Work, Ethnography and System Design

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I Introduction

Although social scientists have long shown interest in advanced technologies and information systems [Kling 1991 (1), Kidder 1981 (2), Klein 1989 (3), Mumford & Sackman 1973 (4)], a pivotal moment in the understanding of what social science might offer the design of interactive computa-tional systems was Lucy Suchman's *Plans and Situated Actions* [Suchman 1987 (5)]. More than any other previous (or subsequent) study, Suchman's work seems to demonstrate the potential which social science analysis based in ethnographic fieldwork could have. The impact of *Plans and Situated Actions* is all the more impressive when one remembers that its central argument is an attempted rebuttal of the prevailing conception of how to represent use and the user in human computer interaction. Suchman argued that there ought to be a place for detailed observation and careful analysis of the actual work practices of ordinary users alongside the standard "user modelling" approaches deployed in design. This position was buttressed by evidence drawn from an actual case where what its designers had thought to be a user interface to a complex system (a photocopier as it happens) which would not generate all the familiar problems of comprehension and interpretation, had in fact failed. Suchman was able to show how, from the point of view of the user interacting with the system, interpretation and sense making *must* always be an issue. The lesson, as Suchman and her colleague Randy Trigg assert in a later study, is:

> Designers interested in augmenting or replacing current artefacts...do
> well to understand how they work, as well as what their limits are. In
> addition, those interested in supporting the design of modifiable
> artefacts do well to understand the everyday processes of modification
> ....
> Design realism can be achieved, we believe, through new methods for
> understanding the organization of work practice in detail. [Suchman &
> Trigg 1991, p. 73 (6)]

The methods they have in mind are ethnography and interaction analysis. Although many see these two as almost inseparable, they are, in fact, quite distinct. I will concentrate only on the former in this discussion.

The effect of Suchman's analysis was salutary. Although, strictly speaking, *Plans and Situated Actions* is not an ethnography in the classical sense (see below), from that point onwards, social science, and in particular *ethnography*, became the subject of much fascination and, sad to say, equal amounts of misunderstanding.

Powerful though its arguments are, the impact of Suchman's book has also to be taken in the context of other emerging trends. For a while, perhaps the most prominent was the development of a new application domain known as Computer Supported Co-operative Work (CSCW) to which Suchman among others [Grief 1988 (7); Wynn 1991 (8); Bannon & Schmidt 1991 (9); Ehn 1989 (10)] was an early contributor. As CSCW technology began to develop, so interest turned to possible marketplaces and hence issues of use. Because the focus was on the collaborative, quintessentially social, aspects of these technologies, over time CSCW became almost the paradigm arena for design-oriented ethnographic work to be done (Moran and Anderson 1990 (11)].

In its turn, the emergence of CSCW was the consequence of a number of radical changes taking place both within Computer Science and within the "information systems" businesses. The dominant "mainframe" paradigm had been overthrown. In its place was "distributed computing". Alongside this development was the increasingly rapid convergence of communications and computational technology. This combination led to the dissolution of the design paradigm associated with "the single (isolated) user at the workstation". Emphasis was put on the co-located or distributed work group whose working lives were premised on the need to co-operate with one another in the performance of their roles. CSCW was and is about building systems for these users. Last, but not least, there was the ever present spectre of the productivity paradox [Strassman 1990 (12)]. Even though companies and corporations had invested hugely in information and communication technology, no real (or at least measurable) improvements in productivity were being seen. CSCW systems appeared to offer an alternative approach which it was hoped would not fail so miserably.

Social science was introduced into this confluence as part of a decisive re-orientation in understanding and defining the requirements for CSCW and other interactive systems. Related elements were participatory design and socio-technical systems approaches [Greenbaum & Kyng 1991 (13), Adler and Winograd 1992 (14)], the emerging field of the social studies of technology and science [Button 1993 (15), Mackenzie & Wajeman 1985 (16)], and, until recently somewhat surprisingly overlooked, organisational theory [Malone & Rockart 1993 (17), Feldman 1989 (18), Feldman & March 1981 (19) Simpson & Simpson 1988 (20) Sproull & Kiesler 1991 (21)]. In their own ways, each of these currents shared an appeal to or reliance on qualitative methods of investigation. To the unfamiliar, such methods look extraordinarily alike. Indeed, for many they are all "sort of ethnographic". This undoubtedly led to some unnecessary confusion. To many designers, qualitative analyses in CSCW concerned with or focused around "working with users" came to mean "doing ethnography".

The general re-orientation I have described was profoundly to be welcomed and, overall, the attitudes and concerns it expressed were both long overdue and extremely important. What is less certain, though, is the real progress we have made in learning how to capture and articulate the value ethnography and social science actually bring to design. To a fair degree, this lack of certainty derives as much from a puzzle over what those enthralled by social science actually thought they were being given as it does from what social science thought it could provide.

On the face of it, Suchman and Trigg's remarks, and many like them, seem to indicate that software engineers and designers were being offered a methodology deployed by (some of) the social sciences. When properly used, this methodology (or so it was hoped) should enhance current ways of understanding and representing the end user requirements for interactive systems. [Jirotka and Goguen 1994 (22), Blomberg et al 1993 (23)] Requirements capture is, of course, one of the banes of any design process.

As I said earlier, these general discussions made almost no distinction between fieldwork, participant observation and ethnography. They were treated as much of a muchness. This is unsurprising if only because many social scientists themselves are prone to be cavalier about the differences between them. However, different they are [van Maanen 1983 (24)] and these differences do matter for the kinds of *analytic outputs* which can be derived from or legitimated by them. Fieldwork can take many forms and utilise many different data collection techniques, among them participant observation. Ethnography is a particular analytic strategy for assembling and interpreting the results of fieldwork gathered very often by participant observation. Designers have, by and large, been more likely to be interested in fieldwork in general than in ethnography in particular.

In failing to appreciate these differences, it is not surprising designers were also unable to understand (still less appreciate) the often Jesuitical fervour with which alternative analyses were justified and pursued. Where they thought they saw a relatively stable and conventional methodology, what they found themselves presented with were almost religious wars [Button 1990 (25), Anderson 1994 (26)].

To see what was at issue *for the social scientists*, one has to appreciate the heritage upon which they draw. My task here is to describe just one element in this lineage, ethnography. In doing so, I hope to help designers understand what I think of as ethnography's "attitude". At the same time, I hope to raise a few questions against which to assess how far ethnography might actually be what design might *need and be able to use*. This last set of concerns is, of course, the vital one but rushing into it without first laying out the groundwork will only slow us down in the end. To decide if ethnography is what systems design ought to call upon, we need first to get straight what ethnography is and where it came from. Once we are clear about that, we can turn to the integration of ethnography and design.

Before letting fly, I want to enter just one hugely important caveat. I make no bones about the superficiality of the treatment I give both social science and social scientists. I do not have the space to explicate their ideas in any depth, let alone to the level of analytic rigour which my social science friends quite rightly demand. Nor am I even going to try to round up and dust off every ethnographer and ethnography which has talked about advanced technology and its design. This is a tour for the uninitiated but mildly interested, not a disquisition for *aficionados*.

II   The Ethnographic Heritage

In this section, I outline ethnography's recent and not so recent history. As any ethnographer will tell you, it is through the myths and legends associated with its history that a culture makes sense of itself and others.

A.   *Inventing the Professional Stranger*

By his own account, and by almost everyone else's as well, ethnography was invented in 1915 by Bronislaw Malinowski. [Malinowski 1967 (27); 1987 (28), Kuper 1983 (29)]. Until that time, ethnology and anthropology were largely the preserve of somewhat eccentric, classically trained and library bound academics with a taste for exotic reading. Their books and treatises were, by and large, speculative histories on the emergence and diffusion of physiological and cultural traits based upon the second and third hand accounts of travellers, missionaries, and more and more, Colonial Officers e.g. [Frazier 1922 (30)]. Around the turn of the century, a few Young Turks with a yen for adventure in foreign parts had begun to take advantage of the funding on offer from the Government to join survey expeditions. But in general, the discipline was bookish in the extreme.

Then, in 1913, the Australian Government encouraged C.G. Seligman (the pre-eminent scholar of the time, and someone who had been to visit the peoples he wrote about) to put together a party to study Aboriginal family systems in Australia and New Guinea. One of the team was Malinowski, an expatriate Pole working at the London School of Economics. As luck would have it, Seligman's group arrived in Australia just as war was declared. As a citizen of the Austro-Hungarian Empire, Malinowski was technically an enemy alien, and despite his tenure at the London School of Economics liable to be interned. However, Malinowski proposed and officialdom concurred that it would be more advantageous if he were to spend the duration of the war working at his profession in some remote place rather than languishing in an internment camp. The place he chose was the Trobriand Islands off the coast of New Guinea. For the next three years, Malinowski spent most of his life on the Islands, living and working among the Trobrianders. In so doing, he invented the modern form of "fieldwork" as a social science investigative technique and ethnography as its analytic complement. The results of

his experience were published in a series of quite astonishingly detailed (even to-day) monographs the most famous of which are *Argonauts of the Western Pacific (27)*, *Coral Gardens and their Magic (31)* and *The Sexual Life of the Savage (32)*.

For Malinowski, the purpose of fieldwork was to become intimately familiar with a way of life through learning its language and culture and living according to its regime. From this intimacy would come the *verstehen* (the subjective understanding) necessary to integrate and correlate the various types of data to be collected. These fell into three general kinds: synoptic charts which lay out the relationships between various kinds of institutions, customs and activities. The information for these charts was provided by native informants and observation of events. Second, there was the detailed description of day to day life and activities. Third, the ethnographer must collect any and all stories, narratives, myths, magical formulae and the like through which the society represented itself and the world around it. From these three, an ethnography was to be woven.

The motivation for creating the ethnographic account begins with "Things are not what they seem", and appearances are certainly not the whole of the story. This need to look behind appearances in careful, detailed and systematic ways is, of course, the common inspiration of all scientific and investigative work. It is also why ethnography insists that the native, the participant, the actor, the person on the Clapham Omnibus, is not necessarily the best judge and their views the final arbiter on what they are doing. Of course we must ask them. But we must also colligate their answers with many others forms of evidence.[1] What the ethnographer must do is set all this evidence in a frame. For Malinowski, this was one in which accidental forms are explicated in terms of universal needs. The forms are those of social life; the needs those of social structure.

We have here, then, the first important point to note. An ethnography is a "post hoc" representation or account of what has been seen, heard and found "in the field". Writing the ethnography is not just "writing up" the field notes. It involves their interpretation and analysis. Take *Argonauts* for example. The centrepiece of this work is a famous description and explanation of "The Kula Ring"; that is the practice among Trobrianders of sailing of hundreds of miles across dangerous seas simply to exchange in very strictly ritualised ways apparently worthless amulets and necklaces. What on earth are the Trobrianders up to in doing this? By dint of his detailed descriptions of what is exchanged, with whom and how often; how the amulets and necklaces circulate around the islands; what the required formulae are; what associated activities may be carried out in association with Kula; what magic governs the journeys; and so forth, Malinowski demonstrates how Kula promotes social integration. In carrying out the Kula, Trobrianders, willy-nilly, are solving one of the central problems of all societies. The function of the Kula is social cohesion.

Although it did not appear that way to Malinowski's devotees and disciples, ethnography and functionalism are not identical. Other theoretical frameworks can be invoked [Barth 1967 (33), Harris 1978 (34), Levi Strauss 1969 (35)]. What Malinowski solved was the "credentialisation" problem. That is, he laid down the criterion by which one could claim the right to say: "The Tutsi behave in this manner" or "The central beliefs of the John Frum Cults are thus and so". To say the Yanamami do this and the Borroro do that, and to be taken seriously, you have to have been there, seen them, and if not done it and brought back the T-shirt, at least captured and recorded their lives (these days on

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[1]   In some "user centred approaches" to design, this central aspect of ethnography appears never to have been fully understood. Asking the user seems to have been the only investigative technique.

videotape). Fieldwork became *de rigeur* for ethnography and every practising anthropologist worth his or her salt had "his" (or "her") tribe.

This, then, is a cardinal thing to note about ethnography. Its practice is a particular form of legitimation. Ethnographers "know" in ways others don't and can't. And what they know derives in part from personal experience. This will become important in our later discussion.

Until quite recently British anthropology continued in this vein [Kuper 1983 (29)][2]. Year after year, anthropology students scattered across the (then) Empire serving their apprenticeship by carrying out fieldwork on "their" tribe. Their task was to gather, order and analyse information on patterns of kinship, trade, warfare, law, agriculture, and whatever else they encountered during their stay. On their return to their Universities, they transformed their fieldnotes into ethnographies to be submitted for their Doctoral theses.

In the United States, anthropology was born as a fieldwork-based discipline under the aegis of Franz Boas and his studies of the native Americans of the Pacific North-West Coast [Boas 1921 (37)]. What distinguished American anthropology from its European (mainly British) counterpart was, first, the availability (albeit already in decline) of a "pre-industrial" or certainly "non-western" culture close to hand and second the interdisciplinary outlook of many of the seminal figures. Where early British anthropology drew its inspiration from the biological sciences, Ruth Benedict, Margaret Mead and others drew upon Psychology and the emerging field of Psychiatry. This is not to say that "banana boat" ethnography was unknown among American anthropologists. New Guinea, the Polynesian Islands and the Pacific rim became favourite field sites. Rather, they sought instinctively, or so it seemed, to compare what they found "over there" with what was left "at home". Ruth Benedict in particular sought to provide such a synthetic integration of cultural types [Benedict 1935 (38)]. Margaret Mead's comparative goals were much more ambitious. She sought to compare "modern American" ways of life with those of Polynesia and New Guinea. Her classic studies *Coming of Age in Samoa* and *Growing Up in New Guinea* [Mead 1943 (39), 1963 (40)] explicitly addressed the cultural origins of the *stürm und drang* associated with adolescence in the West. One scholar who bridged North American and British traditions was Gregory Bateson whose monumental study *Naven* [Bateson 1980 (41)] is a landmark not just in classic anthropology. It also opened up new approaches in Psychiatry and Cognitive Science. Bateson himself went on to develop the influential theory of the "double bind" and to become one of the first to promote systems thinking in regard to human cognition.

If European ethnographers tended to focus around institutional and hence "objective" matters (I include both British and French Anthropology in this), Americans often oriented towards the experiential and hence "subjective" [Turner & Bruner 1986 (42)]. With Clifford Geertz this latter tendency has been developed and refined into a whole paradigm which he calls "Interpretative Anthropology" [Geertz 1983 (43)]. In his approach, Geertz has been determined to represent what he calls "the native's point of view".[3] Geertz, almost uniquely in modern Anthropology, combines a discursively engaging writing style with a deep fascination for the detail of different cultures and their

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[2]   Jack Goody's recently published account of the development of African anthropology offers fascinating insights into the social, political and (inevitably) financial competition underpinning British anthropology just pre and post World War II (Goody 1995 (36)).

[3]   A term interestingly resonant with "the User's point of view" perhaps.

ways of life. [Geertz 1983 (44), 1988 (45)]. Geertzian interpretivism has had a profound influence both outside Anthropology and within it. Geertz is the literateur's anthropologist and has brought the concerns of Anthropology, both its grand designs and local squabbles, to the notice of the wider public.

The majority of the past three quarters of a century of ethnography have been amazingly *normal* in the Kuhnian sense of the term. Disputes over the role of magic, the structure of kinship, the character of the acephalous state, the interpretation of myth, and the circulation of goods or wives were couched in local detail and fought out at various theoretical levels. Then in the late 1970's, ethnographers found themselves hoisted by their own. These traders in culture (transactors might be better) found themselves out-transacted by French "de-constructionism" [Rabinow 1977 (46), Clifford & Marcus 1986 (47), Foucault 1970 (48)]. Deconstructionism exposed the soft underbelly of ethnography: its cosy confidence in the surety of its own position, view and voice. When, within the literary arts, one could seriously ask "Who is the author?" and propose that novels write themselves against socio-political contexts, the ethnographic tradition in anthropology, dependent as it was on the literary trope, was bound to feel threatened. Ethnographers were made more and more uneasy by assertions about ethnography's "perspectivalism", "constructivism", "ahistoricism" and the like. Once the authority of the ethnographer's experience had been undermined, to what else could they turn [Sanjek 1990 (49)]?

For the sake of simplicity, I will distinguish a 'strong' and 'weak' version of the implication of this deconstructionism for ethnography. The 'weak' version looks for a re-assertion of the differences in attitudes, values and orientations which the various participants in and users of the outcomes of an ethnographic encounter might have. The representations which each deploys (their "accounts") are shaped by their interests in the events being depicted. In one sense, then, this relationism (or relativity-with-a-small-'r') denies the possibility of the Archimedian position from which a single, overarching, description to end all descriptions can be derived.[4]

The 'strong' version is a horse of a somewhat different colour. On its argument, interests are tied very clearly and very tightly to socio-economic and political structures. Representations are, therefore no more than the ideological epiphenomena of such interests. Whether explicitly or not, representations either serve or undermine the dominant ideological structures sustaining existing societal structures. On this view, ethnography should be reconceived as a contribution either to cultural critique or cultural hegemony [Marcus and Fisher 1986 (51)]

This gives us a third thing to note and it applies especially to contemporary ethnography and its implication for system design. As we will see in later sections of this discussion, the encounter with deconstructionism has made ethnographers (even ethnographers as sure footed as Clifford Geertz) unconscionably self-conscious about their "place on the scene".[5] Moreover, so conscious have they become that the very practice of "ethnographising" often appears to be the topic of the ethnography [van Maanen, 1995 (54)]. Hard nosed empiricism (of the Joe Friday kind — "All I want is the facts, Ma'am") is no longer a defensible position for anyone to adopt, if it ever was. This goes a long way to

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[4]   A particularly intelligent overview of the issues which this stance raises can be   found in Putnam 1978 (50)

[5]   The leading practitioner of this "reflexive" self-consciousness in the sociology of technology is undoubtedly Steve Woolgar (Cooper & Woolgar 1993)(52). Another, often more whimsical exponent, is Bruno Latour 1992 (53).

explain some of the diffidence which ethnographers have been prone to show over the implications of their findings for technology design [Hughes et al 1992 (55)].

For the moment, though, let's leave all this on hold[6]. We have followed mainstream ethnography more or less to the present time. We need now to step back a couple of paces and follow another line of development. As well as anthropological ethnography, Suchman's interests drew on sociology, particularly on studies of work and the workplace. These studies too employed ethnographic analysis based on fieldwork and more or less started with "Chicago Sociology".

III   Streetwise in La La Land

I mentioned a number of differences between the North American and European traditions influencing ethnography. The most important was probably their institutional context. American Universities did not (then at least) have the rigid divisions apparent in Europe. Anthropology and Sociology, for example, did not always inhabit two different departments and hence vie for space on the curriculum. One place where they came very closely together was at the University of Chicago. Everett Hughes [Hughes 1964 (57)] had been a professional journalist and saw the absolute importance of studying and understanding the development of "city life" in the 1930's. He insisted on the need to carry out just the kind of fieldwork that Malinowski was formulating in anthropology. The concern to understand "the city" should not be surprising since during the 1930's and 40's, Chicago was creating a whole new mythology of its own and with it a whole new gallery of figures to be mythologised. Instead of the Noble Savage and the palm strewn beach found on Firth's [1936 (58)] Andaman Islands or the Rational Calculator examining chicken entrails in Zandeland [Evans Pritchard 1976 (59)], Chicago sociology offered us the drunk, the hustler and the pimp; and not far behind them, of course, those on whom they rely - ordinary working stiffs. Chicago sociology [7] used ethnography to turn a mirror on American society. In a magnificent stream of fieldwork-based ethnographies and studies (the 'street cred' was just as necessary) we are taken from the precinct house [Manning and van Maanen 1978 (60), van Maanen & Kolb 1985 (61)) to the cat house (Bryan 1966 (62)], from the insane asylum [Strauss et al 1964 (63)] to the jazz club [Becker 1963 (64)], from wine alley [Spradley 1988 (65)] to the dance hall [Cressey 1971 (66)] , the public toilet [Humphreys 1970 (67)] and the race track [Scott 1968 (68)]. Moreover, the view encompassed such diverse working environments as the shopfloor [Roy 1954 (69)], the small town *bricoleur* [Harper 1987 (70)], and the morgue [Sudnow 1967 (71)]. Most recently of all, it has extended to the research and development labs of high tech companies [Gregory-Huddlestone 1994 (72)]. At the heart of all of these studies is an emphasis on the depiction of the very ordinariness of extraordinary ways of life. We see them as not very far removed from our own [Birenbaum and Sugarin 1973 (73)]. Each has its own coping strategies for the familiar slings and arrows, the quotidian vicissitudes of working life.

So, here we have the fourth important point to note in regard to ethnography's relationship to systems design. Ethnography is militantly as much about "us" as it is about "them". And we can find "them" all

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[6]   I am aware that I have not explicitly described current anthropology's ethnographic focus. This is because I believe that although the form may have changed, the approach is still the normal one I have described. The context is less and less the bush village and more and more the shanty town and the urban centre. The most important change, perhaps, is the very belated recognition of the important and distinctive contributions women make in all societies (Strathern 1988) (56): But again, the approach remains the familiar one.

[7]   Incidently, Chicago Sociology has become a style of sociologising and migrated beyond Chicago University. Many of its modern exponents are on the West Coast.

around "us". Indeed, as Oscar Lewis made abundantly clear in his wonderful series of studies of life in Puerto Rico and New York: "they" are "us" [Lewis 1967 (74)].

We now have most of what we need to understand the background to ethnography's introduction to system design. We have a fieldwork methodology which provides the material for literary representations of cultural patterns. We have a focus on the ordinary detail of working life combined with a concern for what life and action looks like from the personal point of view. We just need to add a final couple of ingredients. The first is the re-discovery of culture as "communities of practice", a phrase which has become almost a term of art in ethnographic studies of advanced technology. The second is the emphasis on the importance of context for understanding social action. These two came together when, as did so much else in American life in the 60's and 70s, the epicentre of US social science moved to the West Coast.

IV   Agents, Tailors, Jocks and Burnouts

Chicago sociology demonstrated the value of ethnography for sociological investigations. As a result no nook or cranny of American life was left unexposed. Two institutional milieux were particularly popular: the medical environment and the educational. In *Boys in White* [Becker et al 1961 (75)] we were given a classic piece of sociological investigation and analysis. *Boys in White* tracks the "career" of the medical student and the culturally defined stages by which it is marked. This idea of laying out the trajectory of a particular role has become one of the central ploys in ethnographic analysis. It enables the depiction of the "natural phases" of participation in a social group or community.

In 1989, Penny Eckert (76) published a study of a Californian High School which took (as did much Chicago inspired sociology) the "sub rosa", unofficial aspects of the school as its focus. Seen from the student's perspective, the school was a number of "cultures" each vying with one another and each defined by distinct categories, symbols, codes and practices. Becoming a member of a culture −knowing both how to be and to be recognised as being an acceptable type - involves a complex process of assimilation and learning. The career of "becoming a jock" or "becoming a burnout" involves moving from the periphery of the culture to its centre. However, on the periphery, as a learner, one is nonetheless in a recognised and well understood role - that of peripheral participation. In Eckert's view, legitimate peripheral participation is one of the crucial elements constituting the communities of practice which make up a culture. Learning a culture is nothing other than learning these practices.

Eckert's work drew upon a range of studies where the idea of legitimate peripheral participation in communities of practice had been the *leitmotif* [Lave & Wenger 1991 (77)]. It provided a key notion which gave traction on what, otherwise, had been a slippery problem, namely the social basis of learning [Rogoff & Lave 1984 (78)]. Its emphasis on the implicit, tacit dimensions of learning and hence knowledge in the workplace enabled the concept to condense many of the issues which designers had hitherto been struggling to articulate [Markus & Keil 1994 (79)]. Seeing work groups as communities of practice and work cultures as the expression of these communities unlocked a number of apparently intractable problems or offered new ways to look at old and difficult issues [Wasserman 1990 (80), Brown & Duguid 1992 (81)].

The notion of communities of practice has been deployed by many ethnographers to counter the overly cognitivist interpretation of learning which they felt dominated current thinking in their field [Rogoff & Lave 1984 (78)]. Studies of apprenticeship in West Africa, for example, showed how the skills and knowledge required to be a master tailor resided as much in the hands and the culture as they did in the head [Lave 1988 (82)]. In particular, the notion of planning within conventional studies of cognition

was held to be inappropriate to the modes of thinking and acting often found in many "non western societies". The example of the Micronesian navigator became iconic [Gladwin 1964 (83)]. Moreover, as ethnographers turned their attention to very clearly and unambiguously "western" settings [Hutchins 1995 (84) , Anderson & Sharrock 1987 (85)] there too the cognitivist model seemed to fail. An alternative approach referred to as "distributed cognition" was developed [Olson & Olson 1991 (86)]. In turn this notion of distributed cognition became a key notion for CSCW (cf earlier).

V   The Devil's in the Detail

The final component with which Suchman and in turn, ethnography, has become associated is the "situatedness" of action. Although all ethnographers pride themselves on their ability to notice and capture the detail of the setting in which they work, very few focus on the kinds of fragments with which Suchman and her colleagues have been concerned. Indeed many are deeply opposed to the "philosophical" roots which the Suchman approach draws on. Her orientation is taken from a branch of sociology known as "Ethnomethodology" [Garfinkel 1967 (87)] and a particular interest group within it concerned with the analysis of conversation.

Harold Garfinkel, the creator of ethnomethodology [Garfinkel 1967 (87)], had been a student of Talcott Parsons. Drawing upon Husserlian phenomenology (and in particular the work of Alfred Schutz), Garfinkel sought to "radicalise" Parsons [Grathoff 1978 (88)]. In place of Parsons' theoretical construct of the fully socialised actor defined by appropriate sets of norms, values, roles, and outcomes, Garfinkel sets out what he calls "the praxeological rule". This rule is an investigative stipulation which defines any and all phenomena of sociological enquiry as the outcomes of patterned, co-ordinated, *structures* of activities. The net effect is an alternative departure point for the investigation and analysis of the patterns of social action we see in ordinary life around us. Instead of seeing social actors as "programmed by their cultures" to agree on definitions of the situation and hence patterns of appropriate activity, Garfinkel treats the determination of "what is going on" and "what is appropriate" as the outcome of "local", "occasioned" and "situated" action. He proposes that an alternative sociology concerned with the analysis of the detail of everyday actions could be built upon this stipulation. For Parsons, the explication of the stability of social life was by reference to forces and processes external to the interactions under view. For Garfinkel, such stability could be described "from within" those processes.

Harvey Sacks and his colleagues pursued this goal through the analysis of naturally occurring conversation [Sacks 1989 (89), Sachs, Schlegloff & Jefferson 1974 (90)]. Following Garfinkel, they took "analysis" to mean the description of how the overall structure of the conversation could be derived from the observable interactional practices used by co-conversationalists. This structure they termed the "accountable" management of conversation. In other words, Sacks and his colleagues approach the analysis of conversation by treating co-participants as if they co-produced conversational order, and see and hold each other accountable for the preservation of that order. Departures from "orderliness" are themselves "accountable" in manifold ways. In numerous studies and investigations [Button & Lee 1987 (91)], Conversation Analysis detailed how the obvious and thoroughly unproblematic routine and unremarkable features of this aspect of daily life are sustained "from within". One of those features is what they came to call "repair" of the disruption of the routine turn taking order.

The analysis of conversational interaction and the importance of "repair" was the analytic resource for Suchman's ethnographic account of photocopier failure. Examining tape recordings of the actual events, she adapted the turn taking structure of Conversation Analysis to describe what users do. The

point Suchman was making was that while the machine offered a number of "accounts" of its own state, it was incapable of monitoring how those representations were being interpreted by the user. The mutual co-production of intelligibility which had been identified as the central element of conversational interaction simply could not be extended to human-machine interaction. Unlike the case for human interaction, the machine is unable to "read the readings" of its actions and so act in response to repair interactional misfirings and misunderstandings when they occur. On Suchman's view, whatever else the machine might be capable of, it is not *and will never be* interaction in the same sense that we use the term for humans.

VI   Technography

We now have the various strands associated with the "ethnographic" intervention associated with Suchman. There is the dependence upon fieldwork as an investigative technique and ethnography as its correlated analytic strategy. There is the concern to represent the user community of practice and users' actual work practices in and through the examination of the minutiae of their working lives. Finally, there is a predisposition to see the structure and order that the pattern of such working lives display as "situated", "occasioned" and "co-produced". In one way or other, these components define what social science has come to mean for designers in HCI.

In this section, I want to differentiate the various social scientific forays which owe some allegiance to this intervention. My purpose is clarificatory rather than argumentative (and certainly not denigratory). However some of the distinctions I make may well seem forced to those concerned and the subtleties they appreciate in their own work remain unremarked on. Hopefully though, I will have whetted readers' appetites enough for them to seek out this work for themselves.

To begin with, let me reiterate. While Suchman's work marks a turning point, it cannot really be thought of as initiating a social science interest in advanced information systems.[8] There had been many studies concerned with "automation", "computerisation", and the like [Knights & Wilmott 1988 (92), Kling 1991 (1) and perhaps most famously of all Zuboff 1988 (93)]. What it did more than anything else was to contribute to the strengthening of design interest in social science [Winograd and Flores 1986 (94)]. That interest having been aroused, there seemed to be no shortage of social scientists ready to return the compliment.

Broadly the ethnographic studies of technology and work can be scattered across a spectrum. At one end, the study is an end in itself. At the other, the point is to derive requirements. Of course everyone argues (some quite strongly) that their findings are of value to design, but when we look closely we see they derive their *raison d'etre* in very different ways. For the sake of brevity, I will bucket these differences in three broad groups: those *integrated with*, those *complementary to*, and those *independent of* design.

A.   Integration
The primary characteristic here is that the ethnographer is seen as a member of the design team. The ethnographer is called upon at the conceptual stage, the design requirements analysis stage, and at the evaluation stage of the design. The objectives of the studies are, then, set almost entirely by the needs of the design orientation and, in the phrase of Somerville et al. [1992 (95)] they can sometimes provide some surprisingly useful insights. Examples of how they go about doing this and the kinds of insights

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[8]   Certainly Suchman herself has always been at pains to point this out.

which can be offered can be found in Brun-Cottan & Wall 1994 (96), Nardi & Miller 1990 (97), Nardi & Johnson 1994 (98), Wall & Mosher 1994 (99), Sachs 1995 (100) and Harper, Lamming and Newman 1992 (101). For the most part, the worth of the contribution is assessed by seeing how helpful the study has been with the definition of scenarios of use, the "practicalities of implementation", or the coopting of users.

B.   Complementarity

This is by far the largest grouping. Here the questions asked of the ethnography are not set by the design team but oriented towards what the ethnographer perceives their needs to be (even if these are not always what designers feel they want). The aim, often, is not to make the ethnographic analysis tractable for design so much as to raise designers awareness or "sensibilities" [Anderson 1994 (26)] with regard to particular aspects of the setting within which the technology under consideration has to be deployed. The most celebrated series of studies of this kind have been those carried out by the Lancaster CSCW Centre [Hughes et al 1994 (102); Rouncefield et al 1994 (103), Harper & Hughes 1993 (104) and those carried out by Lucy Suchman's group at PARC (Suchman & Trigg 1991 (6), Suchman in press (105)]. Other studies of this kind have been undertaken by Rogers 1992 (106), and Harper & Sellen 1995 (107).

C.   Independence

In the last grouping, an impact upon design is often the last thing the ethnographer wants. Findings are positioned relevant to ongoing debates within or between the social sciences (and sometimes with design itself). This is not to say that there are no implications, only that the investigators themselves are not much concerned to isolate and articulate them. Examples of this approach can often be found looking at design and development [Anderson, Button & Sharrock 1996 (108), Sharrock & Anderson 1994 (109)]. Louis Bucciarelli is perhaps the leading researcher of this type [Bucciarelli 1995 (110)]. Work in other domains has also been carried out [Henderson 1991 (111), Heath et al 1992 (112), Star and Ruhleder 1994 (113)].

VII   So, what's the score and where to now?

In the years following the publication of *Plans and Situated Actions* ethnography has come to fascinate many designers. The studies appear to offer them something they feel they need. In addition, the whole tenor of the approach is grounded in a tradition which expresses concerns some designers feel extremely sympathetic towards [Borenstein 1991 (114)]. But the tightness of the relationship, the strength of the relevance still seem to be in question or in need of justification. Why is this? Why haven't the many and varied ethnographies of the kinds listed above been enough to still the doubts?

I believe this questioning reflects an awareness that a number of very tricky questions have yet to receive sufficient attention, or that the attention they have received has been somewhat misdirected. Until these questions are addressed and the issues which they represent fully worked through, the possibilities which ethnography might offer design will remain unclear. Unfortunately for those who like their issues brisk and their outcomes clear-cut, these questions are complex and very often entangled with one another. Certainly they are not the substantive, concrete, empirical ones which many designers and ethnographers prefer to home in on. They have a more methodological and hence "philosophical" bent which perhaps accounts for why they have been somewhat under-emphasised. In my view, the most important are :

- the character of the method

- the relationship to theory

- the scope of findings

- the politics of intervention

There is also the recurring question of ethnography's capacity "to cope with change". Although I regard this question as expressing both a deep lack of understanding of ethnography and a surprising lack of sophistication concerning the processes of technologically inspired change, because it is so widely discussed, I will devote some attention to it.

In the following section, I set out what lies at the heart of these questions, partly in the hope of engendering the debate I think is required. Until these and other related concerns are worked through, the jury will have to remain out on how the relevance of ethnography to systems design just might be realised.

VIII   Methodology or Gift?

Ethnography has been portrayed as a "methodology" in the conventional sense of the term. This is precisely how Malinowski felt about it, and how it has usually been presented in the basic texts. It is described as a body of procedures and techniques which anyone can learn to apply. However, as ethnography has moved away from reliance on fact collection and simple description of folkways and mores, this outlook has seemed less and less secure. The shift away from empiricist and positivist inspired modes of investigation (the importance of Biology and Natural History for early anthropologists should not be underestimated) has coincided, not surprisingly, with the rise of interpretivism I mentioned earlier. As a result, the emphasis has been thrown not so much on the techniques for identifying and listing of what any particular group does as to how their activities are integrated within their world view. Laying out the weft and warp of "the native's point of view" has become the heart of the ethnographic enterprise. The net result is that the ethnographer's skill has come to seem more and more like gift than a craft and, as such, associated with particular individuals and their genres[9]. In that sense, ethnography has become less proceduralised and more personalised.

One way of responding to this tendency is to try to move the discourse away from a narrow conception of "method" by emphasising the importance of the literary medium. Ethnography is not a way of finding out but a way of writing up.[10] Once we accept this, the reason ethnographers place so much emphasis on the personal in their work becomes understandable. They write with a voice and from a point of view. But this is not a defect; it is the essence of the genre. Inevitably, this way of thinking turns more to the hermeneutic disciplines for guidance than to the natural sciences. It is the fieldwork *experience* not the fieldwork findings which is of importance. And that experience must, by definition, be unique. The replication of findings (and their generalisation—a topic we will come to) has its roots in the conventional experimental method. As it now seems to be understood within Anthropology at least, ethnographic fieldwork is not easily seen as an analog of the laboratory experiment. But if that is the case, what does the anthropological apprenticeship consist in and how is it structured?

At first sight, this appears to be a parochial matter, of interest only to professional ethnographers and anthropologists. However, as soon as we start to advocate an understanding of "ethnographic methods" by designers, it behoves us to say what those methods are and what their outcomes might be.

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[9]   So we talk of Geertzian, Levi Straussian, Bartian ethnographies.

[10]   I have set out my arguments for this at length in Anderson (1994) (26)

Moreover, we also have to indicate just how ethnographic "findings" relate to those of laboratory and similar studies more usual in design. Here we run directly into the varieties of disciplinary conception to which ethnographers hold. Some do indeed see themselves producing findings of the same logical order (but vastly different in style) as experiments. Others would deny this entirely, stressing the importance of voice, perspective and genre. For them it is no surprise at all, for example, that two ethnographies of "the same" setting can be diametrically opposed. Indeed, they would almost expect them to be.[11]

IX   Epistemological Impedance Mismatch?

If the first question has to do with ethnographic method, the second turns on the relationship between ethnographic and other modes of theorising. The introduction of ethnography into HCI came during a period of re-appraisal and opportunity. Others responded to the same challenges by proposing somewhat different strategies. Newell and Card 1985 (116); 1986 (117), to take but one leading example, proposed the only realistic way forward way was by "hardening up" the science of HCI. By this they mean the development of "technical engineering-style......theory" (p. 257) of a psychological form in the case which they were discussing. No doubt their aspiration is echoed by many within the HCI community. The theory they want could take many forms: "rules, theorems, grammars, simulations, formulae, or other explicit means; and might operate on data that was symbolic, structural, or numerical" [1986 (117) p. 258]. Most important of all, it will encompass both quantitative *and* qualitative data and offer each a complementary place in the construction of theories. The approach which they believe allows this ecumenism is what they dub as "divide and conquer" and which they assert is a hallm**ark of any progressing science.**

> The divide-and-conquer heuristic is that it is better in the long run to
> work out pieces of a complex problem in some detail, setting aside
> some interactions with other parts until later, than it is to try to handle
> everything in one big confusion. [1986 (117) p. 265]

Although it looks eminently reasonable and practical, ethnographers working in HCI are likely to have an allergic reaction to Newell and Card's (117) proposal.[12] And what will upset them most is precisely the divide and conquer strategy. The basic premise of the divide and conquer approach seems to be that it must be possible to define a medium of exchange by means of which the disciplines can transfer concepts, theories and findings. Although Card and Newell do not say so, one can imagine such a medium to be a general scheme into which the various disciplinary conceptual structures can be translated. In as much as it is articulated at all, ethnography's epistemology takes an entirely different tack. It begins in the denial of the realism which underpins divide and conquer. Putting it another way, for most ethnographers, the social, psychological, biological, and even the physical worlds are theoretically constituted and hence the disciplines listed may well be incommensurable. To them, it is simply not sufficient to suggest that science can carve 'the world' at the joints and allocate the various portions to the currently available disciplines.

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[11]   The classic instance of this is Oscar Lewis' analysis of Tepotzlan originally studied by Robert Redfield [Lewis 1951 (115)]

[12]   The most explicit and lengthy discussion of Card and Newell I am aware of is by Carroll and Campbell (1986) (118) Though they do not discuss it from the ethnographic viewpoint.

This response is not a rejoinder to *the content* of engineering style theories, although this is how it is often interpreted. It is rather about the possibility of theorising in the mode for which Newell and Card (and as I say, many others probably [see Marchianni & Sibert 1991 (119)]) are arguing. What are at odds are epistemologies and what we have is an epistemological impedance mismatch. In such circumstances, nothing is gained by ignoring both the fact of difference and the motivations producing it. If they do not find Newell and Card's vision attractive, ethnographers have got to say why, and what they would like to offer instead. This makes the central points at issue here *philosophical* in nature, since it is the epistemologies expressed in and represented by the studies to be carried out which are at odds. As a consequence, a resolution of the differences will have to go much further than an acknowledgement that the research approaches or foci are "complementary", with ethnographers giving paramountcy to the particular. Both the ethnographer and Newell and Card (should they wish to rebut the claims) will have to engage with the arguments of people such as Putnam 1994 (120), Quine 1981 (121) and others who are currently struggling to articulate and resolve what the issues and choices are.[13]

This is another deep question which has to be argued out in the open if only to lay out how extensively it is connected to some of the questions I discuss in other sections. We need to understand on what epistemological grounding ethnography wishes to stand and how that relates to the other approaches to which the HCI general community holds. Unless and until we do, the tolerance of ecumenism is likely to mask the impossibility of integrating ethnographic research within more experimentally oriented and engineering inspired approaches. Or rather, it is likely to leave such integration to the wit and ingenuity of the individual researcher. And that would be the worst of all possible worlds.

X   Does it all add up?

If from the outside it is hard to see how the theories knit together, and if it is not clear if we have a method in the more conventional sense, it is not surprising that the most common reservation expressed about ethnography in systems design concerns the apparently idiosyncratic character of the findings. Not only does each ethnographer have a distinctive point of view; every situation is different, with the differences being complex in manifold ways. Or, at least, that is how it appears. Since the point of ethnography is to ground generalisation in the detail of the specific, it is only with the greatest reluctance that when pressed for generalisations which might be of value to design, ethnographers offer statements such as these:

1.   Working life involves collaborating with others and this has significant (but highly context-dependent) implications for work design and the technology which supports it.

2.   The organisational settings in which work is carried out are complex beyond description and in ways which even those who inhabit them don't fully grasp.

3.   In the end, it all comes down to managing the details. Working life is experienced as a plenum of detail.

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[13]   See Leplin (1984) (122) for a reasonable survey.

The problem is that such "generalisations" satisfy no-one: not the ethnographer who finds them empty nor the designer who finds them unusable. Here is a comment from one set of not unsympathetic critics.

> " .... detailed design guidelines are typically absent from the standard format of CSCW conference or journal papers, which tend to offer a description of a case study, followed by an 'implications for system design' section at the end of the paper in which a number of highly generalisable or semi-intuitive recommendations are made." [Plowman, Rogers & Ramage 1995, p 312 (123)]

The most frequently mooted way of rebutting this criticism is to offer systematic ethnographic generalisations. What the designer needs, it is said, is generalisation or generality. Generality is held to reside in summarisation or abstraction and moves beyond the specifics of the individual case. But ethnography, with its emphasis on particularity, is often said to resist this. Summarisation and abstraction require the use of notations or formalisms of some kind. And in defining the importance of the particular, ethnography is often accused of rejecting formalisation. Nothing could be further from the truth. Many ethnographic works are deliberately formal in their structure. What they do not use very often is symbolic notations, and certainly not mathematised ones [though Hage & Harary's 1984 (124) work is an exception here]. Take, for example, Levi Strauss' analysis of myth [Levi Strauss 1988 (125)]. No-one would deny, I hope, that Levi Strauss was pursuing a formalisation of what he explicitly refers to as the cultural code. Moreover, taxonomies are formalisations. And ethnographers are the taxonomes *par excellence*.

The problem is that the issues of summarisation and formalisation have become distractions. They lead us away from the real issue to be resolved. This is the examination of what exactly the criteria for evaluation should be. How do we tell the difference between a good ethnography and a poor one? To what criteria do ethnographers hold and how do they relate to the ones designers favour? Once we have both sets out in the open, and this may be an unsettling exercise for both ethnographers and designers, we may have a better chance of making progress. If abstraction and summarisation are necessary for design, then we know what kinds of ethnographic representations to recommend. But, were we to do so, any interest in "the user's point of view" is likely to disappear. And that, you will remember, was one of the attractions of ethnography for design in the first place.

In the end, it is a matter of being clear about and ordering the criteria of evaluation. If summarisation and abstraction are vital to design, ethnography can provide them but at the price of giving up on something else.

XI Whose Side Are We On?[14]

Classically, the ethnography has eschewed intervention in ways of life though, of course, it has always been deeply engaged by them. The deconstructionism mentioned earlier has led to considerable revision of this stance [Hameda & Sibley 1994 (127)]. Even so the kind of involvement which the most committed anthropologist wants is often orthogonal to that required by the design sciences. Most designers are not motivated by what I will loosely call "social engineering". By and large, they do not

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[14] I've taken this question from Howard Becker's famous paper (Becker 1970) (126)

see their role as changing the *fabric* of working relations within which the technology they design is to be deployed. Usually they argue for this by drawing a line between technological and social/organisational effects. They clearly feel themselves responsible for the former. The latter are out of the designer's hands.

Those who argue for commitment on the part of ethnography in design often assert that the distinction between the technology and its social relations cannot be sustained. When designing technology we are designing social relations [Suchman 1994a (128), 1995 (129)]. In addition, they express the conviction that since the design process is fundamentally about intervention to create change, it must be steered by value-orientations. If the values of the users of the technology are not dominant, then those of others (often those in whose interests it is to exploit or control the users) will be. On this view, technologies are drenched in value and hence their design motivated by assumptions about value legitimation. This makes design (or so it seems) inevitably political. All design is design in/for someone's interests. These interests construct what the technology "is". They also construct what the activity the technology supports is or how it can be represented [Suchman 1995 (129)], though the case may be made in either of the 'strong' or 'weak' versions outlined above.

The entanglement of the politics of design with the social construction of technology has its roots in a standard analytic move in the sociology of knowledge [Mannheim 1982 (130)]. Its use has created a mare's nest which will only be unravelled with some difficulty. As Richard Rorty 1991 (131) has pointed out, these same ideas have proven almost irresistible to those whom he identifies as "The American Cultural Left" and which he attributes to a fascination with (and uncritical acceptance of) the philosophical and social analyses of Michel Foucault. Such analyses Foucault describes as "archaelogies".

> ...(T)he archaelogical description of discourses is deployed in the dimension of general history; it seeks to discover that whole domain of institutions, economic processes and social relations on which discoursive formation can be articulated; it tries to show how the autonomy of discourse and its specificity nevertheless do not give it the status of pure ideality and total historical independence. (M. Foucault, The Archaeology of Knowledge , p 165 (132))

For Foucault, discourses (forms of argument), especially philosophical discourses, are not to be grounded in pure reason ("ideality") as the enlightenment "myth" would have us believe, but in social interests. For Foucault, that makes all such discourses ultimately about power.

On Rorty's view, this "politicisation" has occurred in literature, the humanities and of course, science. The problem is that the claim that ways of thinking serve social interests is a reflexive (philosophical) *argument*. It too must serve social interests. But if social analysis serves social interests, how do we choose the social interests our analysis should serve? Can we? Should we? The same questions arise in the application to design. Can we choose our politics for design? How?

These may look like straightforward questions but they are not. On the one hand, if the argument is pursued relentlessly to its conclusion, it becomes corrosive. It leads to the abandonment of reasoned debate because any attempt to use "Reason" as the basis for such decisions must, on Foucault's analysis, itself serve (existing, exploitative) interests. What, then, could be the bases on which to evaluate arguments, conclusions and the decisions derived from them? On the other hand, if we allow the possibility of reasoned debate, once we look beneath the surface the issues reveal themselves to be

age old and extremely difficult ethical and moral dilemmas [Nagel 1991(133)]. If, as Suchman (1994b (128)) explicitly reflects, one works for a large multi-national company, helping to design systems by means of which it creates profits, whose interests should be put first? The Company? The users? The discipline? What obligations do we have to each and how are they calibrated?

These are tough questions to think one's way through. The easy way out would be simply to divide the world into camps and then claim the moral high ground by joining one or other side. But that would convince no-one. Such attempts to cut the gordian knot of difficult argument rest ultimately on an appeal to sentiment. As Nagel 1986 (134) points out, that is likely to be in vain and provide only the most fragile basis for consensus.

These problems and issues swirl around in the discussion of design methods. Many [e.g. Greenbaum & Kyng 1991 (13)] believe the only response is to grasp the nettle of commitment. Yet, as we have just seen, the bases for making the choices (choosing which nettle to grasp) are not themselves uncontested nor uncontestable [Rorty 1982 (135), Rawls 1971 (136)]. In the end, the direction individual designers choose to take are likely to be independent of their decision to draw upon any deeply ethnographic approaches. To be sure, certain ethnographies and ethnographers may sit more easily with certain design outlooks, but the determination of that outlook is to be found, either explicitly or implicitly, in the design philosophy (broadly construed) to which the designer holds.

XII Panglossian Ethnography

One argument which often surfaces in discussions of ethnography, but is only now and then articulated clearly enough to warrant discussion, is the claim that ethnography tends to be inimical to change, or at least to design-induced change. Probably the most reasoned exposition of this claim is to be found in Grudin and Grinter 1995 (137). In their examination of it, Grudin and Grinter find this "conservatism" to be a corollary of ethnography's fine tuned attitude to the detail of social practice. Any intervention, or so the ethnographers seem to argue, is bound to be devastating for existing practice. For Grudin and Grinter, surely some disruption is worth the eventual gains to be made?

This argument about the resistance to or incapacity to provide for change is interesting if only because it is, in many ways, a re-running of a very similar debate which raged over structural functionalism in social science many decades ago. And, like the previous version, it is just as muddled. The central confusion which it is vital to straighten out is that what ethnographers say about their ethnographies should not be confused with what their ethnographies say. As a result the question at issue should not be "Why are ethnographers loathe to endorse design-induced change?" (if they are) but "Does ethnography *necessarily* militate against it?".

We can begin to piece together a preliminary way of sorting out what an answer to this latter question could be if we look at Suchman's reply to her critics (one set of whom are Grudin and Grinter) [Suchman 1995 (129)]. In brief, what Suchman refuses to endorse is the inadvertent butchery of fine grained and complex practices solely in the name of technologically driven change. Moreover, and here her argument turns to the politicisation which we have just been discussing, she insists that it is not the fact of change which is in question but "what" is changed and why; and in whose interests; and at whose behest.

On Suchman's view, then, the objectionableness of change is very much to be determined case by case and as *a result* of reflecting on the import of ethnographic materials among other matters. Because of the imbrication of social and technical relations as well as their attention to latent dysfunctions, in some people's eyes ethnographers are, in such circumstances, likely to be overly cautious.

But even if they are, that says nothing in general about ethnography itself. Indeed, what Suchman is making partial use of is precisely the framing move at the heart of all ethnographic analyses and an understanding of which will lead away from the muddle. If we set the context for the ethnography at the level at which many ethnographers feel most comfortable, we will find they are almost obsessed with change of one sort or another. In picking their way through the minutiae of routine action, prominence is (endlessly) given to the innovative, the *ad hoc*, and the unpredictable rife in the workplace and elsewhere. Change, here, is the very stuff of ethnography.

If we move to another level of context, that of social structure, not only is the scope enlarged socio-spatially, socio-temporally it is too. In their concern for the mediating character of social institutions, ethnographers are apt to point to the *relative* stability of social relations. But this stability *is* relative. Change is still part and parcel of the phenomena under analysis. The point for the ethnographer is the exploration of how change at the structural level gears with change at the level of practice. Where are the sticking points? The lines of tension? The zones of free flow? And how are they to be accounted for?

Ethnographies, then, are representations of changes brought about by the interaction of different force fields operating at different levels and the myriad unintended and unnoticed ways in which the consequences and functions of such changes play through. When called upon to draw out the implications of their analyses for explicitly engineered change, it is ethnographers not their ethnographies who are cautious. And, given our experience of explicitly engineered change, perhaps we should be grateful they are.

XII   Conclusion

Ethnography has been drawn into the circle of design.[15] And, while things will never be the same again, we should not expect future studies to have the same impact which Suchman's did. The ideas which she promulgated, the orientations she stood for, have become part of our design *lingua franca*. The task we now face is to try to understand what the incorporation of this and other modes of social science might mean and just what their investigative methods can contribute. I do not believe this is a straightforward, quick, or painless process. Resolving the questions I have just outlined, if they are taken seriously, will be none of these. Yet they are just the beginning and in some ways the easiest to grapple with. There are many other only marginally less relevant issues which HCI itself has to deal with, the outcomes of which will have profound implications for the role which might be offered to ethnography. Not the least of these are the claims which the designers of interactive systems will be able to sustain for the value *they* offer. The key question here may turn out to be how far interactive systems have contributed to creating the productivity paradox in the first place [Kraut, Dumais & Koch 1989 (139) and Landauer 1995 (140)]. But we now move away from the local concerns of investigative techniques and forms of theorising to Corporate strategy, new management models, the economics of high technology (what has been called technonomics), and the much discussed shift of Western economies from a production to a knowledge basis.

All of these issues I have touched on in the last sections may seem a long way from the concerns of designers and the even more prosaic obsessions of ethnographers. But they are not. The turn to the social, together with the interest in ethnography, began with a disillusionment with prevailing ways of understanding use and users and with ways of drawing on such understandings as there were through to the design of systems. This still remains the challenge. There is no doubt that the arrival of social scientists in the systems world has done both them and systems researchers a great deal of good. Thus far, though, with the notable exceptions mentioned in Section VI, this seems mostly to remain in the form of consciousness-raising. How far we can build on the leading exemplars to develop a real, practical, design-oriented social science remains an open question. Unless and until the fundamental issues I set out above are openly, collectively and thoroughly addressed, that's how things will stay.

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[15]   Many have contributed to this but none more than John Seely Brown whose vision and perspicacity has been a resource for many, including myself. (Brown 1991 (138), Brown and Duguid 1992 (81)).

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