

# The Limits of Ethnography: Combining Social Sciences for CSCW

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## ABSTRACT

This paper addresses some of the divergences between social sciences, and proposes the development of hybrid forms of participation in CSCW. It offers a critique of the theoretical isolationism of some ethnomethodological ethnography. It reviews the prospects for interdisciplinary collaboration, and seeks to motivate it with some “core propositions” which expose the inescapable character of the problems (although not necessarily of the solutions) which are “owned” by different disciplines. It illustrates hybrid forms with discussion of some issues in two areas: the cognitive versus the ethnographic; it further describes the politics of participation.

**KEYWORDS:** CSCW, interdisciplinary relations, ethnography, ethnomethodology, cognitive science, participative design, distributed cognition.

## INTRODUCTION

Issues of interdisciplinary work are among the most fundamental in CSCW research. Some almost go so far as to define it [20]. But a consequence of this interdisciplinary nature is an ever-broader range of voices offering to speak to designers’ problems. Older paradigms have continued; in addition, newer ones have been overlaid on these older ones to create a bewildering diversity of disciplinary perspectives. Software engineering, information systems, activity theory, participative design, cognitive ergonomics and cognitive engineering, linguistics, linguistic philosophy, soft systems, management science, organizational studies, and ethnographic sociology and anthropology can all stake a significant claim, and there are certainly other claimants besides. While the relations between the social and computer sciences have properly received the most attention [1, 7, 8], relations within the social sciences are also problematic. As Graham Button [workshop contribution]

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CSCW 94- 10/94 Chapel Hill, NC, USA  
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has aptly put it, when designers “open the box” of the social sciences, they find their prospective partners locked in desperate struggles with each other, both between disciplines and within them.

The struggles within the social sciences concern long-standing foundational matters of philosophy, theory, and method, and there can be no prospect of “disposing” of them as a sideline along the way to a participation in design. But the field of CSCW has matured to the point where it can no longer ignore these issues. This paper argues that a practical orientation to design gives them a particular cast, which it seeks to explore.

I start with a consideration of ethnographic approaches, and the distinction between ethnography in general and ethnomethodological ethnography in particular. The reason this is important is that ethnomethodology sets for itself a strict agenda which separates it in certain ways from most mainstream social science. It insists on a rigorously descriptive rather than theoretical program, or an explanatory one (in the sense that most social sciences would understand it). This lends it its strength in producing rich descriptions of work in context, but creates particular kinds of difficulties in meshing its work with that of other disciplines.

I review these approaches briefly, suggest some difficulties with the “purist” approach, and advocate the development of hybrid forms within the social sciences for CSCW design. I argue that the time is ripe for these hybrid forms, and I try to ground them in practical motives for collaboration by developing some “core propositions” which reveal the inescapable character of the problems (although not necessarily of the solutions) which are “owned” in some sense by different social science disciplines. I illustrate this with discussion of some issues in two areas: the cognitive versus the ethnographic, and the politics of participation—engaging, in passing, the debate on THE COORDINATOR rekindled by Suchman [33].

## ETHNOGRAPHY OR ETHNOMETHODOLOGY?

Ethnography has achieved high visibility as a contribution to CSCW. While “ethnography” as a term strikes a useful contrast to traditional methods of requirements capture,

within sociology and anthropology themselves it denotes rather little. It marks a distinction from quantitative approaches to social science and carries with it a commitment to a period and degree of immersion in the social setting being studied that is sufficient to reach a qualitative understanding of what happens there. These are important matters, but beyond this, ethnography can be put to the service of virtually any theoretical school: there are, for example, functionalist, structuralist, interactionist, Weberian and Marxist ethnographies [cf., 17].

There are also ethnomethodological ones. These have dominated in CSCW at least since Suchman used this approach as the basis of her *Plans and Situated Actions* [34]. Recent ethnomethodological studies of work settings have also been influential in the field, such as that of Heath and Luff [18], which describe the collaborative work practices and procedures through which communication and coordination takes place in a London Underground line control room. It has become a shorthand or simplification in CSCW to speak of ethnography when in fact what is often involved is ethnomethodological ethnography.

Nothing which I argue in this paper constitutes a proposal to replace ethnomethodological ethnography with other varieties. Rather, I am concerned with its borders: with the connections that can be forged with other contributions to CSCW design, and some limits which the classical ethnomethodological program may place on these connections.

### ETHNOMETHODOLOGICAL CHOICES

The founding works of ethnomethodology [e.g., 13] and introductions to the field [e.g., 31] explain that most mainstream social science sees itself as proceeding theoretically--that is, by proposing concepts and logical relations between them which abstract successfully from the state of things in the real world and help to explain them. Ethnomethodologists (in the wake of phenomenologists) claim that attempts to do this fail for a variety of reasons. One is that the theoretical approach has to assume that the social order is an external "given" which people "enact" (although this is not incompatible with them doing so voluntarily). Yet attempts to explore this (by, for example, trying to provoke the breakdown of social order in interaction) reveal that people in fact construct their social order in radically creative ways in the very process of acting together. Because of this, attempts by "conventional" social science to explore the empirical implications of theoretical accounts fall down, because they are attempting to operationalise on the back of a set of "common-sense" entities which are, in practice, being constantly remade in significant ways by members of the social order. They therefore do not exhibit the constancy and stability that is required for them to be investigated (e.g., measured) in ways consistent with the theoretical approach. (This is, of necessity, an absurdly simplified account--for a discussion of varieties of ethnomethodology, with particular reference to the cognitive, see [26]).

That is part of what is meant by the "indexical" character of the terms and concepts employed in common-sense talk and interaction: that they are radically contextual and cannot be fully specified independent of the circumstances of their use. Garfinkel proposes that rather than seeing this as a weakness, it should be recognised as the practical achievement whose properties make everyday sense-making possible. Indexicality is the triumph rather than the shame of ordinary language. Yet the concepts which conventional social science tries to deploy in its theories--and which it therefore requires to exhibit stability--are themselves, and/or rely for their sensible application, on irreducibly indexical expressions (and actions).

The ethnomethodological program proceeds by making certain strategic choices about how to do sociology. In the relationships between theory, research, and phenomenon, it sees two possibilities. First, it persists with attempts to theorise on the basis of concepts which cannot be adequate to their phenomena. Second, it follows the phenomena (pursuing the data) wherever they may lead [31]. Mainstream social science chooses the former; ethnomethodology chooses the latter. Hence, one of the most distinctive claims of ethnomethodology: that it is not driven by theory or explanation but by the stringent discipline of observation and description. In this it separates itself from "mainstream" sociology and from the other (in effect, from all) "Galilean" social sciences.

It is for these kinds of reasons that ethnomethodology can sometimes seem so recalcitrant to those who turn to it for enlightenment, in terms which would be familiar to them, about topics which are important to some constituency. For any given question, the approach is to ask: What methods do practitioners within the domain use to give sense to, answer, and resolve that question? So, for example, the question, "What functions are embodied in these work practices?" will be answered by exploring the methods members within the setting use to find functions and to attribute them to practices; and the question, "What is the cost/benefit of this technical-cum-organizational change?" will be answered by exploring the grounds on which members within the setting identify, allocate, and assess costs and benefits. In doing this, they will certainly expose many ways in which the questions as they were posed are trickier and more ill-formed than their posers imagined them to be--but most probably not to the point of their agreeing to drop the original form of their interest in such questions altogether. It is therefore fairly arbitrary how pertinent the results will be for the problems of practitioners within that constituency *as they see them*; or, to put it slightly differently, how much and what kinds of further work are necessary to make them pertinent in those ways.

### Documentary Method

Because people constitute the social order in radically creative ways, this means that they must themselves be involved in a constant process of analysing and exploring it, so that they are able to act successfully--intersubjectively--in relation to others for everyday practical purposes. Two

things follow. First, the difference between members acting in society and sociologists studying it is not so great--both are involved in the analysis of social order, although for different purposes. Second, this provides a very valuable resource for studying the constitution of social order, namely the methods which members themselves use in everyday social action. This indeed is how the term ethnomethodology was derived.

Since the whole complex patterning of social activity--its "structure"--is seen as deriving from the practices of members themselves and not from the enactment of some underlying scheme which they have simply internalised, then they must both generate and know how to absorb the requisite information. For this to work, members must actively make the social organization of their activities "accountable" to each other. This is a powerful resource for the observer, since it can be followed "in the doing." For those sufficiently geared into the setting, this "signalling" is a publicly available matter in which any competent member can engage. At its most basic, this means talking about what is going on in ways that are specifically sensible for the setting: "that X is late again" indicates (with astonishing economy) a familiar exception to the expected pattern; while particular handling may be required, it will be in (some variant of) a way that has already been practised. "War stories" about events may be structured around (and hence display) the features that make them relevantly remarkable, in terms of the play of understandings and misunderstandings, expectations met and missed, strategies of repair, and so forth: a particular combination of circumstances might, say, turn what would routinely be easily distinguishable events into confusable ones on this occasion.

Through their social organisation, activities are "corrigible" in mid-flow: an apparently tangential remark or query may be seen to be a tactful way of calling a colleague's attention to something that has been overlooked; an apology offered, however fleetingly, may indicate a departure from the normal course of events that requires some additional work for a colleague to cope with or repair. Where an activity attracts particular attention from colleagues, then this is a resource for investigating the character of its "exceptionality" (this is somewhat comparably to Bittner's [2] discussion of "stylistic unity" in organizations). There are also "breach experiments"--exploring the characteristics of a setting by deliberately acting in ways which provoke a breakdown in its normal course. (In the context of our work on air traffic control we did not think these very desirable.)

It is on account of the documentary method that the "rich descriptions" offered by ethnomethodology can be claimed to be systematic and not arbitrary ones. As is well known, this is best developed and achieved in conversation analysis, where small segments of data can be recorded and richly transcribed for exhaustive repeated study. (The analysis of work equivalent techniques, such as video analysis, are rapidly developing [25] but are still far less secure.)

## BREACHING

I find some difficulties with various steps in these arguments. What is more to the point, however, is that many ethnomethodologists also seem to find problems in adhering to the program. There is no question that it yields many tremendously interesting and useful insights, but how valid is its claim to be regarded as a purely *descriptive* agenda? I argue that it is "honoured in the breach" in at least three respects. The first is that the description is "just descriptive" *only given* the choice that has been made. It is equivalent to instructing the researcher to describe those and only those aspects of the setting which can be used to demonstrate its self-ordering properties, and to organize the description such that it emphasises those properties (for example, concerning how to account for the phone company's recommendation to let the phone ring ten times, see [29]). It does, therefore, specify not only how to look but also what to find.

The second concerns the interpretive character of what are supposedly "first order" observations. For example, in their treatment, also of telephone conversations, Sharrock and Anderson [31] discuss that when a friend calls up for some particular purpose they often do not want this to be recognised, and will introduce the reason into the conversation at some later point as though it were an afterthought. But it is surely stretching matters to hold that this explanation, however satisfying and plausible, is actually provided in the data. It rests, rather, on theorisations of how people are liable to feel when they call their friends for instrumental reasons. Thus, it has shifted from a statement about how people handle conversations to an interpretation of how they handle their relations. There is a "residual move" of explication and interpretation which goes beyond what is directly to be found in the materials and effects some kind of transition to concepts, categories, and arguments which relate to sociology's concerns as more conventionally conceived. Hence it is arguable that rigorous adherence to the data does not fully succeed even in its most developed heartland, conversational analysis. When it comes to work, the problems are much greater due to the difficulty of recording work activities with comparable accuracy and accessibility, and due to their open-ended and multi-faceted character.

Third, there is the apparent willingness on the part of ethnomethodologists to intersperse "second order" observations in their accounts, which connect them to other kinds of sociological interests, or indeed to ethical, political or personal interests. For example, in her discussion of the language/action perspective, Suchman [33] refers to one of Sacks's published lectures in which he uses his analysis of categorization devices specifically to interpret the categorizations produced by teenagers ("hotrodders") as revolutionary acts of resistance against their assignment to a place in a system of categorization that is not of their own making. She goes on to argue, referencing the work of Foucault, that the categorization devices of speech act theory have been taken over, in THE COORDINATOR, as a discipline for organizational communications (more on this

below). In these and many other instances, the work of ethnomethodology is recruited into the *explanatory* “sense-making” self-indulgences of sociology; it would lose a substantial part of its interest, for many of the researchers themselves as well as for their readers, if it was not. Clearly this sows some confusion about exactly what lies inside and what outside the paradigm. It makes it much harder for those advocating a “purist” ethnomethodology to reject, or continue indefinitely to postpone, an engagement with the more conventional concerns of social science.

### PURITY OR DANGER?

It would seem that ethnomethodology now has on offer a fork of two possible routes. The “purist” route is to try ever harder for a proper, painstakingly empirical sociology which is fully grounded in the phenomena that it studies. For this reason (and not because of any counterposing of “micro” and “macro” perspectives), it will have a very concentrated focus of attention, and may fail even there. The alternative is to combine in some form with the messy, contestable, provisional, iterative scene in which the formation and deployment of concepts struggles to carve workable entities and relations out of the seamless flow, and to cope with its simplifications, indexicalities, inconstancies, and *a priori* theorising; all for the prize of having *something to say* across a wide range of social and sociological concerns.

Most observational methods have (or claim) a circular or dialectical relation to theory, in that it is the relation of observation to theory (not necessarily a relation of simple consistency) that lends a pattern of observations its plausibility and rescues it from the arbitrary. Ethnomethodology seeks to deny itself this recourse or comfort, arguing that nothing prevents this from resulting in a collusion between theory and observation in arbitrary fictions. If, as I have suggested, this strategy does not wholly succeed, then it may be helpful for ethnomethodologists and others to consider explicitly its relation to other purposes within social science, and to other bases of observation.

It is evidence of the clarity and sophistication of ethnomethodology that it directly foregrounds its exercise of choice in philosophical and methodological direction. It is typically accepted that other choices are possible, and that a return in some form to the classical theoretical concerns of sociology is a reasonable long-term objective, once an adequate foundation has been put in place. Much hangs on the integrity with which ethnomethodology can maintain this distinction: its refusal to tender “advice” about its objects of study in the ways that its practitioners might expect, its refusal to generalise or abstract from its findings, its refusal to engage in theoretical speculations, its refusal to miscegenate with other forms of social science.

Although the problems raised about the relationship between theory, method and data, and about the constancy of the “object” of study are very real and important ones, the consequences of accepting this as a total bar on the

possibility of any further inquiry of this character are--to borrow a term from legal discourse--“too onerous.” It disqualifies us from being able to speak directly about “preserving” the activities in new forms of work, or about the ethical or political character of actual or proposed changes. There must be some middle way, although it will indeed involve accepting that there is a clear drop in standards in the relation between evidence and argument, together with a shift in the nature of the objectives, when a certain point is passed.

Sharrock and Anderson claim [31] that it is ironic that sociologists sometimes try to incorporate ethnomethodological studies into conventional sociology, since this completely misses the motivation which underlies the work. The question, however, is whether one may fully understand the motivation but choose nevertheless to recruit the results for “conventional” theorising (to say nothing of the equivalent irony that many who affiliate with ethnomethodology themselves choose to do so). To what extent and in what ways are the fruits of ethnomethodological inquiry available to or incommensurate with work of other kinds?

There can be no doubt that brilliant work will be done by continuing to pursue to its uttermost possibility the object and doctrine of faithfulness to the materials. The ethnomethodological “strong program” is one of the most impressive in current sociology, but no one should look to it for quick results, in the sense of quickly building out a sound empirical basis for the [reconstructed] concerns of other areas of the social sciences. But there is also, I believe, worthy and important work to be done in pursuing and exploring hybrid forms, and I try to develop in the second part of the paper something of what this would mean. For this “fudge” to work, however, there must indeed be compromise on *both* sides, whereas at present ethnomethodologists are fully justified in their critique that most mainstream social science simply ignores these problems.

### The Distinctiveness of Design

These questions of methodological choice receive a particular twist when taken in the context of systems design. For academic purposes, one is entirely free to pursue theoretical and methodological choices rigorously and to their logical conclusion: endless qualification and elaboration is possible, and indeed appropriate, as is the maintenance of a rigid boundary demarcating one’s concerns. But one of the virtues and interests of systems design as a practical endeavour is that it creates a constrained arena within which some set of compromises must be worked out. While it should not be taken as an excuse for relaxing, nor allowed to obscure the stunning advances that are sometimes achieved, it is helpful to recognise that design is not an exact or an absolute matter, but is about simply “doing one’s best” on a range of dimensions--functionality, time, cost, etc.--and in relation to large numbers of divergent and conflicting objectives [14]. In a messy and imperfect world which is already full

of tragedies and ironies on every side, systems developers build on the past in the hope of coming up with something better (for a reasonable expenditure of resources) than was available before. Borrowing from H. A. Simon [32] we could say that design, as with almost every other practice in the real world, is to do with “satisficing”—not with achieving perfection, but with attaining a set of compromises and outcomes which are “good enough” for the purposes and “the best that can be done” in the given context. All kinds of systems are going to be designed willy-nilly, with all kinds of imperfections and compromises. But the reverse is also true: when we are concerned with matters in the real world, theoretically-based critiques are simply not a sound enough basis for rejecting contributions which might be useful. Too many of our confidently-held essentialisms have fallen away in recent years for anyone to be satisfied, on a theoretical basis alone, that they have either vindicated an approach or definitively demolished a competing one.

I believe this raises an interesting dilemma for ethnomethodology. Sociology’s purposes are self-defining and self-pacing (which is not, of course, to say that there are no external influences on them). The *fact* of a system and its time frame (if not its detailed content) are externally given, in a “satisficing” context, in which the contributions of many different parties must be made to work together. What then is the purpose of treating the ethnomethodological analysis of work for system design as a hermetically sealed endeavour which brooks no compromise? What one chooses not to do for sociological purposes, one may be forced to do for design.

There are obvious differences between an ‘adequate account’ for the purposes of social science and one for the purposes of design. Sociologists’ purposes will typically be such things as to evidence or refine a general principle (e.g., that the orderliness of activities is produced in and through the actions of members, or that labor process changes are driven by imperatives of capital accumulation). Alternatively, it may be to identify a particular kind of process or mechanism in the work setting (e.g., intersections of class, gender and ethnicity in the allocation and definition of jobs, or the place of trust in enabling the flow of work). But having once “captured” such instances (provided, if you like, an “existence proof” for them), it is not a natural part of sociology’s interest to catalogue them exhaustively for a given work setting and to identify minutely and in their totality the flows of activities which they make up. Yet this may be precisely what the software engineer needs as a basis for requirements capture. Hence a “natural” expression of ethnomethodology’s interest is to show, for any given work setting, its self-organizing character, and the ways in which parties to that setting will collectively “bend” the resources to their purposes as best they can. It is far less natural an expression to seek to judge the adequacy of alternative sets of resources, even less to “invent” a better-suited one.

One recourse is to abandon any such questions to the designer, having contributed to the “pot” key additional (and previously invisible) knowledge of what is salient to the organization of the work setting. The designer, or the design team, is saddled with the responsibility for the system and so this must undoubtedly be where all final resolutions are achieved. Designers bring to the process diverse kinds of technical knowledge, a range of techniques, and a wealth of experience. But they have no magic means available to them for reconciling orthogonal perspectives or working through the detailed consequences of social scientific studies. It seems odd to impose the entire responsibility for the redesign of work on systems designers while those whose speciality is supposed to be the analysis of work run for cover.

In practice, ethnographies of work in CSCW are now sometimes concluded with a discussion of design implications, but these are typically reticent and show a marked, if understandable, change of level by comparison with the rest of the research. One might describe this as a kind of “covert theorising.” Because it is not supposed to exist, it cannot be acknowledged, and so cannot surface amid the fray of other theoretical contestation and debate. I believe this is evidence of the need for further hybrid forms of interdisciplinary work for design. Of course, this equally applies to other disciplines. If they allow themselves to give the “satisficing” of a successful design priority over particular disciplinary concerns, then they too will need to suspend various of the “necessary fictions” through which their boundaries are maintained and imperative calls on their attention are contained. I suggest in the conclusion that the gains from this are not merely pragmatic ones.

#### HYBRID FORMS

There are various reasons why it may now be quite a suitable period to try to forge such connections. For the social science researchers who are engaged in questions related to CSCW design, the usual perception from the heartland of their respective disciplines would be that they are engaged in an applied field of work. Despite the dubious validity of the distinction, a classic view of these relationships is that bodies of theory and of well-established knowledge in the core of disciplines will strongly determine the way that these are applied in various practically-oriented fields. In practice the relation is far more complex (with respect to HCI see, for example, the contributions to [6]). In particular, I want to suggest that the relationship between core theory and application in different disciplines is variable over time.

In periods of theoretical confidence and coherence, the relationship is a strongly directional one, and application fields tend to be subordinate within a discipline. Indeed they will always *tend* to be subordinate, but in these conditions that subordination will be much more effectively policed. In periods of theoretical disintegration and hesitancy, this tie is much weakened, and it can even be the case that application fields move to the heart of development within a discipline, and disciplinary boundaries are much more open—not only

with regard to “applied” work, but to empirical research and to theoretical influence from other disciplines. I suggest that in a number of areas of social science we have recently made such a transition, and that this is having substantial effects on the nature and prospects of applied, interdisciplinary work.

Over roughly the past two decades, many of the social sciences now contributing to CSCW have enjoyed periods of theoretical confidence and coherence. So, for example, cognitive psychology was harnessed to the modelling of user requirements. Systems theory and cybernetics developed optimised models of processes. The triumphs of structural linguistics in the 1970s helped to bolster the view that “structured communication” could be modelled and supported. In sociology (although without at that time any thought of *contributing* to systems design), Marxist-derived labor process theory offered to explain both the *uses* to which computer systems were put, and developments in the division of labor *within* systems development, such as software engineering; restructuring theory and world systems theory, at the same time, offered to explain the changes in the global economy brought about by the harnessing of informatics.

While that confidence reigns, there is a strong directionality to the relationship between core theory and the field of application. Under these circumstances, applying an approach in an interdisciplinary field means bringing those theoretical strengths and certainties to bear on it, and seeking homologies with the orientations and certainties of other contributors. Where such confirmation cannot be found, the inclination will be to contest whatever contrary perspectives are encountered. On the whole, then, this makes for a program either of corroboration or disciplinary imperialism.

But what happens when these certainties fall away? In each of these fields, their “strong programs” could be said to have encountered some deep-rooted problems and limitations which cast them into doubt. I have already suggested something of this kind for ethnomethodology, but it is certainly not alone. Cognitive modelling of user requirements has encountered the indeterminacy and lack of hierarchical decomposability of activities, and is now trying to come to terms with the “sociality” of work. Systems theory has similarly encountered the indeterminacy and complexity of work, and the inevitability of initiative and discretion. Structured communication has encountered the difference between heuristics and pragmatics in language use. Marxist sociology has encountered the seeming impossibility of planning a complex economy, and the difficulty of envisaging anything other than a “market” alternative; the difference between the interests of direct producers and the “general interest,” so that a trade union perspective (even an idealised one) is no longer sufficient; and the seeming inevitability of competition in a global context and increases in labor productivity as key agents of development.

Across the board, therefore, the prospects for determinacy and predictability seem to be increasingly thrown into doubt, and with them the prospect for such classically modernist projects as the “rational design” of artefacts, organisations, institutions, political systems, personal relations, or whatever. Under these circumstances, what would previously have been seen as a discipline’s main strengths, its theoretical empowerment, suddenly reappear as essentialist baggage of questionable utility. What are also called into doubt here are the twinned “necessary fictions” through which disciplinary boundaries are maintained: that their own perspectives are both coherent and self-contained as accounts of the world; and that practical ends can be satisfactorily achieved on the basis of them--in this case, that the depictions of the target domain which they produce are adequate for systems design. These are not, in the main, explicitly affirmed. But if it is regarded as unnecessary and unimportant to look over the boundaries of a perspective in order to feel confident of one’s own pronouncements, then they are present by implication and in effect.

### **Some “Core Propositions” for Interdisciplinary System Design**

What are the implications of this for practical design? I believe it gives rise to new puzzles of both a technical character (what is “good design”) and of ethical/political choices (how does one exercise “good will” in design). I return to the latter further below.

I earlier urged a “satisficing” perspective on design, and argue that this can be a liberating perspective for considering who should be involved in design, and how. However, and despite the weakening directionality between core and application which I proposed above, the first instinct of academic researchers when attempting interdisciplinary collaboration is to bring together and try to reconcile their theoretical perspectives and core precepts, so that these can be brought to bear in a “well-grounded” way on a practical field of application. Yet it is at exactly this level that the differences between disciplinary approaches will seem most intractable: as between, for example, the formal modelling of users’ requirements drawn from cognitive psychology, the specification of functional requirements drawn from information systems, and the flexibility and situated character of action in context drawn from ethnomethodology. Researchers will be understandably reluctant to buy into the solutions to design issues proposed from other perspectives (the competing necessary fictions).

One way to approach this is to acknowledge that disciplines are the custodians of certain core perceptions which anyone setting out to achieve success in the design of certain kinds of system would ignore at their peril. However strange some disciplines’ theoretical self-understandings may seem to be, and no matter how much one may wish to contest them at the level of theoretical or methodological essentialisms, ultimately there are certain “core propositions” which relate to particular disciplinary points



of origin; and if one ignores any one of them in designing systems, then sooner or later they will return to haunt one. Another way of putting this is that, even if one is unwilling to buy into any of the particular *solutions* proposed, does one nevertheless need to accept the *problems* which different disciplinary perspectives somehow represent or “own [cf., 27]? It is important to appreciate that these “core propositions” are not advanced as lasting absolutes, as key definitions, or as entailing a particular set of sub-propositions which further delineate each field. Their intent is much more “positional”: to act as a means to express these problems in terms that reveal their mutual necessity, even to the satisfaction of those with no prior commitment to that discipline’s participation (the concept of “position” is taken up further below).

Some candidate examples of such “core propositions” for the social science contributions are as follows:

1. *Activities are socially organized and flexibly situated in context.*

This looks towards ethnomethodological ethnography, and some other forms of work analysis.

2. *Organizations make deliberate strategic changes; these engage highly differentiated interests.*

This recognises “work redesign” and “requirements engineering”—that work and technology arrangements are changed (invented) for instrumental reasons. Hence too there is a necessary politics of design.

3. *Users can easily be alienated from a system for reasons of presentation, interface, and usability.*

4. *Using a system imposes a variety of cognitive loads; these can be assessed only in relation to practice and training.*

These (Items 3 and 4) look to cognitive ergonomics and cognitive engineering and acknowledge interface and training issues, regardless of how secondary they may sometimes seem to the designer of the functionality.

5. *Sociotechnical systems are mutually constituting and adaptive.*

This recognises some of the dynamics involved—that new systems change ways of working in intended and unintended ways; and that users change, indeed constitute, the technology from the moment of getting hold of it.

6. *Users are the ultimate custodians of and experts in their own practices.*

This looks to user participation. No analysis by any method can do the job alone—designing by and with users is indispensable, at the least for settings where a close fit with particular work activities is required.

7. *Organizations and activities are continuously evolving.*

This recognises continuing everyday dynamics of many different kinds, in addition to those which arise from the introduction of the technology itself. It implies that there can be no serious prospect of getting a system “right the

first time,” once and for all (although it certainly remains possible to get it wrong the first time).

Items 5, 6, and 7, taken together, look also to the arguments about the need to shift from a “product” to a “process” focus in design [15]. It is sometimes argued that this is unrealistic for “industrial” design, but if we have grounds for thinking it a more appropriate (and most probably in the long run a more efficient) model, then we should surely promote it. For an industry as recent and as volatile as systems design, and a set of techniques as current as software engineering, their immanent imperatives should not be assumed too easily.

8. *The cost-benefit of systems should be optimised (though not necessarily in financial terms alone).*

This recognises that all of these matters, and what to do about them, cannot be divorced from resourcing.

The key question is how some list of this kind could come to be received by potential contributors to design from diverse backgrounds and perspectives. Are they sufficiently self-evident, and sufficiently independent of a precise disciplinary affiliation, as to form a basis for persuading contributors to reconsider their own work in the light of them? For if it is accepted that a list of “core propositions” something like this does apply, then it will be necessary, despite the difficulties, to find some way of living with the consequences of making them mutually intelligible, since they impinge drastically on each other. At least for some purposes, this may not be just a matter of inclusion or mutual tolerance, but may require the development of “hybrid forms” which differ substantially from their progenitors. It would involve disciplinary contributions which “know their place” in the wider scheme. The alternative is to say which of this list one would be prepared to jettison, and what the costs and consequences would be. Not, of course, that this should be regarded as out of the question—it may well be claimed, from one perspective, that another’s problem (and not just their solution) is falsely conceived and disappears within some alternative formulation. There can be no formula for what kinds of hybrid forms will be appropriate. That can only be discovered through collaborative work. But that will not start unless the reasons why it is needed are clarified and accepted. In the remainder of the paper I seek to explore some of the issues of hybrid forms through a consideration of two sample areas: “the cognitive versus the ethnographic,” and the “politics of participation.”

### **The Cognitive versus the Ethnographic**

As more researchers with psychology backgrounds in HCI become interested in ethnographic methods, so the question of how exactly we should regard the relationship between sociology and psychology as “partner” disciplines for computer science becomes a more pressing one in CSCW.

There are various ways in which this relationship can be treated. First, the theoretical bases of the approaches can be considered, as is done, for example, by Coulter [9] in presenting a wholesale critique of cognitive theory from the

standpoint of ethnomethodology. Second, the implications of the approaches specifically for HCI can be critically reviewed, as is done, for example, in [27]. Third, there are critiques by psychologists in the HCI field themselves (e.g., [5]). It remains very unclear to what extent this represents a development of psychological approaches, or a conjoining of psychological and sociological approaches, or a displacement of psychology by sociology. There is already a significant “politics” of this relationship, and we can expect it to grow. Over the last decade or so the psychological contribution in HCI, cognitive science and human factors has burgeoned into an academic industry, and as yet only a small section of its members--and for the most part those more sympathetic to the shift--have noticed the threat of displacement lurking at the fringes. Currently CSCW enjoys a certain “suspension of disbelief” which is sometimes accorded to newly developing fields which seem to have potential, but we cannot expect this to last.

Despite the criticisms, there may nevertheless be a desire on both sides to collaborate or even a sense that collaboration is necessary; in neither but very little sense of how to do so. This is partly because each approach is inclined to regard itself as generating a “core” account of the activity, with the other handling some specialised details or aspects. So, for example, ethnographers will be happy to leave such matters as screen colours, screen layout, the proximity and spatial relation of different work elements, the memory load to be imposed, etc. to cognitive ergonomists, while assuming that these will follow relatively straightforwardly from the analysis of work in context. Equally, those engaged in task analysis will see their work as identifying the core of the activities being undertaken, with sociological approaches supplementing or modifying these with reference to their specifically communicative or cooperative aspects.

I have considered aspects of the relationship between ethnography and cognitive task analysis in some detail elsewhere [28 Part 1, 30], on the basis of a comparison of our own ethnographic work--on the issues surrounding an electronic replacement for the paper flight progress strip in air traffic control [21]--with a representative of a task analysis approach addressing the same substantive problem [35]. It was suggested that the outcome of the approaches differed in various ways from standard expectations--generating both unanticipated opportunities and unanticipated problems--and concluding that certain kinds of hybrid form might indeed be useful. The possibility that the behavioural analysis might help address the ethnographer’s “completeness” problem was not fulfilled because of the extreme and potentially arbitrary statistical reduction of the data. This, in turn, was necessary in order to avoid the combinatorial explosion of possibilities. Nevertheless, the behavioural analysis generated significant new information and interpretations which were not in the ethnography.

The empirical basis of the behavioural analysis was called into question in various ways. Its categories were derived from domain experts, but that is a process which, although

absolutely critical for the analysis, is entirely hidden. Further, it proved impossible for the authors to interpret the findings in any significant way without appealing outside the behavioural data to some externally derived sense of what, substantively, is “really going on” in air traffic control--though, again, the derivation of these insights is not available for inspection in the way that it would be with an ethnography. Hence the “behavioural” character of the study may be somewhat illusory. While a systematic behavioural recording of the flow of events is potentially a very useful resource for an ethnography, it could only be helpful on the basis of properly situated categories--an interdependent and sufficiently flexible iteration of approaches.

The development of theories of “distributed cognition” [e.g., 22] is of obvious relevance to such a project. In studies such as that of distributed cognition in an airline cockpit [23], Hutchins and his collaborators draw on several strategies, techniques, and concepts, and on substantive and methodological literature, which are familiar to ethnomethodological ethnography. Where their objectives clearly differ is in continuing to specify what they find in terms of individual cognitive properties such as the memory states of individuals and objects and contrasting expectation states. They also engage much more directly info-logical issues and are concerned, for example, with mapping the flow of information availability.

A number of issues would be relevant to further understanding and developing the relationship between these approaches. These include: the basis on which the ethnography is done, the purpose and usage of the mapping of information availability, the sense of intersubjectivity which is proposed, and the meshing between intersubjectivity and distributed cognition. It is also implied that the elements of the account can still be placed within a hierarchical and extensible task structure. It is not entirely clear what the purpose of this would be, how central it is to the approach, and how the issues of combinatorial explosion and lateral entanglement would be handled.

### **Working Positions**

It may be helpful to consider these kinds of questions in two ways. First, in terms of the normal standards of academic debate: the persuasiveness of the arguments and their relation to evidence. Second, in terms of the situations and contexts of those who are party to the issues. In reception studies of the uses made by audiences of media texts, and in cultural studies more generally, it is common to consider the “reading position” of audience members. So, for example, the meanings drawn from the self-same item of television coverage of an industrial dispute (and not just the evaluations made of it) will vary among audience members on the basis of such things as their existing political stance, their personal experience of comparable events and their coverage, and their independent knowledge of the events [16].



I wish by analogy to invoke a concept of “working position” for parties to CSCW design. That is, the sense and the usefulness—one might say the “validity”—of different perspectives and methods will depend on their relation to the position from which different parties are attempting to “work on” CSCW, as well as on their merits in the standard terms of debate. In that sense we might, for example, say “positionally” of the field of CSCW as a whole that it is moving past the point at which “ethnography” is a sufficient term, and finds it both necessary and possible to cope with the question of just what kind of ethnography, and with what consequences.

The concept of distributed cognition could be seen to be working both as a means to identify, for example, flows of information availability, and also positionally to approach sociological issues of the organization of work. So the point about distributed cognition is not only in what sense it involves a “better” or “worse” set of concepts than intersubjectivity, but also that distributed cognition is a vehicle by which those coming from a cognitive point of origin can orient to the sociality of (in this case) work, which those from other backgrounds might collect under intersubjectivity. Of course it does not follow that they amount to the same thing: there are particular costs and consequences, together with a new potential for a dynamic relation between the two traditions.

### **The Politics of Participation**

While ethnomethodology’s documentary method, discussed earlier, is intended to get to the heart of “what is really going on” in a particular setting, there are obvious limits to what it can achieve. In the case of the air traffic controllers we studied [21], they undergo highly selective recruitment, two or three years of training, and specific validation for the individual sectors in which they will work which needs to be regularly renewed. Controllers report that quite short periods away from a sector destroy their capacity to master it, and they also (self-) report that the stress and the concentration required can make it difficult for the more demanding sectors to be managed over the age of around fifty. No ethnographer stands a chance under these circumstances of “completing” an analysis of the work. Even under much simpler conditions, there can be no certainty of interpretation which avoids the need to involve users closely.

But the converse also holds. It may well be asked why, having succeeded in some measure in getting psychologists “out of the loop”, designers should now be asked to tolerate the intermediation of ethnographers [Liam Bannon, workshop contribution]? It is a tenet of ethnomethodology that its findings are only “what every competent member already knows” (although I have, in fact, claimed that the schematisation that is involved goes beyond this). Yet even so, it is evident that these renderings can seem very peculiar when relayed back to those members, who do indeed find a world of difference between what they “already know” performatively and what they could know discursively (for myself, I have found my facility at turn-taking in

conversation considerably debilitated since reading the literature). This means that there is a point to the ethnomethodological intermediation—the rendering of activities that results from it is, for good or ill, substantially different from a collegial “muddling through” of users and designers.

This raises directly the question of the relationship between an ethnographically-informed CSCW and Participative Design (PD). Although also a varied program [see, e.g., 3, 11], the most distinctive elements of PD could be summarised as:

- (1) It is politically committed to workers and direct producers, and to the enhancement rather than the destruction of their skills, autonomy, and quality of working life. It has sought to realise this through working closely with trade unions at various levels, and enhancing their capacity to devise and to achieve alternative technology policies.
- (2) It is action research, intended to make a direct and immediate difference to the working conditions of their research “subjects” and to foster industrial democracy.
- (3) It proceeds by a direct and unmediated partnership between designers and the users of systems, and has devised a range of methods for rapid prototyping of systems in collaboration with users.

While the political engagement of PD is always stressed, some commentators suggest that its main rationale has shifted from democratic participation to effectiveness in design [27]. It might be more accurate to say that the concern with effectiveness has always been there, but that its grounds have shifted. In the earlier formulations, the capitalist context and its labor process implications were themselves seen as the main fetters on development, so that a thoroughgoing industrial democracy was the most obvious way to unleash more effective systems. But this is now part of PD’s problem: as confidence in this straightforward solution has weakened, it is less evident how to replace it. While PD has always been concerned to try to exercise “good will” in design, the decline of trade union power even in Scandinavia means that this is a good will which PD has increasingly to define for itself. How, in effect, should one attempt to assess the politics of CSCW systems?

Just as the earlier assessments were grounded in contemporary conceptions of the labor process founded in the work of Braverman [4], a current assessment needs to situate itself in relation to present debates about the changing nature of work in advanced economies [19, 24], especially concerning “flexible specialisation” in the globalised post-Fordist economy. The orienting significance of these debates for redesign is obvious, though it could be argued that it is not very specific. But their import is far more direct when it comes to seeking to evaluate the politics and the ethics of particular interventions in systems design, since these depend on an appraisal of the realistic

possibilities and their balances of advantage, however difficult this may be. A broad spectrum of possibilities for evaluating CSCW and comparable leading edge systems philosophies might be as follows:

- 1 That the new forms of work and their accompanying technologies are simply a *systematic aid to exploitation* on lines of class, gender, race, the global division of labor, etc. Perhaps the character of this exploitation is more shrouded and obscure, more “ideological” (as, for example, in “quality circles”), but this simply represents a more modern form of the same labor process developments that have always characterised new technology.
- 2 At the other extreme, that the *technology itself is inherently democratic* and liberating. Of course there will always be differential access to and through any technology, but CSCW cannot help but be more democratic than the “privatised” technology that it displaces, in much the same way that the telephone might be said to be an inherently democratic (uncontainable) technology.
- 3 That the *forms of work are themselves* becoming less oppressive. As the most advanced forms of production shift from Fordism to post-Fordism, this requires increasing flexibility and intelligent initiative from a larger proportion of the workforce. There is therefore a corresponding shift from “direct control” to “responsible autonomy” as the management strategy for these sections of the workforce. This is akin to some of the “human relations” developments of the 1960s and 1970s, but this time *forced* as part of the material development of the labor process, rather than *chosen* as an act of “good will.”
- 4 That we can exercise the moral choice only to develop *systems that are “liberating”* and humane. This involves making the brave assumption that we can know the uses of systems, and that these uses cannot be “turned” [cf., 36].
- 5 That we can choose only to support *subordinated groups* of people, whose voices would otherwise not be heard in the development process.
- 6 That even if our intervention *is* exploitative, we may have to accept that enhancing productivity in a globally competitive environment is simply necessary as the engine of growth and of survival. If capitalism is now the only show in town, then it is in workers’ interests to be exploited by successful rather than unsuccessful capitals. If the objectives of skill enhancement and job creation are to be pursued, then it may be necessary to shift the focus to a societal or even global level, rather than expecting to be able to achieve it *within* a given unit of production.
- 7 That people resent and are frustrated by systems and setups which force them to do a bad job. Though people are sometimes so completely demoralised or antagonised that they seem not to care about their

work, basically they prefer to be able to take pride in their job. Even if they do intensify accumulation, good systems and procedures are a major determinant--maybe now *the* major determinant--of the *quality of working life*.

- 8 That we have to make continuing choices, project by project, day by day, and even hour by hour, about what to get involved in. In doing this, we are drawn into a continuously unfolding succession of more-or-less shabby compromises.
- 9 That the character and consequences of what is done in systems design, in both the broad and the narrow context, are extremely complex and largely unknowable.

Essentially, I believe the current politics of design are best represented as a mixture of all of these, with the last two predominating. This raises some issues with regard to Suchman’s critique of THE COORDINATOR, mentioned earlier. Foucault’s analysis of the birth of the prison in *Discipline and Punish* [12] focuses on a particular institution, but explores the ways in which its regimes of surveillance were “rehearsed” over other institutions, and argues that these are the main techniques of diffused power through which our society in its totality is now maintained. In an *age* of surveillance, the character and significance of any particular *artefact* of surveillance is not so clear, and depends on the relations within which it is embedded. A system like THE COORDINATOR is one thing when imposed on functionaries in a strongly hierarchical firm; but what would one say of it being applied to monitoring compliance by police custody officers with prisoners’ rights as set out, for example, in the UK Police and Criminal Evidence Act? The enthusiasm with which, it seems, the system is sometimes received by ordinary users might have something to do with its contrast with uncompromisingly hierarchical systems of the project planning type. If the previous experience was of simple *fiat* delivered by the system (complete with critical date), then to be offered the chance of agreeing or negotiating a commitment may seem like heaven (while under other circumstances--regular standard procedures, for example--it could be an infernal nuisance). Of course this is not at all to dispute that one could and should try for something much better (which may mean much “less”).

If it is hard to assess the meaning of “good will” in design, how hard would it be to realise once decided? Of course this can only depend on the particular circumstances, but amid the guruism of “modern management” there seems, in many cases, to be real indecision about the direction to take, with modernist “scientific” management and postmodern flexibility and autonomy holding variable sway in different sites, in different forms, with regard to different sectors of the workforce, and with different consequences. Amongst other things, those seeking new systems will probably be uncertain about what they want and about what is possible, and may well imagine themselves to be far more constrained by existing design techniques, methods, and

philosophies than is actually the case. In such circumstances a politically realisable intervention from systems design is not entirely out of the question. That it is sorely needed is shown by studies such as that of Egger and Wagner of time management in the allocation of hospital surgery resources [10]. A revealing irony which is relevant to the previous discussion is that the strengthening of management in the UK national health service, brought about by a government-enforced pseudo-market orientation, is likely to enhance the prospects of some such system here.

An upshot of all these considerations is that a sensitive and successful redesign is, at least for many of the most interesting and significant kinds of systems for CSCW, unlikely to be achieved without the development of mutual understanding on the part of very diverse contributors. Consultants and management scientists will propose forms of work and technology redesign which will often be rooted in fashion and hype, but which may also be grounded in experience of comparable settings and of the developing competitive environment. A participative design or a participative CSCW which takes no account of the problems, as they understand and experience them, which are faced by organizations in engaging with a hostile and increasingly global environment, has no chance of being taken seriously. This does not foreclose in any way the imaginativeness with which this "taking account" might be done. At the same time, managements and their consultants are likely to have a severely truncated view of what their staff really do, and of the dependence of their enterprise on their routine and flexible production of cooperation. The objective is to invent and develop appropriate forms of work and work support by and with diverse groups of users. Among the most important factors for this will be the space and time for attempts at workable resolutions to be negotiated amongst the parties when the prospects of redesign bring them to light.

## CONCLUSIONS

I have tried to indicate that there is a need and a virtue in developing hybrid forms of social science contribution to CSCW, and that ethnomethodological ethnography should also engage in this program. The objective would be for each participating discipline to recognise these hybrid forms, not as a generous accommodation to *others'* problems, but as necessary for satisfactorily addressing various of *their own* core problems, properly conceived. At present the context for such attempts is very obviously a research one. If it can be made to work there, then it will be time to consider such crucial matters as when and for what range of systems it would be cost-effective to attempt it.

If this were only a matter of improving the contribution made by different social sciences to systems design, then that would be a worthy but slightly dull result from the perspective of the heartland of a discipline. But, of course, the long term hope must be that such stern tests, over terrain not entirely of their own choosing, will provoke and reveal core developments for their methods and theories.

This paper has also tried to indicate some glimpses of such prospects.

## ACKNOWLEDGEMENTS

The ideas in this paper have benefited from discussions with many groups of colleagues, including especially the members of: EC Working Groups on Interdisciplinary Theoretical Approaches for Cooperation Technology (INTACT) and on Interdisciplinary Practice for Cooperation Technology (IMPACT), the EC HCM Research Network on Appropriate Cooperation Technology (ENACT), the EC ESPRIT Basic Research Action on Computer-Based Mechanisms of Interaction in Cooperative Work (COMIC), the Vienna-based "Software Cultures" project, and the Lancaster CSCW Research Centre.

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