Ethnography, Ethnomethodology and Interaction Analysis

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This paper addresses the problematic status of ethnographic enquiry in a specific interdisciplinary context, that of H.C.I. and C.S.C.W. related study. In particular, the paper raises issues that relate to analytic relevance, and specifically how analytic 'problems' are translated into methodological procedures in interdisciplinary work. It deals specifically with the varying analytic auspices under which ethnographic study can take place, and in detail with Nardi's (1996) accusation that 'situated action models' fail in some important ways to provide design-related conclusions, owing in part at least to their 'slightly behaviourist' leanings. It seeks to examine whether these alleged failings can or should be remedied, and suggests that the 'problems' that Nardi identifies largely disappear if and when we take the relationship between Interaction Analysis (IA) and 'ethnography' seriously, as advocated in outline by Jordan and Henderson (1995); Blomberg (1993), Heath et al (1993) and others. Nevertheless, we find that some central differences between 'behaviourist' and 'behavioural' approaches to the study of work are in practice elided in the IA stance, differences which allow Nardi to make the claims she does. Recovering behaviour as sense making procedure is, for us, a matter of finding ways of explicating notions of 'skill', 'knowledge', 'memory' and so on as interactions-in-the-organization. These issues, we believe, might prove central to the problem of what workplace studies are 'for' (Plowman et al, 1995).

Analytic Choices in Ethnography and Design

The argument we present below should be viewed very much in terms of a trend towards 'moving out of the control room' (Hughes et al, 1994) in design related fields such as Computer Supported Cooperative Work (CSCW). That is, we are beginning to see a shift away from the highly limited sets of domains in which CSCW research has been carried out - the co-located, task-focused workgroups of one kind or another - towards other organisational domains as computer-mediated communication increasingly facilitates geographically remote collaborative working and the use of distributed systems, not to mention the possibilities of mobile, 'information rich' technologies. This has led, we argue below, to some techniques which have hitherto been seen as informative in design decisions being implemented in domains where their appropriateness might be questioned. Our concern here is with the way in which approaches informed by the ethnomethodological 'turn', such as CA, the Institutional Talk Programme (ITP), and, more latterly, IA are being taken up in domains which, for us, are demonstrably more complex than these studies would suggest. That is, the findings of such studies are decontextualised in a very specific sense, that of context as a 'members' category'. It is our view that the link between IA and ethnography requires serious consideration of their analytic relationship, one which allows us to delay and keep provisional our view of organizational 'complexity', 'richness' and 'contextuality'.

Ethnomethodology's project can be considered, as Button (1991) has argued, as the 'respecification' of sociology and for that matter the other human sciences (see for instance Coulter 1989, 1991; Anderson, Hughes, Sharrock 1989). In pursuance of this theme there have been attempts to place ethnomethodology itself on a methodological footing to address criticisms that ethnomethodological enquiry removed itself from the methodological concerns of sociology. [1] One such strategy was Conversation Analysis. In keeping with Sacks' original search for a 'natural, observational, science' (Sacks 1984; 1992), Conversation Analysis developed methods for, in effect, disposing of common-sense dependency by utilising a strict 'warrantability' clause for its enquiries. Ethnomethodologists of whatever stripe have always insisted that the foundation for any argument concerning 'what is going on here' must be the data, and in the spirit of Sacks' work have argued that the warrant for any given assertion should be visible in the data. We have no objections to this position aside from the fact that it elides some possible differences in what our understanding of relevant data might be when conducting research on how people use technology in organisations. Our argument in this paper is that Interaction Analysis, which we treat as a natural adjunct of Conversation Analysis, is founded on a perceptively driven and behaviouristic notion of 'warrant', one that is relevant only, if at all, to a restricted subset of

technological possibilities. For it to be otherwise requires us to take the notion of ethnography – as – study – of-work much more seriously then it has been in practice in such accounts.

Our reflections on the method and analysis is driven by our interest in contributing to the design process. The pace of technological change has increased so dramatically that the three to five year term from idea to product is now arguably a three to five month term. Finding alternative core business for organisations whose products are facing extinction is forcing designers to look to alternative ways of ensuring that they have a useful, useable and viable outcome to their endeavours. Thus 'usability testing' of one kind or another may, perhaps for the first time, be taking precedence over extended design periods. Nevertheless, the difficulties associated with designing products to be used by people do not disappear just because technological change accelerates. In particular, the effectiveness of new technological systems relies to a great extent on the effective elicitation of requirements. Traditional methods of requirements elicitation, however, are predicated on the assumption that all system behaviour is governed by sets of rules about how work should be done. Thus researchers produce idealised models of the work process rather than examining the human activities which constitute those processes in practice. The result, as has frequently been attested, is treatment of the 'user' as a secondary consideration, where 'usability' rather than 'usefulness' is the main issue. Yet it is clear from a whole range of studies that there are factors which are critical to the efficacy of new systems which may not be identified by traditional methods. It is this which has drawn attention to a range of candidate methods from the social sciences,

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[1] If ethnomethodology does not share the same concern over methods of data collection as are evinced in standard sociological accounts this is because such approaches appear excessively preoccupied with questions to do with obtaining the data and less with what to do with the data once obtained.

and in this context, of course, that the value of ethnography for design has been examined.

There are all kinds of debates in the in the social sciences about how to conceptualise and conduct ethnography including debates between ethnomethodologists and conversation analysts (see Crabtree, this issue). In contrast, within an interdisciplinary context like CSCW there has, until recently, been relatively little such controversy. What controversy has arisen has tended to focus on issues of 'context' and 'motive'. Thus Nardi, for instance, takes issue with Suchman (1987) over what he sees as the 'behaviouristic' assumptions of 'situated action theory' which lead to a presumption that plans are 'retrospective reconstructions', a point we return to later in this paper. We are interested here, though, in understanding how debates within ethnomethodology can be related to ethnographic analysis in CSCW in view of the increasing popularity of approaches which are derived from conversation analysis such as Interaction Analysis.

Ethnography and Technological Systems: Nardi's Critique of 'Situated Action'

The apparent advantages and disadvantages of ethnographic app-roaches to investigation of the social world have been well-rehearsed and have informed many approaches to the use of ethnography in such fields as HCI or CSCW. Here we discuss some of these points in relation to systems development requirements. According to Anderson (1994), effective requirements elicitation needs some knowledge of the context of use. The users themselves are important because they and they alone understand and appreciate such things as the practical

management of organisational contingencies, the taken for granted shared culture, social relations and locally specific skills. Formal methods just cannot get at these because they focus purely either on the data or on the processes. Ethnography can, in contrast, identify such features because it is 'motivated looking'. The relevance is, of course, not what a system of work is or what it is like, but rather a way of examining or working at analytic possibilities. In other words, ethnography as conducted by ethnomethodologists may be construed as a loose set of analytic orientations, founded on rigorous commitments to 'members' methods'. What is much more contentious is one of the analytic orientations involved in what, for convenience, we will describe as the Lancaster school of ethnographic enquiry. This has to do, as we shall see, with the interest in 'skilful' or 'knowledgeable' working. Leaving this aside for the moment, both 'ethnography' and 'situated action' analysis have come under attack for failing to provide solutions.

Nardi has argued that proponents of "situated action analysis", such as Lucy Suchman (1987), fail in their methods because they have no means to deal with concepts of motive, goal, plan, and so on. That is, uncovering the 'goals' of educational tasks is more problematic because they are not directly visible, and by implication the problem of 'something other than that which is directly visible' going on will always compromise naturalistic research under ethnomethodological auspices. Nardi, then, suggests that the failure of 'situated action theory' (throughout, Nardi uses neologisms such as 'situated action model', 'analysis' or 'theory', rather than the term ethnomethodology) lies in its 'slightly behaviouristic' assumptions (p.81), which lead this 'theory' into

presuming that plans are 'retrospective reconstructions'. One aspect of these apparently theoretical choices is methodological - 'situated action theory' fails to take interviews into account, because 'interviews are treated as more or less unreliable accounts of idealized or rationalized behaviour …" (p. 81). Nardi further points to the refusal of parties to 'situated action analyses' to engage with cognitive notions of goal or intention. She also points to the failure of situated action analysis to deal with 'persistent structures such as artefacts, institutions, and cultural values …', and suggests a tension exists in such analyses between their concern for the 'emergent, contingent, improvisatory and that which is routine and predictable' (p. 84). Thus, the 'appearance of routines in situated action models opens a chink in the situated armour with respect to mental representations'.

Nardi, then, has a clear conception of what is entailed in the ethnomethodological conception of 'situatedness' vis a vis the plan. It is a more or less behaviourist position, relying on objectivist assumptions concerning some kinds of data rather than others. It also cannot deal with the intentionality of plans, and is thus forced to describe them as retrospective reconstructions. Nardi is, of course, right about ethnomethodology's refusal to engage with various cognitive categories (excepting that she does not seem to recognise that ethnomethodologists would seek to deny their existence other than as folk-psychological or ordinary language terms). She is, however, wrong at every other point.[2] Regardless, our

task here is to discuss how this view of ethnomethodology and its methods can have arisen. We will argue that it arises directly from a response to how techniques derived from conversation analysis, such as Interaction Analysis, are applied in systems design (see e.g. Jordan and Henderson, 1995).

When thieves fall out ...

Members of any church, or for that matter any radical political group, will recognise the way in which internal squabbles can often take on a significance which appears much greater than the position of the group vis-à-vis the 'world out there'. For this reason, we are insistent that our argument be seen as 'friendly', in that we are not seeking to critique IA per se but to examine what the limits of its analytic commitments might be, and how they connect, both to a presumed

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2 Here, we hope we will be forgiven for a small diversion. Critiques of Suchman like Nardi read her as suggesting that plans and conversely situated actions are different types of phenomena, such that plans constitute one

way of doing things, and 'situatedness' another. We see no reason to accept such a view. The critique of cognitivism Suchman presents is clear and unambiguous (although her discussion of 'plans' construed in other ways, such as 'artefacts', is less clear). It specifies, no more and no less, that the principles that underpin some forms of cognitive science cannot fully account for human behaviour. It is important to be clear, because the source of the confusion lies in the conflation of quite ordinary terms like 'thinking', 'memory', 'plan' and so on – terms with which everyone is familiar – with the same terms as they are sometimes used within cognitive science. To express a scientific and causal view of the relationship between 'things which exist in functional relationships in the mind/brain and 'what goes on in the world'. Ethnomethodology, for good or ill, is concerned with critiquing a model of the relationship between the mind and the world, not with denying that people plan and think. No variety of ethnomethodology that we are familiar with would subscribe to a view which stated in response to any ordinary language proposition about memory, thinking, or for that matter planning, 'nonsense, human beings do not [think, remember, plan]'.

relationship with ethnography and to design issues. That is, we want to take pains to avoid the impression that we are vying for the title of 'best in show' with colleagues who in many ways are working in a similar way. After all, and on the face of it, there is little in the claims made by, for instance, Jordan and Henderson that we might in the first instance object to:

> Interaction analysis as we describe it is an interdisciplinary method for the empirical investigation of the interaction of human beings with each other and with objects in their environment. It investigates human activities such as talk, nonverbal interaction, and the use of artefacts and technologies, identifying routine practices and problems and the resources for their solution. (Jordan and Henderson 1995).

This position is clearly quite consistent with the ethnomethodological line. Moreover, they are insistent on a relationship between IA as they practice it and some kind of ethnography:

> Another widely shared assumption among practitioners of IA is that verifiable observation provides the best foundation for analytic knowledge of the world. This view implies a commitment to grounding theories of knowledge and action in empirical evidence, that is, to building generalizations from records of particular, naturally occurring activities, and steadfastly holding our theories accountable to that evidence .... analytic work, then, draws at least in part, on our experience and expertise as competent members of ongoing social systems and functioning communities of practice" (Jordan and Henderson 1995).

Interactional Analysis draws heavily on conversation analysis, although they do not seem aware of the "squabbles" that have taken place within ethnomethodology over the extent to which this approach can address institutional talk. If there are indeed problems of 'context' and 'generalisation' as they apply to interdisciplinary work, then we might understand them through Hester and Francis' (2000) critique of the Institutional Talk, where they drew attention to what they saw as two developments. Firstly, they argued that CA in some studies was being compromised by its incorporation into theorist-imposed sociological agendas, and secondly that its focus on the sequential organisation of talk was being required to carry a far greater analytic burden than it is capable of bearing. It is this latter point we wish to focus on. As Hester and Francis put it:

> Our second point is a concentration on matters of sequentiality and turn-taking alone whilst perfectly legitimate in themselves, cannot provide an adequate answer to the question of the recognisability of 'institutional activities and identities' (Hester and Francis 2000).

They go on to suggest that such adequacy:

> can only be achieved when a concern with sequentiality is combined with other organisational (e.g. categorical) features of such phenomenon (Hester and Francis 2000).

Their difficulties here are similar to our own. In attempts to get around the problem of the reification of context in mainstream sociology, ITP (Insitutional Talk Programme) introduces the concept of 'structure in action' which involves

> treating social structure as an object that members attend to as a condition and resources for recognising various occasions of interaction for particular

kinds of action and inference' (Boden and Zimmerman 1991, pp.11-12).

What Hester and Francis argue, however, is that examples of analyses in this tradition actually show a tendency to produce contextually informed readings appearing to originate from conventional sociological perceptions of categorical relations of procedures rather than from participants' contextual orientations. [3] We make a similar point here: that studies predicated on some presumed relationship between IA and ethnography actually show conceptually driven readings which appear to emanate from the methodological concerns of the discipline rather than a concern for informing design.

It seems to us that this has much to do with the presumed generic superiority of a particular methodological/analytic perspective rather than the effectiveness of the techniques themselves when applied to a range of possible design problems. We will argue that there has been a serendipitous connection between Interaction Analysis and quite specific technical problems. When the nature of the design in question, however, shifts (in the context of rapid technical change or moves out of the control room, for instance), it may well be that the IA/ethnography relationship being specified as valuable to design may not

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[3] Their example makes reference to analysis of a conversation between a health visitor and a recently-delivered new mother which suggests the latter is 'passing' as a professional and laying claim to medical expertise in the use of terms such as 'episiotomy' or epidural'. Anyone with an experience of pregnancy and childbirth will use these terms as the names for procedures with which a familiarity may at some point be necessary. It is hardly 'passing as' or 'laying claim to' to 'call a spade a spade!

continue to be so. Moreover, and to our concern with Nardi's reading of 'situated action', it is this practical focus on procedures for completing task/interaction sequences alone, along with a set of methodological choices concerning the observation of task completion sequences, that allows Nardi to view this kind of analysis as behaviourist.

What's the ethnography for?

Jordan and Henderson have always argued that IA should be used 'in conjunction' with ethnography. Thus:

> "We rely on participative observation, in-situ interviewing, historical reconstruction, and the analysis of artefacts, documents and networks *for providing the framing context*. In the course of this ethnographic work, we attempt to identify interactional 'hotspots' - sites of activity for which videotaping promises to be productive. Ethnographic information then furnishes the background against which video analysis is carried out while the detailed understanding provided by the micro-analysis of interaction, in turn, informs our general ethnographic understanding" (Jordan and Henderson 1995, our italics).

They advocate, then, some reflexive relationship between ethnography and I.A. and seem quite content to interview people in addition to making video-recordings. How can this quite extraordinary set of confusions have arisen, if in fact they are confusions? Part of the problem here is that it is not exactly clear from the above extract what role ethnography is expected to play. There appear to be two possibilities: Firstly, it may be that ethnography and IA provide us with different and complementary analytic viewpoints, and thus feed from each other in providing design related assessments or it may be that the

ethnography merely provides a background context for the serious analytic work of interaction analysis. At this point, we will refer to a recent study conducted under the auspices of this conjoined methodology, a study of 'time delayed' distributed interaction conducted by Ruhleder and Jordan (1999).

This paper, presented at CSCW '99, is indicative of the kind of study that results from presumptions about the relationship between IA and ethnography. We should note that we do not consider it a particularly bad (nor particularly good) example of such a study. We choose it because it typifies the work done under the auspices of Interaction Analysis; it is a thorough and polished analysis of a highly specific set of interactional sequences, and explicitly recommends that studies based on video analysis be accompanied by something that looks like an 'ethnographic' approach. Furthermore, the paper addresses the application of technology in a distributed fashion and is therefore appropriate for exploring the methodological implications of moving out of the control room. The paper is concerned with the effect of time-delays on video based communication systems and specifically addresses a video segment of pre-meeting and meeting activities between three software developers on the East coast and three accountants on the West coast, using video-conferencing technology. The following extract serves to illustrate the type of 'problem' they identify for analysis:

> We rely on participative observation, in-situ interviewing, historical reconstruction, and the analysis of artefacts, documents and networks *for providing the framing context*. In the course of this ethnographic work, we attempt to identify interactional 'hotspots'- sites of activity for which

videotaping promises to be productive. Ethnographic information then furnishes the background against which video analysis is carried out while the detailed understanding provided by the micro-analysis of interaction, in turn, informs our general ethnographic understanding … (Jordan and Henderson 1995, our italics).

The following extracts from their discussion which in keeping with the CA line, focus on turn-taking and sequen-tiality, are indicative of the problem we are identifying with IA:

> Talk is not just about the exchange of information, but about shared meaning making on multiple levels. The examples above illustrate how delay impacts the ability of conversational participants to create shared meaning through talk via remote communication technologies. In each case
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> > Some kind of trouble arises
> > This trouble disrupts the turn-taking system
> > The trouble source **cannot be identified** by
> > participants (our emphasis)
> > Participants may sense that something might be wrong …
>
> … The nature of distributed technology, however, may preclude people from identifying the trouble and making repairs.
>
> The potential consequence is a pervasive sense of unease".

The points we want to make below have to do with the absence of what we would term an ethnographic 'sensibility' from both the data itself and from the analysis. This in turn has to do with a particular vision of what constitutes the 'visible'. That is, available data results from presumptions about the 'here and now'. Moreover, we argue, the analytic sensibility that sees the sequential

organization of talk, and notably the turn-taking mechanism as being central to the understanding of members' methods, is in full view here. Our point is that a number of possible analytic questions are absent that could only be addressed using ethnographic methods. The questions might include:

1. Does the task-in-hand make any difference?
2. Does the status of the participants, construed by these participants, make any difference?
3. Is this work they could equally well do by using another technology (e.g. telephone), and if so why did these participants not opt for that?

If serious organizational tasks need to be completed, then members will use whatever technology is to hand to 'get the job done'. If these people do not do so, then it suggests that the interactional problems they are experiencing are not terribly serious in the first place. If it is wrong to read the situation that way, then we should know why. One might point out in passing that in other contexts, such as TV journalism, people seem to cope with the problem of 'time delay' rather easily. That is, what seems to be missing here is some conception of an 'organizational context'. It might be presumed that such an appeal is to approaches which are distant from all forms of ethnomethodology, but that is not the case. We are arguing precisely that the skills, knowledges and competencies that members possess, and which enable them to orient to various here and now problems of work-in-organization may not be directly visible in this here and now, given that some set of (unspecified) procedures has produced this particular context as the relevant context for analysis, but will be visible in other contexts within the organizational

setting - contexts which the proficient ethnographer will have investigated and analysed. To put this another way, construing the relevant analysis as a matter of understanding 'interaction' is one thing; construing it as a matter of understanding this interactional data is quite another. That is, we are never clear what kind of relevance criteria are applied such that we can justify applying this data to this technological problem. Construing the analysis as being a matter of understanding 'a job of work' which takes place in the context of various organizational relevancies, and seen by members in those terms, entails seeing a range of other possibilities, including for instance how members define and orient to an organizational context.

Conclusion: 'Interaction' and 'Work': The Problem of Situatedness/Context

The case we refer to above, and to re-iterate the point, is a purposeful investigation of the sequential organization of conversation and activity/interaction. Our point here is not to critique such an attitude per se, nor to critique the principle that Interaction Analysis and ethnography might mutually inform one another. Rather, it is to suggest that if that is to happen, we need a much better understanding of what the relationship between them might be. We feel that this relationship is singularly under-specified to date, within the interdisciplinary context of CSCW, and may be appreciably more problematic than implied. This is, we feel, because there are distinctive analytic foci implied within the two approaches; foci which need to be rendered more explicit, and which may well depend on differing conceptions of 'situatedness' or

'context'. There are, we suggest, at least three discernible views of what 'situatedness' or 'context' might be, especially in the range of papers that owe allegiance to ethnomethodology in CSCW:

1. Situation or context is a members' category
2. Situation or context is analytically driven, and relies on the degree to which it is subjectable to prior categories.
3. Situation or context is driven by relevance to design issues.

We might describe 1) as the stance taken by those who undertaken ethnographies broadly within the 'ethnomethodological studies of work' programme; 2) as the stance taken by Jordan and Henderson where 'situation' is construed as the exhibition of sequences and turns by parties to interaction, and 3) as a stance taken by various practitioners in CSCW who assert precisely that the point of observation is to present observations which can/may be used by designers, and that what we choose to observe will be influenced by those considerations.

1) clearly derives from the conception of ethnomethodology originating with Garfinkel which has it that social order is locally produced through the sense making procedures of participants. In addition, according to Garfinkel, they do this reflexively - not in the sense of self-examination or self-monitoring of their behaviour that Giddens (1991), for example, might intend but rather in the sense of it being through the reflexive accountability of ethnomethods through which actors make sense of their world. By accountability, the ethnomethodologist is referring to the means by which the social world is describable, intelligible, reportable and analysable. The accounts which an actor gives of his/her social world are not descriptions of social reality but are treated as situated accomplishments and analysed to show how participants construct and maintain a social order to allow them to communicate. The ethnomethodological commitment to rendering the social world in terms of what people actually do and say, and how that is accountable in terms of their own sense making machineries has, unsurprising, led many ethnomethodologists to focus on studies of 'work'. Garfinkel uses the term 'work' in a dual sense. On the one hand, it is or may be the things people do as a job. It is also the work of in situ accomplishment. All activities are conceived as 'naturally occurring ordinary activities' from the standpoint of those engaged in them. Such activities might not be ordinary to outsiders but are for the members who perform them because they take for granted the competencies, skills and knowledge involved. The method thus begins by seeing the social world from the point of view of the participants and by examining the members' methods for the practical accomplishment of work- how the work actually gets done - and seeks to elicit clues as to the how and why of everyday working practices. So it may be possible to illuminate the rationale brought by people at work to their various practical tasks and problems. Of course, such insights have subsequently been rendered design-relevant through the work of, for instance, Suchman (1987), and subsequently any number of studies have sought to show that this kind of insight might have a relevance to the design of computer systems.

We do not see the same concern for 'work' in the I.A. version. Rather, the notion that 'context' here might, at

least in part, be analytically driven we think is persuasive. Thus, and for instance, Jordan and Henderson assert:

... much has been written about the complex organization of turn-taking in conversation. For IA the situation is ever more complicated because an IA turn-taking system has to take into account more than talk; it encompasses the whole range of behaviours through which people can take a turn", that is, participate in an interactional exchange system. Not only "turns with talk" must be considered, but also "turns with bodies" and "turns with artefacts … (Jordan and Henderson 1995).

This presumption - that the 'situation' or 'context' for investigation will involve the turn-taking mechanism – explains in some part much of the analytic focus of Ruhleder and Jordan's study, and why it is that Nardi (1996, p.82) can make the quite extraordinary assertion that: 'In situated action, what constitutes a situation is defined by the researcher'. We can only say, 'No, it is not'. Even so, we might ask why it is that we need understand gesture, gaze, glance, etc. as forms of turn-taking at all, but more importantly in an interdisciplinary context why we should presume that these mechanisms constitute the 'problem' in the context of computer use. The relevance of any 'context' here is not that of context as a members' category, for that context would have to derive from how members construe and manage their 'work' and we have been at pains to argue that the 'work' is not in view. Rather, 2). and 3). are implicated. After all, appeals are made to the design relevance or the evaluative implications of IA on a more or less regular basis, both in this study and elsewhere (see for example Heath and Luff 2000). Moreover, it is the fact of these analytic presumptions in practice

that allows Nardi's egregious description of 'situated action analysis'.

She makes three errors. Firstly, she presumes that ethnomethodology logically involves a 'behaviourist' stance whereas it clearly does not. Nevertheless, we should acknowledge that in some of the methodological and analytic choices we see in IA, we see something akin to 'slight' behaviourism in practice. Secondly, Nardi presumes that IA's apparent omissions can only be remedied by recourse to cognitive notions of plan, goal, motive and so on. This is plainly wrong. Thirdly, she also misunderstands what ethnomethodological analysis involves. Thus and for instance, she supposes that ethnomethodologists reject anything that looks like interview data. What many ethnomethodologists would, in fact, reject is the notion that an interview is a privileged and formal setting where data of an approximately 'scientific' value can be collected. Ethnomethodologists working in CSCW have in the past, are currently, and will continue to talk to people on the grounds that it provides useful data. In contrast to the approach taken by other analytic traditions, ethnomethodologists are, however, much more concerned with the warrantability of data. That is, what justification we have for arguing that any particular thing is 'going on' should be evident in the data and open for inspection. In this way, the plausibility of any given account of 'what is going on' can be tested against other accounts, and it is for this reason that ethnomethodological accounts typically contain rhetorical flourishes such as 'can be heard as …' or 'can be seen as …'. It means that when we review interview data we should recognise that one of the things that may be going on is the 'social

construction' of the interview itself, including the way in which respondents may design their responses to questions in part through their knowledge of the context in which the questions are asked (including for instance inferences concerning what interests the questioner).

Again, however, it may be that in some IA accounts there is a very strict notion of 'warrant' and one which opens the way for Nardi's accusation that such accounts deny experience. The strict notion of 'warrant' we referred to earlier creates a major methodological issue within the context of interdisciplinary work, and that is the problem of rendering the 'meaningful' nature of human interaction, or 'work' as a 'lived experience'. Ethnomethodology is founded on the principle that it is possible to provide descriptions of 'lived experience' without recourse to cognitive categories, without any appeal to 'mental' states or functional architectures of the 'mind' because 'lived experience' itself is socially rather than cognitively constituted. However, such a position entails a different methodological stance from the one rehearsed by the version of IA that Nardi critiques, since it suggests that any description of 'lived experience' must be founded on our own knowledge of-and-in the social world. Such knowledge is always open to review, contingent, available for re-description, and accountable. That is, analysis does not and cannot, be reduced to observation of behaviour because our assessments of 'what is going on' must be founded on our own knowledge of the world. That is, briefly, the standard for analytic understanding of any social encounter is a standard of plausibility. A warrant for analysis exists under this regime as well. Data in this version must also be

available, and there is some sense in which the grounds for analysis must be visible in it. The sense in question, however, looks rather different here. In this version, 'warrant' suggests that within the data we can see the plausibility or otherwise of any given version of events, and moreover that, in opening up the data for scrutiny, other more or less plausible versions can be explored.

Much of the difficulty we see in linking IA to ethnography lies in the way these varying conceptions of 'situation' or 'context' slide in and out of the narrative. Of course, and precisely because the context of enquiry is interdisciplinary, we are not suggesting that any single version of 'context' should take precedence whatsoever the purpose (it has been argued before that analytic choices, and thus relevant context, can shift during the course of interdisciplinary enquiry (see Hughes et al 1992) Nevertheless, we are saying that adopting a stance which adheres rigorously to members' conceptions of 'context' entails analytic commitments to understanding organizational 'work' rather than simply to understanding any specific interactional sequence, and this can only be undertaken by taking the analytic work of ethnography seriously. That is, the work of ethnography is rather more than to provide a frame for the serious business of Interaction Analysis. This, because interaction, like 'order' is everywhere (Sacks 1984), and we may in principle dip into it wherever we see social life. Our problem as analysts in CSCW is precisely to identify how and in what ways interactions may be germane to design, and if so, what interactions. We are most likely to conduct this enterprise successfully if we recognise the elements of 'at work' organizational life. These elements, as mentioned above, have to do with the

skills, knowledge and competencies that organizational members have and display (see Harper et al 2000 for one example of this kind of 'organizational ethnography').

This argument, as we stated at the outset, should be seen in the context of the ever-increasing complexity of design. The move to less 'bounded' domains, we have argued, raises the prospect that what has been seen as an in-principle coupling of Interaction Analysis and design problems may turn out to be, at best, a serendipitous relationship between one set of analytic interests and a specific set of problems to do with 'control room' technologies. As we see a shift away from the highly limited domains of CSCW research towards more complex, and arguably more interesting, problems so we may see a progressive decoupling of this relationship. In part, our concerns lies with when our analytic interests should become design relevant, and what we see in IA is a rush to focus on presumed 'design relevant' matters which may, in highly complex organisational situations, miss the point. The point of the ethnographic stance we advocate is that it paints a picture of organisational 'complexity', 'richness' or 'contextuality' which allows us to identify what matters might turn out to be relevant to design in the first place. We do this not merely by 'framing' the work of IA, but by taking a quite specific analytic turn towards what are, visibly and accountably, skills and knowledge in work. That is, and in sum, IA's specification of the relationship between ethnography and 'video data' is, as Nardi asserts, radically wrong, or 'turned on its head'. There is no reason, however, to equate this with analysis of 'situated actions', for 'situations' as we have shown can be construed in a variety of ways. Seeing

'situation' as a members' category and recognising the fundamentally social nature of our own analytic procedures, we have argued, disposes of every one of Nardi's objections.

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