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The Organisation in Ethnography

A Discussion of Ethnographic Fieldwork Programs in CSCW

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Abstract. Ethnography is now one of the key approaches used within the CSCW community to specify the role of computer based systems in work practice. Yet what ethnography involves as a program of inquiries is only discussed in a piecemeal way in the literature. This paper attempts to make up for that absence by describing one fieldwork program (or programme) developed over a number of projects in which ethnography has been allied with computer systems and work practice design. The discussions will be of interest to both expert practitioners of ethnography and novices.

Key words: computer systems design, CSCW, ethnography, methods, organisations, the International Monetary Fund, work practice

1. Introduction

In this paper I want to consider one of the key methods used in CSCW research. The method is called "ethnography", but this terms hides much. For ethnography is a label used by sociologists and anthropologists in a somewhat casual way – a catch-all phrase for a range of different things, just as long as they involve fieldwork of one sort or another. Within CSCW, where ethnography is expressly linked to system design, the term is even more troublesome: as a member of one organisation I have studied once put it, "Ah, the E word", alluding to the fact that, as far as he understood, ethnography is something that whiz kids from the CSCW world use to transform what is "just" a means of looking at what people do in organisations into something that is a marvellous new technique that will revolutionise office systems.

Unfortunately, ensuring that ethnography is part of a research program does not, by any means, ensure that the results which are generated solve all the problems the research sets out to investigate. Ethnography is simply one way of looking at how people do their work. It is, and has been demonstrated to be a very valuable addition to the methods that have evolved over the years to understand and represent work. But it is not (most emphatically not) the answer to such things as the problem of requirements capture (i.e. how to get a perfect specification for a system, a specification which leads to a system that does all the users could want and more).

Nor is it the unique technique that will enable managers to properly understand "the culture" of their workplace. Nor yet is ethnography the trade that can ensure that systems are evaluated properly. Certainly ethnography can go some way towards these things, but it is like any tool. It is part of a toolbox – an artefact that has to be used alongside other tools.

In this paper I want to discuss what ethnography involves as a structured, organised set of activities. My concern is not with the structure of activities into which ethnographic research fits, as with the structure or program of the ethnography itself. With empirical illustrations, I will outline one particular *fieldwork program*. By program I am thinking of those series of work packages undertaken, their sequential order and the inter-relationship in an ethnography.

Such a structure is, of course, only the foundations of effective ethnography. Built on top of this is what Anderson and many others have come to call the "analytic sensibility" of the trained ethnographer (Anderson, 1997). This sensibility is of vital importance and is reflected in the extensive discussion of what it comprises. Nonetheless, without an effective program, the deployment of this sensibility will be seriously weakened. This is particularly so in undertaking organisational ethnography. For here certain steps are required to ensure that the ethnographic materials cover a sufficient spectrum of organisationally situated tasks to enable proper examination of any particular subset of those tasks. It is only then also that the materials generated by the research get taken seriously by members of the organisation itself.

Ethnographic research in organisations can still be effective even if these steps are not followed; my view is that such success is less likely to be achieved without it. Moreover, the program I will be outlining here is not the only way of undertaking fieldwork, though the success of the program in question has demonstrated that it is certainly one fairly effective way. In any case, one will scour the CSCW literature to find any information on what programs have been used in other ethnographically informed research activities (presumably these activities have involved using some kind of fieldwork program). By and large, most discussions and reports of ethnography in CSCW say very little about what the organisation of the fieldwork might have been, treating those things as taken for granted matters that can be left aside. Most of the papers that do discuss ethnography are intended, as one of the authors of those papers puts it, for *aficionados* who are well versed in the programs of relevance (Anderson, 1997). Exceptions to this are few, but worth reading (Hughes et al., 1994; Jordon, 1996; Anderson, 1997).

Needless to say, the imbroglio surrounding ethnography and its role in CSCW (and indeed elsewhere) is not something that can be tided up in this paper. Others have been investigating this topic and their work, too, needs to be referred to (see, e.g., Anderson, 1992, 1994; Blomberg et al., 1992; Hughes et al., 1993a; Shapiro, 1994; Norman, 1998, pp. 185–201). In any case, it is my belief that research such as is reported here is part of the material that will enable some untying of knots in this area, but as yet we are on the path to that tidying up and are not quite there yet. One

does need to be very careful when one starts discussing and reporting ethnography. It is not always quite what it seems. It is not as facile as it sometimes appears, nor yet as elusive and difficult to undertake as some discussions pretend. But it is useful and can uncover important materials that need to be taken into account when systems are being designed, implemented and evaluated; it can make the difference between good and bad, between the nearly good and the just right.

To achieve these goals will require more than simply an adequate fieldwork program, whether that program be the one advocated here or some other. Though my main concern in this paper is with that program, I will conclude with some discussion of the alternative processes of research and design into which ethnography can fit. I will be particularly concerned with the need to better link ethnographic research to the goal of "uncovering the organisation" in any organisation studied. In this, ethnographic research not only has a role to play in systems design but in those activities more traditionally associated with business consulting, particularly business process re-engineering (BPR). I will not comment conclusively on how ethnographic research can play in these processes since I believe much work needs to be done before one can confidently ascertain the powers of each, and hence the suitability of certain of these processes for specific design tasks. Nonetheless, I think it appropriate to conclude on this question, since the processes in which ethnography will fit will be the next set of concerns that the reader will need to consider if, as I hope, I will by then have persuaded them that there are good reasons for systematic properties in the undertaking of ethnographic research.

1.1. BACKGROUND: WHAT IS ETHNOGRAPHY?

Ethnography is one of those catchall words whose meaning is extremely general and vague. So varied is the interpretation that can be given to it, that one of the organisations who took up ethnography has claimed to have invented it – thereby ignoring decades of research in anthropology and sociology.[1]

Their arrogance may be excused by the fact that if they were to look at the sociological canon of ethnographic work (i.e., not ethnography under the guise of CSCW, of which something in a moment, but that done in the tradition of sociology and anthropology), they are unlikely to come away with an exact sense of how it is done. Worse, they may not even learn what is systematic about what "it" comes up with. Just to take what comes to mind from the long and rich history of ethnography, one finds Goffman's studies of how inmates in an asylum deal with their predicament through personal and social habits (Goffman, 1959, 1961); Wieder's examination of the ways in which ex-cons andstaff in a halfway house instruct each other as to what is and is not acceptable conduct (Wieder, 1974); Skolnick's classic study of the way police officers are compromised by tensions between "due process" and what they perceive as "moral justice" in their dealings with criminals (Skolnick, 1966); and Blau's report of the changes brought

about by new productivity measures in public welfare agencies (Blau, 1955). The list of contemporary studies could include (though nothing more than the mere fact that one recollects them easily is implied abut their quality): Lynch's studies of biochemistry labs (Lynch, 1982, 1985a, b, 1988); on the same theme, Latour and Woolgar's work (Latour and Woolgar, 1986; Latour, 1987, 1988); Anderson, Hughes and Sharrock's account of a small time fast food retailer (Anderson et al., 1989); Hockey's participant observation of the parochial world of Squaddies (Hockey, 1986); and this is to leave aside the anthropological literature.

Under the term ethnography one will find, then, great diversity of inquiries. Moreover, what makes so many of these studies so interesting to read is that the way they evoke the situation in question, their own reference to previous ethnographies, the interweaving of argument and description, and more, seem so diverse, broad and various as to defy formulation (see Atkinson, 1990; Hammersley and Atkinson, 1983; Harper, 1991a). Ethnographic tools, techniques and presentational formats would appear, therefore, something of a collage, a mish-mash of things. Consequently, it is difficult to know quite what to make of claims that ethnography can help in "requirements capture" or "domain specification" in system design (two amongst the various possibilities), for the simple fact that it is difficult to know what is meant by the term "ethnography".

There are some books that report on some of the mechanics of ethnography. These are at best rather simple, advising the use of tape recorders, daily note keeping, and recommending the sharing of draft descriptions with those studied.[2] More than anything else, they are encouragements for those solitary individuals, sociology and anthropology post-graduate students, who are obliged to commence their career with the injunction from their supervisor, "go do an ethnography". Most, if not all, will have had no training, no guidance, and certainly no experience in fieldwork. Consequently, they are full of doubt when they undertake their research, unsure of what to do at each stage, and unconvinced that they are finding anything interesting.

Some of the senior players in the ethnographic field offer another set of encouragements by writing frank exposes of the serendipitous nature of their own research. Van Maanen's charming *Tales From the Field* is of this kind (Van Maanen, 1988). But these books serve only to add to the problem. For what the novice discovers is that senior researchers got access to a setting purely by chance, had no systematic plans for exploring that setting, and found interesting materials by "bumping into them". What hope then for the post-graduate student, the novice, except trust in Fate?

Within current CSCW one will find no solace. Here one finds three basic strands of commentary on ethnography. One, a set of papers that simply use the term ethnography to label their study of some work place by close observation. The program of inquires that presumably structured this fieldwork are typically left unspecified. Each particular "CSCW ethnographer" apparently chooses this structure for him or herself, or more likely, doesn't impose one at all.

A second strand focuses on what one might call the analytic or theoretical programs that ethnographic data may be used to inform. This is certainly a vital and necessary component. It is not appropriate to talk about this here, since my argument is that this sensibility comes into play on the basis of a well worked program of inquiries. One reasonable complaint about these papers is however, that many are rather arcane (Button and Harper, 1996). Amongst the topics discussed is the nature of ethnographic reportage as Text – an obsession in current sociology.[3] Other concerns include such things as various sociological conceptions of "organisation" (see Jirotka et al., 1992); sociological interpretations of negotiation (Bannon and Schmidt, 1992), and sociological views of conversational interaction (Luff et al., 1990). In the *Limits of Ethnography*, Shapiro remarks that these debates have more to do with conflicts within sociology and anthropology, and very little to do with a purposive ethnography for CSCW (Shapiro, 1994). He recommends something of a hybrid form of ethnography, one, which is specific to CSCW. More recently, Norman has argued the same, albeit using the term, "Rapid Ethnography" (Norman, 1998, p. 195).

As to the third set, these remark on the apparent unwillingness of most ethnographers in the CSCW field to make any serious attempt to specify design choices (see Plowman et al., 1995).[4] The concerns of these papers are not with the actual work that gets done in the field, but the motivations behind that research. Their analysis of these motivations has provoked some resentment, but that analysis is, broadly speaking, correct: many of the ethnographers in CSCW have a tacit agenda that is opposed to technology in general and technologically-driven change in particular. Irrespective of whether ethnography as a general modality of fieldwork needs to carry these oppositions is a moot point (Anderson, 1997). In practice, this is what most ethnography in CSCW consists of, and complaints about it not generating the right materials for design are then largely accurate.

So in short, those who want to undertake ethnography for CSCW will find plenty of materials on the analytic concerns, whether it be to adopt an ethnomethodologically informed ethnographic sensibility or any other. But they will find much less on the structure of fieldwork programs. It is certain that what ethnographies that have been undertaken have had some kind of systematic properties. But the absence of any discussion of them may lead one to think, "anything goes", as Feyerabend so memorably put it in another context (Feyerabend, 1975).

Of course the key element of Feyerabend's argument was not that anything does, in fact, "do" in science. His point was that most second order discussions of method in the history of science (and related disciplines) miss the actual nature of methods by oversimplifying them, making them too rigid, and misunderstanding the relationship between method(s) and action. What needs to be recognised, he argued, was that methods are treated flexibly and pragmatically by those scientists whose business it is to use them. Feyerabend wrote long before CSCW came into existence, but if he were to have written about it one might suggest that he would have noted the opposite situation. If in studies of science there is an exaggera-

tion of method, in CSCW, there is virtually no discussion of method at all. Given his polemical bent, Feyerabend might have attempted to remedy this situation by saying that in "CSCW method is all!"

It is that method or program, as I prefer to call it, that is of concern to me. Before I say anything about that, I need to reiterate some basic assumptions that underscores any and all ethnography, irrespective of fieldwork program of analytic sensibility associated with it. It needs restating now because if an inquiry commences without these underpinning assumptions, then it is not ethnography. It is some other kind of fieldwork or observation technique.

1.2. BASIC ASSUMPTIONS ABOUT SOCIAL LIFE IN ETHNOGRAPHIC RESEARCH

Underscoring ethnography is one basic assumption: it is a method for understanding what activities mean to the people who do them. This sounds rather facile; in a sense it is something that we all do everyday, that is, we understand more or less what we are all about. But as a program of research it means setting out to understand the circumstances in which some set of activities occurs – the circumstances which give those activities meaning.

Let me illustrate. To understand what it means to go into a church is not to describe the "mere" mechanics of that event: the passing through a doorway, the observation of the darkness in the building, and so on. Rather, it is to recognise what a church means as a "holy institution". That means it is to understand the sense of awe and sense of the sublime that equates, in part at least, to the experience individuals will have on entering a church. The issue here is not to do with whether one believes in God, nor whether it is an empirical fact that individuals will feel a sense of the sublime when they enter a church. It is rather that if one is in the business of understanding what people do, then it is in the evoking of these kinds of matters that one should concentrate. One must not endeavour to create alternatives to that which is experienced.[5]

This begs a whole range of questions about how one can describe these matters adequately. There are many impenetrables here, and no number of formulae and rules of thumb will prove sufficient for all circumstances. Gilbert Ryle, a philosopher of unusually laconic humour, suggested that what we have here is the problem of "thick" description (Ryle, 1958a, b).[6] That is to say, a description should be rich enough and detailed enough to make some observed behaviour understandable. An example that Ryle makes use of has gained some fame within sociology through Clifford Geertz (Geertz, 1973, pp. 3–33). The example is of clowns performing at a circus. Ryle's view was that if we had a film of a clown doing somersaults, and nothing else (i.e., we knew nothing about circuses, about the history of clowns and so on), then the film would not tell us what we needed to know to make sense of what the clown was doing. It would not matter how often we were to view the film or from what angles the film was taken. Instead, if one wanted

to understand what clowns were about, then one would need to know something about how they are part and parcel of circuses, and how their somersaulting is viewed as a kind of sentimental self-mockery. In other words, one would need to know the circumstances in which clowns have their life, their meaning, their purpose and their point; these are the things that give their conduct the meaning it has. If one could describe all this (and thus do so much more than a mere film), then what one would have is a "thick" description.

These two examples, what it means to enter a church and what is meant when clowns perform, are good examples because they highlight the issues. Clowns are odd; churches do make many of us feel – how can one put it? – peculiar (even for those without faith). Unfortunately, ethnographers are often confronted with activities that are so mundane and so ordinary that it can become a little difficult for them to know what needs explaining – the events in question almost speak for themselves. All the ethnographer needs to do is to restate that. The difficulties of that notwithstanding, the point is clear: the enterprise is to describe the world as perceived by those within that world. As I said at the end of the last section, if that is not the goal, then though one may be undertaking fieldwork, it is not ethnographic.

2. Field work programs

So how does one gather sufficient materials to concoct a thick description? It is the program for this, the organisation of the fieldwork, and the absence of any discussion of it which motivates this paper. There can be no doubt that to be successful in the field requires a systematic, organised approach. This is particularly important when it comes to delivering materials for CSCW. For though it may be reasonable to argue that ethnographers who are in the straight sociology or anthropology trade don't need to bother with such organisation, and like so many others before, can just wait and see what they bump into (if anything at all), such an insouciant manner is inappropriate in CSCW.[7] For here the outcome of the research can lead to consequential matters – systems design, work process alteration and so on.[8]

The program of work is in many ways simple. It has three main components. The first of these has to do with following the life cycle of information (and its various modalities) through an organisation. This generates the basic materials for the ethnography, an overview of the organisational processes, and a basis for the determination of key sub-processes, in ways that I shall explain.

The second component has to do with going through "ritual inductions". I say ritual because these events do not, in themselves, necessarily guarantee that what the ethnographer understands is the truth of the matter, so much as that they symbolically display an experience that members of the setting believe everyone needs to go through if they are to "know what that workplace is about". In each setting what these rituals are varies, but they can normally be determined by evidence gained from the life cycle examination. Going through these rituals will

ensure that the materials generated by the ethnography get taken seriously or to put it another way, "count".

The third component has to do with the motivation behind interviews and related to that, the observations that are undertaken. By and large ethnographic interviews are reported to be semi-structured or informal. But a well-organised ethnographer with CSCW concerns has very particular purposes when undertaking interviews. These have to do with getting to the actual, practical organisation of the things people do as against such things as what they think the ethnographer might be interested in, what they ought to be doing, or even the things they would like to do. Observations are also important to see these actualities. Although it is often important to learn about what people would like to do in their work, the bottom line, in my view, is to get to the practical realities which confront individuals in organisational life. This is what motivates the CSCW ethnographer because it is knowledge of this that provides the raw materials for analysis.

I want to illustrate these different components with reference to a number of research programs I have been involved with: into air traffic control, hospitals, police work and the International Monetary Fund, or the Fund as it is more familiarly known. These are sufficiently different for the general applicability of the program to be demonstrated and to allow me to explicate fully the relevant details.

2.1. INFORMATION LIFE CYCLES

The term "life cycle" may sound as if it comes from the corporate world: seemingly pointing towards something, but at the same time vague and a little pretentious. Certainly corporations like Xerox like to use the term, but the meaning they give to it is quite unlike the meaning I give it. When I use the term, I am pointing towards the fact that information is marshalled, is worked up, reviewed, circulated, used, stored, and then forgotten about. Information within organisations has, if you like, a birth, a life, and a death. Further, during its life, information will exist in various modalities. The most obvious is when it gets presented in documents. But sometimes the information can be in a database, and at other times it can be latched to some artefact (i.e., information may exist in the form of marks on an object going through a production line). But whatever its form, one way an ethnographer can get around an organisation is by following the life cycle of the key information in that organisation.

This can be illustrated with the research program used at the Fund. Since I will be using examples from the Fund later on it is perhaps worth allocating a paragraph to saying something about this institution – all the more so since so little is known about it. Moreover, this will also allow me to introduce the broad form of the information life cycle within the organisation.

The Fund may be thought of as a financial "club" whose members consist of most of the countries of the world. Member countries contribute to a pool of resources which can then be used to provide low interest, multi-currency loans

should a member find itself facing balance of payments problems. The Fund has some 3,000 staff, of which 900 are professional economists. These economists analyse economic policies and developments – especially in the macroeconomic arena. They have particular interest in the circumstances surrounding the emergence of financial imbalances (including those that lead to a balance of payments crisis), the policies to overcome such imbalances, and the corrective policy criteria for making loans. Key to this work is undertaking "missions" to the country in question. A mission normally involves four or five economists, supported by administrative and secretarial staff, gathering and discussing macro-economic data with key officials in the authorities of the member country. This leads to the creation of a picture or view of that situation which in turn is used in policy discussions between the mission and the authorities. Member authorities are obliged to agree to Fund policy recommendations if Fund resources are to be made available to them. Missions normally last about two weeks. A mission team's view of a member country, the outcome of policy discussions and any recommendations for the disbursement of funds (or otherwise) are documented in "staff reports" prepared by the mission teams once they return to Washington. These reports are used by the organisation's Executive Board for its decision-making.

In terms of an information life cycle, it can be seen that the Fund's cycle involves a process where information is gathered about member countries in the mission process by desk officers and chiefs. It is then worked up by the same during and immediately after missions. It is documented in staff reports, and then is used by the Executive Board. Thereafter, the information (as embodied in staff reports) becomes archived and, crudely speaking, reaches the end of its life.

Before I say anything about how I organised my ethnographic fieldwork around this, I should make clear that I had to discover what this general life cycle looked like before I started the bulk of my fieldwork. I did so by first reading as much material as possible on the organisation, and second, by asking those to whom I had made my original presentation, what were the most important information processes, and what were the key modalities of that information.

On the basis of this, I was able to organise the bulk of my fieldwork in such a way as to ensure that I would look at the activities involved in the information life cycle from beginning to end. In the case of the Fund, this meant I interviewed desk officers and chiefs who gather the information prior to mission and who participate in missions and whose job it is to author staff reports. I interviewed also secretarial and research assistant staff who help in the composition of staff reports and participants in the review of those reports, ranging from the most junior economists to the Front Office chiefs and senior managers (including the deputy managing director). I interviewed Executive Board directors who use those reports (or more exactly, I interviewed their alternates); the clerical staff who issue and release staff reports once they have been "cleared"; those who copy and print staff reports; translators; and finally archivists. In all, 138 personnel, including 90 economists were interviewed.

The point of the program of work was to:

(1) map out the key processes of the organisation
(2) understand the diversities of work within the organisation
(3) understand how different sets of persons depend upon one another
(4) and determine what were viewed as the salient junctures in the (information) life cycle.

Now, the fact that the Fund is, in an obvious way, document-centred, might imply that it was relatively easy for the ethnographer (i.e. myself) to discover the key aspects of the information life cycle. For, in a sense, the life cycle of the staff report is a manifest proxy for – if not the very embodiment of – the Fund's information life cycle. So one might say that the program involved following a hypothetical staff report around the organisation. The task was accordingly to interview all those parties involved in the life cycle of staff reports. But in other places it might not be so easy to determine the information cycle. After all, some organisations are not so clearly document-centred as the Fund. But nearly all organisations can, in practice, be mapped out in this way.

2.1.1. *Other settings*

One can justify this assertion by remarking briefly on the use of the same technique in air traffic control settings and hospitals. As I say, these settings are in themselves quite distinct, and once added to the Fund make for a bundle of work places that are in many ways extremely different. But nonetheless, the key features of these organisations and hence the organisation of their study could be mapped out by use of the same device: following the information life cycle.

To explain: when I entered the operations room at London Air Traffic Control Centre, I had very little idea of the organisation of controlling work, and though I knew them to be important, little sense of how *flight progress strips* played an important part in the work at all. To be sure, I had a vague idea of what went on, but that was largely superficial and wanting. To map out the organisation of the work, I traced through the life cycle of the information that seemed to be central to their activities, as I understood them. This information related to flight progress data.

In the first instance, this information has its source in the flight plans prepared by pilots before they enter the airways. Pilots are obliged to send this information to the ATC authorities, and these authorities enter it in their own flight progress data computers. Here additional data are added, including weather information and radar tracking data. These are combined to generate estimated times of arrivals for aircraft at various key points in the airways. These in turn are used to print data on flight progress strips and, alongside "real-time" radar tracking data, are the key tools that the various individuals working at the control suites within the operations room use when they actually control aircraft.

Of most importance at the flight progress strip stage is how information on these strips is "worked up" in such a way as to ensure that what is presented is a highly accurate and cogent set of information about the dynamics of air movements. This information is only accurate for a specific and quite short period of time – usually about 5 to 10 minutes. This is, of course, the rub of the matter for the much celebrated ATC Project at Lancaster University.[9]

Once this intensive period of interest has passed, the strip gradually works its way on to another stage in its life cycle. In so doing, the modality of that information changes from being both paper-based (on the flight strips) and electronic (on the radar systems) into being solely electronic. For the information ultimately enters another giant database, this time to be used for the calculation of ATC charges for airlines.

I shall say more about the ATC setting in a moment, but before I do so I now want to remark on how a study of anaesthetists at an open-heart surgery hospital was organised. The purpose of the study was to determine what role electronic document systems could play in their work (Harper et al., 1996). Of importance was to determine the balance between those documents that would be best suited for paper media and those which would benefit from existing in digital, electronic form. To understand this, my colleagues and I organised the fieldwork to follow the key information used in that work. This information related to the patient and was embodied in what is called the Pre-operative Risk Assessment form or PRA.

Accordingly, we followed a hypothetical PRA around the organisation and observed what activities were undertaken at each stage in its life cycle. Anaesthetists start the information life cycle by going through a patient's file at the ward reception and using that to create a PRA. They then take this draft PRA to the patient's bedside and undertake a pre-operative interview. Notes from this are added to the PRA, which is then used in the anaesthetic room on the following day. The patient is brought here prior to the operation. As its name suggests, it is here that the anaesthetic is administered. We then observed what anaesthetists do in the operating theatre, and finally, traced how the PRAs were used post-operatively. As part of this process, we interviewed all those involved, namely, the ward sisters who prepared the patients' notes, the trainee anaesthetists, the registrars and consultants who constructed and used the PRAs, and the secretaries and administrators who used the PRAs post-operatively.

2.1.2. *Comment*

Now I do not want to say too much about the results of the research either at the hospital or in ATC. What I do want to do is make some further remarks on what I have hinted are the basic purposes of such programs of work. In particular, these are (1) to map out the key processes within an organisation and (2) to enable one to recognise the salient junctures in the processes. With this, one not only gains an overview, but one can also make effective comparisons.

Take ATC, for example. Here, I was able to use the information life cycle to compare civilian and military ATC. I was interested in what were the important junctures in the work, the key stages in each domain. As it happened, both had more or less identical overall processes (i.e. the same information life cycle), but what was crucial, what was important within each cycle (what was a "problem" if you will) was different in each.

In simple terms, in civilian ATC the important stage occurs when the controllers are using the strips. The activities here are complex and subtle, and this reflects the overall organisation of the airways and the relationship between pilots and controllers. In a phrase, because the civilian airspace system is flexible, the controlling is complex. In contrast, in military control the juncture in the cycle when controllers actually use strips is one that causes few problems. Controlling in military operations is, to put it bluntly, rather facile. Controllers rarely have any difficulties undertaking this part of their work; it is relaxed and unproblematic. This is because of the character of the relationship between the pilots and the controllers and the different flight procedures in operation. Military controllers have to bring military pilots together, and it is the pilots who solve all collision difficulties. However, military control does have its problems, but these occur at another juncture. This juncture is when the rostering of staff occurs. For in military Ops, there can be very little prediction of what the workload will be: air exercises will be undertaken at short notice reflecting such things as the weather and the need for the practice of emergency operations. As a result, it is difficult to determine how many staff should be on duty at any one time. Often military control finds it has staff idle; a few hours later it may have too few staff on duty. Their problem within the information life cycle is here: being able to predict what work will arrive and when.

These examples help demonstrate the claim that reference to the information life cycle can enable one to map out fundamental differences in the organisation of two (or more) work settings. Further, it enables one to determine matters that are relevant to any specific locus of work within that life cycle. If one wants to design systems to support work, one needs to bear in mind these factors, amongst others. For instance, an interface for the use of electronic flight progress strips in civilian ATC needs to be designed in recognition of the fact that the relationship between the controller and the pilots and the patterns of airspace procedure they are part of is extremely important. It is not solely a matter of an interface between an individual and database. Rather, that interface is located within a broader process, as has been extensively noted by Randall, Bannon and many others in the CSCW community.

Similarly with anaesthetists, observation of the life cycle of information in their work led to the discovery that although one might imagine that the main part of their work was undertaken in the operating theatre, in fact what was crucial was getting data on to the PRAs in the pre-operative interviews and from the ward rooms and being able to use that information in the anaesthetic room. Thereafter their work was merely to observe what happens to a patient, making notes, but

effectively taking a back seat. What was salient for an anaesthetist, then, was the information gathering stage, not the drama that unfolds when an incision is made into a patient's chest.

2.2. RITUAL INDUCTIONS

One of the problems ethnographers often have to deal with is how to be accepted by those studied. This is sometimes called the problem of *rapport*. Unfortunately the problems involved are all too frequently exaggerated and claims that the ethnographer has to take the injunction "go native" literally, for example, are best taken with a pinch of salt. But this is not to deny the fact that there are problems. From the CSCW point of view, the concern is not so much to ensure that the ethnographer is "one of them" as it is to ensure that his or her views are treated with respect by those observed. By respect is meant that the ethnographic analysis is treated as cogent, accurate, worth taking heed of, in short, is sufficient to base work process or systems design decisions on.[10] Here the issue is whether the ethnographer is entitled to have a view which those studies ought to accept, or whether that view will be rejected out of hand. The question is not just to do with how thorough an ethnographer has been in his or her program of work (how many individuals they have interviewed and the extent of the observation of the information life cycle, and so on). The issue is what activities an ethnographer will need to undertake for his or her views to be treated as from the "inside" rather than as from the "outside" of work. This is not an empirical question. This is a *moral* one.

Let me illustrate. During my ethnographic study at the Fund, many members of the organisation said to me "Look, to understand this place you have to go on a mission" and "Missions are what it is all about". Although I was always keen to go on one (as part of my fieldwork program), just why *having* to go on one, what it was about *being there* that would transform my outlook and identity, was unclear. Nonetheless, once it had been agreed I would go on one (and of course once I had been on one), the treatment I received from those I interviewed altered. Interviewees were much more forthcoming; people were more frank and offered me materials that hitherto had remained closed.

Why was this? One phrase comes to mind here. When I told one of the senior staff I was interviewing that I was about to go on a mission he suddenly paused and said, "Oh, well you are going to really understand this place then! You are doing it properly. Well, let's get down to business!" At first I was slightly insulted by this. Was he implying that if I had not arranged to go on a mission I would not have been undertaking a serious study? Once I had got over the pique, I realised what he was alluding to. He was pointing towards the fact that to understand the experience of life at the Fund one needs to understand what goes on on a mission. Although this can be described, there is no equal to being there. For it is in this way the observer can suffer along with everyone else on the mission. They too can see how hard everyone works, how tempers get frayed, how difficult it sometimes is to make the

figures work. Going on a mission is, if you like, the most important stage of the initiation ceremony transforming an outsider to an insider. In going on a mission knowledge would not necessarily be altered, but the moral identity of the observer would.

As it happens, my observation and experience of the mission did lead me to deepen my understanding of what many economists had called "facts of life" about policy work, and this, I hope, helped me enrich my analysis. More specifically, it helped me recognise the interactional processes through which their work had to be done, about which I have written extensively elsewhere (Harper, 1998, ch. 8). My concern here, however, is to draw attention to the general problem of how the ethnographer needs to attend to these moral transformations if he or she wants to be successful.

Sometimes these transformations do alter the materials that are made available (as at the Fund), and sometimes they do not. Further, what the transformation might be in any particular setting will vary. In some places these rituals are rather ordinary, everyday events. In other settings, they are considerably more dramatic.

To provide another example: if observing discussions between a Fund mission chief and a Minister of Finance is obviously a privileged "insider" thing to do, in other work places just turning up can be sufficient. Take ATC. The fact that I was willing to spend several weeks, indeed months, watching controllers at work was key to getting my analysis taken seriously by those controllers, their managers and even the controlling authority the UK's Civil Aviation Authority. If truth be known, I reached a threshold of understanding within weeks of starting my study, but further stay was needed to pay respect to the operations room staff. I also had to get my "hands dirty". In this case, this did not mean actually controlling aircraft but meant turning up when no one else wanted to: for the night shift on Saturdays. The fact that I turned up for several Saturdays in a row became proof that I "really wanted to know".[11]

In the case of the anaesthetists, the rituals were more difficult to specify. It was certainly not in the watching of an operation. (For one thing, in the hospital in question, there were so many operations that it was like a production line. An operation was thus hardly a special event. For another, there were too many strangers observing operations to make it a privilege to be there.) Rather, it came through recognising what one might loosely describe as industrial relations. One facet of this was the considerable bitterness felt by trainees about consultant anaesthetists. One did not have to take sides here since the consultants recognised and accepted the grounds for the bitterness (though most did little about it).[12] Another had to do with the animosity between anaesthetists and other clinical professions, particularly surgeons. By commenting on these topics and discussing them in our draft analyses, we came to be treated as viewing anaesthetic work from "within".

Other inductions take even more simple forms. Office life is obviously something that CSCW researchers look at a great deal. Much of office administration is uneventful and perhaps more importantly, one often has difficulty getting those

studied to talk about their work. “You don’t really want to know” they say. So how does the ethnographer get into an insider’s view? One way is to pick up on the industrial relations circumstances in the office in much the same way as I have mentioned we did in hospital life. But another and sometimes more effective way is simply to ask to do some of the work in question. This advice is now as commonly expressed in CSCW as it is in HCI. Its commonality reflects the fact that it is only when one does the work that one gets treated as genuinely interested in that work. It is then that those who ordinarily undertake it will start explaining what one needs to know to do it. From their perspective, their work is still boring and unremarkable, but “since you really want to know”, they will share the details of how it is done.

Sometimes the ethnographer simply has to wait. In police work, for example, which I haven’t mentioned so far but which I have studied extensively (see Harper, 1991b), the induction does not occur when the ethnographer turns up for night duty, nor when he or she helps break up a pub brawl. The transformation comes about when the ethnographer is taken to a *domestic dispute*. Such a dispute gets its name since it involves people co-habiting in some form or another (either as married couples or otherwise). The ethnographer will only be taken to such events once the police officers trust in the ethnographer’s ability not to make premature judgements about who is or is not at fault. These matters are nearly always very difficult to determine in “a domestic”, and it is often very divisive to try and ascertain. What the police officers need to do, and require the ethnographer to do, is to grieve with the victims over the damage they have done to their bodies and the mess they have made of their lives.

The point of these inductions, the ritual transformations of identity that they involve, is not that they are interesting unto themselves (though they often are), it is to ensure that what the ethnography delivers in terms of findings gets taken seriously. It is only once these inductions have been gone through that the ethnographic materials come to take a place at the table along side the more traditional views of authority on system and work process design. This is not to say anything about whether these findings will be properly understood or that they will jostle other views (say produced by BPR consultants) off the table. Getting the implications of the ethnographic research to have an effect requires further skills of exposition, persuasion and discussion that I cannot elaborate here, even if I could. Moreover, even with the best of will, the results of these efforts are not always what one expects. For example in ATC, our analysis was used not to reinforce the need to design better interfaces for the ATC systems, ones that might incorporate paper as well as electronic displays, but rather to justify the unusual shift system in LATCC, which resulted in large numbers of controlling staff working side by side. In the police, it led senior police managers to accept that “outsiders” were capable of comprehending their work processes. Before the research, there had been an assumption that only police themselves could specify what they did or needed in terms of systems. In the Fund, it lead to confirmation that the selection, design and impact of new technologies needed to be understood in terms of culture and

"organisational politics" as well as what were called "workprocess" requirements (related to such things as the process for selecting times-series applications, for example).

2.3. UNDERTAKING INTERVIEWS AND OBSERVING WORK

The last component of the program relates to the motivations behind the interviews and observation of work. As I say, ethnographic interviews are typically described as informal and open-ended. One mistake is to think that anything at all is of interest to the interviewer. As Anderson notes, for the sociological and anthropological ethnographer there certainly is a baggage of concerns that they carry with them and this lies implicit in the questions they ask (Anderson, 1994). In CSCW, this baggage retains its importance since it consists of that ability to bring to bear the appropriate analytic sensibility mentioned earlier on. But underscoring this sensibility is something more fundamental, a seeking for certain empirical details that need to be made available before this sensibility can be applied.

To explain: one of the most eloquent exponents of ethnography, the aforementioned Clifford Geertz, commenced his "Interpretation of Cultures" with the following definition of sociological and anthropological inquiry (Geertz, 1973). "*Believing, with Max Weber, that man is an animal suspended in webs of significance he himself has spun, I take culture to be those webs, and the analysis of it to be therefore not an experimental science in search of law but an interpretative one in search of meaning*" (1973, p. 5). The subsequent essays in that book deal with sacred symbols, death by Suttee, and perhaps most famously, Balinese cockfighting. These are aspects of human action that it seems quite reasonable to claim can only be analysed through reference to such things as religious systems and moral conduct, patterns of social status and more. Quite obviously what Geertz describes is not a world of silent orbs and eddying bodies (i.e. one without meaning), but a world of complex symbols, of religious faith, arcane rituals, and, moreover, to Western eyes at least, peculiar characters. In other settings, nearer to home than the Far East or the Atlas Mountains, it may be claimed that there are no exotic ways of seeing, no shocking and inexplicable rituals like Suttee. Therefore, what is the level of meaning one is after? What would a thick description look like here? What would such a description be of?

Consider office work. Does one need to delve into the inner recesses of an office worker's mind to understand what he or she is about? Does one have to link their working routines to deeply held beliefs about the relationship between, say, the stars above and the Managing Director below? Perhaps in some cases, but not as a whole. Similarly, at the Fund. Are there complex meanings to the things that staff of this organisation do? Are the Fund's staff like clowns to a Martian, living in a world whose meanings are nigh on inexplicable?

No. Office work in the Fund as in other organisations is much more mundane than the things that Geertz writes about in his studies of Indonesia and elsewhere.

But consequently it becomes difficult to see the parallels between the stuff that he offers as *thick description* and the stuff one might seek in interviews of say, the Fund's staff. So what, then, was I after when I interviewed the economists, the chiefs, the staff assistants, and the administrators?

Let me answer this question at first indirectly. Let me describe some of the things that I wasn't interested in these interviews, or rather treated as interesting background information.

At the beginning of each day, there are gatherings of senior economists at the Fund, often but not always divided according to area department, in the canteen. These "breakfast clubs", as they are known, are informal forums for the exchange of gossip and intrigue, and for the sharing and debating of current Fund "views". But membership of them is also a symbol of status: newcomers rarely venture into them; individuals can only join when invited. Those who do not participate, outsiders as it were, talk about these clubs as the cliques that "really control the Fund". It is also believed that this control has to some extent abated in recent years as the Fund has increased in size, reducing the knowledge individual members have of the organisation as a whole. Prior to these changes, when every one knew everyone else, "in the old days", these clubs are said to have been an important mechanism whereby information about candidates for promotion was exchanged. Here, careers were made or broken.

Not only are there these clubs in the Fund,[13] but there are broader divides that are often spoken about as setting one part of the institution against another. Here I am thinking of how the area and functional departments are said to often engage in battles with one another over budgets, staffing, the review process, access to data over the computer network, and more. When they are not busy fighting these internal battles, they are often at loggerheads with the executive staff, the Managing Director and his Deputies. All these disputes are often characterised in medieval terms: the area and functional departments are fiefdoms, their directors being medieval barons, and the chiefs feudal lords. The rights and privileges of these fiefdoms are fiercely defended against the intrusions of one another and particularly the advances of the centralising monarch, the Managing Director, and his coterie of staff in the Managing Director's office and its sheriffs, the Deputy Managing Directors, or the DMDs.[14]

As it happens, breakfast clubs, and the conflict between the Barons in the area departments is very much the kind of thing those I interviewed would happily talk about. For these are the kinds of things, the kinds of stuff of which gossip and work chat are made. And, to be sure there is some truth in these conversations. But the concern that drove my interviews was focused on something else, something more detailed, and more mundane, if you like. What I was after, and indeed what an organisational ethnography in CSCW is after, are the details of the work that individuals do, the particularities through which the various processes that need to be done get undertaken.

The purpose here is not to elicit "juicy examples" (though sometimes these can be very useful in illustrating the character of the work).[15] The question is to get to the organisation of the work at a level that reflects the practical concerns of the individuals who undertake it. It is the practicalities that are at issue here, not the gloss, as sociologists sometimes put it.

Unfortunately it is not always easy to get those interviewed to talk about these details. Often interviewees don't think that it is these kinds of details that interests the ethnographer. As I remarked a moment ago, some individuals cannot believe that the ethnographer really wants to know. Sometimes one has to go through certain routines or tricks, if you like, to get the interviewees to believe in that interest. But more often it is little more than directing the interview.

This is well illustrated in the work of Eleanor Wynn. The example I take from one of the papers she co-authored with L. Suchman and presented below is, I think, one of the best illustrations of how to get interviewees to talk about the matters of concern (Suchman and Wynn, 1984). The example consists of a transcript of an interview Wynn undertook. I will break up the transcript to highlight some issues. Here is the first section, commencing with the claim by the "user" that there is not much to her work:

> **E (interviewer)**: Did you describe your job to me already? Have we gotten that far?
> **G (User)**: I'm in the Collections department. I'm the Lead Collector. You've met Jerry and Christine? OK, they're in my group.
> There's not much to say about it. ... We just collect past-due balances, and we do a lot of things at – you do a lot of customer care, you do a lot of check refunds, and you're cleaning their accounts, and a lot of it is not even delinquent accounts, but *problem* accounts, they have the money ... and it's just not the fact that you convince the customer that they *owe* the money, number one, which, you know, is a big step right there, but then to get it through their system, you have to understand what they have to do to get a check cut, and you have to understand their system a little bit. You have to be able to ... know how to set a fire under somebody to get that moving, if it needs to be prioritized. And that, at times – people don't understand how hard that can be.

The key to the interview is to pursue the pointers that are nearly always made in the first few remarks. Wynn shows us what to do:

> **E**: What is involved in that, "setting a fire under somebody"?
> **G**: Well, see, I have to – you begin to immediately move it upward, up through the management, okay? You start with a clerk, okay? They definitely cannot solve the problem. It's out of their hands. Okay, you ask whose hands is it in? Who can make the decision – the buyer.
> Okay, you call the buyer, the buyer says, "Um, the reason why I'm not paying this is, I said I would pay *twenty* dollars and seventy-*three* cents for a carton, not twenty-*four* dollars and seventy-*two* cents, which you bill me on this five

> thousand dollar shipment of paper." So then you say, "That's all I need to know, let me get back with you." You get back, you go through your billing system, you try to find out, you know.

She then goes on to describe a process that involves a number of complex stages, negotiations, more telephone calls. There is no need to report it all here. This complex, one might also be tempted to suggest long-winded procedure, may appear to be just the kind of thing that one would want to tidy up with technology. That would miss the point I am wanting to make, however. I am not interested in this particular process, so much as wanting to illustrate the level of detail that it is necessary to uncover in an interview. Perhaps this process should have been automated in some way; I don't know. As it happens Wynn and Suchman are rather quiet on this point too. What I am drawing attention to is that the ethnographer needs to investigate all the oddities, the strange cul-de-sacs, the apparently arbitrary social processes and ritualised organisational protocols, that embody the work in question. They need to understand what it means to "light a fire", as the subject described her work.

The importance of this cannot be underestimated, so let me reiterate with a brief discussion of it in relation to the Fund. Amongst the ethnographic tasks I undertook, was interviewing the Fund's desk officers. As I have remarked, desk officers are responsible for maintaining information about a particular country. But what does this mean? What do they do on a day to day basis? During interviews, I sought what they found it *economic* to do rather than what they thought it might be ideal to do. I was not motivated by a concern to ironicise their work, to find evidence to show that the desk officers were failing in their task. I was primarily interested in prising open what it is they did in practice; the mundane organisation of their work if you like. What I found was that for many desk officers, the collection and management of information was undertaken on a demand-driven basis. Their data management tasks tended to take a lower priority than the demand driven tasks. In this sense their work consisted of a matrix of competing concerns, prioritised on a day by day, and task by task basis.[16]

This level of analysis, or, getting to the mundane details as some sociologists like to describe it (namely, ethnomethodologists), should be the basic fabric of material that is gathered. Although from the first moment an interview is undertaken, members of an organisation may well volunteer information about a whole range of issues, including what they believe is their organisational culture, the ethnographer should put their remarks on hold until they have understood the essential details of the work. It is only in this way that they can ensure the empirical adequacy of their analysis. It is in reference to these that interactions between systems and users can be mapped out. This does not limit what the ethnographer can create in the deriving analysis of work practice. For my case is that one should start with this level, since this provides an empirical foundation. One can then add to that foundation a whole host of elaborations that may, in one way or another, move on from and develop matters first pointed out in the details.

Interviews are only a part of the techniques used to learn about the work in question. Another is to observe the work, and even to undertake it if possible. *Participant observation* is sometimes treated as the *sine qua non* of good ethnography. This is arrant nonsense. In some settings it is certainly easy to observe, but down right impossible to participate. In ATC one can while away hours watching the work, but it would be extremely unwise to actually undertake that work (even if one were allowed to). One needs to observe, and to supplement sometimes that with participation, if it is appropriate. It is certainly not essential.

Even so, in some work places it is difficult to know what to observe. In police organisations, for example, certain of the middle managers (namely inspectors) have quite literally nothing to do: they cannot patrol the streets since they are too senior, but the organisation does not require them to do any administration. They find themselves in the symbolic middle: neither Bobbies fighting crime nor senior staff determining strategy. How does one observe that in useful ways?

But other activities, which might seem intangible, are especially good to observe. Meetings are a case in point. At the Fund, meetings during which a mission team prepared a briefing paper were particularly informative, as were meetings between the member authorities and the team itself. As regards the latter, here there were intricate rituals of negotiation and discussion, elaborate patterns of deference and status that one expects to find in all high level meetings (though one cannot predict beforehand what these forms will take). Meetings between the mission members both before and during the mission were worth observing because it was during these that the key concerns of the team were discussed, worked over, and refined. It was here that the ambiguities were clarified, and the rationale of the mission reiterated.

Observation of these meetings was also of vital importance in understanding the context and purposes behind activities that specifically involved technology. The use of laptops and networked PCs to support spreadsheet and time series work is a case in point. For these "data processing" tasks (as they are sometimes called by the Fund's staff) are actually the outcome of and are intimately related to social processes of agreement and discussion. This has implications for how to design spreadsheets and time series applications that more effectively supports discussion, mutual sharing and annotation, all of some interest to the CSCW community and office information systems researchers more generally (Harper, 1998).

3. Conclusion: Ethnography within a process

My last concern in this paper is to explain what implications my arguments will have for the location of ethnographic inquiries within a larger process. One paper that attempts to list the various roles ethnography can have in this larger process is Hughes et al.'s *Moving out of the Control Room: Ethnography in Systems Design* (Hughes et al., 1994). In this paper, the suggestion is made that there are at least four ways in which ethnography can connect to design. I will explain how the

program I have outlined is related to these four ways insofar as it brings a different slant to the argument and justification that Hughes et al. provide. It will also enable me to reiterate the benefits of deploying a program can have.

The first role Hughes defines involves undertaking an ethnography concurrent with design. This was the process used in the Lancaster ATC project. The ethnographer would visit the work site and then report back findings to the design team. The team would then develop new, more detailed questions and the ethnographer would return to the site to find answers (Hughes et al., 1993). Hughes' account of this role is something of a gloss, however, because what they are talking about was a research project that is undertaken only in reference to itself. For the project they describe did not lead to the implementation or design of new systems in the setting itself, but only some implementations and prototype ideas shared, developed and published in the CSCW community. This does not in any way invalidate that research. For what I have wanted to argue is that one way of ensuring that such research does more than merely add to the current stock of knowledge (i.e., being purely academic) and for it to have material implications for the organisation studied, the ethnography needs to be organised in the way I have described. Thereby the research outcomes, whether they be ethnographic descriptions or specific technological recommendations, are much more likely to be taken seriously by the organisation itself. Those who are studied will accept the views as valid, those who make decisions about technology and processes will accept those views as equal to those produced by the more traditional methods, such as business process engineering techniques. It is important to remember that this may not make any difference to the output of the ethnographic research or systems design recommendations, only in its "acceptability".

The second process Hughes describes is one where the ethnography is undertaken quickly and roughly. Hughes and colleagues use a pejorative term here to distinguish between the "proper length" that they suggest is required for ethnographic studies and the short time periods sometimes available in design processes. I think the problem may well be exaggerated, since a well-organised ethnography can uncover a great deal in a very short time, depending upon the setting. Their own comments derive from a study that involved only a few hours of observation and the quality of that work seems to deride their own complaint about it being "quick and dirty" (see Rouncefield et al., 1994).

In any case and more saliently I think, the issue that is brought to mind here has to do with ritual inductions. For it seems to me that Hughes is suggesting that enough time was not spent in the setting not because of what members of that setting themselves felt or demanded, or even what the research team itself believed. Instead it appears that Hughes is worried about the reaction that mainstream anthropologists and sociologists might have to the time periods in question. For these communities do indeed have a their own notion of what is sufficient time to "properly understand" a setting. I suggest that these notions are as ritual and arbitrary as the notions that, say, air traffic controllers have about what is the time

necessary to understand their jobs – i.e. they are not necessarily about how long it takes for a fieldworker to get to grips with the setting. They have to do with how long it takes an outsider to become an insider. In anthropology, a fieldworker is an outsider to the anthropological community until he or she has been on an extended trip abroad – a trip that has to be minimally months, perhaps even years. Only then will they be viewed as having passed the "professional apprenticeship". Prior to that they are novices or worse dilettantes. In other words what is at issue here is a moral tansformation, not one based on empirical understanding. Given that Hughes is writing about ethnography in CSCW, then I suggest it would more appropriate that any judgements about the length of time an ethnography takes is made on the basis of what those who are studied feel is appropriate as well as the research team's own assessments of their empirical understanding, rather than on the basis of the views of the orthodox practitioners of ethnography.

The third process that ethnography can be part of is in evaluating systems. Hughes explains that ethnography can be particularly effective here, since it can deal with issues and complexities that impinge on system use that more formal evaluation techniques simply cannot handle (Randall et al., 1996). I agree with this but suggest that the program I have outlined here will help ensure that such complexities are more effectively understood. For one thing, following the information life cycle will enable the ethnographic research team to recognise the respective role of various tasks within the entire organisational context. Situating systems in this way is often of crucial importance in evaluation. For another, it will make sure that they get sufficiently rich detail. For a third, it will ensure that their findings are taken seriously.

According to Hughes, the fourth process involves using ethnography to re-examine previous studies of the workplace in question (Hughes et al., 1993b). As with the third process, the suggestion that the fieldwork be structured around the information life cycle is, I think, of vital importance. This is because it can enable the ethnographic researchers to reassess the relative importance of some particular task with the passing of time. For example, if, in the early 1980s, the flexibility of civilian ATC lead the interface with the systems to be complex, then by the 1990s, flow control may have reduced that flexibility. Only by looking at the whole process, the life of information relating to air flights from beginning to end, would that be made clear.

The four different processes in which ethnography can play a part in CSCW are not likely to be the only ones, needless to say. I do not think it appropriate to remark on what other processes might be, however, since my main concern in this paper has been to outline what a program suitable for organisational ethnography in CSCW needs to involve, and this latter irrespective of the part it plays in a broader scheme of activities.[17] Nor have I commented on which techniques of presenting ethnographic findings are most appropriate, since this is, in part, determined by the larger scheme of action. My purpose has been to outline how the materials used in such activities, what one might loosely call *ethnographic findings*, may be

gathered. My hope is that on the basis of what I have presented, others will be persuaded to undertake ethnographic inquiries on their own and will find useful guidance when they find themselves asking "Well, what do I do now? How do I organise this ethnography to make sure I get to the organisation in question, and get listened to when I finish?" What I have outlined will certainly not guarantee this. But it will help.

Notes

1. Though for obvious reasons I cannot identify this organisation.
2. See for instance Hammersley and Atkinson (1983). My personal favorite, though it is getting old, is Lofland (1971).
3. The book that is most often cited in relation to this debate is Clifford and Marcus (1986). A more recent collection is presented by Van Maanen (1995).
4. For a response, see Pycock and Bowers (1996) and Grudin and Grinter (1995).
5. This example is taken from Wittgenstein (1993).
6. The ease with which one can slip into confusion when thinking about meaning and action is also examined in his more famous *The Concept of the Mind* (Ryle 1963).
7. Though this particular policy can sometimes pay dividends even in CSCW – my point is that one cannot rely on it. Some commentators think that it is the essential characteristic of ethnography to come up with surprises. See, for example, Nardi (1993, pp. 6–9).
8. Of course, those in the straight sociology and anthropology trades, as I have put it, think that their research is consequential too. It is, but my point is that by and large those consequences are not material to those studied.
9. It is perhaps worth noting that this research followed on from one I had been involved with previously. This previous research had mapped out the processes of ATC work as a whole. The later project focused in more detail on the interface and required the ethnographer, Dave Randall, to make closer observation of controlling work at the radar screens. Key research publications on this first project are Hughes et al. (1988) and Harper et al. (1989a, b, in press). As regards the second project, a number of the papers dicussing ethnography have already been cited. But see also Shapiro et al. (1991).
10. Gaining the respect of those studied can be very difficult when an ethnographer has been "tainted by technology". See Harper (1996).
11. It also led me to discover that UFOs are rather regular occurrences in the sky at night. This had nothing to do with the research but was very interesting. To describe the events: all the controllers would see, if see is the right word, is what the pilots described, for the radar processing systems systematically removed all UFOs from the radar screens on the grounds that an unidentified flying object is of no interest to a controller. Sometimes these UFOs would cause a great deal of commotion with pilots shouting at each other and calling excitedly at the wonders in the night sky. The controllers would sit around the radars and listen to the radio talk, gazing at the purple blips on the screens that told them nothing of what was really going on.
12. There is no news in this. Many studies of hospital life have noted the same. But to illustrate from our own research: Trainees often described the consultants as "braindead" and the consultants described the trainees as "cheap labour for the dirty work".
13. Those who are interested in these sorts of informal communities in organisations see the now aged but nonetheless fascinating Dalton (1950).
14. This view would appear to be current in many organisations. See Davenport et al. (1992).
15. Such examples can be found in Randall et al. (1995).

16. For a more general discussion of this, see Gasser (1986).
17. For discussion, see Shapiro (1994).

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