

Of Ethnography, Ethnomethodology and Workplace Studies

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I fully realise that you all wish that Lucy Suchman was standing here to give this plenary talk. Wisely but politely she declined the honour. It is no fun having to give a talk to an audience amply peopled by one's friends, on a Friday when the call of the Grafton is already gathering momentum in the minds of many.

What I want to say is a few words about ethnography, ethnomethodology and system design mainly by way of clarification rather than any new insight. Save that for the Grafton. As many of you already know, one of the surprising turns in the last decade is the acceptance of ethnography as an important contributor to system design. As it also happens, many of the people who pioneered this move were also ethnomethodologists. It would of course be easy to construct some high minded story about why it was that ethnomethodology, after being ignored for so long by the sociology mainstream, became accepted by hard-nosed system designers. But, as with many such developments, it was much more low-minded and contingent. In fact, it had little to do with ethnomethodology at all and much more with ethnography as fieldwork - and a happy coincidence of amiable boozers.

I was asked to review all the work in the area focussing on the work at Lancaster (and one might as well include in that Manchester, too), and saying something about its 'usefulness' - an interesting notion - and its relevance for foundational issues in ethnomethodology and wider debates in social theory - another interesting notion, 'social theory'. I will, of course, do my best on all of these counts. Perhaps the best way to

proceed would be reasonably historical and focus the story around the experiences of Lancaster. This is simply a device to tell a story rather than a claim to any kind of intellectual pre-eminence. Nobel prizes are not at stake here. However, purely contingently, Lancaster has been one of the foremost institutions in CSCW, the major package within which ethnography entered into system design. Notice I did not say ethnomethodology - and for reasons which, I hope, will become clearer as the story unfolds.

In the beginning was the word ...

As with so many things, it all began with the word and the word was written by Lucy Suchman and the word - or, rather, a number of words - *Plans and Situated Actions*. A book which seemingly gave voice and demonstration to the sociological point of view that technology can be understood as a feature of interactional work. The fact that it was also an attack on cognitivism did not go amiss. Nor did the fact that it was written by someone then a member of Xerox Parc which, then, was trying to bring a social dimension to bear on system design and, largely through the persuasive powers of John Seely Brown, one of the senior managers, added yet more lustre to the reception of the book. The further fact that Lucy had actually been taught by Harold Garfinkel and knew Harvey Sacks meant that it could not fail to be of interest to ethnomethodologists.

However, in the UK the story begins earlier with what was known as, for those of us involved - Bob Anderson, Wes Sharrock, Dan Shapiro,

Richard Harper and myself - as the first air traffic control study in 1987-98. Although ostensibly about the automation of air traffic control (but really about trying to understand more about rule following in a 'real time, real world' environment) it made the point that although aspects of ATC 'naturally' lent themselves to automation it would be wise to understand how the work was actually done before doing so: a position which has become widely held as a major tenet of CSCW though we would not wish to claim any originality to the thought.

At a paper on the ATC project given at the first ECSCW Conference at Gatwick, a colleague from Computing, Tom Rodden - whom I had never met even though he had been at Lancaster for over a year then - said that we were doing CSCW and that we should get together to work on research projects which we did. This was closely followed by the second ATC study this time in conjunction with computer scientists who sought to use the findings of the ethnographic study - this time done by Famous Dave Randall who replaced Richard Harper - for the design of systems. By this time the "juggernaut" was rolling. Bob Anderson had taken up an appointment of Cambridge EuroParc and, shortly afterward attracted Richard Harper there - a process which has been going on for some years subsequently as that institution has raided Lancaster and Manchester for talent! Research grant followed research grant in what seemed like a never-ending treadmill.

There are, I think, two things worth noting about this first phase, to call it that.

Ethnography vs. ethnomethodology

The first is that it was not dramatically ethnomethodological though perhaps

the Report on the first ATC study contained more explicit ethnomethodological discussion that did subsequent ones. Regrettably this did not result in a publication - other than some papers - which could have reached a wider audience and situate itself among the growing number of ethnomethodological studies of work. There were many practical reasons for this not least the problems of more and more research grants to deliver on and little time to do the necessary concentrated thinking that such a book would have demanded. This apart - and it is not a trivial matter - what is important to note is that what was of interest to CSCW was not ethnomethodology but ethnography. I am not suggesting that ethnomethodology was irrelevant but, rather, that its presence was rather muted. To a degree this is reflected in the clumsy phrase, 'ethnomethodologically informed ethnography' which was often used to describe what we were doing. I still cringe at this inelegant and not very brave phrase.

There were, I think, a number of reasons for this. One was the refreshing fact that computer scientists were not, on the whole, very bothered about sociological wars. One did not need to bother reviewing ontological, epistemological reflexivities - or whatever passes for sociological debate these days - in order to convey the results of studies. But, more than this, what the computer scientists wanted was not so much ethnomethodology, or any other approach for that matter but descriptions of 'what was going on' when, say, controllers did the business, or what processing a cheque looked like. In short, what they wanted was ethnographic reportage rather than sociological analysis.

Nor of course is this surprising. No matter how much distributed system designers wanted to introduce a social dimension to their thinking, they did not want sociology. Theirs is not a sociological pursuit. What they wanted was information about the 'real world, real time' activities of people working within a setting and into which their technologies would have to situate themselves. But, in my view, this is just the advantage that ethnomethodology has in the context of design, in that its studies of work begin from the point of the details of the work. As we all well know, ethnography can be the vehicle for many persuasions. Even Actor Network Theory can. Moreover, in the field of CSCW ethnography is almost mandatory irrespective of whether one espouses Activity Theory, Distributed Cognition, Grounded Theory or even Task Analysis. One could say that it has become fashionable to do ethnography in system design, so much so that a few interviews with users can be described as ethnography. But, it can be argued, ethnomethodology has, as its first requirement, the detailed study of work activities as the orderly in and through product of intersubjective action.

In this respect one might recall Garfinkel's promotion of a programme of studies of work originating on the observation about the 'missing what' of the sociology work, namely, the work that people actually do. In the sociology of work one will find a concern with all kinds of topics - employee morale, job satisfaction, unionisation, political affiliations, etc. - but not very much about the work itself. Thus, the 'missing what' was the actual work. And this is the theme that has preoccupied much of the work in CSCW in Lancaster and elsewhere. How members accomplish their work tasks, knowing what they know.

It was this motivation which, I suppose, resulted in the clumsy expression 'ethnomethodologically informed ethnography', that is, looking at how people conduct their work in real settings, doing what they do in the mutual accomplishment of sometimes divergent intentions, treating work as socially organised and, above all, interested in how it is socially organised in that setting. This means looking at the actual working division of labour as routinely and ordinarily manifested in persons' meaningful orientation to their work, not work as some idealised conception. The task is simply to report in adequate detail how they go about doing what they construe as the things to be done. It was this kind of approach which was like a breath of fresh air to computer scientists brought up to believe that the social sciences meant cognitivism, task analysis and other curiosities of that ilk.

The 'discovery' of teamwork and the sociality of decision making

There was, however, a more meaty and directly ethnomethodological thrust in the analysis of controlling work. Up to that date - and as far as I know it probably still is predominantly so - much of the research on controlling work was cognitivist in orientation and had been so for some years. For example, one of the issues that preoccupied the then current thinking had to do with the cognitive overload that controllers were likely to face as air traffic densities rose. Thus, there were numerous psychological studies directed toward building a mental model of controlling in order to inform the design of the next generation of control suites, to address which tasks could be automated, and to facilitate the selection and training of controllers. It was this kind of thinking

which had produced a number of technical facilities on the control suite - such as conflict alerts - which controllers refused to use thereby creating a constant puzzle and worry to the scientists involved.

Being suitably unencumbered by such a tradition but armed with a more sociological interest in the work, we were able to bring out two things: the teamwork of controlling and the sociality of decision-making. These were, in significant respects, aspects of the same thing, namely, the social organisation of controlling around the suite and in the control room. What came out early in the fieldwork was that the controller, along with the rest of the personnel around the control suite, are involved in a 'working division of labour'; that is, a division of tasks and responsibilities which while specified in the job descriptions, etc., not surprisingly conspicuously failed to adequately describe the actual coordination work going on in 'real time' decision-making around the suite. Without going into detail I do not have time for, effectively it was a pattern of work motivated by 'looking out for each other' and each member of the team being able to 'at a glance' monitor the various resources furnished by the suite - radar, strips, and RT.

But, importantly, it was also clear that the notion of 'mental model', again not surprisingly, also failed to describe the decision-making that was involved in making the traffic flow - again, as a 'real time, real world' activity. Behind the idea of the 'mental model' was the notion that the controller was almost following a plan which was implemented in the actual control. While there was a colloquial sense to this idea, it was clear that a literal interpretation of a 'mental model' could not account for the coordination work mentioned above which was 'at a

glance' publicly available. These are, of course, familiar arguments within ethnomethodology. However, what we were able to do was describe the 'actual' course of decision making in terms of a sequence of tasks following the ethnomethodological injunction to ask, 'what do I do next?' While clearly there is some planning in that the traffic flows have fairly predictable rhythms, the strips contain flight plans of aircraft about to come into the sector, and there are rules to follow for particular routes, but there are always 'normal, routine' contingencies which have to be dealt with. In many ways, the decision-making process is one of 'buying time', reducing the costs of coordination with adjacent sectors, and leaving time to deal with the more troublesome elements of the traffic flow. The process is not so much the application of a mental model as the sequencing of a series of practical tasks, dealing with the traffic flow as it happens and arranging it in a manner consistent with the spirit of the air traffic rules.

Even though I have had to gloss over the rich analysis that came out of these two studies, it would be nice to say that it had an impact on the sociology of work but, regrettably, this cannot be said. The sociology of work had all but disappeared presumably on the grounds that Mrs Thatcher had already killed off most of the work done by what were the hoped for cannon fodder of the revolution. And anyway, it was, apparently self-evidently, clear that the latest crisis in sociology was to be solved by examination of the media, film, diasporas, identity - not forgetting more theorisation. Apparently in a post-modern world nobody works.

The studies did, however, gain some prominence in the CSCW community and are still cited as exemplary studies of work for CSCW

though, it has to be said, this focus was not a prominent one at the time.

But it was one thing to begin working within CSCW, but quite another to work out just what this might involve. There were, of course, issues to deal with which were unfamiliar within sociology, many of which we tried to address in the COMIC project.

The COMIC Project

I'm sorry but I can never recall what this acronym stood for. Suffice it to say that it was one of the first major research projects - funded by Esprit - in CSCW and where, among many other things, the place of ethnography in system design was set out along with reports of a number of work studies. In addition there were a number of other problems addressed, problems posed initially by computer scientists.

The first was how to communicate ethnographic findings to designers. This may sound odd - after all why not talk to each other? But essentially it boiled down to trying to relate a graphical medium and a verbal one. Software engineers love working in diagrammatic symbols whereas ethnographers are nothing if not discursive. The problem was, I think, more to do with unfamiliarity with respective domains rather than any principled problem, though we did, for a time, play with DNP, a support system trying to connect ethnographic findings with a diagramming tool so that designers could explore the implications of the findings.

Another was the problem of scale. Studying relatively focused work sites, such as control rooms, is one thing but often systems are designed for whole organisations. What possibly could ethnography contribute to these kinds of design problems? Of

course, there is an important sense in which the very statement of the problem prejudices the nature of the social phenomena subscribing to what Jeff Coulter has called the 'container model of organisation'. In various forms this has always been the argument against ethnography, namely, that it is essentially the study of cases and is not a generalising method such as the social survey.

This was an embarrassing issue from an ethnomethodological point of view. Partly because, and familiarly, it wants no truck with the methodological agenda of positivistic sociology or with fanciful ideas of macro and micro levels of social phenomena, but one could see that generalisability was an issue even for people with no background in either of these issues. We came up with basically two defences. The first was the familiar ploy of pointing out that what passed for generalising studies in HCI and CSCW was in the main to get a few students to use a system prototype - and this hardly warranted strong claims to generalisability. The second was to identify types of fieldwork studies in terms of their duration and purpose within CSCW: a ground clearing exercise which, again, has assumed more prominence within the CSCW community than it perhaps deserves as little better than an act of cowardly desperation in a period when we thought that we could be found out any time. The success of the COMIC Project proved to mark an important turning point in the careers of many of those who took part, both here and in Europe and, in an important way, established 'ethnomethodologically informed ethnography' as the main contender in establishing the importance of fieldwork in CSCW. Since then, and again speaking of Lancaster, the studies of work within the ambit of CSCW have proceeded

adding studies of aspects of banking work, office work, among others.

For now I want to move on to some issues of more direct relevance for ethnomethodology and ethnography but, again, drawn from experiences of work study within CSCW.

The benefits of atheoreticism

Earlier I remarked ethnomethodology had particular advantages in its use of ethnography to inform system design when compared with other approaches which also use fieldwork methods. It is here where I can simply point to the massive difference between ethnomethodology and constructivist sociology and the methodological decision of the former to look at the phenomena in its 'raw state' independently of the apparatus of sociological theories and methods which obscure the phenomena they presumed to address. The 'raw state' of the phenomena is, of course, the 'everyday world' and the 'ordinary affairs' of the members of society. Against the idea that the order of society is a 'hidden order', ethnomethodology offers the notion that the 'real society' is itself socially determined according to practices which are themselves provided by the society.

Here I want to borrow - as so many times - an assessment of ethnomethodology from Wes Sharrock to the effect it says the kind of things you couldn't disagree with. It ought to bring us back in touch with the things we recognise as the most normal, ordinary, natural things in the world. The aspiration to general theorising is an aspiration to novelty, to things that are not ordinary, to give new insights to people, to portray society as something different to the way it is usually experienced. This means that the commonplace, the ordinary is not

only regarded as beneath attention - is it just my impression that most of current sociological theorists are middle class? - but also gets left out of the theoretical picture. If ordinary things are considered this is through the theoretical lens. Thus, if ethnography is a means of looking at the commonplace, the ordinary, then from an ethnomethodological point of view, it requires this be done independently of any sociological agenda which seeks to state in advance what is of interest in ordinary affairs; to look at people as if they lived their lives without reference to what sociology finds interesting. The ethnomethodological researcher is, in effect, relaying understandings appropriated by an apprenticeship in the field.

One can see the differences if we look briefly at another approach used in CSCW which makes use of ethnography, namely, distributed cognition. This draws on the venerable if mistaken distinction between the 'inner' mental world of individuals and the 'outward' world of behaviour. While starting from the individual's mental operations as conceived in cognitivism, it realises that rigorous insistence upon this stance is restrictive and needs to be expanded by 'extending' the mental domain into the 'outer' world. Accordingly, artefacts can be regarded as extensions of mental functions - filing cabinets, records, and computer storage systems, for example, can be characterised as 'memory operations'. It is such thinking, I suggest, which encourages system designers to think of Organisational Memory and the like, and to design systems which bear little or no relationship to the activities in which record keeping, filing cabinets, etc. have their voice. The ordinary, practical activities get left out.

As I see it, because ethnomethodology eschews any commitment to any sociological theory but has to approach the most normal, ordinary and natural things in the world, then ethnography done under its auspices and relayed to system design ought to have a number of benefits. The main one being that it can - the caveat is always 'if done properly' - tell it the way it is ordinarily, routinely, everyday. This may not be what the designer is designing for - it may be something innovative which changes the way things are done - but it is, I would argue, an essential background to informing even the most radical of system designs, not to forget that not all things require technological solutions.

Another benefit is that engineers and designers need not get involved in the kind of debates which have preoccupied social scientists for centuries and are no nearer resolution than when first articulated. I do not intend any criticism here for the problems of the social sciences are deep and troublesome. So are the problems of engineers and designers and which would be made even worse if they had to contend with the problems of the social sciences. Of course, it does not deter all of them by any means.

Nor do these benefits, if they are such, always allay the anxieties of designers, especially engineers and designers from industry. In our experience, while there is a widespread recognition of what CSCW is trying to do, namely, introduce a social dimension to the design of distributed systems (and they are generally quite happy at the lack of social science theory for which they typically have scant respect for, anyway) they do express some anxieties over the lack of method in ethnography. These are people whose whole working lives are

surrounded by software engineering methods of one sort or another. Getting the right method for the job is a constant preoccupation - a Holy Grail one might say. Against this kind of mentality, ethnography looks to be impossibly lackadaisical. They, again in our experience, very often like to see, at last and with much relief, an approach which produces descriptions of the work which they can recognise, and glimpse that perhaps the lack of this is what makes systems fail.

As we all know, ethnography is very much a practical activity, no matter how much we might, say, want to engage in the 'How-was-it-For-You-School of Ethnography?' - which is not, in my view, ethnography at all but self-indulgence. The fieldworker has to collect his or her material not as dictated by strategic methodological considerations but by the flow of activity within the setting. Nor does it require any special or arcane skills for obtaining access and information. On the whole, people are more than willing to talk to fieldworkers and let them observe their activities. But despite the apparent lack of method, the fieldworker cannot really fail. Even a few days of fieldwork is likely to produce more material than one can use. Ethnography is a diffuse exercise and is as much a trawl of some setting as it is something methodical, a matter of seeing what turns up when you hang about in a particular kind of place. This does not mean that 'anything goes' but it does make the exercise a very practical one of making do with what has been gathered and assembling it into an account of the work in the setting and as a 'real world, real time' set of arrangements. Of course what else is done with the materials belongs, in an important sense, to what I was talking about earlier, namely, the different social science agendas. But, at the level of the ethnographic

materials, fieldworkers will very often come up with much the same kind of things and it is at this level, I suggest, where it is most useful to the engineer and the designer.

Transformation of design?

Has ethnography – ethnomethodologically informed or otherwise - transformed system design? What kind of claims can we make on its behalf?

The answer, of course, is that it hasn't transformed design which is not to say that it hasn't made a contribution to design thinking. Its use remains still firmly rooted within research rather than in industrial and commercial contexts and there are places in the UK and Europe which would not be seen to design without it. There are signs, albeit small ones, that there is a growing interest in industry in using fieldwork as an additional resource for system design. But this is a slow business.

In any case, it is important to remember the conditions under which most system design in industry is done. The specification of requirements for a system is not, or rarely, controlled by designers themselves. All kind of parties get into the act including marketing, higher management and information systems departments (or their equivalent), they often change their minds (not unusually when the design process is well underway and fateful decisions already made) and have to respond to changes in the economic climate and more. All of which makes the design process as much an organisational one as a process of technological design. All of which makes it hard to determine at which point an ethnographer should enter the design process. Ideally, it ought to be right at the beginning before matters are settled. Unfortunately, this is unlikely to be the

case. Much will already have been decided. In a study of the police we did some years ago, one of the systems we looked at was HOLMES, a system devised to cope with, and coordinate, the masses of information that large-scale inquiries generate. There were a lot of problems with this early version but one knew that, at a cost of £8m, it could not 'fail'.

For a further example, in the retail bank we have been studying for some years now, it is clear that the technological changes have as much to do with dispensing with labour and its consequences, as it has to do with the supposed benefits of new distributed technologies. And, despite the manifold problems that the incorporation of these new technologies generate, things are not going to change except in the direction of more of the same. The organisational imperatives, many deriving from the economic conditions in which such organisations operate, are too strong for that. The technology, in other words, is only a small part of the story of organisational life and, moreover, one which is often very dependent on what else happens in that life. Losing the kind of local knowledge that members of banking branches had by removing many of the banking functions to regional cost centres might be a price worth paying, especially if one could recreate this in some way by using new technologies and organising work responsibilities appropriately. But even if these attempts fail the cost may still be worth paying, not least because it is too late, and too costly to change back to the way things were before. Further, some features of a system may be desirable but can they easily, and cheaply, be obtained, and will they have negative consequences? These kind of questions simply indicate the difficulties of organisational change - a

lesson which perhaps those 'disciplines' which are much more effective in design, namely, accountancy and management science, ought to take more note of.

Of course, ethnography might have more purchase if it had determinate solutions to the problems of design. But, of course, it does not and, probably, cannot. The best it can hope for is that by explicating the work setting as a lived 'real world, real time' collection of activities, the design is better informed. And, even though this sounds modest - and it is meant to sound so - compared with what was previously available, it is no small achievement. Sensitising system design to the social dimension and, moreover, offering it a means of realising that without the confusions of social science theories is, in my view, a considerable achievement even though largely unrecognised by the rest of sociology - but then who cares?

What more can ethnography and ethnomethodology do in this domain? Except for more studies of work, it is hard to see quite what more is involved. Since ethnomethodology eschews the aspiration of generating theory then it can hardly lay any claim to improving sociology beyond Brewstering other approaches or, more tactfully, issuing reminders that sociology ought to be a more serious business than is usually allowed by sociologists and, as part of this seriousness, abandoning its exaggerated ambitions. Ethnomethodology has been doing both of these things for a long time now but without very much success.

In the field of work study and design the opportunities are enduring. Work and computers are not going to go away. If anything they will intrude more and more upon our lives even worse than mobile phones do now. This will mean new challenges for

fieldwork in leisure, in the home and anywhere else where system design is going to make its presence felt. In this respect the future looks promising. Further, there remains the problem of making fieldwork less dependent upon the survival of the ethnographer and the enduring problem of making ethnographic materials and analyses available in a form that designers will find intelligible and useful. To this end experiments are ongoing with stories, vignettes, and scenarios which are familiar enough to fieldworkers but, also, and perhaps more tentatively, drawing on the idea from architecture of pattern languages' as a means of representing patterns of activities in a form more usable by system designers. One can see the dangers of such an enterprise but it is one worth consideration and research.

The role of ethnography as the vehicle for ethnomethodology and I suppose I should add, sociology, is a modest one. And, of course, ethnomethodology's agenda is wider than that of system design and it would be quite wrong to suggest that the future of ethnomethodology lies in system design beyond the fact that it may offer better employment opportunities than sociology. But, for me, one of the attractions of ethnomethodology compared with the rest of sociology was not its modesty - one has only to mention the names Harold Garfinkel and David Sudnow to suggest that modesty is not a paramount virtue among at least some of its significant figures - so much as the seriousness with which it pursued the sociological project. An important part of this was its refreshing honesty about what was achievable, what was do-able, that it made the idea of a recognisable and familiar world the centre of its studies, and did not make sociology out to be a cult for the marginalised middle class academic.

Let me not, however, end on such a sour note. Ethnomethodology and ethnography may have much to contribute to design - and today we are going to listen to some of these contributions - but let us not be over-ambitious or run away with the idea that we are the elusive 'silver bullet' that will solve the problems of design - or get rid of werewolves. Interventions in the design process are often about second-guessing designers, that is, showing what the unforeseen consequences of design decisions have been. But, of course, the problem of design is anticipating such problems, making it a very difficult business and a challenge we have yet to rise to let alone fail at. There is no reason to think that we can do better than anyone else which is not to say that we have no contribution to make.