

ETHNOGRAPHY IN THE WORKPLACE: Remarks on its theoretical bases

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INTRODUCTION

In this paper we want to offer some remarks about ethnography and its relation to system design but on this occasion focussing upon, for want of a better word, ‘theoretical’ concerns rather than the practical ones of design itself, though we will also have something to say about these. Much of the existing literature on ethnography and system design has, and quite rightly in some ways, concentrated on the practicalities of bringing ethnographic findings to bear upon system design. However, typically there is ‘ghost at the feast’ in such discussions, namely, the theoretical agenda which supposedly underpins the ethnography but which often rarely gets addressed.

As readers will no doubt be aware, there are different kinds of ethnography. However, the differences between them are not so much in respect of the nature of the fieldwork reports they produce, so much as in their theoretical affiliations. It is the disciplinary agendas (sociology, psychology, cognitive science, ethnomethodology) to which they are attached and by which they are, at least in part, driven that makes for the important differences between them. In many respects, these differences need not matter to those involved in system design and development. What they require is some more adequate – often, more detailed – rendering of the domain that is being designed for. Nevertheless, in sociology and the other disciplines which contribute to system design the agenda issues are usually the ones that matter. In what follows we will say something about the ‘varieties’ of ethnography and their associated agendas used in system design and will also suggest that these are largely immaterial to system design.

Ethnography as done in Computer Supported Cooperative Work, and in other connections with system design and development, is not a specifically sociological pursuit but draws its various inspirations from disciplines such as socio-technical systems and cognitive science, disciplines which came, so to speak, early ‘to the game’ but which were found to be in need of supplementation by a more ‘social’ focus with the advent of more distributed systems, and the rationale behind the incorporation of some sociological contribution to system design. The ‘some’ in this case were sociological perspectives which, among other things, made routine use of field studies in

their research. However, it is important to note that within sociology ethnography is a minority pursuit. Although the discipline's aspiration to become like a natural sciences has faded somewhat, sociology remains a predominantly theoretical pursuit. Insofar as empirical work is legitimated let alone prized, this tends to be the kind of research which retains the vestiges of the aspiration to be a natural science, namely, social surveys and multivariate analysis. Within sociology, ethnography is not only a minority pursuit but is not held in particularly high esteem either. Accordingly, it is somewhat ironic that the least 'scientific' of social research methods – ethnography – and the one held in least esteem by sociology should have become prominent in hard-nosed software engineering and system design.

There are, of course, some who complain that much of what goes on in the field of CSCW is not really ethnography (Anderson, 1994) Words do, of course, change their meaning and 'ethnography' is no different in this respect. We use the term 'ethnography' because it is the term now widely used to talk about fieldwork, and it is this that we mean by the term. Compared with the period of protracted residence with the people being studied that became the norm in anthropology, the visits of a few days or weeks that are made to organisations to study work practices hardly even deserve the title of fieldwork. However, the important feature of ethnography, and which contemporary uses retain, is the very simple but important feature, namely, that of taking a first hand look at the phenomena that one is purporting to talk about. And it is this simple point that is at the heart of one of the sociological issues that ethnography represents.

The phenomena of sociology, the events of social life, are all around us. They are not ones which are difficult to access in the way that those of the natural sciences are requiring incredibly sophisticated technologies in order to capture particular phenomena. And yet sociology in general takes little interest in the availability of its phenomena. Cicourel (1964) once remarked that sociologists are 'archaeologists by choice'. Archaeologists are forced to study the societies they investigate through the fragmentary remains that are contingently left behind with time. They are grateful for a few fragments of pottery or a portion of a clay tablet with writing on it. The archaeologist cannot walk the streets of Rome, or any other dead civilisation, but has to attempt to reconstruct a picture of that civilisation from whatever remains of it. Any archaeologist would seize the chance, may even sign the Faustian pact with the devil, to spend even a day observing the ordinary life of a past civilisation. Sociologists, on the other hand, can walk the streets and frequent the multifarious settings of contemporary societies, but most prefer not to. If they intend to investigate the society at all, they prefer to operate rather like the archaeologist, reconstructing the society from its fragments and leftovers, for example, by analysing the contents of little used files collected by the many administrative organisations present in our society or pouring over tables of correlations.

We mention this not only to reinforce the point that 'ethnography' is not a leading or a prestigious pursuit in sociology but also to draw attention to the fact that the differences between those who do ethnography and those who do not are not primarily differences of method or technique. To set out the issues as briefly as we can: those who work within the mainstream of sociology have good reasons for supposing that we cannot understand society's real nature by taking note of the details of ordinary daily life. Such an approach would be naïve. What is required is a theoretical scheme. Moreover, what can be observed of daily life is too small-scale. Social phenomena can only really be understood in relationship to society as a whole. But more than this: sociology seeks to be a generalising discipline whereas 'ethnography' is a case study approach from which it is impossible to generalise. To an important extent fieldwork is idiosyncratic and so even if we were to collect ethnographic case studies together this would generate only a collection of heterogeneous and non-comparable cases. All that we would have is 'islands of data' not a comprehensive picture of society. On this estimate, ethnography is not really relevant to the main aims of sociology.

Of course, many of those who do fieldwork find the refusal to do ethnography equally strange. It is the activities that one can observe in the course of daily life, witness on the street, in the workplace, in public spaces, in the home, and so on, that the schemes and abstractions of sociology seek to talk about. To ignore them is, again on this view, to wilfully neglect the very phenomena of the discipline.

Despite its often confident tones, sociology is a discipline beset with innumerable problems. This does not, of course, do much to mark out sociology from other disciplines which are equally beset with problems. Sociology does recognise these problems but often in muted ways. In our view, one such is precisely that of the ‘empirical reference’ of the problems that its abstractions seek to designate. There is a chronic problem in sociology of relating its large and purportedly general theories to anything that might be called observable phenomena. It is also a problem which provides a motivation for at least some of us who are engaged in ethnography. It does not follow that we do field studies because we are especially interested in the cases being studied. Rather, our motivation is a concern with the chronic methodological problem – in sociology - of relating abstractions to actualities. Thus, ethnography in sociology may sometimes have a strategic role. That is, a means of attempting to address some of the key difficulties and problems of sociology and to do so in ways very different than is perhaps usual.

However, it is our view that sociology’s problem of ‘empirical reference’ is one which besets other disciplines which offer contributions to system design, largely because they share many of the methodological presumptions of sociology at large not least those to do with the salient role accorded to theory as the preferred means of developing knowledge of phenomena. We will return to this particular point later. For now we want to review some of the main social and psychological approaches used in system design. These are Activity Theory, Grounded Theory, Distributed Cognition and, finally, Ethnomethodology.

Activity Theory

Activity theory originates in the humanistic psychology of Vygotsky (1962, 1978). As such, it should be understood both as a critique of the simplistic ‘behavioural’ tendency in psychology and involving an explicitly ‘humanistic’ programme. Most of the original work was done on language learning in children though the framework was later generalized beyond this context (Wertsch, 1985). For Vygotsky psychology should emphasise at least 3 primary elements:

- It should be developmental
- It had to relate ‘elementary’ psychological functions with ‘higher level’ mental functions
- It had to take account of ‘socially meaningful activity’.

In an important way (and we may note the parallel, though from a different intellectual tradition, with the work of George Herbert Mead) Vygotsky’s work is an attempt to bridge the gap between the ‘psychological’ and the ‘social’ individual. He was also interested in methodological issues noting as early as the 1920s that facts were ‘theory laden’ which meant that psychological facts expressed in terms of psychological theories – such as introspectionism, behaviorism, psychoanalysis – would acquire different meanings and be different facts. It is for this reason that psychological investigation needs to be ‘layered’. It cannot restrict its attention to basic functions

such as ‘attention’, ‘memory’, ‘concept formation’, but must also be aware of the ‘inner evolution’ of psychological characteristics and formations. In a nutshell, cognition is developmental and much of this development is owed to interaction with others.

An initial finding was that thought and speech are not synonymous, and have different origins. This, remember, was many years before Sapir-Whorf. Thought and speech merge at particular moments. That is, they merge at the level of ‘higher’ mental functions; functions which form an existence culturally. In certain contexts, then, thinking about technology, for example – and here we may impute from subsequent and more contemporary work – can be preconceptual. This is sometimes referred to as spontaneous concepts located entirely in the reflection on immediate experience and distinguished from scientific concepts. This has an immediate corollary in learning in that this, too, can be systematically organised or spontaneous.

The best known concept to emerge from this approach is that of ‘zo-ped’, or the ‘zone of proximal development’. In effect, it refers to the pre-scientific world of spontaneous concepts inhabited by children encountering the systematic, organised world of the rational adult. Of course, it can equally refer to the way in which the ‘tyro’ user meets the expert. The point is that this meeting place is a rich learning experience. The learner learns by transforming the one into the other in and through the dialogue between the learner and the expert.

Building on these general ideas Activity Theory as an approach orients to 3 elements: actor, object and community. Central to the approach, and the reason why it has proved attractive to system design, is its emphasis on tool mediation between the external reality and the meaning and understandings the person acquires. Tools shape the way in which human beings interact with reality, and it is through such interaction that ‘internal’ activities are also shaped. Moreover, since tools emerge out of the activities of a number of people trying to solve similar problems, they represent the accumulation of knowledge relevant to human interaction with the material environment. Tools are created and transformed through the development of the activity itself and, accordingly, carry a culture, and its history, with them. Tools are one of the means for accumulating and transmitting social knowledge influencing not only external behaviour but also mental functions.

In Engström’s (1987) summation of Activity theory, it deals with ‘consciously constructed activity system that is not reducible to a series or sums of individual discrete actions’. Despite its appellation, Activity Theory is not so much a theory as a framework for the analysis of human activities; a framework emphasizing both ‘object-oriented production’ and ‘person-oriented communicative elements’ as integrated within the activity system. The activity system is analysed as a process of subjects working on objects mediated by tools to deliver outcomes. The process takes place within the setting of a community of individuals linked to some common objective and subject to a set of rules and part of a division of labour.

Although the above summary of Activity Theory cannot do it full justice hopefully one can see why it has attractions to system design. The central role it accords to tools has already been mentioned but, in addition, the interest in concept formation has relevance to Computer Supported Cooperative Learning as well as to CSCW (Kuuti, 1994, Bardram, 1997). Moreover, in terms of the original motivations for CSCW, it seems to offer the prospect of providing a more social perspective so overcoming the rather arid behavioristic and experimental psychology which, hitherto, had been drawn upon to inform design. However, as plausible and interesting as the framework might be it provides little guidance as to the research methods that might be used to implement it in a series of studies. As it happened fieldwork did become one of the main means of bringing empirical materials to the analytical framework (Kuuti, 1992). In other words, the framework, the theory, became the means of not only ‘sensitising’ analysts to what they were seeing ‘in the field’ but also imposing on that ‘seeing’

concepts derived from Activity Theory itself. Again, this is a point we will address more fully later. For now we want to turn to another approach, one more methodologically driven if you like, which seeks to root the development of theory within the ethnographic process itself: Grounded Theory.

Grounded Theory

Grounded Theory grew out of the Chicago School tradition prominent in the 1920s. Although eclectic in the research methods it pioneered, the School became best known for the first-hand observations of urban life and laying the foundations for what became known as Symbolic Interactionism. One of the distinctive aspects of this approach was its argument that sociology's central concepts would, possibly even in the very long term, necessarily remain qualitative and operate only to 'sensitise' researchers. Barney Glaser and Anselm Strauss (1967) were concerned with one of the standard complaints against quantitative techniques, namely, the way in which data is forced to 'fit' pre-organised categories and which, for them, prevented researchers from refining and developing theory further. For them, adequate theory can only be developed by the systematic investigation of the real world. Contra the hypothetico-deductive model of theory proposed by positivism, Grounded Theory is inductive and relies on moving from the particular to the general. Methodologically, this means that the structure of research cannot be specified before one actually starts doing the research. One cannot determine in advance who needs to be studied, how to study them, and what 'correct' evidence will be. It further means that what the standards of counts as evidence need to be relaxed. They propose a standard of 'plausibility'. In this respect, their conclusions have something in common with Ethnomethodology, as we will see below.

Two main techniques for grounded theory are offered. The first is the 'constant comparative method'. This is a method for generating categories and concepts. Categories, in turn, have properties as can be illustrated by the studies of nursing care for the dying. Two categories of 'nursing care' are given – 'professional composure' and 'perception of social loss' – which are to be found in the perceptions and activities of the nursing personnel studied; perceptions and activities which affect the fateful decisions as to whether a patient should or should not be resuscitated. The death of a young mother, for example, is likely to have devastating consequences for the family and children, but who will miss an old vagrant?

Glaser and Strauss look at lots of examples of things that appear to be similar from the point of view of the analyst – in this case lots of examples of nurses dealing with the death of patients – deriving from these observations some categories which make sense of what they see. The point is that at some point in the research, the categories so developed help guide the researchers as to what to look for, something that cannot be done at the start of the research. Analysis should continue only so far as it helps develop new categories or properties.

The second technique is 'theoretical sampling'. By this they mean that cases are chosen to best serve the theory that is being developed. Unlike positivist research where a population has been defined before the research begins, with theoretical sampling the choice of who to study – the cases – depends upon the theory being developed. This does not mean ignoring cases that do not fit the early theory. It does mean emphasising the cases that are relevant to the early theory; the cases that, at the early stage, seem to best exemplify what the theory states. Again they provide examples of deciding what to study from their nursing research.

"Visits to the various medical services were scheduled as follows: I wished to look at services that minimised patient awareness (and so I first looked at a premature baby service and then at a neurological service where patients were frequently comatose). I wished next to look at dying in a situation where expectancy of staff and often of patients was great and dying was quick, so I observed on an Intensive

Care Unit. Then I wished to observe on a service where staff expectations of terminality were great but the patient's might or might not be, and where dying tended to be slow. So I looked next at a cancer service. I wished then to look at conditions where death was unexpected and rapid, and so looked at an emergency service."

".... So our scheduling of different types of service was directed by a general conceptual scheme - which included hypotheses about awareness, expectedness and rate of dying."

By this process, a conceptual scheme is developed which becomes progressively more abstract. The distinguish between 'substantive theory', which comes directly from the data, and 'formal theory' which is more abstract and general. But, and this is another important departure from positivistic approaches, formal theories cannot be applied to new areas of research. Rather, substantive theories must be developed and formal theories selected according to their relevance.

For some committed to qualitative research in sociology as well as those dissatisfied with positivistic approaches, Grounded Theory was influential. On the one hand, it recognised, indeed insisted upon, that fieldwork research was an 'evolving affair' and its role was to allow the analyst to produce reasonable categories with which to describe 'what was going on'. So, in this regard Grounded Theory gave respect to the requirement to gain first hand experience of the social world being investigated. On the other hand, it did so by seeming to offer a systematic way of building generalisations from such first-hand materials. In CSCW, too, the method has had some influence and examples include the work of Fitzgerald et al (1996) in their study of the work of system administrators, and Becky Grinter, who has used insights from Glaser and Strauss in her study of the development of a Workflow System (1997)

Distributed Cognition

Distributed Cognition is a relatively recent theoretical resource for CSCW investigation and owes much to the work of Ed Hutchins. Its growth perhaps owes much to the fact that cognitive psychology played an important part, and still does, in HCI and the development of interfaces. But, of course, the problems of system design began to expand well-beyond the interface ceasing to be simply matters of interaction between an individual and a machine and becoming more and more a matter of serving the needs of groups and teams. The need arose, then, to extend the cognitive model and to recognise that, as Hutchins put it, 'cognition is a fundamentally cultural process'.

Distributed Cognition starts from the perspective that cognition and culture are two parts of a larger system. Its insight is that cognitive processes take place both 'inside' and 'outside' of the head. To put it another way cognition and culture are intimately connected.

For Hutchins, the marginalisation of culture by cognitive science has had reductionist effects leading to an underplaying, not to say ignorance, of context or 'situatedness'.

"The early researchers in cognitive science placed a bet that the modularity of human cognition would be such that culture, context, and history could be safely ignored at the outset, and then integrated later. The bet did not pay off. These things are fundamental aspects of human cognition and cannot be comfortably integrated into a perspective that privileges abstract properties of individual minds." (Hutchins, 1995,

The second significant feature of this perspectival insistence is that it places too much emphasis on the 'boundaries', and hence mistakes the properties of the system for those of the individual. This kind of mistake, he argues, can be seen in the assumption of 'primitive minds' in technologically primitive cultures. Moreover, and equally important, 'culture' is a great deal more than typical anthropological accounts would have it (at least according to Hutchins) in that culture is material. Distributed cognition makes much of the idea that 'we cannot know what the task is until we know what the tools are' (p114). In other words, understanding the task is a matter of understanding human activity in an environment – an environment which contains physical artefacts which themselves are culturally evolved.

Hence, and by way of summary, Distributed Cognition relies on:

- 1 The notion that it is a 'complex functional system consisting of many media in simultaneous coordination' (288);
2. Sees systems as having both computational and social properties;
3. Sees learning as 'adaptive reorganization in a complex system' (p289).

Taken together, we begin to see what the task of the analyst might be. The system is a material environment which can take a symbolic form according to the kinds of media used to make representations of it. We need, therefore, to understand the nature of the system in its widest sense, and the various conditions and artefacts that make it up.

These media have to be coordinated, and human beings, using their social experiences- knowledge, skill, expertise, etc., work out ways of doing exactly that; ways which then become internalised and so become cognitive.

The benefits of distributed cognition for Hutchins are clear:

"Among the benefits of cognitive ethnography for cognitive science is the refinement of a functional specification for the human cognitive system. What is a mind for? How confident are we that our intuitions about the cognitive nature of tasks we do on a daily basis are correct? It is a common piece of common sense that we know what those tasks are because we are human and because we engage in them daily. But I believe this is not true. In spite of the fact that we engage in cognitive activities every day, our folk and professional models of cognitive performance do not match when cognition in the world is examined carefully." (P 371)

Thus, for those who see cognition as a fundamental part of understanding human experience, and who recognise that attention to 'group' behaviour is a matter of understanding 'context', distributed cognition provides a way of integrating two historically contrasting perspectives in cognitive science and social anthropology.

Distributed Cognition has been criticised for 'colonising' other approaches, notably ethnomethodological studies

of work. It is certainly true that the notion that cognition is social is hardly original. Nevertheless, what its defenders would argue is that by retaining a conception of 'cognition in the head' it provides a means for relating 'mental' processes to cultural processes. There is some ambiguity in our view in the literature about the nature of this relationship, but if our position on it is accurate, it constitutes the main point of departure from ethnomethodological studies, in that ethnomethodology for the most part would have little interest in what goes on 'in the head', following Garfinkel's formulation that 'there is nothing in the head except brains'. At which point it is necessary to discuss Ethnomethodology.

Ethnomethodology

Ethnomethodology originated with the work of two sociologists, Harold Garfinkel and Harvey Sacks, in the late 1940's and 1950s. In fact, its origins can be traced back to what is usually called phenomenological philosophy and to Alfred Schutz, whose work began in the 1920's. One aspect of the ethnomethodological programme that tends to be lost in CSCW (and it is probably just as well) is the radical nature of its relationship to other disciplines such as sociology and psychology. Its stance on problems such as 'meaning', 'cognition', and 'behaviour' is both methodologically at variance with standard disciplinary assumptions, and perspectively distinct. In a nutshell, ethnomethodologists (to a greater or lesser extent) would argue that most disciplines confuse theoretical with practical matters, and are imbued with dubious philosophical commitments of one kind or another. That is, ethnomethodology refuses any epistemological or ontological commitments, and limits its enquiry rigorously to what is directly observable and what can be plausibly inferred from observation on a known-in-common basis. Such a posture, for example, is counter to the foundational assumptions of cognitive science which have to do with inferences about 'mental structures' or 'states'.

Another way of capturing these different commitments is in terms the two contrasting concerns which motivate Ethnomethodology and the more orthodox social sciences. The latter focus on what can be objectively known; a scientific worldview evolving from the rationalism of Kant and Descartes in the 17th century. Their topic of inquiry was with what can be known and how it can be known. That is, concerned with the construction of an epistemology. Just by way of example, the work on the logic of mathematics associated with Bertrand Russell, and work on the nature of sensory inputs associated with some cognitive psychologists are examples of this kind of perspective.

By contrast, Phenomenologists rejected the notion that it is possible to know an objective reality derived from logic or from the senses. Rather, they were concerned with the prospect that what we know is inevitably constructed from our experience. This experience may be made up of many things, but will always include the concepts and words that we deploy. The Phenomenologists started from what is called the 'natural attitude', which refers to the way we mundanely, ordinarily, without any problems, see the world as made up in a particular way. The point is that the natural attitude contains no fundamental doubt about the way the world is, or, to put it another way, does not question the reality of the world. By contrast philosophies which rely on that familiar device 'the persistent sceptic', as well as those disciplines deriving from them, are based on doubting our experience of the world as real. Rather than doubting whether the world is 'real', the phenomenologists were interested in analysing the 'natural attitude', trying to understand what it is like to experience the world in the way that we experience it, and suspending any beliefs about whether it is 'really' like that.

One important consequence of this position is that if we are to interpret behaviours we cannot 'just look at' them since we have to impute some motive, desire, rationale or whatever, to the actions in order to explain them. This, in effect, is what Garfinkel meant by the Documentary Method. It is an important point since it means that

commonsense is at least in part how we always interpret the world and, just as importantly, it is largely unproblematic. We do not, for the most part, have any trouble interpreting the world.

For Schutz, and briefly, our experience of the world is organised in terms of ‘typifications’, stocks of knowledge about the world, including knowledge about objects and people along with ‘recipes’, or ‘how to do it knowledge’ rather than a general rationality. These stocks of knowledge are contingently valid; that is, they are treated as right until something crops up which makes them problematic. Moreover, stocks of knowledge are not normally consulted, we simply take them for granted.

No two people can have identical viewpoints, but we can behave, again without problems for the most part, as if we do. Intersubjectivity relies on two fundamental assumptions on the part of actors which are part of a general Reciprocity of Perspectives. We assume, until it is proven otherwise, that if we stand in the place of the other person we will see and think about things in much the same way and we see and think about them now. We will share, that is, typifications. Moreover, although our personal biographies make us different, we are sharing a common perspective on the world when we interact. That is, we are selecting shared and common objects and features to talk about. This shared, known-in-common world is actively sustained by participants to it.

This is ethnomethodology's starting point. How do we achieve and sustain this known in common, typical, world as a matter of ordinary, practical, purpose?

Returning to Garfinkel and Sacks, the fundamental problem is how people maintain orderly conduct, how they produce order in and through their intersubjective action. Garfinkel elaborated the 'Documentary Method of Interpretation', mentioned above, which refers to the way in which we treat appearances as standing as the 'document' of, or 'pointing to', an underlying pattern of meaning or intention.

Two strands of ethnomethodological enquiry have been influential in CSCW:

Ethnomethodological Studies of Work

The promotion of a *programme* of ‘studies of work’ was an incidental, rather than of an essential feature, of ethnomethodology. It originated in an ironic observation about ‘the missing what’ in sociological studies, of which ‘the sociology of work’ was a good example. If you read sociological studies of work you will find a concern with all kinds of topics, ranging from the factors that affect employee morale, to the gossiping that goes on among managers, but what you will not usually find is anything very much about the work that people actually do. Thus, the actual work was ‘missing’ from the sociology of work: a point which connects to the issue mentioned earlier about the intriguing problem of the relationship between sociology’s abstractions and the affairs of daily life. Accordingly, work offered an ample opportunity to investigate something that was not otherwise studied: what did people do when they were working, how did they get on with their work?

Ethnomethodological studies of work takes as its *only* topic, how members accomplish their work tasks. The stress is important, for it implies no commitments of any kind as to the merits or otherwise of members’ world views, attitudes, assumptions and so on. By contrast, most sociology investigates the social world from a set of prior categories – for example, class conflict, gender, control, and so on. Ethnomethodology is interested in the commonsense categories that members themselves deploy. Further, Ethnomethodology brackets ontology and epistemology. That is, it is not interested in the truth of members’ claims, but how they arrive at those claims. To

put it another way: it treats accounts, all accounts, as versions. It is not interested in the fact that the social world is ordered but in how it becomes ordered in and through processes of interaction. The focus of ethnomethodological work is thus processual in a very specific sense. In a nutshell, it treats all social situations as the accomplishment of members. In the context of work, it focuses on how people actually order their working activities through mutual attentiveness to what has to be done.

Put simply, Ethnomethodology means people's methods. It involves seeing things from the point of view of participants and trying to understand how their 'form of life' can be construed as the outcome and accomplishment of their interactions. Taken together, these points indicate what an Ethnomethodologically informed ethnography would look like. They suggest it would be an analysis which is interested in how people conduct working life in real settings, doing what they do in the mutual accomplishment of sometimes divergent intentions, treating work as socially organised and interested above all in *how* it is socially organised in that setting. This means looking at the actual working division of labour as routinely manifested in peoples' meaningful orientation to their work, not an idealised conception of it such as that which might be associated with task analysis or process modelling. One important feature of this is that it would not treat work and technology as analytically separable. It would treat technology as technology-in-use.

The task is merely to report in adequate detail how they go about doing what they construe as the thing to be done. The relationship between ethnomethodology and design, then, on the face of it looks slightly puzzling, since design must be about commitments.

There have been a range of responses to this, from:

‘we just provide the data- leave the rest to designers’

to:

‘start ‘innocent’, become ‘informed’

In either version, ethnomethodology does not and cannot do any strategic work.

Conversation Analysis.

The second major strand of Ethnomethodology which is relevant to CSCW is Conversation Analysis (CA). This has been a major enterprise within Ethnomethodology for many years. It has taken a number of directions in this time, including, for instance, attempts to formulate conversation as manifesting gender inequalities (Dorothy Smith, 1988) and power structures (Atkinson and Drew, 1979). It is not the place here to enter into debates about whether these attempts make sense or not. Rather, our emphasis will be on the notion of rule following since it is this feature which, on the face of it, offers an interest to system design.

At the core of CA is the idea that conversation embodies rules. Without going into too much detail here, examples of such rules are as follows:

1. At least and not more than one person talks at a time, and any overlaps will not persist.

2. All conversations are structured in terms of adjacency pairs (first part – second part) with the first part spoken by one person and the second part spoken by another.
3. The second part will be relevant to the first part.
4. Turn taking is allocated with a minimum of gap or overlap. Sometimes 'speaker selects' methods might be used – e.g. 'what do you think, Paul?', and sometimes 'self select' techniques might be used when a speaker might use a pause to offer an utterance.
5. There are particular structures to the opening of conversations. These include summons/answer; identification/recognition; greeting/greeting.
6. There are particular structures to the closure of conversations.

Perhaps more importantly than this, CA work has focused on the way in which Conversation can be repaired. That is, a normal feature of conversation is misunderstanding, but what is clear from real conversation is that we normally recognise our mistakes and correct them in the course of the exchange. In turn, this has led to a focus on CA as a resource for solving some problems associated with cooperative work. Examples of work in this area include ethnomethodological work such as Frohlich and Luff's 'advice system', Heath and Luff's seminal study of the London Underground and Gilbert, Wooffit and Fraser's 'Sundial' project. In a nutshell, the insights afforded by this perspective include

1. The idea that we can uncover a 'grammar' of communicative acts, which would include gesture, gaze, etc., (thereby, in principle, providing a logic of enquiry for all similar work);
2. That a focus on the detail of communication work has specific resonance for design, through its recognition of the semi-tacit nature of some communication.

There is some debate among ethnomethodologists about the degree to which C.A. constitutes a general rule-based understanding of communication. The fact that there are rules which can describe some aspects of conversation does not mean that conversation can be described strictly or always in terms of rule following behaviour. But there is the problem of what is meant by 'rule'. Is a rule something which provides for a causal relationship? It might be but it does not have to be. One way of looking at the 'rules' of conversation would be to view them causally, and if you can, then the sophisticated simulation of conversation is only a matter of uncovering every rule and its relevance. In principle, this may be no easy task, given that there may turn out to be a huge number of rules, but few would argue that the number of 'conversation generating' rules is that large.

The kinds of ways in which some systems use this kind of analysis include Frohlich and Luff's advice system, which is designed to preclude or prevent certain kinds of response. Thus, if the system asks a question, users are forced into conditional relevance by giving answers to questions. The 'Sundial' project, designed for airline bookings and enquiries has a similar structure, by including identifiable adjacency pairs, turn taking mechanisms, opening sequences, closing sequences and question/answer structures.

Back to agendas

In the introduction we referred to the agendas underpinning these various approaches to ethnography as used in system design and, following the review of these approaches, it is time to explore these agendas and how they impact, if they do, upon design itself.

If we reflect for a moment on two of those approaches – grounded theory and ethnomethodology – the relevance of agendas in differentiating them becomes apparent. The dominating idea is that what makes the sciences scientific is that they have general, formal and deductive theories. The role of research in the scientific cycle is to provide confirmation or refutation of hypotheses that derive from the general theories.

As far as sociology is concerned, this strategy was meant as a cure for what was seen as a chronic problem, namely, the discontinuity between theory and research. The two seldom make contact in ways which genuinely informed one another, and the aim was to develop a logically tight sequence of derivation from most basic theoretical axioms to specific empirical predictions. The development of hypothetico-deductive theorising was not, however, any genuine success. Whilst it might seem a convincing idea in principle, it proved difficult in practice to set up theories that really fulfilled the requirements of formal theorising or that carried any interesting empirical consequences. One of the leading presentations of this strategy for sociology was offered by Hans Zetterberg, in his *On Theory and Verification in Sociology*. The ‘grounded theory’ approach consisted in a simple reversal of Zetterberg’s strategy. The attempt to develop general formal theory was premature, since there was a lack of real knowledge about the phenomena being investigated. There was, therefore, a need to give research a more positive role in the theory-development process. Rather than theory being tested by research-accumulated data, it needed to be formed from it: it needed, in another word, to be ‘grounded’ in research. The kind of research envisaged was that which was rich and qualitative.

This strategy arose out of the Symbolic Interactionist Herbert Blumer’s ‘critique of the variable’ (Blumer, 1956). Natural science inspired sociologists imagined that sociological theory could be like theory in mechanics, a specification of a set of quantifiable and systematically interrelated variables. What were designated ‘variables’ in sociology were, however, it was critically argued, based on an insufficiently rich knowledge of the phenomena they were designed to represent. The adoption of fieldwork as the basis for grounded theory follows from this critique. However, the ‘grounded theory’ approach in many ways shared the basic convictions of the natural science approach in that the eventual aim was to develop formal deductive theory, theory constituted out of interrelated variables. It differed from the natural-science approach in that it envisaged a different, more realistic and, hopefully, an eventually more successful *strategy* for the development of such theory.

The ‘grounded theory’ project originated in a concern with the lack of fit between sociology’s theoretical abstractions and the data which was collected through research, and sought a way of bringing these closer together. Ethnomethodology was no less strategic in its thinking that the grounded theorists were, but it was motivated by a very different set of considerations. It undertook to question what is almost a sacred assumption in social science, namely, that it is only through a set of theoretical generalities that the nature of a phenomenon may be captured. This conviction had the effect upon sociology of inducing a drive toward the production of purportedly universal generalities. Grounded theory, too, sought those, although it viewed the development of more restricted generalities as a *first step* toward the eventual delivery of universal generalities. But even if one grants the desirability of obtaining a generalised theoretical system, it does not follow that the way to attain such an ambition is to set out to explicitly develop such theories. Rather than suppose that the problems of connecting theory and research in sociology reside in the problems of constructing theory from data one could alternatively propose that many of sociology’s problems originate in its treatment of data. The favoured methods of sociology

involve the presentation of data which has already been extensively processed, that is, rendered, for example, into the form of statistical tabulations. However, one cannot characteristically compare the raw data with its processed form, and could not specify the procedures actually involved in the transformation of one kind of data into another. At the same time, we know that the practices of research are replete with problems in terms of the handling of data. One might, therefore, take the view that both the theories and the methods of sociology were *obscuring* rather than exposing the phenomena that they presumed to talk about. The strategic move was, then, to problematise the relationship of sociological discourse to the phenomena it talked about, and to seek to recover the 'raw data', the phenomena in its most primitive representations, in order to look at these afresh independently of the usual apparatus of sociological assumptions.

It has been said that this strategy is a kind of naïve empiricism, the idea that one can discover the phenomena in themselves. But this is not the case. This strategy drastically reconstructs the whole conception of what 'the raw data' might be. The return to the 'raw data' turns out to be a turn toward 'the everyday world' and the 'ordinary affairs' that the members of society conduct. The return to these phenomena does not demand the adoption of any *special* point of view, but only a recollection of the point of view that the sociologist *already* (though largely unreflectively) occupies, namely that of 'the member of society.'

Behind the point we are now going to try and make clearly and briefly lies a huge amount of sophisticated philosophical and 'theoretical' reflection on the problems of sociology but which we cannot develop overmuch on this occasion.

The massive difference which this latter strategy makes is upon this conception: that the 'real nature' of society is to be determined through the development of a set of theoretical categories and associated methodological procedures, which are articulated by a set of professionally specialised community of students [call them 'scientists'] of society. Against this is offered the following view: that the 'real nature' of society is something which is itself *socially determined*. The ways in which 'what is really going on' in society is determined according to practices which *the society itself provides*. The determination of 'what is really going on' on any social occasion is something which is *itself socially controlled*.

These *might* seem like arcane and convoluted arguments, but, in our assessment of ethnomethodology, the kinds of things it says ought to translate into things that *you couldn't disagree with*. The point of this line of argumentation is not to take us somewhere we have never been before, but to bring us back into touch with things that we [otherwise] recognise as the most normal, ordinary, *natural* things in the world. The aspiration to general theorising is an aspiration to *novelty*. The sociologist's imagined objective is to bring *new insights* to people, to present society in way which makes it seem different to the way in which *it is usually experienced*. But this emphasis upon finding out what is new and potentially surprising means that that which is commonplace, familiar and otherwise unremarkable is not really deserving of attention. It gets *left out* of the theoretical picture, taken-for-granted and [systematically] disregarded. Insofar as ordinary commonplace things are to be considered, then, they are really to be considered *through* the theoretical scheme. The means for such a reviewing of 'the commonplace' is that of ethnography, of taking a first hand look at the affairs of daily life. The point is however, to see these independently of a sociological agenda which seeks to lay out, *in advance*, what is of interest about these ordinary, everyday affairs. The aim is *to see them for their own sake*, for themselves. The big difference is, then, whether you look at people *as if they lived their lives in order to solve the problems that professional sociologists find interesting* or whether, instead, you look at them as if they lived their lives without reference to what sociologists find interesting or otherwise.

A key difference between the ethnomethodological, and other kinds of ethnography, then, is concentrated upon the kind of relationship that is conceived to obtain between ‘the inquirer’ and those who are the subject of the inquiry. The other approaches are apt to conceive the relationship after the ‘scientific’ conception of a relation between the observer and the observed. It is the observer’s task to identify and conceptualise regularities in the phenomenon. Ethnomethodology’s approach, by contrast, conceives the relationship as much more akin to that between teacher and pupil – in other words, much more as an instructive relationship. Thus, the first approach seeks to yield ‘what the researcher has found out’ whilst ethnomethodology’s offers only what the researcher has been taught by those being studied – the researcher is effectively *relaying* understandings which have been appropriated from within the field. This places an emphasis (a) upon the extent to which the researcher’s report is a *joint* production, is something which has been compiled *between* the researcher and those under study (b) emphasises, also, the extent to which the organisation of the social setting is also a ‘joint construction. omething that is done between and *together* by the participants in the setting.

In some ways conversation analysis is a best example of ethnomethodology’s approach. There are reservations about this. Conversation analysis can really be treated as existing in two phases, one before 1974, a date marked by the publication of a remarkable, and very influential paper, ‘A simplest systematics for the organisation of turn taking in conversation.’ In the [more or less a] decade prior to 1974, ‘conversation analysis’ was mainly the pursuit of a trio of researchers, Harvey Sacks, Emmanuel Schegloff and Gail Jefferson, but the 1974 publication provided a very condensed and schematised summary of their work in a form which became a template for numerous others to make studies. In the first, ten year period, these individuals – especially Sacks – were working out a way in which to examine the organisation of conversation. Conversation’s organisation was very much understood to be a *joint* production, a matter of the way in which the participants to it co-ordinated their activities to put together an organised structure of conversation. The attention centred very much upon the ways in which conversationalists could fit their activities one to the others in such a way that they could, for example, be seen to be collectively talking to a common topic. The actual practice of analysis consisted in looking at the relationships between utterances, with the emphasis upon the way in which any single utterance was to be understood in terms of

its location in the ongoing course of the conversation as a whole. Understanding a single utterance in a conversation required seeing both (1) how the utterance depended for its sense, for its meaning, upon its relationship to what had been said [or even not said] previously and (2) upon how the utterance projected the further development of the conversation, gave rise to next things for someone to say. The approach analyses conversation as a *real time* activity, one which is built up step-by-step, as decisions are made as to what to do now-and-next in the course in intricate interaction with other conversationalists. The ‘real time’ approach is one which is an expression of ethnomethodology’s interest in the ways in which people ‘put together’ their activities in the production of collective phenomena.

The influence of conversation can be extensively seen in the presentation of data in the form of transcriptions, and in the analysis of such transcriptions in terms of a turn-by-turn organisation.

One can see, in the emphasis upon the *joint* nature of activities a considerable difference of focus from *distributed cognition*. The latter is based upon the inherited distinction between the individual’s ‘inner’ mental world, and the ‘outer’ behavioural world - it is an approach which is centred upon *the individual’s* mental operations, but which has come to realise that the rigorous insistence upon this mental as a strictly *inner* domain is a handicapping one, which may be corrected by ‘extending’ the mental environment into the ‘outer’ world. This means that, for example, artifacts may be regarded as extensions of mental functions: the storage of

documents in filing cabinets can be characterised as a ‘memory’ operation. This is also a means of acknowledging the indisputable role of culture in the conduct of human life, for, of course, the artifacts are unquestionably cultural contrivances. However, the difference between distributed cognition and ethnomethodology [though not it alone] reflects one of the most constantly contested issues in reflections on people’s activities, to what extent are these *essentially* individual or social matters?

The point is, then, that within the context of the so-called ‘human sciences’ an activity such as ‘ethnography’ is characteristically undertaken according to an agenda, and between those different agendas there can be the most profound disagreement as to the what the nature of a discipline, such as psychology or sociology, can possibly be. See, for examples, the (very similar) critical – in our view, misguided – comments on ‘situated action’ (ethnomethodology’s *nom de guerre* in CSCW) by Jean Lave (1993), by Yngve Engstrom and by Bonnie Nardi.(1996). Engstrom’s complaint that ethnomethodology’s studies

‘focus on dyadic interaction, attempting to define contexts as social situations, as spaces of interactive experience, or as fields of discourse. Although contexts are seen here as interpersonal constructions, they are commonly treated as purely linguistic, symbolic and experiential entities. This makes, as if independently of the deep-seated material practices and socioeconomic structure of the given culture.’ (Engstrom, 1993, 150)

The complaint is wholly reminiscent of the kind of ‘mainstream’ sociological, theory-motivated views mentioned above, and which Engstrom shares. It is a concatenation of misunderstandings, but we cannot undertake to correct those here. The interdisciplinary turf wars have been going on a long time, but they continue to animate contemporary discussion: what is the fundamental nature of the phenomena to be understood [are human activities primarily individual or essentially social in nature] and what is the appropriate methodology to be followed in making studies of that phenomena? We have only adverted to these differences here, but you can be assured that *these* are the real issues which engage the minds of practitioners practitioners in those areas, and which are addressed *through* their empirical studies, be these ethnographic or of other kinds. By being addressed *through* those studies, we do not mean that the issues which divide the different approaches are specified as hypotheses and tested against the data – that is not what goes on at all. It is, rather, that studies stand as exhibits of the features and merits of the favoured approach, as displays of the difference between these different approaches.

Does this mean that engineers must perforce/designers – if they are to make use of ethnographies – to be involved in following out these disagreements and, indeed, making up their own minds where they themselves stand with respect to them? Fortunately, we think not. To employ a term from the world of design, the problems of the ‘human sciences’, certainly as they stand, are wicked problems: ones that do not have any clear cut criterion for a solution. Insofar as engineers/designers are looking for solutions to their engineering and design problems then it would be, on the whole, a regressive matter to engage with these issues – they have not been settled after even hundreds of years of debate, and there is no likelihood of any immediate solution to them. However, the fact that different kinds of ethnography are associated with different agendas need not comprise a deeply problematic fact for the designer/engineer.

If one does ethnography in organisations, one is likely to find that they have, there, their own terms for ethnography: for example, it will be spoken of as ‘sitting with’ or ‘following around’ members of the organisation. The first thing to realise, then, is that ‘ethnography’ is in the first instance a matter of the most *ordinary* kind of activity. ‘Ethnographers’ are apt to make themselves out to be the possessors of refined skills,

and to present their work as a matter of ‘methodological’ problems, but the plain truth is that much of what the ethnographer is doing is the kind of stuff that *anyone* in – say – an organisation will be doing :sitting through meetings, engaging in conversation. They will usually be doing this as a supernumary participant, and so they have to e.g. keep themselves out of the way, recognise the priority of the others’ business and so on but they are only having to learn their way around a social setting, something that all those they are studying have had to do and have already done. affairs’ of their practical life, and they are certainly *willing to talk*. It is not so much a matter of getting them to ‘open up’ about their affairs, but even more of *stopping them* from doing so: people will put aside even pressing work tasks for the opportunity to explain ‘how things really are’ around here to the ethnographer. Further, the issue as to what the fieldworker will collect as data will be dictated not by strategic methodological considerations, but by the flow of activity within the social setting Nor is ethnography a matter of possessing particularly arcane skills for instilling trust in people or extracting information from them. We do not say that this is as a general point, but it is in our experience overwhelmingly the case that people are very receptive to the ethnographer, that they are more than willing to have people ‘sit in’ on and ‘observe’ their activities, and that they are only too eager [*much* too eager, often] to be told what the ethnographer has found out about them, has to teach them about how to improve the organisation of their affairs. They are frequently *eager* to draw the ethnographer into the ‘inner’ reaches of their collective life. Someone who wants to study transactions in traffic court, for example, is most probably going to have to do this from one of the seats made available to the public within the courtroom, someone who is studying project organisation will likely spend a great deal of time in meetings, because that is where the people they are studying spend their time. There will also be organisationally given restrictions in the nature of the access that they have to study: they will often not, for example, be able to accompany those to whom they have access into meetings with their superiors. There will be a great many decisions to be made as to what is to be studied, but these will be common sense decisions. They will also be ones in which you just don’t know what the consequence of an alternative decision would have been. One cannot study everything at once, and when people disperse from a group you can only follow one or other of them. Whether you go with this one rather than that depends on such things as whether (a) you are trying to get around as many different people as you can or (b) whether you are trying to stick mostly to one kind of person. There are lots of these ‘common sense’ decisions to be made, but it is worth remembering that they are not decisions by which you can really lose.

Another simple truth about ethnography is that, given access, you can very quickly collect far more data than you can ever possibly use: a day’s work can generate several hours of audio or video tape recording. Nor is there really much meaning to the idea that some things are *crucial* data – ethnography is a pretty diffuse exercise with [characteristically] vague objectives, if indeed, they can be called objectives at all: often the aim is just to see and hear as much as you can, and to get as good a record of what you can see and hear as possible. In the ethnographic setting it is *all* data, though there is no sense to having *all* the data. Harold Garfinkel astutely remarked that whereas natural scientists can ‘lose’ their phenomena social scientists cannot. Social science data can characteristically be made to do, to serve any purpose that one cares to bring to it. One of the best ethnomethodological field studies was made by a man who, in attempting to study the activities of convicts in a ‘half way house’, a post-incarceration residence, found that the convicts would not speak to him, that they regarded him as an agent of the staff: out of the fact that he could not do the fieldwork he initially wanted to do he fashioned an exceptional work (Wieder, 1974). What is collected by ethnographers are *snatches* from the affairs of the social setting, and there are ways in which, inevitably, the assortment of snatches that is put together will be an *arbitrary ensemble*. We have already said that these generalities are not meant to be exceptionless. Clearly, field studies can be made which are pretty narrowly focused and which are oriented to specific kinds of observations and data: I’m preparing to undertake a field study to see how lawyers make use of a document processing technology, and most of what I collect is, therefore, going to be around the issue of what they do at

their desk with this desk based technology – do they use it? With what fluency? What kinds of problems are there with it? Do the users problems suggest refinements or new uses which have not been anticipated? Again, though, the decisions will be ‘common sense’ rather than agonised: where should I spend my time, given that there is a choice: well, which one is more likely to be informative about the possible uses for the new technology? And so on. But ethnography is usually very much *a trawl* of the social setting, a matter of seeing what turns up when you hang about in a particular kind of place.

If these remarks sound sceptical or diminishing about ethnography, then they are not meant to be so. The important thing about the ethnographer is not that he or she brings particularly arcane skills to the collection of data [many of those are the skills of office administration, cataloguing and classifying documents and records], but that they bring the *willingness* to pay attention to people’s activities, to attend in detail to how people actually go about their affairs, however ordinary and otherwise unremarkable these affairs might be. This willingness may complement for the reluctance of those who do engineering, design or software development to spend *their* time in close contact with ‘customers’ or ‘users’, let alone to spend their time rather *aimlessly* watching what they do and listening to what they have to say. That kind of thing is not what *they signed up to do*, and it just does not interest them, nowhere near as much as, say, code writing does [and this is not a comment at the expense of programmers, either]. Relative to the engineers/designers point of view the good news is the practical and rather commonplace nature of the ethnographer’s work – the sitting with and following around of people-at-work – ensures that much of what one ethnographer will collect will be pretty much the same as another one will: if one tapes or videos a meeting, then you get to see and hear what everyone in the meeting gets to see and hear. Further, what the engineer/designer might want to hear from them are not professionally informed sociological analyses, but simply common sense observations about fairly obvious [to anyone who spends any time there] matters. Much of the *descriptive* stuff in Hutchin’s distributed cognition account of navigational work (1995) need not differ a great deal from what an ethnographer with an attachment to ethnomethodology would produce – though what they would go on to make of that descriptive work certainly would. Similarly, people with an ergonomics orientation did note many of the same things about the work of air traffic control that ethnomethodologists do. Of course, it ought not to be, for engineers/designers as it might be for protagonists of the different kinds of fieldwork, a matter of scholarly priorities, of who comes up with, first, or distinctively, a certain kind of information.

Of course, we are not saying that there are no differences whatsoever involved between the different kinds of ethnographers, though these will extensively manifest themselves in the ways in which the ethnographic materials are taken up, interpreted and written up. Here significant differences in *sensibility* and in *analytical concerns* will manifest themselves. However, we have taken the view that the contribution that ethnography is as an input to the design process, particularly in respect of requirements capture.

Designers are now sensitised to the importance of the details of work practice in designing effective systems, and the provision of evidence about the organisation of actual work in particular cases is a critical feature. The connection between the *ethnomethodological* ethnography and the input of requirements is an *adventitious* matter: the ethnographer can service the design process by just doing what he or she would be doing in any case, which is attempting to witness and record *whatever goes on*, whatever that might be. Of course, the requirements of a project can be tailored to designer’s needs as these emerge.

All of this might seem a bit disappointing when compared with the expectation that people often have that ethnomethodology will help them redesign the design process. What I have in mind is this: Clearly, there are many respects in which the system design process confronts great difficulties, and there are many ways in which

design processes fail to satisfy those who procure them or who must work with them. Sociology is not, and never has been a unified discipline, nor does it show much sign of becoming one, but it has, during the time of my professional career, developed a very broad consensus. The consensus is that the way to understand society is: as a struggle for control. This consensus can spring from otherwise diversified and competing sources: Karl Marx, Max Weber, the Frankfurt School, or Michel Foucault all provide very different theoretical rationales [and, of course, specifically different ways] of implementing this idea. The workplace is, therefore, to be understood as a primary site of such struggle.

Part of the formation of this view of social life as a struggle for control has been the development of an idea, taken particularly from Max Weber and the Frankfurt School, that formal reason is an instrument of oppression, that the employment of formal techniques is no neutral matter, not simply a matter of adopting the best tools available and appropriate to the job. Max Weber refers to a civilisation-wide tendency toward a process of ‘rationalisation’, the systematic introduction of formal techniques into all areas of life, and his successors have taken up the idea that this is done to serve the needs of regulation and control. There is, thus, an inspiration for a certain *antipathy* toward those aspects of the design process which involve the use of formal techniques, and to welcome evidence of the failure of design operations as demonstrations of the inadequacy of formal techniques as such (cf Kjeld’s ‘On Scripts and Plans’ as a reaction against this, revealing the extent to which it is a prejudice, not a thoughtful understanding??). Further, there is inspiration for the idea that the design process is itself involved in the struggle which must [inevitably] feature in the work process, and that, therefore, one of the important requirements is to change the degree of control within that relationship. Thus, for example, the concern with ‘the user’ does not necessarily spring from the conviction that the user will know the domain of application much better than a software designer, but also from the idea that the involvement of the user is a means of wresting [some] control over the design process itself.

Thus, from a sociological angle, there is often a concern with the possibility of restructuring the design process as part of a wider agenda of changing the balance of power within organisational relationships.

Ethnomethodology does not share in this consensus, and it is on this point that, in particular, it is likely to be severely chastised. For our part, we regard the conviction that social life should be considered as a ‘struggle for control’ (or as a natural, spontaneous harmony) as both *apriori* and *essentialist*. To decline to join the [near] consensus around this point is not, of course, to deny that there are differences of power in social life [what could possibly be a more unremarkable and trivial observation] nor to question whether ‘struggles over control’ may occur. It is, rather, to suppose that *whether or not there are* struggles for control is something that the study should establish, and that it may not necessarily be as simple or straightforward as all that to establish whether or not a struggle is taking place. In many ways, of course, the *essentialist* character of the idea that social life is a ‘struggle for control’ is one which is ostensibly at odds with the facts. In many ways, ‘struggle’ might seem to be more notable for its absence than for its presence in actual workplaces, but the value of the essentialism is this: that things may not be *overtly* matters of struggle, but that is what they are *in reality*.

It is not to say that we could not contribute to the design process at all, only that the contribution would have to be *more ethnography* – ethnography on the design process itself, study of design *as work*. The input into the design process is likely to be, too, into particular design projects, where ethnography can be *one of the sources* of information that designers can use, in trying to develop requirements, where, presumably, its specificity is part of its value: it refers to those matters for which the designers are trying to tailor their design.

It should be borne in mind that there are considerable differences between *research exercises* in which

ethnographers collaborate with designers, and the incorporation of ethnography into the industrial process of system development. In the former case, the capacity for flexible, mutual accommodation is perhaps readily available, but in the latter, the matter is likely to be much more difficult. Finding a way of fitting the ethnographer into the design team is not necessarily going to be straightforward, and the problems of developing a working relationship with designers will present a range of unresolved problems. Fitting the ethnographic work within the framework of the design process and of enabling it to make an effective contribution are, too, issues which will arise. Collaborations with system developers in large organisations show that, for example

- a. the specification of requirements is not something necessarily controlled by system designers – all kinds of parties (such as, for example, the advertising department) get into the act, and the motivations for introducing or upgrading IT may simply be those of having such a system, being up to date, being able to match competitors in the claims that one can make. The problems in organising the design process are often as much problems in *organisational design* as problems in system design.
- b. the point at which the ethnographer enters the process of requirements capture may be one at which much has already been decided, and that whatever problems there might be, the fact of commitment to change is irreversible, things have already been altered and substantial resources sunk into them – whatever happens, the system will have to go ahead and whatever uses are made of it will have to constitute its ‘success’.
- c. Ethnographic information does not necessarily carry a determinate implication. For example, the concentration of much banking work in cost centres has been primarily motivated by cost centres, and with this there has been a huge loss of local knowledge, as well as the possibility of a ‘personal’ relationship between the customer and those who service them. There has been some attempt to recreate this by making particular operators responsible for a specific area, but nonetheless there are tangible effects of the loss of local knowledge – but for the bank this may be a price worth paying.
- d. There is, in other words, the administrator’s question: will some effect be worth what it will have cost? That it might be a desirable feature does not mean that it can *easily* be obtained, or that the effort to institute or recover it will not itself have other negative but unforeseen consequences.

Having inserted the ethnographer into the process, there are problems of making the ethnographic materials accessible in a form that designers find intelligible and useful, and the employment of stories, vignettes and scenarios as means of presentation is something that is being experimented with. There is also the problem of the extent to which the field materials are tied to the ethnographer, and of capturing these – what are we to do if the ethnographer suddenly drops dead – and of generalising them – we do not want to just go on doing ethnographies but want to learn from these and accumulate the understanding that they give, but how to do this? Attempts are being made to draw upon the architectural idea of ‘pattern languages’ in various areas of software development, and they might also be tried out in this connection too.

The role of sociology as represented here through ethnography is a very unassuming one. It might be thought that more might be expected from sociology than this. However, sociology, considered as one of the policy sciences, has always seemed to me subject to the kind of problems which Charles Lindblom and David Cohen, in their

sorely neglected book, *Useable Knowledge* identify. The policy sciences tend to overestimate what they themselves know, to underestimate what those to whom they presume to give advice know, and to underestimate the extent to which problems ‘solve themselves’.

Insofar as sociologists interventions into the design process are concerned these often involve (a) second guessing the designers – one can find out after design moves have been made what unforeseen consequences they have had, but the design problem is, of course, that of planning, and the real problem is to *anticipate* these difficulties (b) a disposition to look for problems and therefore to find them and (c) perhaps to exaggerate their significance, to assess whether these are problems which are practically negligible, and which can and will be ‘lived with’.

Bibliography

1. Grudin, J. (1990), ‘The computer reaches out: the historical continuity of interface design’, *Proceedings of CHI 90*, ACM Press.
2. Hughes, J.A., Randall, D. and Shapiro, D. (1991), ‘CSCW: Discipline or Paradigm? A sociological perspective’, *Proceedings of Second European Conference on CSCW*, ed. L. Bannon, M. Robinson, K. Schmidt.
3. Chaiklin, S. and Lave, J. (1993) *Understanding Practice*, Cambridge, Cambridge University Press
4. Cicourel, A.V. (1964), *Method and Measurement in Sociology*, New York, The Free Press.
5. Hutchins, E. (1995) *Cognition in the Wild*, Cambridge Mass, MIT Press.
6. Lave, J. (1993) ‘Introduction’ in Chaiklin and Lave.
7. Nardi, B. (1996) *Context and Consciousness*, Cambridge Mass, MIT Press
8. Vygotsky, L.S. (1962), *Thought and Language*, Cambridge, Mass., MIT Press.
9. Vygotsky, L.S. (1978), *Mind and Society*, Cambridge, Mass., Harvard University Press.
10. Wertsch, J.V. (1985), *Culture, Communication and Cognition: Vygotskian Perspectives*, Cambridge, Cambridge University Press.
11. Wieder, D. L., *Language and Social Reality*, The Hague, Mouton, 1974.
12. Engeström, Y. (1987) *Learning by Expanding: An Activity-Theoretical Approach to Developmental Research*. Helsinki: Orienta-Konsultit Oy.
13. Kuutti, K. (1991). The Concept of Activity as a Basic Unit for CSCW Research. In L. J. Bannon, M. Robinson, and K. Schmidt, (Eds.), *Proceedings of the 2nd ECSCW*, (pp. 249-264). Amsterdam: Kluwer.

14. Bardram, J (1997) Plans as Situated Action: An Activity Theory Approach to Workflow Systems. In *Proceedings of ECSCW'97*, Dordrecht, Kluwer. Pp17-32.
15. Kuutti, K., and Arvonen, T. Identifying CSCW Applications by Means of Activity Theory Concepts: A Case Example. In Turner, J and Kraut, R. (eds.) *Sharing Perspectives. Proceedings of the ACM 1992 Conference on Computer-Supported Cooperative Work (CSCW'92)*. New York: ACM Press.
16. Glaser, B. and Strauss, A. (1967), *The Discovery of Grounded Theory*, Milton Keynes, Open University Press
17. Smith, D. (1988), *The Everyday World as Problematic: A feminist sociology*,
18. Atkinson, M. and Drew, P. (1979), *Order in Court: The organization of verbal interaction in judicial settings*, London, Macmillan.
19. Zetterberg, H. (1954), *On Theory and Verification in Sociology*, New York, Tressler Press.
20. Blumer, H. (1956), Sociological Analysis and the "Variable", *American Sociological Review*, 21, pp. 683-90
21. Sacks, H., Schegloff, E., Jefferson, G. (1974), 'The simplest systematics for the organization of turn-taking for conversation', *Language*,.50, pp. 696-735.
22. Lindblom, C. and Cohen, D. (1979), *Usable Knowledge: Social Science and Social Problem Solving*, New Haven, Yale University Press.