Representations and Requirements:
the value of ethnography in system design

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Abstract

For a number of reasons, systems designers have recently shown considerable interest in ethnography. Principally this has been as a method for the specification of end-user requirements for systems. This paper argues that most of this interest is predicated in a misunderstanding of ethnography's role in social science. Instead of fixing upon its analytic aspects, designers have defined it as a form of data collection. They have done this for very good, design-relevant reasons. But designers do not need ethnography to do what they wish to do. In the central part of the paper, an approach to analytic ethnography in HCI is set out and illustrated. The latter sections take this approach and show how it opens up "the play of possibilities" for design. These are illustrated by counterposing a summary logic of organisational structure such as that associated with the calculus of efficiency and productivity with the "local logics" of daily organisational life.

Key words:

Ethnography, design, requirements capture, qualitative methods in design.

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1. INTRODUCTION.

Discussion of the contribution which ethnography (van Maanen, 1988) might make to the design of complex technological systems has largely moved at two intricately related levels: the substantive and the methodological (Button & King, 1992, Harper, 1992, Hughes and King, 1992, Shapiro, 1992 and other discussions at CSCW 1992).[1] The issues addressed are, first, the specific kinds of information ethnography makes available to system design and the value it might be; and, second, the stratagems which ethnographers employ to access that information and how they might be deployed by and on behalf of system designers. Important though these questions are, and much though there is left to say about them, I feel they are in fact inadequate characterisations of what is at stake here. This feeling is reinforced by the frequency with which discussion degenerates into a friendly but unfocused debate over what I think of as the notational and normative distractions. The former refers to the presumption that to be of value to designers, any description must be couched in a formalised or semi-formalised notation of some kind: as if design consisted in jig-saw puzzle solving and only certain shaped pieces were allowed. The latter is the age old (and tired) prescription vs. description debate, with the ethnographers staunchly appearing to refuse to be prescriptive in the face of designers' demands for requirement specification. Here what seems to be being missed is the extent to which design involves *sensibilities* as much as models and predictions, programmes and prescriptions. These sensibilities define the boundaries of the design problem and possible approaches to it. Sensibilities, then, provide what Schon (1988) calls 'the world for design'. One of the things which the discussion of ethnography might be instrumental in starting, perhaps, is an examination of the dogma of formalism and the proper scope of design sensibilities.

To me, the fact that the debates so often do end up pursuing these red herrings indicates that there are deeper, more substantial matters to be resolved here than the theoretically specified substantive and methodological questions. Such issues are, if you will, *aprioria* for the theoretical. The often implicit choices made over them are expressed in the theoretical and methodological decisions which are then made.

In this paper, I try to draw out and deepen the discussion over ethnography in and for design by focusing on two different types of pre-theoretical choices and tracing their implications. On the way, hopefully, it will become apparent why they are so often condensed into the notational and normative distractions. The first is a choice over what I will call *genres of reportage* and is discerned in ethnography's analytic aspirations. This discussion occupies Sections Two and Three of the paper. The second choice is over *the play of design possibilities* and is found in the practical recommendations (or lack of them) which

ethnographers are often accused of making. Section four is devoted to this. A third choice which has to do with *degrees of engagement*, will only be touched on here and there in this discussion. A fuller treatment of it awaits another occasion. The failure to appreciate the choices over genres of reportage has led many within the CSCW and HCI community to misconstrue the character of ethnography. Presumptions about the investigative framework for design, that is, the conventional assumptions about how design should view the world, have led to the confusion over prescriptivism. Failure to explicate the choices over engagement have, in turn, led to what John Hughes and Val King (1992) call 'the promissory' character of much ethnographic (and indeed social scientific) work in this domain.

My contention is that it is here, to these choices, that discussion of ethnography in design should be directed. Once we gain an appreciation of their import, it will be possible to lay out their dimensions and explore some ways for making them more tractable. At that point, we will see that requirements and representations might just be different views on the same hologram.

2. GENRES OF REPORTAGE

The turn to ethnography as a research strategy in HCI and especially in CSCW seems to be motivated by one major issue: a disenchantment with conventional forms of 'requirements capture' for the design of end-user systems (Luff et al, 1992). This disenchantment is expressed in many ways, usually as the search for a 'design methodology' which gives equivalent value to the orientations, expectations and understandings of the (end-)user of the system as to those of the software engineer or developer. The justification for this is, roughly, that the working knowledge of the context of use which the user has is at least as vital to the eventual success (or failure) of any system as the technical knowledge of the system designer. I use the term 'working knowledge' here as a deliberately large portmanteau within which to stow the differing perspectives and emphases found within research into user-centred design, participatory design, socio-technical systems design, end-user customisation, as well as qualitative methods of requirements capture (Norman and Draper, 1986, Ehn, 1992, Mumford, 1992, Mackay, 1990, Luff et al., 1992, Woolgar, 1992). What all of these approaches share is a determination to achieve the re-balancing of the role of user knowledge and a concern to complement formal models, methods and representations with relatively informal ones. What the user is held to know about and to orient to in the daily routine of their workaday world is the practical management of organisational contingencies, the taken for granted shared culture of the working environment, the hurly burly of social relations in the workplace, and the locally specific skills (the 'know-how',' know-what') required to perform any role or task. Formal methods

of requirements capture, or so it is supposed, are incapable of rendering these dimensions visible, let alone capturing them in the detail required to ensure that systems can take advantage of them. On the view thus espoused, ethnography is at least *a* method which will provide access to these dimensions.

But is *ethnography* as practised in the social sciences what designers with the motivations just described really want? To get access to the things which they think they need, do designers have to engage in ethnographic research? My contention is, simply, that failing to see the background upon which it draws has led to some profound misconceptions about ethnography and its applicability in design.

To get at the origin of the misconceptions, I want to introduce two simple but very important distinctions: that between data collection and reportage; and that between impressionistic and analytic forms of the latter. Here is the first major point. Within the social sciences, ethnography is a form of reportage and not a form of data collection. This might seem a trivial point to make, but it is not. Once one is aware of it, all the emphasis is thrown onto understanding the processes for patterning observations and their interrelationships rather than the methods for recording and summary. The ethnographer's eye is always *interpretive*.

Second, these interpretations are rooted in the exploration of *conceptual* or *analytic specifications*. Data, observations, stories are important only as means to working through and working out the analytic possibilities currently in view. In this sense, the supposition that ethnography conveys an overall impression of 'what life is like' or to 'tells it as it is' is profoundly mistaken.

Both distinctions are vital, not just for my argument but for the conclusions which can be drawn from it. As I will try to show, designers might well work closely with users, engage in fieldwork among the end-user organisations for whom they are designing, focus on the intersection of the technological, the organisational and the social dimensions of the working environments within which their designed systems will find a place, all without ever engaging in the kind of *analytic ethnography* (hereafter just ethnography) to be found in the social sciences. In fact, doing ethnography might prove a barrier to achieving the goals which designers want to set themselves.

On the other hand, and this is the second part of my argument, if we set our ambitions slightly differently (and perhaps slightly higher), it could be that ethnography *as I define it* will prove to have a major contribution to offer *research* into innovative systems of all kinds. This research may then be used to inform design decisions. What we will be asking of ethnography is not that it should be a way of getting to know and articulating 'the user's

point of view' or whatever, but that the analyses it offers us should be directly germane to the interests and issues which confront designers. To repeat: this is not to say that getting to know users and their knowledge and practices is unnecessary or irrelevant, or that observational fieldwork and impressionistic reportage can be of no value in this. Far from it! It is simply that you don't need ethnography to do that; just minimal competency in interactive skills, a willingness to spend time, and a fair amount of patience.

2.1 Some reasons for the misconceptions

There are several very straightforward reasons why those unfamiliar with ethnography might have come to hold the misconceptions just outlined.

The importance of style

Ethnographers tend to write in a conversational voice and trade in the exotic. To the unwary, it can appear as if style is all and that stories of strange ways of life and doings will always carry interest, depth and insight. But this is not so (Harper, D., 1992, Harper, R., 1992). We should not be beguiled by stories from the field and need be no less discerning about ethnographies than we would any other form of analysis. Most importantly, we should always seek to be sure that what is on offer in the account given is what we could not otherwise have had.

The mis-homogenisation of fieldwork

While most ethnographers are fieldworkers in some sense, not all fieldwork is ethnographic in orientation. Going into the field does not necessarily mean doing ethnography, nor should it. Related to this is the fact that not every investigative problem is amenable to ethnographic treatment. Indeed, some of the lapses in quality hinted at just now derive from the attempt to use ethnographic techniques (again I will be more specific later) on problems which are wholly unsuited to them. I often think it would help if ethnographers (or at least those who professionally claim this title) were slightly less reluctant to admit both these points, and thus less generous with their praise.

Social skills as a professional ideology

The lack of self-criticism is understandable though. First, successful ethnographers are almost invariably *nice* people. They like to get on with others, prefer smooth relationships to abrasive ones, tend to play down differences and seek to take the other's point of view. These traits are part of their stock-in-trade, what makes their fieldwork possible. Second, they often feel their approach is under attack by fundamentalism of the left and right. The attacks from the right are couched, of course, in terms of representativeness and generalisation. Can one say anything valid or of general value on the basis of single (albeit

detailed) cases? The defence of ethnography against this and other attacks is set out in Hammersley and Atkinson (1983). From the left, come the issues commitment and intervention. Since, at least traditionally, the ethnographer has generally been in a position of relative power, authority, status and affluence relative to those who are studied (or, indeed, often actively sponsored by those in such positions) what role can they play in the dynamics of relationships between the underclasses and those that dominate them? 'Whose side are We on?' in Howard Becker's famous phrase (Becker, 1970)

And then there are the ethnomethodologists and Critical Theorists, sounding for all the world like methodological anarchists and asking about the occasionality of fieldwork and deconstructing the reflexive grounding of ethnographic communication itself.[2]

Family resemblances are only skin deep

One important aspect of the misunderstanding of ethnography is the tendency of HCI practitioners to identify it and its approach with all of the rest of social science. The various factions and schools within the social sciences are not all of a piece, except at a very superficial level. Moreover, for them, the differences make a difference. This is especially so for ethnomethodologists who have deep and theoretically inspired reasons for wishing to resist being co-classified with ethnography *tout court*.

Ethnomethodology is a particular form of sociological analysis (Garfinkel, 1967, Sharrock and Anderson, 1986) which is premissed in the phenomenology of Alfred Schutz (Schutz, 1967). In contrast to other modes of sociology, it seeks to describe social institutions and structures from within the midst of their daily operation rather from an external or overarching point of view. For the ethnomethodologist, description from within means identifying and elaborating the methods by which ordinary members of society construct and reproduce the stable structures we all recognise as characteristic of social life. This is what they call society's "lived-work". Considerable effort has been expended describing the formal structures which such "practical action" might take (Garfinkel and Sacks, 1970). Sacks (1992) has extensively analysed ordinary conversation, for instance, while Lynch as done the same for science (Lynch, 1988). Such analyses differ markedly from those provided by the more usual ethnographically inspired approaches to talk (Bauman and Sherzer, 1974) and laboratory life (Latour and Woolgar, 1979) where naturalistic description predominates.

Ethnomethodology was first introduced to the HCI and related worlds by Lucy Suchman (Suchman 1987) who does indeed practice both fieldwork and ethnography. But she, like other ethnomethodologists, has her eyes on very specific, theoretically defined and philosophically informed goals outlined above. Ethnography *per se*, however, is not so

narrowly defined and can be used (indeed, has been used) by almost every perspective in the social and anthropological sciences.

Hanging around is not the point

Although ethnographers (even the best ones) are prone to talk of what they do as 'just hanging around' and 'seeing what goes on', this is in reality a form of professional understatement. It is a deliberately designed insouciance which hides the effort required first to get into the field and stay there, and second extract oneself from the field and say something coherent and comprehensible about what went on. To view it in ethnographic terms, you could say it is a form of 'role distance' (Goffman, 1971) and probably has much the same professional function as heart surgeon's talk of what they do as 'plumbing' or 747 pilots' talk of what they do as 'driving'.

The trouble with all this is that it encourages the image of the ethnographic fieldworker as 'transducer' - an open, unmediating channel of information flow from the setting, onto the notebook and into the report. The result is that many impressionistic ethnographies are little more than typed up, spell-checked, anonymised fieldnotes. That this image may be justified by a wish not to be caught 'the wrong side' of the apparent contradistinction between 'objectivity' and 'realism' and a concomitant concern for the 'preservation of naturalness' makes it all the worse since it seriously underestimates what consideration of those issues might actually involve (Bitner, 1973).

Furthermore, it also hides the complex of social, practical and personal skills required to take and sustain a role in the field, whether this involves 'passing' as an insider or maintaining the role of 'the stranger'. Just as not everyone can be a Nigel Mansell or a Madonna, not everyone can be an ethnographer (nor should they want to).

The ethnographic take on the world

It is important to remember that to the extent that ethnography involves a looking (and I will get to that in a moment), it is a *motivated* looking. That is, the local scene, be it a bazaar in Morocco, a station house in Los Angeles, or the Air Traffic Control Centre at West Drayton, is *seen and surveyed* as a constellation of organised activities. It is the patterns and the patterning which the ethnographer is looking for and not simply a realistic, behaviouralised description or natural history. What the ethnographer is most interested in, and thus what makes some ethnography of particular interest, is not lots of everyday detail about some local scene. Neither is it some hitherto unsuspected, beneficial or deleterious aspects of an activity. Rather, the ordinariness is rendered somehow extraordinary and yet recognisable. The deeper patterns being played out in and through the detail, come to the surface.

2.2 The ethnographic prejudice

So much for what ethnography is not. What *is* it? At bottom, ethnography is a literacy practice: an analytic strategy which deploys certain modes of representation. The modes consist in a small number of devices such as metaphor, analogy, and metonymy. While all of these are widely used in HCI design too, they are not that well understood as analytic devices. Representations are constructed or construed around these forms. Given the importance of representation, especially written representation, ethnography is, at heart, an inscriptive discipline rather than an experimental one. What all of these representations seek to resolve is what I will call the *synecdoche problem for cultural forms*.

The synecdoche problem is the paradox (or apparent paradox) presented by, for instance, the problem of indexicality in linguistics, frames of meaning in hermeneutics, and contextuality in Gestalt Psychology. For all of these domains, it is suggested that to understand what any individual item or part 'means', you have to see it against the backdrop of the whole. But in every case, the whole is constituted through the arrangement of the parts. Hence, or so it seems, it is impossible to separate the part from the whole. This is the paradox. In ethnography, the framing problem for cultural forms is often encountered and resolved through the deployment of opposing concepts associated with the insider/outsider, individual/collectivity, expert/novice, native/fieldworker, specific/general, experience near/experience distant, emic/etic etc. etc. dichotomies. Most of all, though, it usually begins and ends with the master dichotomy of 'us' and 'them'. What an ethnographic representation seeks is to tell a story which plays through these antinomies and in so doing, synthesises them.

The synthesising ambition betrays, then, a deeper objective for ethnographic work. Clifford Geertz, among the most sensitive and articulate of anthropologists, describes it in the following way

> The real question....is what roles (these) sorts of concepts play in anthropological analysis. Or, more exactly, how, in each case, ought one to deploy them so as to produce an interpretation of the way a people lives which is neither imprisoned within their mental horizons, an ethnography of witchcraft written by a witch, nor systematically deaf to the distinctive totalities of their existence, an ethnography of witchcraft written by a geometer....The trick is not to get yourself into some inner correspondence of spirit with your informants. Preferring, like the rest of us, to call their souls their own, they are not going to be altogether keen about such an effort anyhow. The trick is to figure out what the devil they think they are up to. (Geertz, 1983, p 56-57)

And, of course, once we have begun to do that, the unfamiliarity, strangeness and, indeed, quaintness of 'the native's way of life soon evaporates. We find ourselves engaging in 'forms of life' which are all too familiar. And then what can we say both about 'them' but also about 'ourselves'? Here is Geertz again:

> ..(A) serious effort to define ourselves by locating ourselves among different others—others neither distanced as Martians, discredited as primitives, nor disarmed as universal Everypersons, bent like us on sex and survival—involves quite genuine perils, not the least of which are intellectual entropy and moral paralysis. (Geertz, 1983, p. 234)

In looking at other cultures, then, not only do we hold a mirror to our own; we also ask questions about ourselves.

These reflections are important not simply because they give a deeper sense of the ambition which motivates ethnography. They indicate the reasons for the strategy of representation which is adopted. The analytic representational devices are designed to have the effect of making the strange very familiar: of rendering the bizarre homely.[3] It is their success in this (their success when well done, that is) which blinds us to the skills required to bring that familiarisation about. Because we see in the ethnographic description what is obviously normal life, we assume that the objective of giving the account must be 'the preservation of naturalness'. Hence telling the tale can only be a simple matter of 'hanging around' and 'soaking up the atmosphere'. In fact, no amount of time and absorption will avail us if we do not have the analytic wit to extract the patterns from the stream of life going on all around.

The ethnographer, then, is a transactor of sorts; a translator, a cultural broker, seeking to achieve an equivalence in value between ways of life by allowing 'us' (although more and more today it is the other way round) to see ourselves in 'them'. It is this transparency of translation which many (both in the systems world and outside it) have misunderstood. They have seen ethnography only as information gathering and have missed the critical importance of representation. They have no doubt that it is difficult, skilful and requires certain orders of talents which most 'system people' don't possess. But, at bottom, it is just a *methodology* and hence akin to all the other methods of requirements capture. Nothing could be further from the truth. Thinking about it that way might allow system designers to become more sensitive towards users and their issues, more circumspect about the processes of technology transfer, more willing to make the trade-offs over system needs and user interface constraints in different ways. All of which are good things to have and to be. But deploying ethnography in systems design? Hardly.

3. AN EXERCISE

Thus far I have been primarily concerned to assert the first parts of my argument. These are (a) that HCI and related design domains have misunderstood ethnography; and (b) that ethnography is essentially a literary practice involving certain kinds of analytic strategies based upon the juxtapositioning of antinomies. What I want to do now is to demonstrate what doing ethnography of this kind in design might look like. To do this I will adopt a standard technique used to introduce the ethnographic approach, namely that of deploying it in areas where the audience is already very much at home. The fundamental point of this technique is to render the *constructed framework* of the ethnographic account visible and open to interrogation.[4] To achieve this, I will deliberately to construe the ordinary, the familiar, as bizarre Those are my antinomies. I will try to find some account of its bizarre features which re-familiarises them, but in ways and at levels which goes beyond their immediate appearances. In so doing, I hope to show the ethnographic strategy is one of finding a form of representation which de-centres the familiar but which provides for its re-integration.

What this section offers, then, is a demonstration, an exercise in how ethnographies are built. I will begin with a phenomenon we, in HCI, know very well, namely customisation. However, and this is important, the point is not to offer, just now, insights into how systems might be better designed to support customisation. To do that would require an extensive discussion all of its own. Rather, I am trying to explicate what I referred to above as the 'ethnographic prejudice' through the examination of materials we are all relatively used to analysing. Having shown, hopefully, how ethnographic construction works, I will extend the demonstration by turning to a comparison of two actual ethnographies of technology, Bruno Latour on hinges and locks (Latour 1992), Graham Button and Wes Sharrock (Button & Sharrock, forthcoming) on the handicraft of programming.

Once we have a clear idea how to go about constructing analytic ethnographers of design, we can then turn to the second choice I identified at the start. This is the choice over the play of possibilities. In Part Four, I will sketch a number of alternative ways of looking at organisational life. Each of these might form the basis of an ethnographic description. The antinomy they explore is that between 'global' and 'local' logics. The opportunity they offer is reflection on that central plank of design thinking, the problem-solution frame of reference.

3.1 Customisation and the Bororo

In a series of studies, Wendy Mackay (1990 (a), 1990 (b)) has outlined the social processes through which the customisation of particular systems took place within a research

laboratory. In this work, Mackay has identified the strategies which various types of customisers engage in, the types of customisations they make, the 'triggers' and 'barriers' to customisation which may be discerned and the pathways through which customisations circulate within the various groupings in the laboratory. I don't know if Mackay would call her work ethnographic in my sense, but for my present purposes that does not matter. The analyses presented could clearly be derived from ethnographic-like research as it is currently deployed in HCI. Indeed, many within CSCW who speak of their work as ethnographic clearly aspire to present analyses as insightful and well documented as hers.

Let us take just one of Mackay's studies: the account of the trajectories of customisation and role of translators in this process (Mackay 1990 (a)). In her study, Mackay identifies three different participants in the dense customisation 'network': systems programmers who have a highly sophisticated and deep understanding of the environment for which customisations are created; non-technical end users who are largely ignorant of and uninterested in the complexities of the system; and translators who, having some knowledge, interest and understanding, act as mediators or brokers between the creators of customisations and their (eventual) recipients. Systems programmers are, of course extremely adept at creating customisations whenever software changes are introduced. They are the major source of such innovations. The trajectory of the customisation is from the programmers to the end users via the translators. End users are either not knowledgeable enough to want to risk making their own customisations or do not have the time. For them, as for the translators, when a customisation was shared it was often done through the direct request of another. Finally, the motivation which translators espoused for the effort they expended was altruistic. They liked helping others and felt a responsibility for protecting their less knowledgeable colleagues and for saving them the effort of learning how to customise for themselves.

All this should look relatively familiar, at least to those of us interested in technology transfer and the propagation of innovation. We may not have thought about it quite like this or teased out the relationships and tracked the steps through which particular technologies or innovations went. Nonetheless we can recognise the plausibility (and insightfulness) of the description. What is being described is a world we know in ways we know it.

What would an ethnographer want to say about customisation? When I first heard this paper, I was struck by the immediate and deep similarities between the social process being delineated and the marriage system of the Bororo which Levi Strauss describes in his classic *The Elementary Structures of Kinship* (Levi Strauss, 1969). This is the de-centering, juxtaposing move: see customisation as it goes on in a highly technical research environment as the same as an Amazonian marriage system. What? Why? How? Some of the answers to

these questions can be found in the way that Levi Strauss approaches the analysis of marriage *as a socially organised phenomenon*. Here is a summary of his view.

> It would then be false to say that one exchanges or gives gifts at the same time that one exchanges or gives women. For the woman herself is nothing other than one of these gifts, the supreme gift among those that can only be obtained in the form of gifts......It should not be surprising then to find women included among reciprocal prestations: this they are in the highest degree, but at the same time as other goods, material and spiritual..........

> But no matter what form it takes, whether direct or indirect, general or special, immediate or deferred, explicit or implicit, closed or open, concrete or symbolic, it is exchange, always exchange, which emerges as the fundamental and common basis of all modalities of the institution of marriage. (Levi Strauss,. 1969 p. 65 and pp. 478-9)

What Levi Strauss is saying, then, is that from the point of view of the cultures he has examined, the arrangement of marriages involves a flow of *socially valued goods* engendered by the sets of reciprocal rights and responsibilities associated with kinship. In all these cultures, no matter what the particularities of their marriage rules, women are viewed as one such species of socially valued good.[5] Marriage is the institution by which this value is exchanged with others through the device of gift giving. The woman is given: value is exchanged. Some time later, value is expected to flow back but not necessarily in a directly reciprocal way. The norms of marriage and kinship ensure ultimately that value flows equally through the whole system.

The purpose of the analysis of marriage systems was to provide an analysis of kinship forms. Using structural analysis in Linguistics as his analogy, Levi Strauss sought to recover the universal generative grammar of marriage (and indeed culture more generally) from the variety of forms which it has taken. Such a grammar would consist of a canon for the concatenation of different 'primitive' or 'atomic' kinship relations. The basic unit, 'the atom of kinship' in Levi Strauss' terms, is the complex set of relations existing between the following pairs: husband/wife, brother/sister, father/son, mother's brother/sister's son, all of which are derived from the basic set of terms (father, mother, sister, son). The dynamics of the binary oppositions among the generational relationships are what provide the atom of kinship with its stability (Levi Strauss, 1972).

It is not necessary for us to delve into the detailed analysis which Levi Strauss provides. All we need to pursue is the symmetry being offered between customisation and marriage. On the analysis just given, the institutional rules governing marriage in Bororo society (what

everybody knows about how marriages should be arranged and what and who should be involved) form part of a broader complex of rules governing the reciprocal exchange of gifts. Men, as Levi Strauss says, exchange women through marriage.

On Mackay's analysis, the circulation of customised files can be seen equally as part of an exchange system which everyone knows how to work. However, the gifts or 'prestations' as anthropologists refer to them, are not all of the same kind. Individuals gain a sense of self-esteem, the knowledge that others respect their technical competence, the chance to solve tricky problems, all in exchange for creating and sharing customisations. (The way in which the sharing occurs may well indicate the kind of value exchanges taking place.) Given the chains of customisation and their extension over time, it would be a mistake, as Levi Strauss' analysis makes clear, to treat the exchanges as either insignificant or market-like.[6] Rather, we have to look diachronically, at the trajectories of gifts and the flow of value embodied in women and customised files over time. What is the grammar of this institution of gift giving as we can see it working in these two instances and how is it reproduced?

Now, of course, we are re-familiarising the phenomenon. Having treated customisation and marriage as *instances* of gift giving, we can move up a couple of analytic levels. We can ask about the function of gift giving for cultures in general and how that plays out in both these cases. Being a mechanism for the circulation of value and hence a device for overall equilibration, gift giving reproduces and thus legitimises the system of value transformation. It binds transformations in mutual obligations. If I receive something, I have to give something (eventually). Now the synthesis becomes possible. Through the push for a strong analogy with language, cultural practices such as gift giving can be seen as *forms of communication*. An endeavour to treat culture as communication becomes the frame underpinning the rationale of the ethnography. Through the rules which make up the institution of gift giving, the society communicates the force and importance of its social arrangements including its categorisations. In so doing, it also reproduces those social arrangements and distinctions.[7] The exchange of gifts (women and files remember) is a code (and not merely 'coded') and hence amenable to formal modelling according to the classic tenets of information theory.[8] This is precisely what Levi Strauss does with kinship systems. He uses information theory to construct a binary system for all such cultural practices. Although Mackay doesn't provide it, one could equally well imagine an analysis of the circulation of customisations as a device for representing and reinforcing the institutionalised structure of the research laboratory she studied. Furthermore, it might be very simple to summarise the process in a formal representation such as a directed graph or state transition diagram. The ethnographic strategy of seeking unity in diversity, similarity in disjuncture, will have been worked through once more.

In the above exercise, all the essential steps in constructing an ethnographic representation and analysis are present. The first is the de-centering of the familiar through its juxtaposition with the bizarre. Second, a description (form of representation) is offered which aligns the two. Finally, this representation is expressed in a formal way allowing the synthesis to be achieved.

Although I did explicitly say at the beginning of this example that I was not concerned to draw design implications from my description, some will no doubt still want to ask what the ethnographic account just provided gives them. What have they got now that they didn't have before? I would suggest that by seeing customisation as a species of gifts-giving, its *significance* is altered in many deep ways. To continue the linguistic analogy, the semantics of customisation have been transformed. The aspects on which one might place emphasis are very different to those which would be of importance if it were to be treated as a form of commodity circulation. The value systems, literally, are quite different. It is not for me but for designers, reflecting upon these differences, to say how they might impact the design of customisation tools, the features to be made available for customisation and the like. I imagine that in exploring the gift-like character of customisations and their circulation, designers might want to draw customisation and personalisation together to allow them to take advantage of the place of gift giving in our cultures. At the same time, through their resistance to the introduction of tools which would measure and charge for their use, designers might resist the commodification of customisations.

The example I have just given is deliberately taken from the world of research with which we are familiar and was constructed to display the de-centering moves in ethnography. It offered a demonstration of how the ethnographic inscriptive strategy might be analysed. What I want to do now is to explore how this might be applied to ethnographies of technology in use. My aim is to begin to tease out the principles we might want to use to construct good ethnographies which were of direct value to design. At the start, remember, I said that just because a description was ethnographic didn't mean it was any good *as* social science or *for* design.

3.2 The sociality of doors.....

Ethnographies of technology are likely to focus on the social relationships within which any technology is embedded. As such, they might well act as a corrective to more determinedly technologically focused accounts.[9] However, there is one other consequence of which we should be aware. As Graham Button has noted (Button, 1992), in many accounts the technology vanishes from view to be replaced by the familiar nexus of social institutions,

power, economic and social interest, and historical contingency (G. Button, 1992, Bijker & Law, 1992, Pickering, 1992).

In a recent discussion, Bruno Latour seems to have found a way to remedy this outcome (Latour, 1992). To adapt a phrase of George Homans (Homans 1964), he wants to 'bring technology back in' but in ways which do not subvert, demean, or marginalise it. In a typically delightful and whimsical account of some very ordinary artefacts such as doors and keys, Latour tries to show how through the description of artefacts, the apparent antinomies of artifactual and social, man and machine, can be rendered fragile if not entirely dissolved.[10] For Latour, despite what appears at first sight, there is nothing inherently irrational in the idea of a sociology of machines.[11]

The strategy for grounding this description is the adoption of a *faux naïveté* about the ordinary world around us. By treating it as 'engineeringly strange', Latour achieves the decentering needed. From this wonderment, he is able to demonstrate the mutual involvement of the technological and the social. Locks, hinges, car seat belts and the like are just as enmeshed in the social world as we are. Indeed, they engage in it in almost exactly the same way we do. Both they and we are *social actants*.[12] The social world is populated, then with human and non-human actants. The purpose of a sociology of machines is to de-scribe their interrelations.

> ...every time you want to know what a non-human does, simply imagine what other humans or other non-humans would have to do, were this character not present. This imaginary substitution exactly sizes up the role, or function, of this little character. (Latour, 1992, p. 229)

Having constituted his social world (a community) of actants in this way, Latour goes on to describe the nature of institutionalised division of labour, the distribution of rights and responsibilities, the political economy and moral order in one specific part of it: the sub-region, we might say the domain, of the door, or better one specific door and one specific type of key. While we are used to talking in this way about the social world as we conventionally conceive it, we are unused to thinking of the mutual obligations of doors and users, of the relative power relations between them, and hence the degree of control which they exert upon each other.

This is not the place to consider the application of social actor network theory to the analysis of technology and Latour's contribution to it. I will confine myself to one observation. The point of this ethnography is to achieve the collapse of what Latour calls the 'technologism' and 'sociologism' dichotomy and hence the appearance of a forced choice between them.

Neither the social and the technological supervenes. Both are engaged with one another. What Latour has his eyes on here is what others (Gaver, 1992) have termed 'affordances', the predispositions to act in certain ways which artifacts and combinations of artifacts and social institutions engender. For Latour, what he calls the "ascriptions" of doors (p.240) is an embedding of them into a cultural world populated by artifacts and people. This embedding is facilitated by certain orders of "set-ups". The result of such set-ups, when well designed, is that people flow through doors and around buildings, without every thinking about the co-ordinated work with human and artifactual actants carry out to enable this to happen.

However, even though the technology has been drawn into the analysis, somehow its character as technology has been weakened or weakly made out. To be sure, Latour describes some of the technological characteristics of the Berliner key and the automatic door which are his examples, yet they do not interest him as species of technology. They interest him as species of social actants: actors of a specific kind in a network of social relations. His eye remains firmly on the relationships among the categories which he, using social actor network theory, has deployed. What we might call the *praxis* of the door and the key, the description of the of door's engagement with us in the social world, is missing. This praxis is what Graham Button and Wes Sharrock call 'the lived-work' of technology.

3.3 ....and the programmer's thumb

While I am not sure whether Bruno Latour would really want his account of the sociality of doors to be labelled an ethnography (even though I think it is), Graham Button and Wes Sharrock's account of some of the mundane practices of computer programming clearly is. The overall strategy remains much as before. An arcane practice, in this instance the writing of computer code in high level languages, is rendered familiar by juxtaposing it to an order of practices for the carrying out of routine tasks. So, while at the end of Button and Sharrock's paper, someone who had never seen a line of code before could not claim they now knew LISP, they could claim that they might be able to recognise some of the handicraft of LISP in a code listing. This handicraft, these production skills, are the mundane or *vulgar* competencies of programming.

The task for the programmer, apart from writing executable code, is to write that code in such a way that its structure is trackable and its intelligibility revealed. In that sense, although one might say that the code is piece of technology which engenders certain causal effects when run, one could also treat it as text in a very literal sense.[13] What Button and Sharrock are interested in are the craft skills, the lived-work, of writing the code.

The description they provide distinguishes just three coding practices. These are: keeping the structure visible; the relevance organisation of names; and strategies of visual representation.

I do not have the space to summarise all of Button and Sharrock's dense descriptions of the practices they notice. However, here is a taste of what is on offer.

One of the support tools available in any programming environment is the screen structure editor and one of the ways in which the editor can support the programmer is by keeping track of the relative parenthetical 'depth' or 'embeddedness' at which the code is being written.

> They may do this by alternately flashing the parentheses of a computational process that is in the course of being worked on so that the writer can disentangle that process from others.....If the parenthesis that encloses one argument is clicked on, then it and its right hand counterpart may flash enabling the writer to bring to the fore that process. (Button & Sharrock forthcoming p. 30)

The reason for this device is very simple. None but the most trivial programs are composed from beginning to end as a single flow of code. Rather, coders build up their program by embedding 'chunks' of code within pre-existing structures. As Button and Sharrock show in detail, one has only to move three or four levels down into a program for it to become increasingly difficult to maintain a picture of the overall structure, and hence of where exactly one is at any particular time. Learning to use the tools provided by the screen structure editor skilfully and competently so that the overall picture of the structure can be maintained is one of the ordinary professional skills of programming. However, in order to maintain their grasp on the unfolding structure, occasionally even the most accomplished professionals find themselves resorting to very familiar practices.

> The screen editors do not seem to serve all the needs of programmers working on the screen for we have observed programmers working on their screen with their fingers. One of these screen fingering practices is to place a little finger on the left hand bracket and search for its right hand bracket with a hovering thumb that eventually traces out the correct right hand bracket or slaps against the screen at a point where a bracket seems to be missing. (Button and Sharrock, footnote 28)

This is quintessentially the handicraft of programming.

Once again, just to make sure there is no misunderstanding, I am not suggesting this observation is amazingly novel or that it offers, on its own, deep design insights. However, what it does do is juxtapose a deeply esoteric set of practices such as programming in LISP with very familiar manual practice we have all employed at some time. Button and Sharrock's purpose is to show that to be successful, many of the esoteric practices not only

rely on the mundane ones and hence on the ordinary skills we all share, but that competence in them is recognisable through their intermeshing with our more general, 'natural' skills.

In Button and Sharrock's description, all the steps in constructing the ethnography are tightly tied to the detailed description of the technology as technology and its practical use. The code is not re-described as some other form or re-categorised in ways unrelated to its use as code. *We and the analysis stay with the technology.* This, it seems to me, could be a principle for ethnographies of technology. Were it to be followed, the gap between design and ethnography about which I spoke at the beginning might be narrowed and the normative and notational distractions less enticing.

We are not now all that far from requirements analysis and the user's point of view. While no-one would want to interrogate Button and Sharrock's work for simple and simplistic design relevant observations, as I have indicated, some linkage does seem possible This is because the descriptions while they remain sociological in form, are construed with *the same order of* interests as designers although not identical with them On the other hand, staying with the technology does not necessarily mean abandoning the ethnographic point of view in favour of the twin obsessions of the designer, practical intervention and practicable outcomes.

So where does that leave us? How can ethnography really contribute to design? If we look once more at the motivation underlying both Latour's and Button and Sharrock's description we can see a common thread. Both have a determination to keep the technology at the centre of attention. In Latour's case this is done by adding technologies and artefacts to the motley of social actors in a setting. With Button and Sharrock it is achieved by explicating their observations through the technology itself - lines of code. With Button and Sharrock as well, there is this focus on the lived-work of programming: the detailed description of the practices of programming in order to display the competencies which programmer's share as part of their professional expertise. In ethnographies with this orientation, any analytic terms used to structure the account of activities, such as 'community of practice', 'tacit knowledge', 'shared competencies', 'embodied skills', 'ready to hand', 'breakdown' and so on, have to be grounded in the 'just-what' actualities of the activities themselves: the technologies-in-use. It is my contention that from these two: the determination to stay with the technology and the focus on the lived-work of use, a design oriented social science, a practical sociology, embodied in ethnographic analyses might be crafted. However, to realise that goal, we will first have to come to grips with another set of pre-theoretical choices; that which involves the play of possibilities.

4. THE PLAY OF POSSIBILITIES

As with the previous discussion, my purpose is to gather together a fair number of resources in order to get to grips with the issues of requirements and design with which we began. Requirements are important, of course, because of the *interventionist impulse* in design. This impulse both shapes and is shaped by a particular design frame of reference, that of *the design problem and its (designed) solution*. The designer intervenes to provide a solution to some perceived problem, a short-fall on some value dimension. One fairly conventional one is where less than optimal arrangements are felt to exist to secure maximum efficiency and productivity in the use of technology commensurate with comfort, health, ease of use etc.

What ethnography might offer to designers concerned with productivity is not just detailed description of work routines and daily life with which to fix the features of the design, but an opportunity to open up the overall problem-solution frame of reference in the context of some proposed solutions to specific identified problems. It might do so, for instance, by playing through the possibility that a number of differing rationalities are on view in any work setting. Seeking to evaluate all practices against a single or narrow range of formal criteria might cause designers to miss much of what is vital to the lived-work of the technology in use. Hence their solutions might even make things worse not better. It might be suggested, for instance, that when more closely inspected, apparent inefficiencies usually turn out to be not problems but precisely shaped (and hence effective) solutions to the exigencies to which individuals in a working order are subject. In other words, the contribution which ethnography might make is to enable designers to question the taken for granted assumptions embedded in the conventional problem-solution design framework.

In this section I want to illustrate this proposal by sketching a general rationalisation of organisational life which could well lead to apparently inefficient and unproductive behaviour. I will claim, though, that far from instantiating problems in need of solution, such behaviour is often exquisitely designed for the practicalities of organisational life with which individuals have to deal. A decontextualised, globalised approach to determining and assessing the fit of means to ends, effort to outcome, behaviour to goals, would in all likelihood be blind to this fit and its roots in the pragmatic rationality of daily life. From the point of view of those engaged in the activity in hand, the giving of primacy to globally defined efficiency and productivity, itself can itself look irrational in the extreme. The ethnographic juxtapositioning of these 'local logics' and the rational calculus of productivity, opens up the play of possibilities for design. If it can do so and satisfy the principle of staying with the technology then it will be both good ethnography and good for design.

Lest it be thought that the rationalisation I propose is too bizarre to take seriously, ('just an ethnographic ploy'), I will locate its dimensions in some established and well respected theories of organisations and organisational life. The point of reference I will take is a *course of action ideal type* (Anderson and Sharrock, in press) — that of someone whose work involves the pooling, sharing and communicating of information. I have in mind activities as disparate as a corporate lawyer putting together a brief, an invoice processor trying to tie up some paperwork, a personal assistant planning a travel schedule, or a project manager making deals on the allocation of resources. From within the mêlée of courses of action such as these, what does the rationality of working life look like? Clearly, I accept it is always possible to reconstruct the logic of action as a ratio of effort expended, calls made, pieces of paper filled in, meetings attended, documents archived, costs incurred, time taken and the outcomes achieved. And, of course, from that point of view (let's call it the strict information engineering point of view) as Feldman and March (1989) and Stinchcombe (1990) emphasise, there is bound to be redundancy, inefficiency and hence lower than achievable productivity. Extracted from the fray and the hurly burly of getting things done within the circumstances currently given, it is always possible to construct more rational ways of matching alternative outcomes and means for attaining them. But, in the midst of the flow of activity, what we might take to be redundancy, inefficiency or ineffectiveness could just turn out to be precision engineering.

4.1 Up to your neck in the Garbage Can

Scientific rationality begins in the premise of control and simplification. The world (that is, any phenomenon under investigation) is defined as a simplifiable, controllable, causal structure. The purpose of the experimental method, for instance, is to control and simplify conditions and so identify clearly substantiable causal effects. However, as March and Olson (1992) make abundantly clear, this outlook is rarely if ever even approximated to in organisational and other ways of life. Instead, decisions and actions have to be taken in the face of too much information and too little. There is unpredictability and indefiniteness, while shifting priorities, changing personnel and changing time lines all add further confusion. None of the important variables are under control and all seem to be subject to random, externally generated and inexplicable forces. The only thing that links problems and solutions, decisions and choices is their simultaneity. (March and Olson, 1992, p. 13). Faced with this, commonsense organisational rationality naturally drives in precisely the opposite direction to that of science. From its point of view, any attempt to investigate the antecedents of action, would reveal them to be chaotic and constantly ramifying. Any simplification is an *ad hoc* convenience. Because the influences which impinge upon anyone's sphere of action are beyond enumeration and beyond control, and because their relative weightings are

constantly shifting, the rational organisational actor radically foreshortens the world's causal texture. Instead of seeking to understand causes, organisational actors treat one another's actions as subject to what Harold Garfinkel and his colleagues once called 'demonically wild contingencies' (Garfinkel et al., 1989). Things happen and no-one really knows or cares about the whys and the wherefores. Furthermore, trying to find them out would be an unending and unrewarding task (as Egon Bitner (1965) pointed out, people have ready to hand good enough explanations anyway). Given this outlook, it makes sense to bound horizons of interest quite tightly and to try to manage events only within that bound. What is beyond is somebody else's business or nobody's business.

Jonathan Grudin (in press) testifies to precisely this strategy in a different domain. He asks why designers on a major project would want to introduce a complex and cumbersome tool to document design decisions for maintenance, upgrade and re-engineering purposes when everyone knows that there is an extremely high likelihood any project will be terminated before it reaches conclusion (but for reasons no-one now can enumerate). Why would you try to gain downstream savings (even large savings) at the cost of current expense, if the calculus of risk that such savings would ever be realised was extremely low? In fact, the very effort put in to providing for these savings might jeopardise the possibility of their realisation. Because you do the work of documenting, recording and shaping the decisions the project might be all the more likely to slip its time-table — one reason for termination.

There is yet a further aspect to this. The point about demonically wild contingencies is not only do you not know what all of them are, you also don't know when any one of them will come into effect. This temporal dimensionality takes on specific colouring when the contingencies extend to social relations. Here parties to interactions often seem to be counting on the stochastic character of social relations. They seem to be trying not, as with conventional game theory, to produce some saddlepoint for each occasion which would enable compromise to be reached, but to so construct the running balance of relationships that the impact of the 'small world phenomenon' is nullified. Maintaining the running balance requires them to attend not just to the syntax and semantics of social relations and the degree to which for example, any communication technology interferes with the proprieties associated with them. They have also to attend to situational pragmatics. As a consequence, they may, prefer to phone someone rather than mail them: travel half across the globe for a face to face meeting rather than a hold video or telephone-conference; insist on going to their location because the effort involved indicates the commitment they want to show, all simply to keep the running balance adjusted.

Each of the behavioural 'ploys' just described and one can invent many more, is unproductive in some sense. There exist more efficient and perhaps in the short run more effective ways of getting the business done. But introducing technologies which mandate these other strategies without understanding and playing through the rationalities imposed by the unique unpredictability of a specific working environment would not be good design policy.

4.2 The multi-valued character of information

The strict information engineering point of view treats information as a medium of exchange. As such, while there are costs of accessing, distributing and storing information, information itself has no use value. However as Davenport et al (1992) and Machlup (1984) have indicated, in many modern organisations and hence in the economy at large, information has become a valued commodity and as such a scarce resource rather than a free good. Competition for information has been created, and as in any marketplace regulation is needed. The point at issue here is not that access to and deployment of information is itself an issue, (i.e. the privacy concern) but that systems of resource control and rules for determining contractual enforcement and the like have to be invented. Davenport et al liken the process to the 'invention' of different forms of the state. In most of the organisations they surveyed, Davenport and his colleagues thought a constitutional monarchy existed. Such a sovereign legitimated the rules for information collection, dissemination, forums for debate and procedures for conflict resolution, and so on.

Within the working life of the organisation, one is constantly reminded of the multi-valued and multi-dimensional character of information, and of the trade-offs between these values which may be required. These trade-offs are not just possible but required because what is rational from the point of view of the individual politico-information actor might be at odds with the summary rationality of the system as a whole. Where, from the system's point of view, information enables freedom of action, control over outcomes, efficient allocation of resources, timely action, and so on, open and as complete free circulation of information as possible is optimal. But, under the same conditions, for the 'rational politico-informational actor', the equivalent of free riding might make sense. In so doing, the individual would be able to take advantage of all of the benefits of openness and the work others do to provide it without himself contributing at all. At that point, the local logic of riding free on the backs of others militates against the centralised logic of the open information system. As more and more see the advantage of free riding, the requirements of the information system are undercut.

In much the same way, constructing and deploying a visible organisation of the information on one's desk and in one's filing system to bolster the kinds of strategies Donald Roy (1961) called the 'black arts' of making out ('It was here just now, I saw it. Come back later and I'll have found it.' 'Look at my 'in-tray', how can I possibly do it now?' 'I must have deleted the message, can you send it again?' 'There is no point expecting a reply from him/her. It always takes a week for them to catch up on their mail'). These might all be construed as perfectly intelligible ways of holding 'the organisation' at bay. Such strategies may make even more sense where everyone knows the effort required to police enforcement of more efficient systems is incommensurable with the gains made thereby (Feldman 1989). Reflecting on both the multi-valued character of information in organisations and on its highly variegated nature, might well lead designers of workflow tools, collaborative calendar systems, document management systems and the like to take very different tacks to the ones which they currently employ.

4.3 Coping with Adaptation

The need for adaptation to changing external and internal circumstances and what is called 'organisational learning' is now a commonplace departure point in commentaries on the challenges facing organisations. (Adler, 1992) But what is less often remarked upon is what such adaptive change looks like when viewed from within the daily life of the organisation. There it can come to have very different connotations; ones more easily summarised as organisational amnesia associated with ever diminishing structural half-lives. Quite recently a colleague, Richard Harper, came upon a very interesting and illuminating response to this combination. Harper had spent some time working with a team of designers in a development group. Everybody on the team he studied was, at the same time, a member of multiple other design teams working on other projects. They all led extremely busy lives and were constantly having to juggle schedules, deadlines and resources. While among the designers, Harper noticed the almost ritualistic importance being attached to formal 'sign-off'. Any decision, any 'contract' put in place, had literally to be signed-off by someone who had the authority to do so. It was not enough to have a verbal agreement or to exchange email. An actual, physical piece of paper had to be signed.

Given the widespread use of electronic media to support the design process and the dependence on email which the groups all had, from the information engineering point of view, this ritual looks like unnecessary duplication and overhead. Why not circulate and store the documents electronically? Why print them and sign them? The answer is found in the rationality of actors faced with an ever diminishing effective organisational memory created by constantly changing structures and permanent re-organisation. Membership of teams is

constantly in flux as new organisations are created and old ones dissolved. Teams themselves are called into and out of existence at will. If one can never be sure just who, in a few months, will be occupying the role with which one is negotiating now, whether a 'dead' project will be resuscitated, and who will be the team members if it is, it makes sense to insist on as tangible and permanent a record of the agreement as possible. Few incoming managers are likely to feel particularly bound by decisions of their predecessors especially when only one side (the other side) of the agreement is now available.

Second, and here we come back to the pragmatics we touched on earlier, tangibility is the inverse of plausible deniability. Because of the effect of demonically wild contingencies, the actual existence or arrival of an electronic document may be 'manageable'. Electronic documents do 'disappear': email is known to fall into black holes only to re-appear months or even years later. So, if one needs to create space and time in order to juggle a schedule, await another decision elsewhere, generally 'get one's ducks in a row', denying the document arrived or insisting it has been accidentally deleted from your desk-top is an easily available and difficult to discount ploy. Insisting on sign-off is a strategy precisely aimed at managing and hence countering that possibility.

In one sense, the 'management' of when documents arrive is an attempt to warp time. This is a clear recognition of the complexities of organisational time and hence something of a contradiction of the view of time is a continuous, even flow measured by the clock and the calendar. In the rhythm of daily tasks, time is tied to the natural viscosity of events. It is not simply that time drags when you are doing boring things, or vice versa. Rather, the differing natural viscosities which events have are the ways in which time can be exploited as a resource, the passage of time marked and details of events recorded.

This is particularly so for personal memory although the extension to organisational memory is an important but as yet unexplored topic. A recent study by Marge Eldridge, Debra Bekerian and Phil Barnard (1992) has shown the ways in which individuals' memories of their working routines are structured in schema themselves shaped by the natural viscosity of events. People know just how long particular routine events and actions ought to have taken and hence when asked to recall their activities, can induce when they did other things and for how long. They recall the familiar first, and then fit the unusual in to the 'spaces' which are left. A similar orientation to the natural viscosity of events was revealed by Philip Tyson's (1992) study of desks. Here, individuals routinely use their desks as publicly viewable mnemonic devices to enable them to mark and recall the current status of parallel continuing events. The piles of papers and files scattered on the desk in an apparent jumble can be interrogated as a log of activities in hand. The morning ritual of sitting down at the desk

coffee cup in hand and riffling through the piles has all the hallmarks of what Air Traffic Controllers call 'getting the picture' (Hughes et al 1992), that is, the gradual immersion in the flow of activities in hand. Once again, taking the information engineering point of view and seeking ordered and highly formal structures although being neater, logically consistent, and allowing information to be retrieved 'at will' might well disable this capacity to track and re-capitulate the flow of work.

4.4 Work is not where you live

In his recent study of life interests, Robert Dubin (1992) has expanded and reconfirmed his earlier finding that for most of the working people in highly industrialised economies, work is not a central life interest. He does not mean they take it lightly or do not seek to give a fair day's work for a fair day's pay. Simply put, work is not where they live; from where they derive their sense of identity. However, the ethos motivating much of the contemporary talk about, for example, the information revolution, new ways of working, high performance work systems, informed work and the like is premised in precisely the opposite of this. It makes an assumption of the primacy of an orientation to 'the organisational form of the Generalised Other'. It presumes we are motivated to work harder because work provides self-realisation, because of our general and quite abstract commitments to the organisation, the work group, the firm, and because success at work matters to our sense of personal identity. This orientation to the Generalised Other goes hand in hand with a definition of work as vocation: that is, a definition of work as a pathway of personal development.

Dubin's research indicates most people do not see work this way. To get through the day and cope with its vicissitudes is not seen as opportunity and challenge but a chore. The primary dimensions for self realisation are found away from the sphere of work, in family, with friends and leisure pursuits. In work, orientation is often to very specific others (e.g. those immediately alongside one or with whom one works directly) while the character of work experience is more like plate spinning than a personal Odyssey. Each day the same tasks have to be performed. Each day the same metrics are applied. There is very little opportunity nor do people really want the chance to define objectives for themselves, to broaden horizons, or learn new skills. That would require an 'investment' which it does not seem rational to them to make. Not only, to borrow Henry Ford's phrase, is work one damn thing after another: often it is the same one damn thing. The rationality which results in the location of central life interests outside of work, is one where one holds one's working relations and commitments at arms length (Wenger 1992). Where engagement, involvement and enthusiasm are carefully managed; where, at the end of the day the measure of success is whether in-tray is empty and the out tray full, one is all the more likely to feel constrained to

seek self-fulfilment elsewhere. There may be some challenge, at first, in trying to empty the in-tray every day. But, if it is always full every morning and if it makes very little difference to you if it is empty at night, why keep trying so hard? Why push all the time? Once work is not where you live, the 'effort bargain' can be very precisely set and constantly monitored but in ways which pay heed to efficiency and effectiveness only in the breech.[14]

4.5 Intervention reconsidered.

Each of the four rationalities I have just outlined, could provide the basis of an ethnographic description of the 'local logics' in play in particular settings. These local logics are the resources which the actors in those settings use to make sense of the activities going on around them. As such, they are deeply embedded in work practices and work communities. By tuning in to this plurality, design can take up the opportunities offered by the play of rationalities and hence design 'solutions' which go with the grain of local logics rather than against them.

Once again, I am not presumptuous enough to offer to do designers' thinking for them. However, the kinds of considerations just laid out might lead designers to predict certain kinds of design choices will lead to deleterious results. As John Bowers (personal communication) has observed with regard to his study of the failure of a major Government Agency to manage the introduction of networking, the desire of users to preserve many if not all of the local logics in place in the organisation, in the face of the globalised logic of the network's configuration, led to 'rituals of resistance' which eventually forced the abandonment of the technology.

The upshot of all the observations just summarised is not the abandonment of design intervention. Rather, what they invite is a degree of circumspection over the focus of that intervention, and the 'problems' we are trying to 'solve'. This is because they can all be summarised as: ***brokenness is in the eye of the beholder***. And, if you cannot be sure it is broken, think twice before setting out to mend it. No doubt some will see this as an appeal for a technological version of Burkean conservatism. Nothing should be done because it will always make matters worse. But that is not so. It is rather an appeal to explore the possibility of a 'play of rationalities'. Such play is where the connection with the requirement for efficiency and effectiveness can be most strikingly made and the implications for design drawn. If the play of rationalities could be part of the problem-definition phase of design, it might be a very effective way of teasing out the span of design requirements and the conditions under which they could be satisfied. One of the rationalities would undoubtedly be the relative balance of resource, effort and outcome (i.e. efficiency, effectiveness and productivity). Others might well relate to the kinds of pragmatic considerations I have

sketched above. The point of the play of rationalities is not be to seek ways of trading one set off against others. To do that subsumes them under the same decontextualised, economising calculus which underpins simplistic definitions of productivity. It is rather to place at the centre of design an orientation to the perspectivalism which is explicitly set aside by the adoption of the problem-solution framework, and hence to pose the interweaving of such perspectives (and not their reconciliation) as the challenge for design. The artefacts we design should not subvert the practical logic of the routine world in the name of the rational view from nowhere. Rather, if it is efficiency, effectiveness and productivity (among other things) that we want, why can't we have an efficiency, effectiveness and productivity which enhances and takes advantage of that logic?

5. CONCLUSION

This discussion has had two major objectives. First I wanted to demonstrate that the usual discussions of the value of ethnography for systems design have largely been based on a misconception. They have defined ethnography as a form of data collection and hence have associated it with what I called an impressionistic genre of reportage. Ethnography is a far more complex and subtle practice than that. Second, I wanted to indicate some of the possibilities which an alternative interpretation of ethnography, an analytic approach, might have for design ways of thinking. Many designers of CSCW and other types of collaborative end-user systems are now turning to ethnography as a means of requirements capture. In my view, it is not ethnography they want but field experience. Getting out into the real world to understand the context of use may provide them with all the access and insight they feel they need. The argument I would make for ethnography in systems design is much stronger than the need to give 'a better feel' for the context of use. Analytic ethnographies can make a contribution to design which may deliberately question conventional frames of reference such as that of 'problem-solution' discussed in Section Four. In so doing, they might well bring novel and deep design possibilities to light.

The liaison between analytic ethnography and design could well form the field of a practical sociology committed to an serious engagement with the design problematic - the interventionist impulse. Such a practical sociology could not eschew prescriptivism, as designers frequently accuse ethnography of doing. But its prescriptions and predictions might well not match those which designers currently seek. Instead of providing yet more grist to the mill of conventional design solutions, ethnography might offer sensibilities which will cause designers to question the pre-suppositions of their conventional outlooks. To be open to this possibility, though, requires a commitment by designers too. Future papers will explore what might be involved in making both sets of commitments.

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[1]   It is my view that what are usually termed 'methodological' issues are, in fact, investigative. 'Methodology' refers to a logically prior set of concerns which embraces analytic frameworks, modes of enquiry, forms of data collection and methods for data summary. Given such complexity, I would, if I had my way, purge the term methodology from this whole debate. However, it has a currency and because setting out my reasons for thinking it misapplied would take us a long way out of our way, I will use it here.

[2]   Interestingly, the most vehement debate over the reflexive character of ethnography has taken place within Anthropology itself and has been carried on between practising or early retired ethnographers. (Cf Clifford and Marcus, 1986, Marcus and Fischer, 1986)

[3]   This true even when the subjects of ethnography broadly share our culture. One has only to look at such classics as Spradley's *You Owe Yourself a Drunk* (1988), or Eliot Liebow's *Tally's Corner* (1967), William Foote Whyte's *Street Corner Society* (1955) to recognise the same motivation at work. The audience for these accounts, middle-class academics, do not share the worlds of the street. As we shall see, when the analytic eye is turned upon our own, technological, concerns, as in Marilyn Strathern's *Reproducing the Future* (1992), the distanciation involved can be very unnerving.

[4]   There is a long and honourable tradition (honourable in the sense that "Brutus is an honourable man") which gratuitously juxtaposes some practice of ours, the favourites are laboratory science and medical consultations, with those of magic and divination in supposedly "primitive" societies. The aim, generally, is that through the shock of ironicising 'our' practice as just like 'theirs' we will come to have less confidence

in the universal efficacy of our logic and our practices. Obviously from what I have already said, I have no such debunking ambitions.

⁵   Notice that saying this does not imply *endorsement* of it as a view of women in those societies, or in general. The ethnographer remains *indifferent* to the value system as a value system. Of course this "attitude" is hotly disputed by some.

⁶   Indeed, gift circulation and commodity circulation are orthogonal systems.

⁷   Those unaware of Levi Strauss' work (Levi Strauss, 1969-82, 1988) should consult his magnificent multi-volume *Mythologiques* and *The Jealous Potter* for accounts of the universal grammar of such communication.

⁸   For those who feel this is all too far fetched, I suggest a reading of Wagner's use of fractal geometry as an analogy of kinship systems on Papua New Guinea (Wagner, 1992), or Crump's (1992) use of Markov chains to analyse children's games.

⁹   That this is so is, of course, one of the reasons why ethnography is so attractive to radical system designers who want to break with the technocentric, technophiliac viewpoint.

¹⁰   De-scription is one of a family of similar terms Latour uses to fix the relationship between a text and that to which it refers. The point of de-scription is to explicate that referential relation. In-scription, on the other hand, is its achievement. See Akrich and Latour (1992).

¹¹   Steve Woolgar pointed this out several years ago, but in a different way (Woolgar, 1986). As we shall see, a sociology of mundane artefacts has a particular twist in Latour's hands.

¹²   This is another of Latour and Akrich's neologisms.

¹³   Latour refers to artefacts as texts in a figurative way (as does Woolgar, 1991) in order to open up the possibility of the artefact as a ground of meaning (a social semiotic). Button and Sharrock mean simply that the code is written to be read by both a machine and, potentially, a human reader.

¹⁴   Dubin's work only touches on one aspect of this rationality. Studies of occupational cultures abound, many of which testify to the institutionalised character of the distanciation summarised here and the way it is visible in the details of working life.