extent, sociological) tradition, where being able to do fieldwork equals being able to do ethnography. Here, doing ethnographic fieldwork depends upon years of prior training into the mechanics of data gathering and into forms of exposition and ‘account telling’ (or ‘narrative’ as it is more often put). Key to the nature of ethnographic fieldwork, and the processes of documenting that work, are how the topics and themes of generations of prior ethnographers are referred to and invoked. That fieldworkers needed to be familiar with prior work and understand the cargo of knowledge that ethnographic practice had built up in that tradition went without saying.⁴ From this view, ethnographers do what they do, are interested in the things that interest them, and communicate with designers as required to establish a design relevance, but design work itself would be separate from the business of narrative construction.

Obviously, we see ourselves as being situated at the ethnographic end of this rhetorically convenient continuum, but with some major reservations which have to do with the intellectual history of anthropology and sociology. Our reservations are twofold. Firstly, we are uncomfortable with the professional status given to ‘method’, by which we mean issues of note-taking, indexing, and interviews against observation. Secondly, we are uncomfortable with the status of theoretical or perspectival claims which

⁴Epistemic revisions mean the themes invoked may be, as with some postmodern reportage, in a rather mocking way.

emanate from specific social and human sciences but are applied, largely unchanged, in interdisciplinary contexts.

The perspectival or paradigmatic issues raised by interdisciplinary working are important. They are key to understanding, for instance, the power of Suchman's (1987) work. The persuasiveness of the various theoretical positions we sketch below depends on an understanding of the distinction between approaches which stress some notion of human conduct as being inescapably bound up in context, locality, situation, and sensemaking (and which imply methods founded variously on a hermeneutic, on interpretation, or on just plain common sense)⁵ and those which are founded on rationalistic, calculative, or computational models of human conduct and which carry with them 'scientific' assumptions about appropriate method. For this reason, we emphasise, at least to an extent, a third variant: the so-called ethnomethodological tradition. This position, for some, puts us at the ethnography end of this scale. In fact, although we believe in something called ethnography rather than simply fieldwork, we are impatient with much of the sociologising of method and theory that we see around us. We want to treat fieldwork as an ordinary and mundane business which requires neither sophisticated skills nor years of training. We suggest, indeed state openly, on several occasions that 'going out and looking' is easy.

The important skills are mobilised back in the office (although not only there). They are analytic skills and are predicated on a sensibility or way of looking.

Sensibility can sometimes seem in short supply. We believe we learn much from a sociological sensibility, but much less from sociology's specific interests. The view that we develop in this book entails building on and expanding our perspective so that broadly ethnomethodological insights might be used in conjunction with models, processes, tools, and ideas from other traditions and perspectives, as one of many tools in the toolkit. Of course, and as stated, these positions are caricatures. There has arguably always been far too much posturing about positions taken in opposition to one's own and we do not wish to be accused of that. We have pointed out that there is a tradition of fieldwork which owes nothing to sociology at all and it is worth examining, briefly, what kinds of assumption are entailed in this work. We can illustrate this by briefly referring to Vicente's (1999) book, *Cognitive Work Analysis*.

⁵Again, interdisciplinary practitioners will have had to suffer, at some point, the ethnomethodologist who insists that what is going on in a particular setting is not a matter of interpretation either for the people working in that setting, or for the person observing it. This is because the term 'interpretation' carries with it, at least when being used in a technical sense, the implication that cognitive work has to be done in order to understand any social setting. This is a strong paradigmatic claim, unlike one which says that cognitive work may sometimes be done in settings that are in some way problematic.

2.1.1 Cognitive Work Analysis

Vicente represents the human factors perspective in a way that highlights a particular view of the need for fieldwork, the nature of doing fieldwork, and the role fieldwork plays in a larger scheme of design. He describes a method specifically suited for highly complex sociotechnical systems where the nature of design solutions might not be easily iterated and where there is a need, because of the task being supported, to get the solution right first time round. Not all human factors and ergonomics research incorporating a perspective on fieldwork entails this method, of course. Nevertheless, cognitive work analysis is a good example of how these disciplines characterise different levels of description and evidence gathered in fieldwork, distinguishing, for example, between normative and descriptive (the kinds of thing that an ethnographer would think of as his or her normal forms of evidence), and on the other hand, what Vicente calls formative or predictive models. These are the things that human factors and ergonomists want to turn data into and in which ethnographers have seemed reluctant at times to get involved.

This work seeks to develop techniques and methods that could enable the design of truly failsafe systems in extraordinarily complex environments. The concern here, as we have alluded to already, is that these complex sociotechnical systems have to be designed to work from the outset, and cannot be gradually iterated and improved. Any failure of any element in the system could be catastrophic. Therefore, there is a need, in these situations, to develop methods that could enable the specification of sociotechnical systems that are well in advance of current systems and practices. These are based on abstraction and modelling of the constraints of a sociotechnical system. The modelling captures both the human and the technical in an abstract schema that the designers can use to determine, assess, and evaluate design solutions before any eventual system is built. This preference for modelling may be taken to its extreme in cognitive work analysis but is the norm in ergonomics and human factors.

Although this preference may seem alien to those trained in ethnographic traditions, it forces us to deal with the question of prediction. If, as we show, contextual inquiry (Beyer and Holzblatt, 1998) claims that one can design new systems through close reference to the current processes and technologies used in any context, then Vicente takes a quite different view. He believes that this approach, which he calls descriptive, can only lead to incremental design improvements and although this is satisfactory for some types of settings and technologies, it is not adequate for all. In highly complex sociotechnical systems, Vicente argues that there is a need for tools which enable designers to specify sociotechnical systems that go well beyond current systems, and thus fieldwork and description of current practice can only be a part of the tools necessary.

Vicente's main goal, his mantra if you like, is summed up by Rasmussen's (in Vincente, IX) observation that,

the pace of change has become so fast. And the need for high reliability of new systems so high, that the design of new work tools and systems cannot be based solely on empirical incremental evolution based on studies of existing work conditions. Predictive models of work behaviour become necessary, models that can serve to predict work behaviour by explicitly identifying the behaviour-shaping features of a new work environment ... we need to replace the normative and descriptive models of work analysis by formative analysis.

In sum, the approach requires understanding of the constraints that shape an environment, and these constraints are both human and technical. To get to these requires a mixture of data gathering, one of which is the task of fieldwork. Fieldwork is useful according to Vicente, because 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 overridealise work, especially those which use laboratory data-gathering tools alone. These Vicente calls normative.

This in turn has implications for work analysis, or, put another way, uncovering these issues enables work analysis to do the following. First, the information that workers require when dealing with the unfamiliar and unanticipated will be specified. Vicente calls these work domain skills. Then the more limited set of information required to get the right work done, irrespective of contingencies, will also be specified. These are called control tasks. The strategies used by workers to learn and adopt their work practices to the context will also be captured. These strategies can be used to design mechanisms that can replace current ones in future systems.

However, this is only part of what designers need to do their work. Vicente claims that these topics only describe or capture what he calls the current 'workaround processes' designed to achieve certain functional ends. These functional ends are also captured by the descriptive analysis. But, to consider what alternative ways of doing the work might be requires a leap beyond what these descriptive resources allow.

There are a number of reasons for these limits, and he seeks to prove the existence of these limits by showing how ways of dealing with them fail. First he explains how rapid or iterative prototyping or scenario-based design invalidates descriptive analysis. He explains this by referring to what he calls the 'artifact task cycle'. According to Vicente, if one introduces a new artefact into a domain (an artefact could be a new computer system), this automatically alters the way work is done and thus invalidates any prior descriptions of the work. Thus the use of rapid prototyping and design iteration, or even a scenario-based approach to exploring what the future might hold is not a solution to peeling away what the future might be, he contends,

inasmuch as these techniques serve only to invalidate the description used to produce the scenarios or prototypes in the first place. Introducing these things, that is to say, results in the description always being one step behind itself.

A second problem has to do with what he calls incompleteness in rapid prototyping. Iterative design prototypes never offer complete solutions, he says, instead offering only partial solutions for bits of the overall process. This means that the overall ecology of the desired sociotechnical system is never prototyped. By not looking at the overall ecology the designer not only fails to understand the full interrelatedness of the ecology but might even be distracted by the specifics of a prototype at the expense of the totality. It leads to a piecemeal approach to design.

The solution to these limits and problems, he argues, is to model all of the constraints on a system, be they human, informational, cultural, or technical. By model is meant an abstract representation of the functions of various elements which in turn allow designers to assess and explore the interrelationship among these different elements and among different solutions to satisfying the needs within each element. These elements he has already defined, and they are the classes above: starting with domain skills, then control tasks, strategies, social, organizational, and so on.

Each of these can be represented as a list of requirements and then possible solutions to satisfying them. Each is placed within a frame provided by the subsequent stage. This reflects what he calls a move from the ecological, related to the work domain and its local features, through to the cognitive, which relate to worker competencies, what an individual can be realistically (cognitively) expected to do. The important point is that the data from the descriptive analysis is to be used to populate these elements or stages, but the stages themselves are universally applicable, and the ways in which they interrelate and are subject to one another in hierarchical order is also universal. By placing the descriptive data within this framing the analyst can transcend the limits of that data and do predictive design.

But to do this, Vicente argues, the descriptive analysis should be used as a resource for the greater task of 'peeling away' current practices to specify 'intrinsic work constraints.' These are the things that are fundamental to the work and have to be treated as given in the design of any new sociotechnical system. Unfortunately, descriptive analysis is not capable of doing this alone.

Now, as should be clear, Vicente represents a perspective that is very sympathetic to the descriptive approaches exemplified by ethnographic inquiry. At the same time he is clear that there are limits to this form of analysis, and these mainly have to do with the gap between descriptions of current practice and the design of new systems which may alter those practices. There are a number of issues that we could raise, but what we want to take from his discussions at the moment, however, primarily has to do with the role and function and auspices of fieldwork.

Cognitive work analysis has, we think, three main threads:

- It sees fieldwork as a descriptive rather than analytic process, and thus in need of remedy (e.g., through modelling).
- It offers fieldwork as an element in a systematic process that leads to generic design.
- It attempts to overcome the gap between descriptions of current events and design for the future.

This approach has one deeply serious challenge to the ethnographic community, particular those who insist on description above all else. It states unequivocally that description is not enough, and that it must be supplemented with some systematic analytic work. It thus incorporates ‘work’ in the way that various CSCW practitioners have been advocating, but argues that the problem in systems design is not to gather data or to understand and describe work practice. It is to reduce the work practice descriptions to abstract models that can then enable, in turn, even more abstract models of the sociotechnical systems. Leaving aside the question of modelling for the moment, actually undertaking fieldwork, getting to understand the relevant aspects of human action and relatedly the role of technology in that setting, is neglected in the discussion. Now of course it could be that Vicente chooses to neglect the issue of how to do fieldwork as an editorial necessity; after all his book is about more than fieldwork. But we pick up on this because we think that this is rather more typical of the attitude of human factors and ergonomics to fieldwork as against a more sociological treatment.

Our reason for discussing this work is specifically to show that cognitive work can indeed show the same sensitivity to context, contingency, complexity, and skill that we advocate, and without any of the specifically sociological training demanded at the other extreme. Cognitive ergonomics, human factors, and so on are not deluded as to the nature of work practice and may well seek to avoid overidealising it. What is curious, however, is the treatment of ‘description’. It is as though description were an undemanding, self-evident set of procedures in which the world ‘out there’ simply has to be represented on a canvas. Our point is that this is true only if one accepts certain kinds of analytic commitment, and in particular a commitment to seeing the world through the eyes of those who inhabit it. The objectivist view avoids the very questions about what one might look for to which we have alluded. After all is it certain or clear what, say, a work skill or a control task might be? Can a fieldworker simply go and gather data about them and if so, where should he go? How are these to be distinguished, described, and clarified? All of these questions, it seems to us, need to be dealt with before the modelling starts.

2.2 Sociological Method, Sensibility, and Analytic Stance

One central theme that has exercised and continues to exercise the social scientific ethnographer is its status as a method. Some flavour of this is gleaned from the vast literature available (for those interested, one can trace various kinds of methodological reflection and the evolution of 'how to do it' manuals by looking at any number of texts, for example, Adams and Preiss (1960), McCall and Simmons (1969), Filstead (1970), Habenstein (1970), Wax (1971), Lofland (1976), Burgess (1982), Hammersley and Atkinson (1983), Bryman (1988), Ackroyd and Hughes (1992), Brewer (2000), Silverman (2000,2001), Schensul and Schensul (1999), Schensul et al., (1999), and Hine (2000). This emphasis can be traced back almost a century.

When Sir James Fraser's (1993) huge work, *The Golden Bough*, was first published, in 1922 it was in twelve volumes. It purported to be a theoretical examination of the roles of myth, magic, and religion throughout human society. Unfortunately, Fraser's grand anthropological scheme foundered on the simple fact that he, like Emile Durkheim (who was also interested in religion), had not actually investigated empirically any of the cultures from which he drew his conclusions. Ethnography was thus a remedy for the failings of this theoretical approach. During the first half of the twentieth century immensely influential ethnographies were published which detailed a number of 'strange tales of faraway places', including Malinowski (1922,1929,1935; see also Malinowski (1967)). From the 1920s onwards, a second motivating force for ethnographic research was also being established.

The Chicago School, as it was known, (because it was based mainly in the department of sociology at the University of Chicago), became famous for its use of fieldwork methods. This was to be found notably in the work of Park and Burgess (1921), Park et al. (1925), and Park (1952) on the social ecology of the city, Thomas and Znaniecki⁶ (1927) on immigration and social disorganization, and Shaw (1930,1931) on delinquency. We need not concern ourselves too much with the themes of these studies, because they are quintessentially sociological. We can and should, however, point to some of their methodological implications. First, Chicago school theorists were fundamentally concerned with the problematic relationship between the subjective and the objective. Thomas, famous for his deliberations on the 'definition of the situation,' was engaged in challenging dominant psychological views of the nature of motivation at that time. More specifically, he was concerned with distinguishing 'values', which he thought were objectively and institutionally given from 'attitudes', which he thought were subjective. This, of course, carried with it the implication that sociology had to be concerned with both the analysis of causal relations and of subjective experience.

⁶We should not exaggerate the commitment to ethnography here. Chicago school sociologists were eclectic in their methods. Thomas and Znaniecki, for example, were heavily reliant on diary material.

Even at this early stage, then, ethnography entailed no ‘purist’ commitments and could go hand in hand with general theoretical laws. Robert Park, in a similar vein, made much of the distinction between nomothetic and ideographic study. The former is basically study which aims to generalise and the latter is more straightforwardly historical and concrete. Ideographic study is what we associate with ethnography, of course, and Park’s by now well-known and much reported injunction to ‘go and get the seat of your pants dirty’ reinforces that idea (quoted in Hammersley and Atkinson (1983)). Park was recommending a ‘social ecology’, whereby ways of exploring urban sites could be found which were analogous to the psychologist’s laboratory. He suggested that our task is primarily the analysis of experience and argued in effect that individual experiences could be collected and compared in a way that had much in common with biological classification. We pick up on the notion of ‘ecology’ at a later stage.

It was really with the work of Herbert Blumer that naturalistic enquiry became more rigorously counterposed to other methods. Blumer’s famous (1940,1954,1956) ‘critique of variable analysis’ was a savage attack on the hypothetico-deductive model in sociology. In other words, Blumer was an early critic of the notion of sociology as a science. In his 1940 paper he makes the case against variable analysis by comparing sociology/social psychology with science. He points out that the scientific approach to quantitative analysis is not statistical, and that consequent scientific laws are universal, not probabilistic. Furthermore, the statistical methods associated with social psychology give us no adequate measures for how things change.

Lastly, he suggests that such statistical relationships presume a behaviouristic model of human conduct (i.e. leave no room for the analysis of human experience). Blumer subsequently extended his critique (1954,1956) by arguing that variable analytic sociology does not succeed in its objectives because it fails to provide precise, unambiguous, and clear concepts.⁷ Indeed, he rejects the idea that there can be any definitive concepts in sociology at all. That is, Blumer is the first to point out that social science concepts involve an inevitable element of judgement.

This argument has had a profound effect. It has led critical sociology, for instance, to explicitly embrace its values, the feminist epistemologists to consider how being a woman can affect one’s knowledge of the world, and the ethnomethodologists to argue that sociology’s project should be limited

⁷Basically, his argument is that variable analysis cannot do what it sets out to do because it is predicated on the view that we can safely ignore any ordinary or ‘pre-scientific’ interpretations of the world on which our more rigorous concepts may be based. That is, variable analysis trades on the assumption that any ‘interpretation’ that goes on has no relationship to the construction of rigorous concepts. Blumer in contrast believes that commonsense interpretation is fundamental to our procedures and that the concepts we habitually use in sociology are saturated with commonsense usage. In other words, Blumer is denying the existence of a neutral descriptive language for sociological purposes.

to careful description and, at best, commonsense generalisation. Even contemporary 'realist' sociologists (who believe that the causal nature of the world is independent of our perceptions of it) have accepted that cause and effect in sociology has a moral component (see Sayer (1992)).

'Naturalistic sociology', for Blumer, must 'respect the world' rather than lurching into explanation long before it has adequate description. In other words, it involves the world as it actually is, rather than substituting a shorthand for it. As Matza (1969) later pointed out, such a commitment requires methods which can maintain loyalty to the world but does not, in itself, presuppose any particular methodological procedures. This is important, for it suggests that one of the problems with sociology is that it is too obsessed with method, and raises the possibility that interdisciplinary research can be the victim of the same overmethodical approach. What looks like a set of tools for doing research might be replaced by a simple recommendation to go out and look at the world, and then think carefully about what you've seen in order to represent it in a way that reflects how ordinary people experience it. We have a certain sympathy for this viewpoint in that our own view of method is an impatient one.

Of course, for interdisciplinary working the problem lies in the terms under which this apparently simple business of going out and looking should take place. Blumer insists (1969) that through processes of 'exploration' and 'inspection', one can provide detailed descriptions of events and patterns of activity, and thus speak from fact rather than speculation. The point here, to reiterate, is that any technique, including observation, interviews, life histories, the study of official and personal documents, and even 'counting' might turn out to be appropriate.

It is as well to remember this, for it forestalls naive questions concerning whether one is 'allowed' to do certain things when undertaking ethnographic work. It, for instance, opens a space for 'inspection'. Inspection, or thinking about what you've got, requires us to develop sensitizing or illuminating concepts. This is an important, if underused, notion in sociology. It refers to the difference between concepts which identify those things that classes of object have in common (definitive concepts) and those that just give the user a general sense of reference, a way of helping decide what to look for and organising your thoughts. The important thing about sensitising concepts is that they can develop and change as the course of study is refined.

Thus, two themes begin to emerge from this tradition. First, ethnography can be viewed as a method that remedies the faults of other methods, and hence training of some kind is critical to the success of the ethnographer. Second, ethnography is analytically distinctive; the task of the ethnographer is akin to a cataloguing of experience. These two emphases are variously present in work on the role of ethnography today and we can discern the first, in particular, in well-known treatments such as that of contextual design (Beyer and Holzblatt, 1998).

2.2.1 Contextual Design

Beyer and Holzblatt report on the development of a practice that has much to do with a need to ensure the usefulness of systems as it does with usability, and has been developed on the basis of considerable familiarity with academic discourse about ethnography (particularly the ethnomethodologically informed variety), although the intellectual auspices of their work are not always apparent, presumably because of their intended audience. Their book is clearly intended as a hands-on practical guide to integrating ethnographic work into the system design problematic. It is intended to be used as a reference guide, a book of rules of thumb and maxims of conduct for practitioners in many industrial settings. In part the success of the book on both counts 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 by students. For industrial settings, it offers guidance for how individual workers might do their own tasks as well as how they might cooperate with fellow workers, making numerous suggestions about how to enable the data gathered by individuals to be made sense of by the group as a whole, and this includes the use of Post-it notes, whiteboard diagrams, and so on.

Contextual design makes it clear that fieldwork is nearly always only an element of a larger process that entails multiple individuals and, moreover, that fieldwork in design itself is nearly always a multiperson activity. This has consequences on how data are gathered, shared, and analysed. In more traditional approaches to ethnography, the task of fieldwork is typically a solitary one, and fieldworkers have sole responsibility for writing up their data (which might go some way towards explaining the postmodern obsession with narrative). In contrast, the multiperson data gathering mechanisms of contextual inquiry are largely oriented to producing a coherent and demonstratively evidence-based perspective on the actions and needs of users that anyone on the project would recognise and to which they would agree. Indeed, ensuring this shared view is one of the sought-after goals of the methods of contextual inquiry. It is also something that we believe is a prerequisite of good practice in fieldwork for design.

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 organized and documented, is always construed in reference to the larger purpose at hand. 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. Although 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.

Focusing more particularly on fieldwork, contextual inquiry treats it as a task that can be done without reference to any particular body of theory or narrative of fieldwork reportage. What we are thinking of here is to contrast contextual design with the idea that ethnography, as one take on fieldwork, exists as part of a tradition that has built up certain ideas, theories, and concepts which in turn have some value when referred to and invoked in any new study. This is what makes ethnography distinct from fieldwork.

At first glance, straightforward fieldwork would seem to be the sole concern of contextual design, in that the main emphasis is certainly on the practical skills necessary in the field. Nevertheless, in our opinion, there are certain assumptions about the nature of fieldwork that underscore the use of the practical skills built into their work, and which Beyer and Holzblatt are keen to justify. These, we would suggest, are the output of the ethnographic tradition and in particular ethnomethodological inquiries. But Beyer and Holzblatt do not dress up these assumptions in histories of their use and importance in prior research, nor do they consider what competing assumptions could be deployed, and what is good and bad about each. Instead, they present these assumptions ‘as is’ and simply justify them, admittedly in a very disarming way.

For example, they point out that one of the problems fieldworkers have is to try to get their minds around the huge amount of data that can be gathered. Beyer and Holzblatt say that this begs the question of what is relevant, what is useful, what is too much, and what is too little. Certainly, they note that everyone is easily persuaded that having a look at the context in which a system is to be introduced is necessary, but the trouble is, they ask, what is it that one is to look for and what is it that one should take away? They claim (Beyer and Holzblatt, 1998, p. 4):

The trick is to give (fieldworkers) tools that let them see the breadth of the data with out being overwhelmed; to see the common structure and pattern with out losing variation, and to understand the wealth of details with out losing track of its meaning.

So far so good, but Beyer and Holzblatt then start bringing in some lessons from prior research, particularly so-called work practice research in the ethnomethodological tradition. They explain that the central problems of knowing what to look for and what to disregard, what is too much detail and too little, what is the patterning that needs to be sought, and what is too broad and varied, has to do with two things: the first has to do with the nature of descriptions; the second has to do with the skills, most especially the tacit skills, used in real everyday contexts. They explain that there is a subtle relationship between descriptions of work and the work as it is done in situ. This has to do with a variation on the old argument about situated action and descriptions of that action.

According to this view, descriptions are always reductive in one way or another. Having users describe their work, getting those who live in the context in question to account for what they do, is not sufficient for good design inasmuch as something might be left out by those descriptions. In this view, ethnography is clearly remedial. Beyer and Holtzblatt draw attention to the potentially limited, somehow incomplete, descriptions offered by subjects and treat the firsthand knowledge of the fieldworker as a compensation for that failure. The prospect that ethnographic data might in some way, for some purposes, be incomplete, does not seem to occur.

Beyer and Holtzblatt think, secondly, that the fieldworker should take on board the ‘tacit’ nature of many workplace skills. Skill deployed in the work being observed often becomes, Beyer and Holtzblatt note, invisible to the subjects themselves. These tacit invisible skills have to do with the things that people have to undertake to get to the work tasks on which they consciously focus. Oftentimes when a clerk, for instance, is asked to describe his work, even if he could more or less provide a good description, he may well fail to mention parts of his work, judging them to be irrelevant or related to a routine trouble that no one needs to know about especially if solving these troubles means breaches of the rules. Beyer and Holtzblatt see the failure of certain methods, notably interviews, as endemic in certain approaches to understanding users. Instead, they want the fieldworker to assume that part of work – any work, whatever it is that is being investigated for the purposes of design – is not easily articulated, even by those who are deeply experienced in it. There is a need, therefore, for a different approach, one that makes direct observation more important, one where watching is as necessary as listening, where mixtures of accounts and descriptions are used alongside other evidence to construct an understanding of the work itself that is greater than the indexical expressions used when the work is done. It is thus that Beyer and Holtzblatt get to the need for fieldwork. So, and in sum, the purpose of fieldwork, then, is as follows.

- To be able to articulate the unarticulated and tacit knowledge used in that work
- To make low-level details that have become invisible and habitual into visible materials that can be considered in design decision making
- To get to the work structure

We do not mean to imply here Beyer and Holzblatt underspecify analysis, for that is evidently not the case. They speak explicitly of a ‘language of description’. Watching, occasionally listening, making notes, and so forth are not sufficient, they insist, to do good fieldwork. They argue that this ‘language’ is necessary so that the actions under observation can be coherently and systematically structured. This language, for them, combines textual descriptions and diagrammatic representations along five different

dimensions. These have been selected, we are told, on the basis of what has turned out to be important over years of fieldwork and design. They are

- The flow of work
- The sequencing of work
- The role of artefacts in the work
- The nature of culture in the workplace
- The physical space in which work occurs

For the most part, these seem eminently sensible ways of thinking about some issues that arise in working life, hardly surprising inasmuch as suggestions about tacit knowledge, artefacts, the flow of work, and so on have variously been on the table for anything between ten and fifty years- and we use similar constructions ourselves later, albeit with a slightly different emphasis. We do, however, want to point to one or two rather problematic usages because they reflect precisely the absence of a sociological sensibility of the kind with which we are dealing.

Firstly, this has to do with the term 'culture'. Culture is perhaps the most ambiguously articulated concept in Beyer and Holtzblatt's exposition, and it is described as consisting in attitudes towards customers, hierarchy, and decision making. Culture here means how organisations who are in the same business might nonetheless undertake their work in a distinct manner. This is, we might note, a somewhat superficial take, and moreover one which is hardly in keeping with a rigorously observational stance, because attitudes and values can be seen as mental constructs. The point here is that 'culture' itself is a highly contested term and one which has been subjected to a wide range of definitions. Indeed, the distinctiveness of say, ethnomethodological work, can be seen, ultimately, as a particular take on culture in much the way, as we show, that Peter Winch recommends. Culture, in this view, is no more or less than what people knowingly do.

Secondly, and similarly, other concepts could be unpacked further, particularly some of those that ethnomethodologists have highlighted. Within the flow of work concept, for example, one of the topics that Beyer and Holtzblatt identify has to do with individual responsibilities. Anderson et al. (1989) have made much in their work of the importance of the individual and his or her relationship to others within a working division of labour. They coin the term 'egological' as a label for how one might analyse work processes in terms of the intersection of the perspectives of individuals constituted by their knowledge of a particular division of labour. This concept is valuable for drawing attention precisely to the way in which individual tasks are melded with orientations to, understandings of, noticings of, and so on, the work that other people have done or are doing.

Nevertheless, our words are not meant to be critical. Beyer and Holzblatt make a significant contribution, predicated on a huge amount of empirical work both on their part and on the part of others, and from which they draw their arguments. Moreover, they are perhaps the first to take the problem of 'design teams' seriously. They very sensibly concern themselves with how, having gathered data, the next step is data consolidation. They note that many of the detailed ways in which people get their work done, details that enable them to get their work done in particular and often rather unique circumstances, are not necessarily required for design. There is a fundamental difference they suggest, between designing what are called point solutions for the specific ways in which a particular individual works, and designing a generic system that can be useful in a set of users across an organisation.

Consolidation occurs in step-by-step fashion as follows. First, once a large number of interviews and observations have been completed, each researcher presents her data to her colleagues in what are called interpretation sessions. Here, the common properties of the work start being teased out and topics clarified. Sometimes the need for further interviews and observations also becomes clear when a researcher begins to see that her understanding of the work does not seem as complete as some of her colleagues'. Once this entire cycle of researching is done, the next step is to use graphical tools to create models of the systemic properties of work across the subject set. This is called an *affinity analysis*. Here, the key stages and elements in the observed work are labelled and presented as Post-it notes on a large wall. Those stages and elements that seem similar or even identical are physically placed near one another and gradually a map of the main features of the work emerges. An affinity analysis is done for all five dimensions of work: flow, sequence, artefact, culture, and space.

The outputs of these sessions are then used in design sessions. Here the goal is to develop designs that can deal with the complexity conveyed in the affinity analysis: a complexity that cannot be modelled as if it were merely a workflow representational task. Instead it is better thought of as a case of designing processes and supporting tools that can enable anyone to comprehend the task at hand given all the ways in which those tasks are typically completed. Necessarily, of course, part of evolving and developing a design solution will entail mapping the balance between human action and technological support, between space and artefact, between workflow and sequencing of work, and so on. It may also mean identifying areas of work that seem unnecessary and redundant. When contentious issues arise, such as apparent redundancy in a work task, the possibility of that redundancy is considered carefully and this often involves revisiting observed subjects and even getting subjects to participate in design iteration sessions.

In other words, Beyer and Holtzblatt argue that a design team will always consist of various specialities and experts, but that the work of design is not

a serially organised event with fieldwork being task one and, let us say, interface design task two. It is rather that the data from the fieldwork, the insights they provide, become the basis for all activities, referred to at an early stage in the work but then not forgotten, being referred to again and again and indeed sometimes even being revisited.

In contextual inquiry, fieldwork is then partly iterative and deeply bound to the overall process of design. It is thus also endemically bound up with the goals and purposes of design and is not a separate task or skill that produces a freestanding object, a description of work practices that is handed over to designers and then forgotten. Fieldwork data, or rather the analysis and consolidated representations of fieldwork observations, the descriptions of the work in question, the ways it is sequenced, flows, is supported by space and artefacts; all these concerns are specifically oriented with the purpose and needs of design in mind. Even when the researcher starts to interview, his goals, the concepts that he has at hand, the tools he will use to transform fieldnotes and other materials such as transcripts into usable evidence, all are driven by and bound up with the purposes of design. For Beyer and Holtzblatt there is then no separation between looking at the work, observing the work, describing and analysing the work, and designing that work.

We want to highlight four important aspects of contextual enquiry and try to clarify our own reactions to them. The first has to do with the assumption that fieldwork can be done without a sociological sensibility, as long as the fieldworker is aware of certain key assumptions about the limits of description, the need to observe work in situ, and the need to ensure that analysis captures the ways in which the complexity of any work task is handled in a day-by-day, routine way. We, in our turn, and as already stated, do not believe that collecting data requires anything more than the most mundane of skills, and agree wholeheartedly with their implicit conclusion that analysis is fundamental to the ethnographic project.

In the ethnographic tradition as practiced by sociologists and anthropologists, researchers are expected to be familiar with prior research and indeed are expected to invoke this research in their own work, even if it is by way of pointing out the contrasts. Whether ethnography can be done without reference to previous traditions, practices, concepts, and so on, it would seem to matter whether the ethnographer-to-be has some sense of what to look for and how to look at it.

At root, the problem is what sociologists refer to as 'reification', which has to do with making concepts 'thinglike'. We can treat concepts as 'things' which we can more or less take from the shelf and apply to the specific circumstances of our enquiry (and we see many examples of people doing so), or we can make use of concepts in the way that Blumer (*ibid.*) suggests. We unequivocally take the side of the latter. Good ethnographies, we think, are those in which the way of looking and its results, however represented, will show not only the use of a set of concepts but will also demonstrate the

existence of an ethnographic imagination at work. If not, the business of doing good ethnography can be reduced to that of following some rules. We have reviewed too many papers, shaken our heads, and sighed too many times at too many conferences to believe that for one second.

The second distinct element of contextual inquiry that we want to mark out has to do with how it entails teamwork both in the field and thereafter in the design process. According to Beyer and Holtzblatt, this means that interview and observational data gathered by individuals need to be reconciled against data gathered by others. This distinguishes contextual inquiry from traditional ethnography, where there is very little if any research addressing this particular problem. For mainstream ethnographers, fieldwork is essentially a task of solitary individuals. Now, these are clearly proposals. They are suggestions concerning how good design might be done where generic software properties are to be uncovered by ethnographic research. The proposals in question seem solid, if largely unremarkable. However, in our view, this is to significantly limit the remit of ethnographic enquiry in relation to design and we have a lot to say about this later. The assumption that ethnography and design can and should be closely linked at all is a somewhat problematic one and we want to point out that even if it were the case that ethnographic insights seldom resulted in clear design requirements this would not invalidate the ethnographic project because the results of ethnographic enquiry can, in principle, be used in a large number of different ways.

A third issue has to do with the insistence within contextual inquiry on being data-driven. This may seem to be an odd issue to highlight. After all, what fieldwork does not produce evidence and what is it for if not to produce evidence-based insights? But what contextual inquiry highlights is how evidence needs to be shared, tested, and reconsidered by everyone in the design team. That is, the assumption in contextual design is that the fieldwork data the contextual inquiry produces intrinsically address design issues. As we have seen, design is constantly present in the task of gathering fieldwork data in contextual inquiry, as it is also in data analysis and in considerations of design options once fieldwork has been completed. This is, we think, an important and contentious claim. It is rarely dealt with adequately, in our minds at least, in many other perspectives and contrasts directly with the 'over the wall' approach which lays out some more or less rigid demarcations between ethnographer and designer, thus preserving professional expertise and implying strict limits to responsibility in either camp. For a variety of reasons we are not entirely happy with either position and once again we return to the issue at various points.

In sum, contextual design is important in at least three ways:

- It sees ethnography in the interdisciplinary process as being saturated with design problems from the outset.



- It sees the advantages of ethnography as being to do with its status as a method and form of analysis.
- It lays out a programme for working with designers.

2.3 The Third Variant: Ethnomethodological Indifference

In 1958, Peter Winch's seminal book, *The Idea of a Social Science*, was published. Winch's book was enormously influential in sociology, and prompted an ongoing debate concerning 'reflexivity'. Winch followed Wittgenstein, and applied his arguments concerning the nature of philosophy to the problem of sociological theory and method. Winch's concerns are the degree to which an empirical investigation of the social world is possible, and what it means to analyse and understand a society. He is adamant that the natural sciences do not constitute a model for the social sciences because aping the former relies on confusions between empirical and conceptual issues. That is, mere data collection in the social sciences is not and cannot be the objective, neutral, fact-gathering it purports to be because our very reality is constructed out of the way we think of facts, and the words we use to describe them. In particular, he argues that 'To ask whether reality is intelligible is to ask about the relation between thought and reality.'

Because thinking, in this view, arises out of language use, we cannot consider the nature of thought without considering the nature of language. In turn, to use language is to act in the world and because we speak in different ways in different contexts, then we must analyse 'understanding' contextually. The problems of philosophy and the social sciences, if they arise out of language, must therefore involve what counts as being in the world. Whatever aspect of a culture we investigate, and indeed whatever culture we investigate, understanding how meaning is arrived at must be our method. Understanding, for instance, what scientists do or what religious belief consists in is, in both instances, a matter of understanding how they make sense to each other. Winch suggests, borrowing from Wittgenstein, that to make sense of social relations is to understand social behaviour as rule-following behaviour.

This argument, of course, is of profound importance because it has a close relationship with ethnomethodology and with Suchman's (1987) argument about plans. This is because arguments about 'plans' or 'situatedness' depend on what we mean when we talk about rules. Winch argues that understanding how someone is following a rule firstly requires us to understand not only how the individual takes action in a sensible way, but also to take into consideration what the reactions of others are. In other words, the rules are achieved out of the sense of the meanings individuals attach to their actions and the way in which others come to understand what is being done as meaningful. They are bound up with cultures, traditions, plans and

procedures, and so on, and thus we sociologists understand them only in and through our familiarity with what is typically done in certain contexts. Understanding what the rules of conduct are, then, must be a matter of understanding how these rules are exhibited in a social setting.

A second feature of rules as they apply to social life is that they exist only in the sense that we act typically. One cannot separate the rule from its application. Hence, as Wittgenstein put it, 'What has to be accepted, the given is – so one could say – forms of life.' We do not do what we do because there are rules (in a causal sense), rather there are rules because we do what we do. All behaviour which we can call rule-governed must by definition be meaningful behaviour. We cannot say someone is following a rule unless they know what counts as behaving in accordance with a rule.

What this means is that rules cannot be applied independently of judgements about their applicability. Activities are things about which we could sensibly say they have a meaning. This has to do with inferring motive and purpose. This is a subtle point and needs analysis; motives and purposes cannot be presumed from mental states, nor can they be equivalent to causes. What goes on in the head is not observable. Neither can we derive the rule which is apparently in operation just by introspection (i.e. thinking about it); we must derive it at least in part from our experience of the world in which the rules apply. Rule following and meaningful activity are, then, the same thing. It would be a mistake to think that this means we cannot break a rule, or make a mistake about a rule, because of course we can. Activities cannot be summed up by precepts (rules about rules). Rules are not recipes which we follow slavishly; neither are they 'rationalist' principles. They cannot be causal in the social world, for they are in their nature normative. (Searle (1995) makes a similar point about institutional 'facts'.)⁸

The importance of all this for CSCW-related work has to do with the kind of analytic procedure it recommends. Sociology's task, in this variant, is to elucidate the concepts which actors use: how actors define the contexts in which they find themselves. Sociology, in a nutshell, can map activities and concepts, or as Wittgenstein put it, identify and explore the 'language games' which are being played. Understanding motive and purpose is the relevant business of sociology, but motive and purpose cannot exist independently of our knowledge of social context. The task of the ethnographer, then, is to understand context in such a way that we can identify the rules which are in place and how people decide on their applicability; understand why people do what they do, and identify the social contexts which make them meaningful things to do. The ethnographer cannot do any of these things without familiarity with cultural activity. This is why we referred

⁸Winch allows that we do, of course, use a vocabulary of cause when we speak of motives. For example, 'I wonder why he did that?' (see also Burke (1945)) but denies the causal status of 'motives' in any strict scientific sense.

above to the problem of ‘culture’, because for Winch there is no thing as culture; there are only activities.⁹

Winch’s argument, difficult and contested though it is, has a relevance to fieldwork in design communities, for it has implications for our understanding of regularities. Regularities are constant recurrences of the same thing, and as such would appear to be evidence of structures or institutions in a culture. There is nothing wrong in principle with identifying regularities, but Winch makes the point that ethnographers who simply describe various activities as being ‘examples’ of a particular thing are missing the point. The problem is precisely what counts as the same thing and what warrant we might have for so deciding. This position informs ethnographic work of the kind exemplified by, for instance, Andy Crabtree’s (2003), *Designing Collaborative Systems*.

2.3.1 Designing Collaborative Systems

Crabtree’s book is distinguished by its resolute insistence on an ethnomethodological stance in ethnographic work, a view common in the CSCW literature but underrepresented elsewhere. Such work often, implicitly and explicitly, criticises cognitive science both for the ‘scientistic’ errors Winch (and Wittgenstein) accuses it of and for the conceptual confusions inherent in the rush to generalisation about regularity. Broadly speaking, *Designing Collaborative Systems* consists of a set of fieldwork-based investigations of how people in workplaces deal with the endemic contingencies of working routines, contingencies, and incidents which result in people ‘working at’ and ‘achieving’ what they construe as the goals of that work. According to this view, the purpose of fieldwork is to capture what these contingencies might be and to document the ways in which they are solved.

Ethnomethodological studies of work would seem primarily empirical rather than theoretical, and thus at first glance well suited to delivering materials for design. Indeed, this very claim – albeit oversimplified here – is made often in the CSCW literature, by ourselves amongst others. But it should not, we feel, be made uncritically nor should the empirical focus of ethnomethodological studies mislead CSCW practitioners into thinking that studies of work are merely empirical. Ethnomethodological studies of work are empirical in a specific and contrastive sense. They deal in the ‘facts’ of the social world in a way that is quite distinct from what sociology has typically done, and do not produce theories of the type that sociology

⁹Note also that, for Winch, motive and purpose are legitimate problems for sociology to deal with, but they have to be dealt with through our reasoning about social context. By and large, ethnomethodologists agree with Winch about this. It is simply not the case, as Nardi (1996b) has suggested, that ethnomethodology (or ‘situated action theory’ to use her neologism) has no account of motive.

normally does. Put simply, for good or ill, ethnomethodology has often been measured against sociology at large. Of course, in design-related contexts, ethnomethodology's relationship to sociology is wholly irrelevant. Designers would be quite entitled to ask, 'Who cares?'

The problem here is, as postmodern sociologists like to emphasise ad nauseam, descriptions are potentially infinite. One can choose to describe any event or situation in any number of ways. We need, in other words, to figure out how our work can be made empirically relevant in the sense of what may be understood as such from the perspective of designers and system engineers, and not from the interests of sociology, whilst retaining whatever it is that makes sociological ways of thinking interesting. The ethnomethodological solution is to insist on the description of members' meaningful actions, their 'lived experience,' to the exclusion of the more constructive kind of analysis to be found in sociology at large. Hence, when we read the condemnations of the 'cognitivism' and 'scientism' (that obscure the 'actualities of work') that characterise Crabtree's book we need to decide whether such critiques are relevant only to ethnomethodology's unremittingly hostile relationship to both sociology and psychology, or to the specific problems of design work.

Now, we have made similar claims ourselves at various times and can hardly complain about this partisan line. At the same time, there is something very important at stake here. This is that those of us who practice something like ethnomethodological work in an interdisciplinary context need to justify the particular advantage involved in doing our work ethnomethodologically. In other words, we need to be able to state, and state reasonably clearly, what we get from our ethnomethodological commitments that we cannot get elsewhere, and which enriches and refreshes design. It is worth reminding ourselves that the likes of Vicente (*ibid.*), who come from a cognitive tradition, can be equally critical of studies that idealise or overly schematise work, that overly emphasise the formal over the informal, that focus on the standard procedures rather than the more flexible, and fail to capture the skills, techniques, and processes that are actually entailed.

Crabtree recommends, in line with standard ethnomethodological positions, quite finely detailed descriptions of particular aspects of work. These aspects to be focused on, he argues, are not to be selected by the researcher who might wish to simply look or record and thus capture what she thinks is relevant; it is rather that the practical concerns of the participants in the setting should be used to guide what the researcher should look at. This sounds fairly straightforward, and for the most part, it is.

Where Beyer and Holtszblatt make specific, and strong, suggestions about analytic themes, Crabtree does not. In taking a resolutely ethnomethodological view, he wants to emphasise the way in which any analyst can see what is out there in respect of the fact that a commitment to understanding the members' world as they understand it is what is primarily

required. This is done by a number of devices familiar to ethnomethodologists, and includes notions of ‘vulgar competence’, ‘*lebenswelt* pairs’, ‘unique adequacy’, and so on. This sounds rather opaque, and for the individual unfamiliar with ethnomethodological language, rather intimidating, but Crabtree tries to show how this pans out in terms of three kinds of explanatory materials that need to be presented when doing design.

The first has to do with the ordinary meaning of what is said by those studied, those ‘endogenous’ formulations as he puts it. The researcher should trap these or in some other way capture them.

The second is to describe the relevant nonverbal actions of those persons, particularly those material resources used in work. Little guidance is given as to what these might be, although presumably whatever the members treat as relevant and as an important resource in their work, should be captured or at least described by the fieldworker.

The third is to describe the work in terms of its sequential organisation and ‘component events’. By this he means the moment-by-moment series of actions that are linked to each other as in a chain. The most obvious example of this is turn-taking in conversation, but as Crabtree points out, quite rightly, we should remember that this is only one form of sequential organisation.

Fieldwork, then, consists of assembled instances of work. Any material could be gathered to do this, whether it be tape recordings or video, fieldwork notebooks, articles, or objects from the field. The ordering, cataloguing, and analysis of the material has to be determined by the context itself. To do otherwise, Crabtree warns, is to produce a version of ‘constructivist’ explanation. What needs to be done instead, is to look at how the ‘vague descriptions’ of the subjects are transformed into ‘tailored descriptions’ (p. 73). This is a kind of filtering work and how it is done needs to be understood. Doing so will lead to what he calls, following Ryle, a thick description of ‘members formulations as they are hear-ably produced and recognised *in situ* by the parties to their production’ (p. 73).

The relevance of the assembled data to design has to be established in some way. Crabtree argues (pp. 94–95),

It might be said that those who contested ownership of the problem were right to do so only in the sense that ethnographers rarely possess the necessary technical competence to formulate design solutions on their own.... This does not rule out a constructive role for ethnography in the formulation of design solutions. Adopting any such role will require that ethnographers move beyond ‘imparting knowledge’ to directly inform the construction of design solutions in collaboration with the other parties to systems development.... If ethnography cannot actively and constructively support system development in the reorganization of work through technology design rather than run for cover, then it has no business in the field. While rightly leaving the actual production of design solutions to designers, ethnographers are nevertheless obliged to assume a constructive role

if their craft is to be of any lasting utility. This means that the ethnographer will have to rid themselves of disciplinary baggage and become a *bricoleur* in design practice.

He goes on to discuss some methods by which this might be done, including using viewpoints analysis (Kotonya and Sommerville, 1992) and subsequently, the coherence model (Viller and Sommerville, 1997) and argues that these models sidestep the problem of communication in the design process. He also rehearses air traffic control material in order to demonstrate the way in which designers could misunderstand the import of, for instance, analysis of the functionalities of flight strips as 'found' data. He is kinder to pattern languages (Alexander, 1977, 1979), admittedly in an adapted form. This analytic device might, at some stage, provide a *lingua franca* for design, through analysis of 'commonsense typifications'. He further discusses aspects of scenario-based design, use case modeling, and so on in terms of how workplace study can contribute to design as a form of co-construction. Similarly, ethnography has an evaluative role to play through its emphasis on situated evaluation or through a relationship with participatory design.

From this perspective:

- Ethnography is a descriptive and analytic process that needs no remedy.
- The appropriate analytic stance is rigorously ethnomethodological, emphasising finely detailed, processual/sequential analysis.
- The ethnographer has the role of *bricoleur* in relation to design.

There is a danger here, although not one specific to ethnomethodological enquiry. It is that readers can presume that there are relevant empirical facts simply waiting to be uncovered. Where Crabtree suggests that ethnomethodological fieldwork produces free-standing objects – the descriptions – that are handed over to the design community, this can be read (mistakenly) as suggesting that there is no particular issue with what kind of description is at stake as long as it is in keeping with the above ethnomethodological injunctions. This will work fine, we feel, in known settings with known technological relevance. Quite how it is to be applied in situations where we have only the sketchiest understanding of technological possibility and of the domains in which innovation might apply, we do not know. Deciding on a programme of ethnographic work, in this situation, will require rather more than an ethnomethodological determination to render the phenomenon for the simple reason that figuring out which phenomena will be relevant phenomena will be far from straightforward.

Given our own research record, we might be expected to be sympathetic to Crabtree's formulations, and of course we are. Nevertheless, we want during the course of our own exegesis to ask a few questions about the

relationship between ethnomethodological studies of work and the design problem. That is, we are interested in the specific context of design-related work, and in interrogating some of ethnomethodology's assumptions in this area. No one would assert, we assume, that ethnomethodology has all the answers. At the same time, it does seem to us to be important to discuss which answers, and to which questions, ethnomethodology might provide. Moreover, we feel that it is equally important to try to understand what relationship ethnomethodology might have with other perspectives. This is because there are a host of as yet unresolved questions about what ethnomethodology can do in relation to the design of what kinds of thing and in what kinds of design community.

Where, for instance, Beyer and Holzblatt suggest that organisational culture is a relevant matter for designers, ethnomethodologists tend in the design context to be quiet about matters of culture, wider organisational context, and so on. There are good reasons for this: ethnomethodology has been (for good reason) negative about anthropological conceptions of culture and about formal and informal conceptions of the organisation. We need, then, to discuss whether ethnography and the ethnographer need concepts of culture and organisation and, if so, what kinds of conceptions they might be. We also, in our opinion, need a much more nuanced view of ethnography's relationship to design. Crabtree uses, as do many others, Hughes et al.'s (1994) 'Moving Out of the Control Room' paper as a basis for his discussions. So do we. We, however, argue that the four functions of ethnographic enquiry they suggest are nowhere near adequate for describing the range of possible inputs into the design process, for it will depend ultimately on what kind of design process we are talking about, and it is increasingly evident that there may be many different kinds.

2.4 Morals and Metaphors

A further turn in the ethnographic project dates from the publication of Howard Becker's famous paper, 'Whose Side Are We On?' (1967) Becker's work was very much associated with an ethnographic approach to the study of crime and deviance, and he was one of the first sociologists to identify the way in which deviance can be a 'social construction'. That is, that what is deviant is to some extent at least a matter of definitions by more powerful people such as the police, social workers, and the courts. This became known as labelling theory and within it the task of an ethnographic sociology in this context, then, was to explain what it is like for the people so labelled. Crucially, for Becker, this entailed an ethnographic stance where one knew where one's sympathies lay.

This can be argued in two ways. For Becker, sympathy for the underdog was a necessary result of ethnographic enquiry because, failing completely

cynical use of the groups in question, it would be impossible to fit in without the required empathy/sympathy. Becker was not suggesting that we should explicitly take sides before we undertake investigations, rather that when one adopts the ethnographic stance one inevitably gets involved in a sympathetic consideration of their viewpoint. One could not, in Becker's view, understand their world unless one did this. In a nutshell, ethnography involves seeing things from the point of view of the participants through a moral/political lens.

'Seeing things from the point of view of participants' takes on a rather different cast yet again, however, when we take a brief look at explicitly 'standpoint' epistemologies. In these, Becker's inevitability is embraced from the outset. Readers will be quick to see a relationship between this initial position and the subsequent stances rehearsed by Marxists, feminists, and, in the design community, participatory design.¹⁰ If we may characterise this move in a rather broadbrush way, we might call it standpointism, after standpoint feminism. Standpoint ethnography by definition refutes any possibility of neutral or objective stance in ethnography. Hence (Edles, 2002, p. 145):

"The questions were political, epistemological and methodological"; who gets to say what about whom, and why? What are the interests and motivations behind alleged ethnographic "realism".

Some feminists have defended the idea that there can be and is a distinctive feminist methodology, characterized in part by its commitments. It must be recognised that there are a number of different versions of feminist epistemology, and indeed some doubts have been expressed whether a distinctively feminist method is possible at all. Sandra Harding (1986, 1987, 1991; Harding and Merrill 1983), for one, believes that the methods feminists adopt are not to any degree distinct, but their methodology is. There is, she argues, something distinctive going on, in that feminism implies rather more than 'adding women' to an orthodox analysis. Standpointism, then, rejects the possibility of a neutral description and analysis and embraces political purpose. For interdisciplinary work, this raises the prospect that ethnographies should recognise such matters as power, interest, and commitment. It places the enquirer 'in the same critical plane' as the subject. Rather than the

¹⁰In passing, one obvious problem with this basic view is that it makes a presumption about the nature of the relationship between the 'underdog/class' and those who label them. For some, Becker is confusing 'understanding all' with 'forgiving all', suggesting they are indeed the same thing whereas they are not. For others, the problem is that Becker misses the point about why there are underdogs. In other words, his interactionist viewpoint tells us nothing about the structure of power relations in society, which in turn might explain why the police, courts, and so on do the labelling that they do. A third, and rather more incidental argument for sociology, which has seldom troubled itself with such problems, is that sometimes it is rather difficult to judge who the underdog is. Hunter Thompson's classic book (1966), *Hells Angels*, for instance, shows considerable sympathetic understanding of a community of people the rest of us might not view as underdogs at all.

researcher being faceless, researchers place themselves alongside the subject by identifying their own class/race/gender position.¹¹

Now, not for the first time we suspect, HCI and CSCW practitioners at this point will be wondering what, if anything, this excursion into standpoint epistemology has to do with design. The simple answer is to invite the reader to substitute the word, ‘users’ for the word ‘women’ at every point. When we do so, we see that standpoint epistemologies are pretty much what underpin approaches to design such as participatory design, and this is our reason for raising the issue. Nevertheless, there are some difficulties with such an analysis. As we show, developments in Participative Design (PD) are very much concerned with how we answer such questions when we apply standpointism to the user.¹²

This debate opens up a space, as we soon show, for the most radical of the intellectual positions that can be associated with ethnography, and the one that is most distinguished from the view that ethnography is just about collecting the facts, for in this (postmodern) view, facticity itself is under scrutiny. Postmodern ethnographies are often described as relativistic (whether strongly so or not) because they challenge the idea of the ethnographer’s authority, and particularly the view that the ethnographer is involved in truth telling. In other words, ethnographic work is to be regarded as just telling a story, and only one among many possible stories; notions of truth are fundamentally problematised for there is no cultural ‘whole’ behind the ethnographic story.

James Clifford (1988), a proponent of this view, argues that the ethnographic narrative which sought to convince an audience that data were ‘objectively acquired’ has been overtaken by a new formulation, one which at least in part, takes cultural relativism seriously (see also Clifford and Marcus, (1986). Part of this, says Clifford, involved the play between the value of experience and of interpretation and, he says, increasing emphasis was placed on the second of these two because experience is difficult to articulate and our claims in respect of it naive (at its worst it is dishonest;

¹¹In contrast, Clifford Geertz (1973) questions whether, ‘seeing things from the point of view of the actor’ needs to be seen as a moral commitment at all. Geertz makes reference to a famous incident in the history of anthropology – the publication of Malinowski’s (1967) diaries. With Malinowski, what became clear was that despite his sympathy/empathy for the native in his published work on the Trobriand Islanders, he was in fact a rather nasty individual who disliked the natives he was studying, thought them ‘primitive’, and generally wished he were somewhere else. The point is that a cornerstone of the ethnographic imagination has always been the notion that we put ourselves ‘in the place of the native,’ suspending our critical views, biases, and what have you. Malinowski clearly did not do that. Geertz asks whether this means that Malinowski’s general conclusions are unjustified, and for him the answer is unequivocally, not at all. Ethnographers can and must be capable of transcending experience. They do this in and through reflexivity.

¹²Standpointism has subsequently widened its remit. Ethnography has, for instance, been accused of promoting a colonialist attitude (Said, 1978), telling us more about the researchers, and their (usually his) attitudes, than the cultures they purport to describe.

see Freeman (1999) for a discussion of Margaret Mead's anthropology. See also Cote (1994) and Murray (1990)).

The claim to interpretation is equally suspect. In entering a foreign culture precisely what is missing is the commonness of cultural background that is the foundation of Weber's '*Verstehen*' and hence that became what had to be recovered. In other words, the ethnographic task became, in part, that of discovering a common language in its widest sense. It suggests a cumulative and deepening knowledge of the culture in question. Clifford doubts whether any such claim can be maintained. This attack has been influential in the production of postmodern ethnography, in which the role of, and status of, the ethnographer becomes part of the topic of the ethnographer. Put simply, the dialogue between ethnographer and subject is scrutinised.

Again, nonsociological readers must be in a state of some agony at this point, and for once we can only sympathise with their pain, for it is hard to see what it is exactly that matters in postmodern accounts, other than the prospect of intellectual *nombillisme*. One consequence, however, has been a growth of 'insider' ethnographies, whereby familiarity with a culture is a given, providing ethnographies which are empowered and restricted in unique ways and this does have some significant ramifications. The choice between insiders who have prior knowledge of the field in question and outsiders who claim some neutrality is not a trivial one. One small caution that results from the postmodern perspective, and worth remembering, is that we should not represent our findings in too complete a way, for our results may not be as reliable as we would like to think, especially when we seek to generalise them precipitately.¹³ In exploring the relationship between purpose and method, Marcus also explores the issue of the ethnographer as solitary individual, as a sole researcher on a voyage of observation. These voyages, like Sinbad's, produce stories. Like John Barth's (1991) postmodern version of Sinbad, however, this begs all sorts of questions about fact versus fiction in storytelling, about narrative structure and the problem of evidence in those same stories, and much more besides. By 1999, however, the crisis in anthropology led Marcus to consider new forms of ethnography which explicitly addressed the need for interdisciplinary enquiry.

¹³We do no justice to the subtleties of postmodern argument here. The textuality debate, for instance, has historical roots in philosophy and critical theory but has recently culminated in the 'ethnographies as texts' movement. See, for instance, Marcus and Fischer (1986) and Hammersley (1990). In one view, ethnographic writing is determined contextually, rhetorically, institutionally, generically, and historically. Moreover, these features govern how 'ethnographic fictions' are produced. In this view the notion of a naturalist ethnography that merely describes the facts of the matter should instead be regarded as, 'An insidious discursive strategy whose underlying purpose is to assert authority, dominate and maintain privilege' (Edles, 2002, p. 151). Dicks and Mason (1998) identify two aspects of postparadigm ethnographic enquiry in particular – the demarcation of ethnography's object of study and its mode of presentation – as areas of vociferous debate.

In case any interdisciplinary practitioners should find themselves seduced by postmodernism, they should be aware that it too has come under attack (see, for instance, Sharrock (1995) and Slack (2000)) and justifiably so. Nevertheless, the postmodern turn, especially in George Marcus's *Ethnography Through Thick and Thin* (1998) does point to at least one other relevant matter. Marcus insists that ethnography needs to be understood as always driven by particular analytic foci. Traditionally these have been construed by the anthropological and sociological disciplines but latterly they have been developed and imposed by other disciplines and concerns (he does not mention design). At this point, we should perhaps acknowledge that the nature of interdisciplinarity is all too seldom discussed and might be a great deal more problematic than is normally acknowledged (see Fish (1994)). 'Multisited' ethnography, for Marcus (1998) is a way forward. The erosion of anthropology's traditional interests in primitive societies and the close investigation of their particularities has been replaced, he suggests, by the need for an ethnography which recognises globalising tendencies. Marcus (ibid) suggests that multisited ethnography represents 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".

The crux of the matter lies in the degree to which we can think of ethnographic research for CSCW purposes as an 'emergent object of study' and the degree to which this informs what questions we are to ask, and what answers we are to seek. Marcus' notion of interdisciplinarity is worlds removed from the kinds of interest we have, but we can legitimately ask what contribution multisitedness – or new forms of comparison – can make in contexts where the primary analytic concern might be (but not exclusively) the use rather than the effect of new technology, and the purposes of the kind of ethnographic analysis we provide might be the design rather than the critique of technology. Regardless, the multisited view of interdisciplinarity might at least give us pause to reflect on problems of empirical relevance, of conceptual orientation, and of the role of comparison.¹⁴

The questions that are entailed have not gone away: what kind of site constitutes an appropriate site for investigation? To what extent can the detailed and local results typically resulting from ethnographic enquiry produce results which are generalisable, and for what relevant purpose? Who decides, in this context, what an appropriate analytic framework looks

¹⁴See Randall et al. (2005) for an extended discussion of these themes.

like, and what should our ambitions be? The questions thus entail some engagement with the relationship between data and theory, or at least with the possibilities for relevant analytic purpose and relevant generalisation. The failure to answer these questions adequately might account for the hitherto rather disappointing set of achievements in the fields we are interested in and address the problems inherent in ethnography's move away from the co-located, 'shared goal' work associated with control rooms, professional work, office work, and even domestic life.

In other words, by looking at the problem of empirical adequacy in anthropological ethnography, Marcus highlights (and we strongly agree) that the first and primary question in fieldwork, whether it be for anthropology or any other discipline, is what the purpose behind that fieldwork might be. If one follows the line of argument then the thesis in respect of design might state:

- That design should produce its own set of concerns that will drive the fieldwork-for-design enterprise
- That it should result in certain types of styles of reporting, with particular tropes at its heart
- That it should deliver a corpus of reasoning that constitutes its essential framework

If all this holds true, then, fieldwork-for-design or ethnography-for-design will be distinct from what we find elsewhere. This takes us back to the beginning of the chapter, to the question of what is done when fieldwork is undertaken, what is looked for and ultimately how these 'seeings' and 'tellings' are rendered.

2.4.1 Issues Arising

We have tried, above, to say something about the social scientific tradition and the way it imparts a sensibility (of one kind or another) to ethnographers. We have used particular texts to demonstrate the variety of takes on the descriptive, analytic, and theoretical roles of ethnography there might be within a broad tradition. Of course, we could have chosen other texts to encompass the range of views we have in mind, but these were chosen for particular reasons. They are precisely that they broadly represent some of the differences we have outlined above, holding positions along the continuum we outlined. Thus and for instance, Vicente holds that data from the field are insufficient to design the future and Crabtree that fieldworkers should produce 'whatever it is' they find as materials for designers to use. Beyer and Holzblatt, in contrast, have a strong commitment to the idea that ethnographic output should strongly orient to design at the outset.

We suggest that all these convictions can, in some circumstances, be wrong and in others perfectly reasonable. In large part, this is for the same reasons in both cases, and they have to do with the purposes of the ethnographic enquiry, which can be many and varied.

It is our view that descriptions of any sort are always a function of the questions that have necessitated those descriptions in the first place, and really what is at stake here is who has the professional right, and at what time, to define what those purposes might be. It is quite wrong, we feel, to suggest that results can stand independent of the purposes behind them. It is not so wrong to argue that these purposes, and rights over them, may themselves be a matter of ongoing controversy.

In many ways, and perhaps here is an example of the sociological imagination at work, the situation is akin to Max Weber's discussion of values in sociology. Weber was much concerned with how, if at all, sociology might intervene to make a difference in social change. He was at pains to point out all of the problems that ensue when enquiry is 'value laden' and nonetheless argued strongly for a sociology which was 'value relevant'. We find this a useful analogy. We do not believe that ethnographers in CSCW and other design-related contexts can and should produce enquiries that stand in no relevant relationship to design: we strongly believe that ethnographic enquiry should be 'design relevant'. We would at the same time strongly resist the notion that our enquiries should be 'design laden'. Ethnographers cannot serve an underlabourer role here. They cannot ply their trade in an environment where they are pointed at a predetermined 'context', and told what to look for and how to organise the results. We argue, rather, that in being concerned with seeking out aspects of work practice with a design-relevant intent, the descriptions that result are naturally design resources; although to be really useful, or better, evocative, they require imagination.

Tacit Knowledge and Ethnographic Analysis

At this point, we want to emphasise the main issues that, for us, seem to arise from these different viewpoints. All, we feel, are important and during the course of the book we hope to establish our position vis-à-vis each of them. Beyer and Holtzblatt make much of the 'incompleteness' of interviews as data-gathering techniques. Now of course some might say that the failure of a person to describe all aspects of his work is a failure on the part of the interviewer to ensure all that is necessary is elicited, but they disagree. They say that such a failure, if that is what one wants to so label it, is endemic and a typical feature of interview-based approaches to understanding users and is thus a methodological problem.

In some respects, and although in others we are in wholesale agreement with these authors, we find this distinction mystifying. We have no doubt at

all that work has its 'tacit' dimensions, but find it very hard to understand why this should be a problem of articulation for the worker. In our experience, people at work have few if any difficulties articulating their work when asked the right questions and we can see no reason why we as ethnographers should not talk to our subjects. If not, the kind of ethnography that seems to result is the 'fly on the wall', 'point a video camera at it' kind where some type of (rather spurious) objectivity is conferred on the data in virtue of the fact that they have been recorded.

We, in contrast, are of the 'when in doubt, ask a question' school of enquiry and doubt whether problems of either completeness or objectivity are of quite the kind that are implicated in contextual design. The importance of this, in our view, lies in the emphasis on method as against analysis. We, we hope, consistently argue throughout this book that issues of method turn out for the most part to be much less problematic than one might assume. That they are issues at all, we feel, comes largely from that long sociological and anthropological tradition of using a language ridden with jargon, and in particular the illusion that some sort of precise technical language will distinguish sociology from the commonsense equivalents of the layperson. Issues of sensibility, in contrast, we believe to be vitally important.

An example of the consequences of this kind of methodological/analytic discussion might be how we go about talking about the notion of 'skill', for instance, and how it might relate to an emphasis on tacit knowledge. We have to decide, for design-related purposes, whether categories such as skill are relevant, and this is a nontrivial exercise. Experienced ethnographers will know, for instance, that in pretty well all jobs of work, operators will know that, and talk about the way in which some people are better than others at what they do. In principle, of course, such matters are observable/reportable in the way that ethnomethodologists say they are.

What these skills are and how best to describe them is by no means simple, however, and a domain such as air traffic control illustrates this as well as any other. 'Skill' used in this way is, without question, an analytic construct. Put simply, our experience of working in a domain such as air traffic control was one of studying the processual character of interaction, understanding the sequential character of what they do, and the artefacts which they use in much the way that Crabtree recommends.

At the same time, however, we became interested in the way in which 'good' air traffic controllers could be distinguished from others and how we might characterise this. Now, air traffic controllers are perfectly willing to talk about matters such as what makes a good air traffic controller and did so for us on many occasions. The general picture of skill that resulted – one that we would defend as being deeply relevant to the allocation of function problem – is not, however, one that depends on an ethnomethodological form of enquiry. Any decent ethnographer, from any sociological or psychological background could, on the assumption that they take the business

of doing ethnography seriously, come up with similar results. This leads us on to what we consider an absolutely central theme.

Why Ethnomethodology?

One of the things that we, as ethnographers plying our trade in CSCW surely have to do is explain why, if it is indeed the case, ethnomethodological ethnographies are in some way superior to others in a design context and justify exactly why it might be that these specific analytic choices confer an analytic advantage. In our view, critique of the idealised conceptions to be found in other perspectives is a little past its sell-by date now and ethnomethodologists working in design-related arenas still have questions to answer. All of these questions relate, in one way or another, to the relationship between what sociologists call case studies (of which ethnographies are an example and which are often criticised within sociology for having very little generalisable consequence) and the existence of generic systems. That is, we need to take generalisation seriously.

We have no particular view on the kinds of generalisation endemic in versions of pattern languages, as advanced by Crabtree, for it seems to us that ‘patterns’ as used by ethnomethodologists and others are nothing more than a convenient coathanger for doing some kind of generalising work. An alternative we mention later is meta-ethnography (Noblit and Hare, 1988). The existence of a framework called a ‘pattern language’, however, does nothing for us in terms of deciding which patterns, typifications, general features, call them what you will, might turn out to be useful and relevant.

Equally, appeals to the commonsense nature of these typifications by ethnomethodologists rather begs questions in the design context of whose commonsense typifications might be relevant given that, in any other context but the classic control room study, this might turn out to be a nontrivial stakeholder problem. Of course, we can argue as ethnomethodologists that this is the kind of question we are not equipped to answer, or that answers to questions of this kind require us to remove our ethnomethodological hats and answer in another capacity. Neither response, once we take design relevance to be our primary problem, seems entirely satisfactory. There is, it seems to us, a more fundamental issue and that has to do with the idea that fieldwork consists of a natural task, that of describing actions and contexts as they are, or whether an analytic sensibility towards fieldwork-informed design is desirable at the right moment. It seems to us that the materials that fieldwork produces need, by definition, to be sifted somehow to become relevant to design thinking and that the progress of fieldwork itself, at some stage, must orient to design interests. These are issues of responsibility and much of what we argue below is about the responsibilities that ethnographers – ethnomethodological or otherwise – can and should take on.

The Variety of Relationships Between Ethnography and Design

Most of the authors under discussion above refer in different ways to the relationship between ethnography and design and make suggestions as to how ethnographers might help in design projects. We feel they are under-nuanced. This has to do with the ordinary practical problem that the ethnographer has when going to do her work for the first time. This problem, depending on experience and orientation, manifests itself in worries about-what one is likely to see and how to determine what to look for, whether one will understand it when one sees it, how long it is likely to take, what one might miss if one is not extremely careful, and lastly how exactly can one talk about it in such a way that someone, somewhere, can draw some design conclusions from it? All of these problems, of course, take place in a variety of design and observation contexts.

Although Beyer and Holzblatt (op. cit.) make clear recommendations about how ethnographic data might be represented to others, they are less clear on the sheer variety of practices that one might find when ethnographers and ethnographies get used. They are, in effect, proposing ways to use ethnographic results in focused commercially based projects. In contrast, Crabtree, for instance, makes no distinction between the uses that ethnographic data might be put to according to what kind of project might be in question.

In turn, this matters a great deal because the purposes of the ethnography in question are, in practice, often up for grabs. We want to say a great deal about this, because in our view the scale, auspices, and timeframe of design projects are hugely consequential for the way ethnography might be used. Much more important, we find the argument that design is a technical matter tiresome. There are, of course, stages in design processes that look rather technical, and programming would be one of them.

The idea that figuring out what a technology is for (high-level requirements analysis in some views) or even decompositional strategies aimed at meeting completeness and consistency requirements are technical in nature is, in our view, absurd. Moreover, some aspects of design can be thought of as 'scoping'. That is, design might be viewed sometimes as what might be possible in environments where relatively little (computer) technology has been historically applied, such as the home or the public space. Design might equally be to do with look and feel, with aesthetic properties, with fun, and with sheer creativity. It might, more prosaically, be closely associated with issues of cost and benefit, with, if you will, accountancy. Now, we are not suggesting that ethnographers can be involved in all these things, for some aspects of design have no discernable relationship with the concerns of ethnographers. Other aspects, however, might well be susceptible to an ethnographers-as-designers treatment that is some way removed from the fairly orthodox views expressed in the literature we address.

Current Practice and the Future

Vicente (op. cit.) assumes that descriptive data (his name for fieldwork data) are about the present and therefore cannot be sufficient for design of the future. This is because any attempt to design necessarily goes beyond the evidence provided by those descriptive data. Such arguments have been around in HCI and CSCW, as well as in other fields, for a long period of time and it is perhaps time to dispose of them. Ethnographic insights cannot, on their own, replace the modeling work associated with design endeavours. Design, by definition, is formalization. Ethnography's analytic interests offer something different in addition to, not something which replaces, other and more methodical approaches to the design of the future.

Whilst accepting the view that analyses of current practices cannot on their own provide future system states, there has been a casual assumption that current and future are two quantumlike states between which there is no movement. One of the things that ethnographers and others in the CSCW community have pointed to is the long-term contingent outcomes of technological interventions. That is, we might envisage a solution whereby, at some future point, everyone is now working in an entirely different fashion as a result of some new and sophisticated computer system, but there are real questions about how we arrive at this point. There are, in the ordinary reality of human experience, and unlike the quantum universe, a whole series of potential transitions to go through before we arrive at our ideal state.

These issues of transition, for instance, were what informed the Lancaster Air Traffic Control project. Thus and for instance, when the functionalities of flight strips were considered in some detail in this work, the argument was never, 'This work is so rich and complex that technological solutions will never be found to replace it,' but that we need a close and careful analysis of what these functionalities actually are in order to begin to make judgements about both the advantages and disadvantages that might be implicated in our redesigned work environment (see Shapiro et al. (1994)). To put it another way, where Vicente emphasises the constraints endemic in current work practices, he is curiously silent on their affordances. Both, it seems to us, might matter.

By way of concluding, it should be obvious that no toolkit, method, or fully fledged methodology can possibly produce a guaranteed successful outcome in design. Designing the future is by definition designing for the unknown. Nevertheless, and as we all know, educated guesses are better than uneducated guesses. In each instance, our tools, methods, and procedures are intended to reduce the uncertainties associated with design. These uncertainties might have a number of sources, some of which ethnography was never intended to address. There might be others, however, which only it can address.

Concepts and Common Sense

One of the authors found himself in a position recently where, upon asserting that he wasn't particularly interested in theory, but he was interested in concepts, got the reply, 'Nonsense, you can't have concepts without a theory.' To which we must answer, 'Yes, you can.' Above, we have presented some of the ways in which concepts might work. For ethnomethodologists like Crabtree, they are of no interest beyond members' typifications (see also Sharrock and Randall (2004)). They work for others in an illuminating way. For the likes of Vicente, they have a more determining quality, albeit in a relatively narrowly defined set of circumstances. For Marcus, they can be thought of as part of an evolving canon of tropes. Ethnographers in this view, do not simply gather data but, in contrast, what they look for, what they invoke as relevant matters, and how they explicate those materials that they find interesting, and because this is what they are speaking about and dealing with, are driven in part by their familiarity with the ethnographic canon. They are dealing with matters internal to the discipline and not producing empirical evidence that would make sense elsewhere. It is precisely Marcus' point that a multisited ethnography, dependent on interdisciplinary objectives, will need to develop tropes all of its own.

One way through all this is to recall, as the anthropologist Clifford Geertz (1973) does, the distinction between -emic and -etic concepts. Geertz (ibid.) is probably best known for providing a semiotic view of culture through which local partial practices (which can only be inadequately described according to postmodernists but which ethnomethodologists see as their task to describe) and more general cultural features can be reconciled. The '-emic' and '-etic' refer to 'experience near' and 'experience distant' concepts. Compare, for instance, notions like 'beggar' or 'unemployed' with notions such as 'underclass' or '*lumpenproletariat*'. Experience distant concepts allow us to generalise and abstract. For Geertz there are two problems:

- First, experience near concepts leave us with nothing but experience, and tangled up in the vernacular.
- Second, experience distant concepts leave us with vague generalisations.

Here we can see something of the same general issue examined by Glaser and Strauss (1967), to wit the relationship between the general and the specific. Geertz is in no doubt that the task is to generalise, but for him the general is visible in the specific. The micro and the macro are, in other words, aspects of the same thing, linked by the symbolic universe. One of the points Geertz is strongly making is that 'the point of view of the native' is not about empathy, or indeed any other emotional position (and again we see a relevance for those interested in the moral/political

aspects of design). It is an argument about whether we can understand and/or interpret the concepts others use. Put another way, we do not experience what others experience, but we can make sense of the way they make sense of their experience. He is primarily concerned with the symbolic forms, the words used, institutional regularities, rituals, and so on to be found in a society.

We are then concerned with what he usually terms ‘thick description’ (borrowing, somewhat inaccurately, from Ryle (1963)), and with the most global or general themes as well. The two modes of description require and articulate each other. Geertz’ position is unusual in that he seeks to dissolve the debate between the relativists and the rationalists, at least in part, by suggesting it is miscast. Whether particular cultural features are ‘really’ science, religion, ideology, or what have you is not the point. Rather the point is how and in what ways are they systematic representations. In particular, Geertz is famous for treating common sense as a systematic phenomenon. That is, within any particular culture we should be able to identify the properties of common sense.

Such matters clearly relate to the role of theory in CSCW for they have to do with the kinds of concept that allow us to relate situations to generalities.¹⁵ Certainly, in the work we discuss, there do appear to be, glimpsed vaguely, a set of tropes that might help us build towards a heartland of field-work-for-design. Thus, in contextual design we found the flow of work, the sequencing of work, the role of artefacts, and the role of space as some of the tropes on which the fieldworker should focus. In Vicente, we found a set of further tools, the most dominant of which is the idea of constraints. These have various forms and levels, and each can be analysed in distinct ways, even with distinct methods.

Thus work domain skills and work strategies are tropes for the things that workers need to apply when dealing with everyday contingency and when focusing on the specifics of work. We also suggested that some tropes were rather weak, and notions of skill, culture, and constraint seem to us to be examples. Our purpose is to try to find some way between concepts or tropes that are so high level as to be useless or, in contrast, so deterministic that enquiry becomes nothing more than slavishly following a method. At very least, later, we try to negotiate a passage.

¹⁵Ethnomethodologists typically dislike constructive sociological concepts because they trade in unacknowledged fashion on commonsense knowledge. For the most part, we share this distaste for sociologising ordinary matters, but of course the point here is whether generalisations are legitimate and necessary for design purposes. We should point out here that we have nothing to say about the semiotic conceptions of culture that lie behind Geertz’ argument (see Chalmers (2003) for a defence of semiotic conceptions in design-related work).

2.5 Conclusion

We finish by identifying some important features of ethnographic practice (as it pertains to design) that are not always well understood by new practitioners, and often seriously misunderstood.

2.5.1 Ethnography Is Part of a Social Science Tradition

Ethnography clearly does form part of the traditions of both sociology and anthropology. At the same time, the term disguises a wide range of analytic and practical commitments. If writings on ethnography up to the 1950s were largely to do with its relationship to science, and the possibility of a rationalistic model of human conduct, then what followed was a progressive break with any conception of science at all. This can be seen in our rehearsal of Becker's work, through standpointism and latterly through what is usually termed postmodern ethnography as well as our treatment of ethnomethodological *trahison*. We have discussed these only because they sometimes have a tangential relationship to fieldwork-for-design, and because they are useful for building sensibility. Readers may even see some relevance to design insofar as case studies and general conclusions seem to be as important in design-related activities as they are in social science.

In briefly ranging through some of the human sciences' obsessions in respect to ethnography, then, we did not intend to give them a dignity they do not deserve but have tried to give a flavour of what it is that concerns sociologists so much and suggest what aspects of sociological obsession we can safely ignore and which might have some relevance to our interests. In so doing, we have given some weight to an ethnomethodologically informed approach to ethnography.¹⁶ This might be seen as a claim to a superior method for addressing some design questions, one which is distinct from, and perhaps runs counter to, the kind of fieldwork that goes on when done within other theoretical perspectives.

It does not have to be seen that way. That ethnomethodology is distinct from social science, and runs counter to many social scientific assumptions, is largely recognised in the social sciences (although there have been

¹⁶This somewhat dubious term has a provenance both in ethnomethodological studies themselves (Dingwall, 1981; Silverman, 1985) and subsequently in design-related work (see Hughes et al. (1992)). In the latter work, it was never intended to carry the weight it sometimes appears to, and serves as a useful term of art for distinguishing the issues of method associated with ethnography from analytic choices associated with ethnomethodology. It further, and given ethnomethodology's insistence on indifference to 'versions', was intended as a signal that design should be taken seriously.

various attempts to integrate ethnomethodology into the wider concerns of sociology, for instance, Giddens (1984)). The idea that the same antithetical relationship exists between ethnomethodology and generalising tendencies when fieldwork is being done in relation to design is much more doubtful. The foundation of this putative hostility lies in the assumption that ethnomethodology is rigorously opposed to the principle of generalisation, or theory construction and thus that theories which use general concepts must be inimical to ethnomethodological work. We suggest that neither of these things is true. We expand on this at a later stage, but suffice it to say that many of the claims made about ethnomethodology are founded on misunderstandings of what is actually said, and moreover that ethnomethodology's undoubted hostility to some sociology and psychology is precisely a hostility to sociological and psychological theorising of a particular kind. It has no necessary relationship with any other kind of theorising. The point is that theories should be treated according to the degree to which they make the wheels turn. It is these sorts of subtleties that need to be understood and appreciated.

We contend that the kind of ethnographic fieldwork we propose is based on knowledge of a great tradition, but is at the same time geared towards establishing a distinct design-oriented fieldwork tradition, with its own tropes and themes. We say that fieldwork is much more effectively done with an analytic sensibility which orients to design but which does not accept design problems as given. Familiarity with prior design-oriented fieldwork as much as with the social sciences can help produce this. Part of this sensibility has to do with a recognition that when fieldwork-for-design is undertaken, the evidence it produces may be of a number of quite particular kinds. The way the fieldworker in design looks, what he looks for, what he captures, all this and more is wrapped up, at least at some point in enquiry, with his design motivation. As we suggest (and reiterate throughout later chapters),

if all design can be thought of as the relevant and timely reduction of uncertainty, then ethnography can be thought of in the first instance as a means to identify the relevant uncertainties.

This has implications as to what uses that evidence can be put, and it distinguishes this kind of evidence from what would be produced if the purpose of the fieldwork were different from, for instance, an ethnomethodological inquiry working contrastively against orthodox sociological interests.

One further consequence of this view on data or evidence is that the kind of materials produced enable or help create a space for design thinking. This space is created in part through the material itself and in part through the ways it is conveyed. By way of contrast, in traditional ethnography, to take its role in anthropology and sociology (and thus ethnomethodology too), fieldwork leads to the production of long, detailed, written texts, often

if not exclusively book length. In design, we show that there is a variety of ways in which descriptions of the setting can be conveyed and presented, and the long written monograph is only one of these. Other ways, as Beyer and Holzblatt have shown, may include graphical representations of elements of the work in question, arranged through the themes and topics we pursue. Another is to undertake design workshops where the fieldworker or fieldwork team acts as surrogate users, offering stories about how they do the work.

If the kind of data that are captured and rendered in design fieldwork are distinct, and so too are the many ways they might be used to create a space for design thinking, then another distinguishing characteristic of design-oriented fieldwork is the possibility that it can be undertaken by teams. This is rare in the ethnographic approaches exemplified in the anthropological and sociological traditions. Multiperson fieldwork requires its own tools and processes, and part of the motivation for these is to enable evaluation and testing of fieldwork understanding. We suggest that doing fieldwork collaboratively alters the mechanisms used to assess when it is complete or sufficiently thorough for design thinking to commence, and for the management of the team itself.

In short, we are proposing that fieldwork-in-design is a new practice, however much it builds on a fieldwork tradition in the social sciences. It is one that has slowly begun to build a corpus of research materials in CSCW and other related disciplines in the past decade or so, but one which is, we believe, of growing importance and use.

2.5.2 Ethnography Is Naturalistic

That is, it predicates its inquiries on the principle that studies should be studies of real people and their activities, operating in their natural environment, whatever that may be. An important justification of the approach is that it is not known in advance of inquiry just what the relevant features of some settings are, let alone how they might be relevant to system design. Thus, and distinct from some approaches derived from cognitive science, it refuses to deal with artificial environments and controlled versions of work but argues that only by studying the natural environment of work and its activities can system design be adequately informed. As Michael Lynch (1993) has put it, in another context,

Stop talking about science. Go to a laboratory – any laboratory will do – hang around a while, listen to conversations, watch the technicians at work, ask them to explain what they are doing, read their notes, observe what they say when they examine the data, and watch how they move equipment around

However, and as already suggested, this naturalism might involve any number of analytic commitments and any range of substantive interests. It may entail a variety of approaches to the notion of context and a great deal of (largely unnecessary) agonising about the nature of the context in question. It could be predicated on more or less behaviourist stances on social action or a concern for meaningfulness. It may find meaning in accounts of motive or treat meaning as an intersubjective phenomenon. At the early stage of deciding what needs to be looked at, however, none of these issues needs to matter much. A simple rule of thumb is that context is exhibited by people in the place being observed.

2.5.3 Ethnography Is Prolonged

This background assumption comes largely from the fact that anthropological studies of other cultures have usually implicated enculturation. That is, the post-Fraser (1993) view has been that to understand another culture means having to learn its language, mores, practices, and values. In virtue of the fact that it is another culture that is under investigation, it is reasonable to assume this will take some time. However, we should perhaps point out here that there is no logical reason why an ethnography should take so long, and it has been argued that, for CSCW purposes at least, there are times when it need not do so (see Hughes et al. (1994)), because the point must be that duration relates to the size of the task.

To elaborate the point, the ethnography of cashier work undertaken by Randall and Hughes (1995) lasted approximately three weeks, and that because it was undertaken in three different settings to ensure some kind of validity. Here, the task was quite specific: to understand and assess the kind of problems that cashiers had working with technology when dealing with the public. The problem, that is, was tightly bounded by those who had commissioned the work. (Nevertheless, the ethnography subsequently opened up and more work was done, lasting another six weeks.) The main reason for prolongation is that in advance, for the most part, ethnographers have no clear idea what they will find. Because there are in principle any number of aspects which may turn out to be interesting, and any number of things which may be mystifying, it will take time to form a coherent view of what is going on. Nevertheless, ethnographic enculturation over time and a range of domains does rather suggest, on a commonsense basis, that experienced ethnographers will have some notion of what they are likely to see next time out. Again, Marcus is relevant here, insofar as multisitedness at least reminds us that ethnographies can be done with varying degrees of intensity, over different periods of time, with the need for comparison, or otherwise, being one thing that underpins choices.

2.5.4 Ethnographic Enquiries Seek to Elicit the Social World from the Point of View of Those Who Inhabit It

It should be obvious, given much of what has been said above, that ethnographic enquiries can, in principle, be into an enormous range of matters which might be of interest, depending on analytic purpose. They might, for instance, be enquiries into beliefs and attitudes, the symbolic universe of a culture, customs, law, gender relations, power relations, discursive formations, and so on. They do not, that is, have to provide detailed pictures of patterns of interaction and collaboration. As a result ethnography is too diverse a set of practices to be described as a method.

At a minimum, however, we would argue that ethnography is (should be) about uncovering the world from the point of view of the social actors within it. For this reason, although it is behavioural (interested in the detail of the behaviour to a greater or lesser extent) it is not behaviourist because it does not consider the behaviour itself as the appropriate or only level of analysis. The appropriate level is the meaningful behaviour of those who undertake it. Moreover, we are not necessarily referring to a commitment to the beliefs of social actors. This is true regardless of whether these beliefs are reality claims or value judgements. As an ethnographer, we do not have to accept that what people believe is true, is true. As an ethnographer, one does not have to accept that what people believe is right, is right. Equally, one does not have to be indifferent to these matters, and something which looked like an ethnography undertaken under the auspices of, for instance, participatory design, might have a very different orientation to it than one undertaken by an ethnomethodologist.

The different commitments entailed in doing an ethnographic study are discussed at a later stage. In the context of the design of CSCW systems, however, the kind of ethnomethodologically informed ethnography we have referred to entails some minimum commitments that in our view are inescapably part of fieldwork-for-design. Firstly, such ethnographies have primarily focused on the social organisation of work activities. There has been considerable discussion of the nature of cooperation in work settings and one stance on this has been that the phenomenon of interest must be limited to a fairly narrow sense of cooperation which implies attention to the kinds of dependency that are necessary for the completion of work tasks (Schmidt, 1991).

We prefer the initial and foundational assumption of ethnomethodology that work is anything done in order that a social activity is accomplished, and moreover that such work has a processual character. That is, that it is sequentially organised. We believe, regardless of the theoretical auspices under which investigation takes place, that the processual character of work should be one central research topic for interdisciplinary work of this kind even if other possibilities are opened up as well by theoretical commitments. We remain wedded to this conception of ethnography-for-design.

The task of ethnography is to take the obvious fact of the social organisation of human life and describe and analyse how this social organisation is accomplished, understood, and achieved by social actors. In the context of HCI and CSCW, its purpose is to relate such descriptions and analysis to the concerns of system design.

Under the auspices of the ethnomethodologically informed version, the issue of generalisation also changes. The ethnomethodological conception of social research presumes the immense orderliness of the social world from the outset and seeks to understand how this is accomplished. This orderliness, its regularity, is there in advance of, and independent of, any social scientific accounts, theoretical or otherwise, that we might produce of it. Generalisation here will draw on ordinary and widely held understandings about the social organisation of the world. They will, that is, treat typicalities as the culturally recognisable kinds of activity we are already, and unavoidably, trading on in our understandings of the social organisation of the world. As Sharrock and Anderson (1991) argue, as with so many of the conventional dichotomies that characterise sociology, ethnomethodology does not acknowledge the separation of the particular and the general. Instead, ethnomethodology suggests that it is impossible to assemble a collection of observable occurrences, instances, without already having introduced some element of generality that identifies the actions – right down to mundanities such as organising the family for breakfast before the kids go off to school – as what they so observably and obviously are.

2.5.5 Ethnographic Data Resist Formalisation

Ethnographies of work, then, have been widely undertaken since the late 1980s. Because the ethnographic stance stresses the importance of context or setting, and thus that there can be no theoretical perspective which can explain in advance what one is likely to see in a new setting (*pace* BPR), nor any data which constitute the ‘right’ data to be collecting, this raises a data collection and organisation issue. Put simply, ethnographic data take a variety of forms and can include general descriptions of behaviours, descriptions of physical layouts, close descriptions of conversation, thoughts and feelings about what is going on, tentative hypotheses, examples, repeated occurrences, and so on. Inevitably, this makes it rather difficult to distil data down to an essential and structured form, and particularly difficult to do so for system design purposes. In the commonplace usage, ethnographic data is messy or unstructured. This has, at times, been used as a stick with which to beat ethnographers inasmuch as systems designers, quite rightly, point to the necessity of structured, not to say formalised data for design purposes. It is important, therefore, to make an obvious point. There is no reason whatsoever why ethnographic data has to be messy and unstructured.

Let us go further:

The commonplace attitude that ethnographic data are necessarily messy and unstructured needs to be challenged, for there is no reason in principle why it should be this way. Instead, we argue that this sometime messiness is actually a result of hesitancy about analytic relevance. That is, it is a question of deciding what data are needed, for what purposes, and especially when.

Once these things are decided ethnographic data can be as structured as any other form. Of course, a world of pain is hidden in that simple formulation. In our view, many of the by-now celebrated system disasters that exemplify what is wrong with traditional approaches to system development are often if not always a result of precipitate decisions about what our problem is.

The whole business of structuring data, no matter how it has been collected, and as Glaser and Strauss (1967) point out, is an analytic matter. In much the same way, ethnography-for-design may be a collaborative task and not a solitary one, which raises the issue of responsibility. If fieldwork is the recounting of the view from someone in some particular position, as Marcus (op. cit.) puts it, then this begs the question as to what other views and perspectives might be gathered and listened to. For fieldwork-in-design, this perspectivalism may be avoided by treating the task as essentially a collaborative one. Thereby the evidence gathered, the iterations of research into the setting in question, will not produce a 'view from here' but a view that anyone within the team agree is a reasonable and adequate description. In other words, by making fieldwork-in-design essentially collaborative, the problems that ensue, and the materials that are produced, are all subject to different possibilities and constraints. By treating it as collaborative, many of the problems that beguiled anthropological ethnography are eliminated.



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