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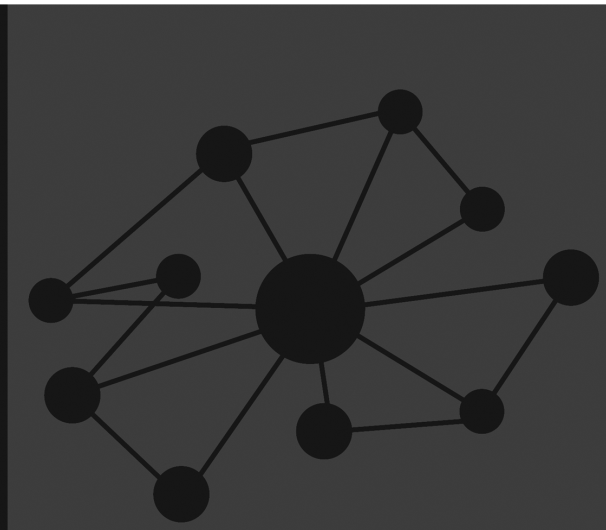

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EPIC 2005

Ethnographic Praxis
in Industry Conference

14-16 November, 2005
Redmond, WA
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INTRODUCTION TO THE PROCEEDINGS OF EPIC 2005: THE FIRST ANNUAL ETHNOGRAPHIC PRAXIS IN INDUSTRY CONFERENCE

It is our pleasure to welcome you to the inaugural Ethnographic Praxis in Industry Conference. A great deal of time and effort has gone into creating a program that we hope you will find informative, thought-provoking, controversial and enjoyable.

Appropriately, we gave birth to EPIC in a coffee shop in Seattle in the Winter of 2004. The idea for this conference, however, has long been discussed among many of the people conducting ethnographic research in and for industry. Over the years, "we" have met in workshops, paper sessions, over dinner, drinks and coffees at a number of other conferences to ask 'Where can we really talk openly about our work?' "Our work" has in the past been left intentionally vague. Who "our" is and will be and what defines the "work" now and in the future, are an integral part of EPIC. We designed this conference around these two words: "our work." We intend to build a community of people, actively doing and thinking about the theoretical and methodological development of their work of ethnography in industry practice.

Conference Theme

The EPIC theme for 2005 was Sociality. Ethnographers working in industry are expected to pay attention to corporate priorities and current trends. One of the predominant corporate themes has been a focus on the individualization and personalization of products and services. Although ethnography can address this issue, one that has received less attention is the social and collective nature of people's lives. We hoped the theme "sociality" would spark debate and discussion around methods we use in our research, present our findings and conduct ourselves in industry settings. By having this common focus in the conference we could collectively expand the boundaries of our knowledge and practice.

Conference Process

In making plans for EPIC 2005, we were mindful of the special nature of inaugural meetings of any conference. The first conference always sets precedents that will likely be followed in later meetings, both in tone and expectations for participants. Knowing this, we were anxious to get as much right as possible. Thanks to the contributions of the authors and the dedicated advisory committee, we think we have succeeded and we trust you will agree.

We divided the conference into five sections: theory, methods, cutting-edge papers, workshops on key topics, and reviewed posters. One critical decision was whether or not to emphasize theory at a conference of practitioners. Too often, conferences consisting of presentations from practitioners that turn out to be nothing more than a series of case studies that fail to address problematic theoretical issues plaguing the field. Practitioners need to incorporate theory into practice, but also struggle with developing theoretical frameworks through practice. We intentionally placed theory as the first session of EPIC2005, highlighting the importance of theory in practice. Further, we allotted theoretically oriented papers more time for presentation, and more space for publication, to allow for the presentation of full arguments and discussion.

In the methods section, we show exemplary case studies; full and well documented examples of the use of ethnographic methods in or for industry. We designed the cutting edge section to address

two aspects of ethnographic work in industry: (1) innovation in methods, topics for research, data collection or representation; and, (2) late breaking findings or insights from ethnographic research in industry. The workshops provide hands-on and interactive experience around key topics in ethnography today. Lastly, the posters present a range of research activities by ethnographers. With EPIC2005 we have striven to provide activities and sessions that will begin to help "us" to create some cohesive sense of "our work."

Another critical decision concerned whether or not to publish proceedings of the conference. Proceedings are the full *papers* that the authors created for the conference, not the *talks* presented at the conference event. Proceedings are unusual in the social science and design fields. Typically, papers presented at conferences serve as pre-cursors to journal submissions or edited volume collections rather than completed compositions of a proceedings. We decided that the proceedings "genre" would allow for quick distribution of timely papers. Proceedings also allow greater participation for a community of practitioners than journals or edited volumes, which are tailored more to people with academic lifestyles than industrial ones. Finally proceedings provide a collective archive of where we are in order for the community to grow theoretically and methodologically.

Over 90 extended abstracts were submitted for the conference, from which we could only accepted 15 papers for inclusion in the program. The response for the call for papers was surprisingly strong. Thanks to all the authors for their fine submissions. Each submission was put through a double-blind reviewing process (that is, reviewers did not know the name or the institution of the authors). Minimally, four reviewers reviewed each submission. Once provisionally accepted, curators and organizers reviewed full papers. Before final acceptance, authors were required to address curator and organizer comments. Paper authors come from a variety of backgrounds; both individual and institutional, and the papers here capture the wide range of thinking about ethnography in industry today.

In Gratitude

We want to express our appreciation to all the reviewers for their tireless efforts in evaluating the submissions and providing feedback to the authors. We also want to thank the EPIC advisory committee for their support and perseverance in working through the issues of a first time conference. Their help and guidance help to make the inaugural EPIC meeting one that will become an important addition to the research calendar for all of us in ethnographic praxis.

EPIC owes thanks to our corporate and institutional sponsors, Intel, Microsoft and the National Association for the Practice of Anthropology, both for their confidence that EPIC will be successful and for their significant and generous contributions of people and resources.

We would especially like to thank the attendees of EPIC 2005 for coming to the conference, even though it is new to conference calendars and a new style of program. Please enjoy the program, learn something from the workshops, and interact with your colleagues from a wide range of disciplines and industries. Most of all, have fun!

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LET'S HAVE A CONVERSATION: THEORY SESSION INTRODUCTORY REMARKS

RICK E. ROBINSON
Luth Research

As an introductory set of remarks for the theory session, this short paper sets up some issues facing both the field of ethnography applied in industry and those who undertake theoretical work in the field. The author proposes some simple dimensions for discussion: how we might consider work in industry a definite and distinct location for theory work; the nature of relationships with key interlocutors that are fundamental to working in industry; and finally, the role, opportunity, and responsibility that theory work might have going forward.

Every conversation has a beginning. A first voice that says, "I'm here." In saying "*I'm* here", that voice invites, recognizes or imagines its interlocutor. And in saying, "I'm *here*,´ "here" becomes a place. A theory paper is one of those first voices. Situating itself. Taking a position. Beginning a conversation.

It is one thing to walk into a crowded space, full of friends or family, and strike up a conversation. You are sure of getting a hearing, you know which gambits of the insider to play, and if you leave, you know that your ability to return is assumed. It is another thing entirely to walk – through a doorway showing few signs of previous use -- into a featureless dark and speak with clarity and confidence. To say "I'm here," without an idea of who or what might be listening, nor what those listeners might say or do in response. Starting a conversation that way requires some faith, some fortitude, and some vision.

The conversations we'll start here this morning are especially important because where "here" is is, to be generous, less than well defined. I doubt that I've ever been in a room with as many other people whose mothers don't really know what they do. Despite the amount of theory-based work that gets presented at the AAAs, SfAA, or 4S on the one hand and CSCW, SIGCHI, UbiComp or PDC on the other, I don't think that there has ever been a dedicated theory session on ethnographic work in industry before. These are, I think, new conversations to have.

At the risk of overextending it, I'd like to take just a few minutes to abuse this metaphor a bit further. If these papers represent some of the first voices to say, "I'm here," which I think they do, and do well, a few critical questions come to mind:

- ? Where's "here"?
- ? Who else is in this conversation?
- ? And what makes theory such an important thing to converse about anyway?

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So first,

WHERE IS "HERE"?

I think it is unlikely that anyone here would have trouble saying in what discipline he or she were trained. Or even in what 'tradition' that training might have more particularly defined itself. That said, it is also almost a given that most of us work not squarely in those original disciplines or traditions, but rather in interdisciplinary gaps and in practical spaces that reflect something of a mismatch between disciplines and organizational definitions: anthropology and technology, or culture theory and computer science, or any of those and design or marketing or strategy.

This would not be so bad if we were -- as both individuals and organizations -- knowingly engaged in the development and definition of a new hybrid field, something along the lines of one of the '*ur* models' in the sociology of science: the emergence of molecular biology from the collision of biology and chemistry described by R K Merton in the "Cold Springs Harbor" paper. A kind of consensual domain formation, through the integration and re-grounding of disparate practices engaged with common problems.

But that doesn't seem to apply here. Terms like "applied anthropology" or worse, "user research" don't seem at all to have the *gravitas* of "culture studies" or "developmental psychology" or even "comparative vertebrate anatomy," yet alone speak to something more richly formulated than the application of an approach to a setting. A bit like saying that Howard Becker is a "cultural institution interviewer."

There is a space here. But we have yet to populate it with theory that has been developed here. It is hard to be either heterodox *or* orthodox w hen all your doxa is elsewhere. We *use* theory, but we tend to *borrow* it from other domains, and too often, it is barely changed in the borrowing. Work in industry is often, at least implicitly, treated like a field site. Industry is a place where theory might be tried out, tested, but it is not what theory is about. For some reason, the very well known movie image of the hero of Mission Impossible, suspended above the floor of the top secret vault, but touching nothing, came to mind. Dropped in from above, getting very close without ever putting a foot down.

Application of methodology to an arena doesn't make a domain, or a discipline. Theory debate does. Theory must engage the conversations here, work in this set of gaps and spaces. And we need to articulate the characteristics of the space itself which peculiarly affect the development of theory. To play a central role, and to have continuity, theory can't simply be a frame around the execution of research while remaining grounded in some other domain's core questions, core lines of argument and central conceptual definitions.

The continual evolution of questions at the heart of theoretical dialogue cannot happen without language and a body of work against which to frame them. We may be looking at women's deodorants for the fifteenth time, but does the fact that we are doing it for a client mean that we need to pretend that we are doing so for the first time? Topics like this will only get more interesting if there are constructs to be interrogated, if there is theory to be extended in and through the examination of them.

I recognize that there is not a particular definition or description of what "counts" as theory here. We bring differences even in that from our different disciplinary roots. Building a definition of what theory is and does for us must be a long arc of conversation and in some senses, it will be a yardstick of disciplinary maturity.

So perhaps it is not so much a question of locating "here" as it is of engaging with the place where we find ourselves already. Best said, I'd argue, in the immortal 70's bumper-sticker phrase of Baba Ram Dass, "Be Here Now."

WHO ELSE IS IN THIS CONVERSATION?

One of the opportunities slash challenges for theory in this space is that there are a lot of interested parties out there in the dark:

Practitioners: One of the great attractions of this space is that so many of the people working in it begin self-introductions with, "well, I used to do… but then I …" or, "I was trained as a …., but I've done a lot of work with …" Interdisciplinarity and multidisciplinarity is nearly an assumed condition of work in industry. In everyday practice we have conversations --whether explicitly theoretical or not-- with one another and with the respective disciplines and theoretical traditions from which we have emerged. There is no single, dominant voice, which is a good thing, and there is the consequent opportunity to bridge and influence thinking with the work from a very wide range of disciplines—which is even a better thing.

Participants: The engagement with the various communities we study – whether conceived of as 'users' or 'consumers' or 'patients' -- is one I think we run the risk of taking for granted. The reality of working in this space is that many of the routines and questions for research and researchers, many of the expectations at work among our clients come not from humanist or interpretive disciplines in academia, but from the practices of marketing and market research. When someone is a "respondent" it is easy to end the dialogue as soon as you walk out the door, and to proceed under the fiction that we know them through their answers. If we are not cautious, we run the risk of substituting interrogation for conversation.

The other interested theorists: Recently, my colleague Hugh Dubberly – a student of both theory and of representations – introduced me to Stafford Beer's *Decision and Control: The meaning of Operational Research and Management Cybernetics*. Beer's curiously dated work -- "management cybernetics" should peg it pretty well for you – has a gem of a model in it that Hugh quite rightly thinks transcends the original context of Beer's investigations.

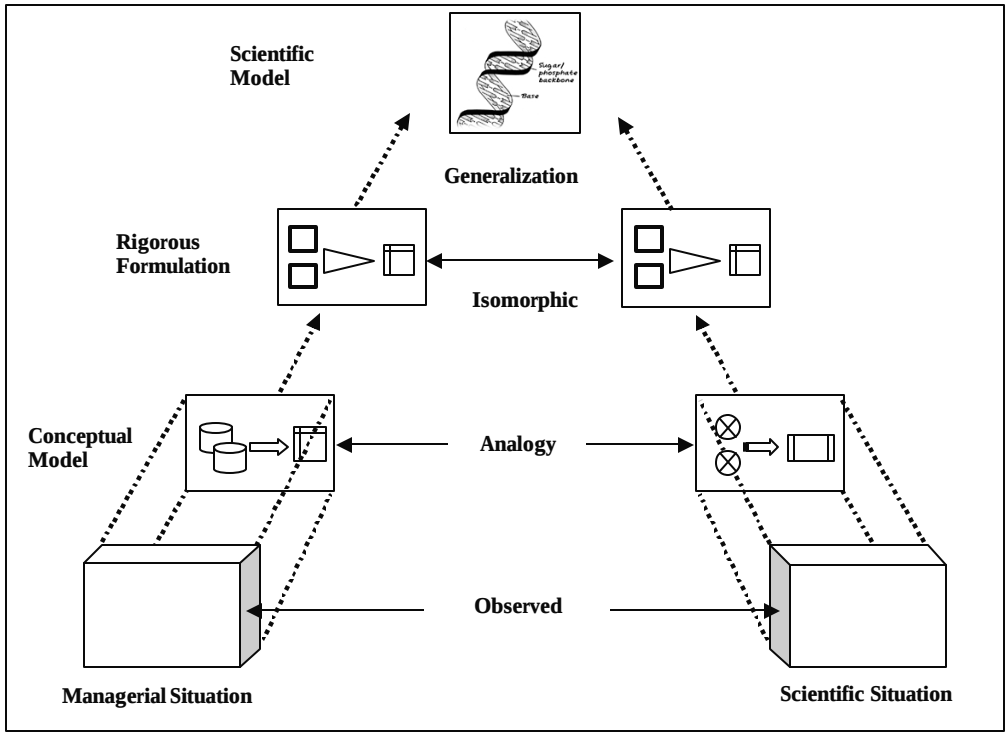

FIGURE 1 After Beer, Stafford: Decision and Control: The meaning of Operational Research and Management Cybernetics.

Part of the work of theory is to move from cases to consensus, from particulars to generalizations. In the interpretive social sciences, this move isn't a brick-by-brick testing of facts and observations, but rather a reconciliation and testing of explanatory frameworks and/or models against what we see in the world. Beer's diagram does a remarkable job of bringing to our attention the fact that *there is more than one model involved in that process.* In applied work, in other words, we don't just reconcile *our* conceptualizations to the 'facts' or the observations we've made, but we must reconcile the models that we build or employ with the models of the people or institutions on whose behalf we are doing this investigation.

It may be forty years old, but I think that a couple of the implications in this representation are very fresh. On the one hand, instead of "management science" he could just as easily have been talking about the idea of reconciling a medical model with a patient model, a user model with a software engineer's model, or any of the other kinds of things we study on a regular basis. That in itself is a nice adjustment.

On the other hand is the notion that the second model of "the situation" belonged not to the managers on the floor of the factory (the 'subjects' of the studies) but to the management science and

management theorists whose theoretical models of production and technology had guided the original design of the systems he studied. What Beer is pointing out here is that the same move we need to make to get from idiosyncratic cases to defensible explanatory reach is deeply tied to the move we make to insure that the model will be useful and effective for what is, for all intents and purposes in our world, "the client". The relationship isn't just between researcher and subject, but between researcher, subject and client, each of whom brings a formulation of the situation to any of the interactions we study.

Clients and clients' frameworks. *There* is that other listener lurking in the dark. And one that many researchers seem to talk to only reluctantly. We study consumers or users or experts or sufferers without hesitation. That relationship is central, comfortable, and expected. But in this space the *value* of the work will always be in part determined by the degree to and the success with which it engages the end users of the research, the clients -- whether they be designers, engineers, brand managers or policy makers. This process of matching the model we make of the situation with theirs, of engaging them in conversation so that what emerges from the process Beer calls "rigorous formulation" is useful as well as accurate is, I think, one of the defining characteristics of this domain with which theory (here) must engage.

To do theory in and of the space, you must accurately and realistically recognize the actors. In industry, that includes clients. In this space, clients are not foundations or public granting agencies; there *are* strings attached—our work must be *effective* in a very real sense of the word. If we approach our work in industry having made explicit the idea of engagement, of moving through analysis to this sort of reconciled formulation of models, it would seem to provide a more sustainable way of 'being here now' than an uncomfortable and distant accommodation with the vaguely threatening notion of 'serving corporate interests.' This means that we need to develop a deep understanding of what different client constituencies do with studies, to understand what constitutes effectiveness in the organizations we work with. They are as deeply part of our conversation as any of the people or places or interactions we study on their behalf.

WHY THEORY MATTERS, ESPECIALLY HERE

In the process that Beer's model suggests, there is one other element that I like a great deal, and which brings me to my last bit of metaphor flogging. That is the idea that through the processes of analogizing and reformulation and testing, *all* the models change. Given that we are talking about pretty fundamental models here – basic conceptions of how things work, about what is involved or what matters in arenas of experience—the idea that we are looking to change how our interlocutors *think* is loaded with both opportunity and responsibility.

There is a final, simple framework I'd like to use to extend that point a bit further.

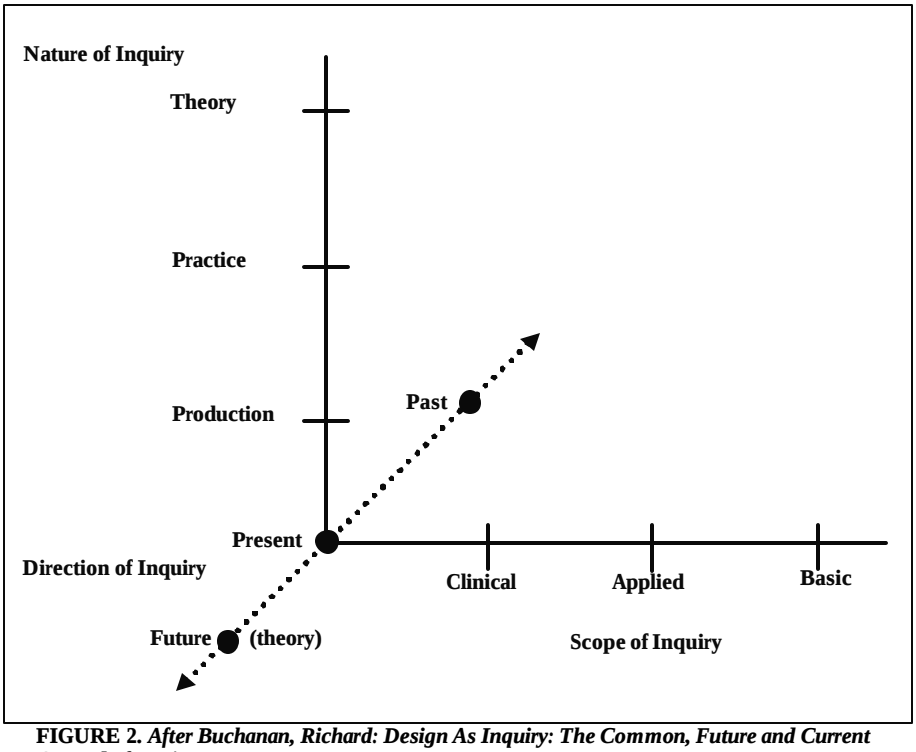

FIGURE 2. ***After Buchanan, Richard: Design As Inquiry: The Common, Future and Current Ground of Design.***

This is the most recent iteration of a way of talking about design research that Dick Buchanan (until recently the chair of one of the best design departments in the country, and who "used to do" Rhetoric in the University of Chicago's Committee on Social Thought) has been working on for quite a while. Buchanan's diagram means to sketch out a conceptual space, and to indicate that in each of its intersections, the nature and requirements of design research are different. Pulling the diagram out of the paper is massively unfair to the argument, making a careful articulation seem much too simplistic. But the points I need it to make this morning are in fact, rather simple ones.

The vertical axis is a pretty clear one, asking what drives a particular inquiry – from the immediate needs of production, through questions of (design) practice out to questions generated by theory. Were we to lay out all the papers about "ethnography in industry" along this axis, the result wouldn't be very top-heavy. That, at least, is one of the things I think we all know, and one of the explicit reasons for this session, this conference.

The horizontal or "scope of inquiry" dimension presses a slightly different question upon us. By "clinical" Buchanan refers to work primarily based on case studies. Again, were we to plot relevant work in the field, "skew" would be a barely adequate description of the result. A single case study is often a powerful thing. But theory cannot be built on cases alone, especially when one case is rarely

connected to the next. It is, as Buchanan's diagram implies, a limited 'scope of inquiry'. If case studies are the only fodder for the conversation, there is no extension, little reach beyond the immediate, and no larger patterns or emergent issues for theory to make sense of. This is almost certainly related to a lack of citation and acknowledgement of one another's work. Without that, there is no central corpus upon which to build meta analyses.

But I think the single most important thing to draw from this model is found on his z axis: past, present, and future as the 'direction' of inquiry. Future has this little parenthesis after it: "(theory)". What does that mean? Obviously, it could be prediction, in the sense of extending our understanding of the current situation into likely sequelae in the future. But there is also a much more potent way to understand it: that in this space --the "here" we are trying to articulate at this conference-- theory of the future also *develops* the future, *conditions* the future.

In the gap between what is (now) and what might be, articulating and developing theory *is* action. This is especially true of the representations of theory we develop and deploy. Because we are in this conversation with the people and organizations who will populate the future with artifacts, affordances, tools, and ways of thinking, we are actively engaged in shaping the future. We are not simply observers, describers, or contemplators of it.

Where there is active engagement, there is both power and responsibility. When was the last time someone asked any of our practices to develop a research project that would do no more than summarize the history of a particular product's use? Or to describe the current composition and behavior of a particular group without the implicit agenda of somehow changing it? We *act* at this intersection, and we act in a way that inflects the directions of the companies and institutions who make the everyday world, who shape power and politics, whose values are literally 'materialized' in a thousand ways each day. We cannot ignore the fact that we have both considerable influence on the future nor the consequent responsibility for using it.

If we only think of theory as what we learned in graduate school before we started to do the work we do here, we do not grasp an important opportunity. When we work in this space atheoretically, or when we think that "real theory" happens in the academic mother ships of our disciplines, we abdicate both the power and the responsibility inherent in it. We shouldn't.

As I've thought about this talk and this topic over the past few months, a line from one of the formative texts of my youth kept popping back into my head.

Something about the blend of conversational directness, an edge of anger, and an optimistic commitment to action makes it a perfectly epigrammatic conclusion: "I don't fuck much with the past, but I fuck plenty with the future." (*Patti Smith, Easter/Babelogue, 1978*).

So, welcome to the theory section of EPIC. Jeanette Blomberg is responsible for the 'P' in EPIC being "praxis" rather than 'practice,' and I think we all owe her a debt of gratitude for insisting on that small change. Practice is a good thing, in all the connotations and implications of the term. But the simplest translation of praxis is "meaningful action" and that's what the conversations we start here today can be.

We were blessed with a wealth of excellent submissions in this area, and the decision to limit the number of talks so that we could give each paper the time to push thought and argument beyond

the usual 15 or 20 minute slot was a difficult one, but the we thought it was the right commitment to make. After seeing them, we hope you'll agree.

Acknowledgments. Thanks to Kris Cohen for his usual blend of appreciation with precise critical perception of an argument's weak spots and omissions. Thanks to Hugh Dubberly and Ari Shapiro for (very enjoyable) conversations and discussions which contributed substantially to my thinking on this topic.

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2

WHO WE TALK ABOUT WHEN WE TALK ABOUT USERS[1]

KRIS R. COHEN
University of Chicago

I begin with some questions: how have the theories and methods which subtend design research been changed by their migration from academy to industry? How have they adapted to their new commercial culture? What languages and customs have they had to acquire to fit in? To address these questions, I consider a facet of design research which I think most problematically bears the marks of this passage: how we choose who we will study. I go on to think about both the causes and implications of exclusions so often resident in this choice. The ideal that drives my analysis forward is that design researchers are in the business of designing not products for "users," but landscapes of possibility for public life. A final suggestion, inspired by my recent work on Internet-based personal photography and here briefly sketched, is that design researchers take the publicness of our work more seriously—that we design for it.

MIGRATORY

We all know some part of the story of how, many years ago or only a few years ago (depending on what you call the beginning), social and cultural researchers arrived in industry, having journeyed from their homes in universities.[2] Most of us are witnesses to that story, if not central characters in it. In a sense, this is a story of migration (emigration or immigration depending on where you sit). And we know from migration stories that nothing comes through such a journey unchanged: things adapt and are made to adapt (Bhahba 1994). If we take this metaphor of migration seriously, it presents us with some questions: how have research theory and method changed in their journey from universities to industry? How have they adapted and been forced to adapt to their new situation? How have they been hybridized with methods and theories from elsewhere? What have they had to give up in order to fit in? What have they had to acquire, what languages and customs? And in their new home, what are their options for existence, livelihood, circulation, adaptation? What can theory and method *become* in their new setting?

More than I want to answer these questions directly, I want to use them, and the metaphor they sprung from, as lenses through which to view certain practices of design research. But I do want to answer that last question directly, about what theory and method can become in their new home, because I think it leads to the most interesting migration story. The answer is the bedrock for

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everything else I want to say here: *In a design setting, especially one where design research is common, theory and method (that is, our ideas about the world and our techniques for arriving at those ideas) will come to exist and circulate materially; they become, quite literally, embodied in products and made public.*

Let's pause here for just a second. This feels to me like a unique circumstance for social and cultural research, for theory; in any case, it has important implications. Foremost among which is this: when people (the ones the design research field often calls "users") interact with products, they are also interacting with—literally using and adapting and negotiating with—particular ideas about how the world should work or might work (for now, call these ideas "theory and method" for short). This is true in three senses that I can think of: (1) all products, whether inspired by user research or not, body forth a kind of social theory in that they try to *predict*, and in some cases, *dictate to* people's uses of that product; (2) products which are inspired or touched in some way by design research perhaps embody social theory in a slightly more literal, or conscious way than those which do not: it is literally the matter from which those products are formed; (3) whatever the intentions of products or their designers, people, individually and collectively, write their own scripts for how the world might work (in a sense, write their own theory) when using products. We could even say that today—as design research becomes accepted as an orthodox feature of product design while the academic production of social and cultural theory remains marginal, at least to most people's conscious lives—people's encounters with products are the closest most will ever come to encountering (consuming or using) social and cultural theory. We should be eager to see what happens in these encounters, to see what becomes of embodied, enacted, public theory as much as to see what becomes of the products which try to embody, enact, and publicize it.

But we should also be slightly humbled, because *if* there is any truth to what I'm saying, it means that design and design research[3] are in the business of conceiving not products so much as what's possible; of making worlds which are born in the collision of design and experience. And design research, or user research, or ethnographic research, or whatever we individually call it, is central to this work.[4]

Because design research, conceived as a world-making endeavor, sits somewhere between the real and the not-yet-real, the projected and the actual, design and use, research and the lives we are researching, I will be calling these worlds "landscapes of possibility." The term is not, I hasten to add, an attempt to name some intrinsic virtue of design, or to speak about design's eminence. At its most sanguine, it names an opportunity. It also names—and this will be my main interest in what follows—a responsibility that is distinctively political.

WHO, WHAT

My qualifications to speak about design research, and my reasons for wanting to, lie in two related episodes: two years (2002-2004) spent as a research fellow at the University of Surrey, in the INCITE

research group, working on a collaboration with the Internet technology company Sapient; and prior to that, four-years spent working at E-Lab (1996-2000), one of the early design research consultancies.[5]

To get to the heart of things, I want to relate a story from the INCITE/Sapient collaboration which I think will help us start to answer the questions I set out in the introduction. Here is the scene. In December 2002, as a newly minted research fellow at INCITE—but not someone who was new either to Sapient or to a commercial context for research—I had been preparing a presentation for Sapient…and I was a little bit worried. Sapient is a technology consulting company, headquartered in the U.S., with outposts all over Europe (www.sapient.com). In order to grow their own user research practice, Sapient had acquired E-lab; this was in 2000, a time when the conditions were ripe for that sort of thing, when companies all over the world were acquiring research "capabilities." In 2002, I joined INCITE in order to take up a research fellowship (already in progress), funded by Sapient and under the directorship of Nina Wakeford at the University of Surrey.[6] At the time of the meeting, I was preparing for the third and final year of work under the grant, the overarching goal of which was to develop new methods for design research, but also to conduct three year-long studies which would be useful to a broad sweep of Sapient's internal teams. My presentation to the advisory board that day was to lay out three possible research directions for the coming year. The work of the advisory board was to agree on one.[7]

My favorite proposal, the one I hoped to persuade Sapient to choose, involved me spending a year in a West London homeless shelter for mothers and babies. This was proposed as an extension of our previous year's work on mobility, during which we had studied pretty much the usual suspects: people—mostly middle class, mostly professional—who used popular mobile devices. I was worrying that day because I didn't think they'd go for my proposal. It didn't really seem like their kind of thing, or, maybe it was their kind of thing and the task of convincing them would be too much for my powers of persuasion.[8]

Specifically, what I was worrying about, what was being debated in my own mind and in the meeting room that day, was the relationship between the themes of the research (in this case, mobility) and our research sample (the residents of the shelter, in comparison to, say, people who own BlackBerrys). We knew Sapient wanted to know more about mobility (at the time they had several clients who worked in the mobile phone industry), but could homeless women—women who had been kicked out of their homes, who were living on public funds, many of whom were political refugees—be considered an appropriate sample, a way to understand some features of mobility?

I didn't like my chances of pushing this through, *not* because we had a bad working relationship with Sapient—just the opposite—but because I knew a little bit about how research themes and research samples are normally connected in design research. It goes a little something like this: study users of X in order to understand the phenomenon of X, where we can replace X with "mobile phones" or "toothbrushes" or "SUVs" or "Internet-based investment banking tools." We identify a thing that we want to study, then look for "users" of that thing. This is common in design research as well as in studies of technology more generally, whether conducted in universities or companies. To

take a popular example: we study mobile phone users, people who own mobile phones (and probably people who own the right *kind* of mobile phone) in order to understand something about mobile phones or mobility (see, for instance, Nyíri 2003, Katz and Aakhus 2002, Brown et al 2001, and most of the studies compiled at this site: http://ist-socrates.berkeley.edu/~nalinik/mobile.html).

The fact that the work of the meeting that day can be described as a debate that was almost exclusively about this relationship intimates that a lot happens in the suturing of a research theme to a research sample, the What of our research to the Who. And that, when nothing much appears to be happening, when the Who of our study seems to come bundled self-evidently with the What, then this ligature, and the means by which it has been effected, have been rendered invisible and, thereby, unthinkable by some assumptions buried deep in our methods, or in the contexts where those methods are deployed. So, we can start to ask: what are *the ways that* design research constructs a relationship between Who and What? How does design research establish Who will tell us What we want to know? Why those people and not someone or something else? What ideas about people, and more specifically, about class, race, gender, sexuality—the various technologies of the self—are smuggled into the methods we choose? And what has been the role of migration here? (From this point forward, as a shorthand nomenclature for the common research practice that I am going to discuss at some length, I'll refer to research samples as the Who of our research and to research themes as the What.[9]).

Back to the boardroom then. One assumption at work here, and in many rooms where people discuss research samples and appropriateness, is that, in order to conduct useful research, one must study people who can be considered a viable market for goods and services. Young homeless mothers can be excluded on this basis alone. Another assumption—in some ways far more problematic—is that users of X are the only people who can tell us about the social life of X. If homeless women are not "users" (and the first assumption has it that they are not), if for instance they use Ys rather than Xs, then they are the wrong people to study, especially if what you want to design are improved Xs.[10] Nothing personal against homeless women or anyone, then, because the benign reigning spirit here is that of just good design, a matter of common sense: you can't design better Xs unless you can watch some people using Xs. Which might be true enough, when heard in context, but when this sort of formulation comes from a user or design research group, one with ties (however attenuated) to anthropology or sociology, then you can be sure that we're inhabiting a site of migration. Some changes have been made to adapt to the new culture. Not all bad, to be sure, but definitely worth investigating.

A somewhat schematic way to talk about what these two assumptions accomplish is to say that they are forces which suture a Who to a What. And in the suturing, the choice of who we study becomes almost a given. And this never need be defended because the structuring assumptions of our methods erase the possibility of choice. It just seems logical to do it this way. Moreover, in most consultative contexts (the typical scene of user research) the Who of our research is given even more ineluctably: the client specifies it. *We*, the client says, *are interested in American teenage boys, aged 18-21, whose parents make between $30K and $60K/year.* They don't need to specify White; that will be assumed. They don't need to specify heterosexual or able-bodied; those too will be assumed (unless directed otherwise). The insinuation being: in this suturing, much gets elided. But my overarching point here is that: these are some of the conceptual and economic conditions under which design research gets conducted, features of the new scene for social and cultural research. Having said so, one thing we can

keep an eye out for in the future, another migratory trace, are places where market-logic has replaced other forms of logic which might guide our research and which might have guided it in the past. Or to put this in a slightly more specific way, we can keep an eye out for places where people have been replaced by "users."

ON BEING AND NOT BEING A USER

By now, a certain implication of what I've been describing might be obvious. Far from being the presumptive background of our research, the relationship between Who and What is the very thing our research should be geared to explore. To say who it is that can answer our—or our clients'—questions is a complex analytical *procedure*, not a given and not something that should be specified by clients. Here is why I think this matters so much: *in a design setting, this relationship, the linking of Who to What, is especially important because the people we choose to study stand in for the people who the eventual product, once designed, actively imagines.* "All works of the mind contain within themselves the image of the reader for whom they are intended" (Sartre 1988:73). I think in the case of design's "works of the mind," we are talking about more than an image of the intended. The people Sartre calls "the reader," and whom we often call "users," are encoded within the thing, the "works of the mind," and design research is (in a way that is only slightly less than literal) the means of that encoding. This is one reason why the experience of using products can be profound: in a sense, I encounter there someone else's idea of myself. Sometimes I like the person I find, and sometimes I don't recognize him at all.

If this way of thinking about design research has any truth to it, then it matters who designers and researchers choose to study because the products we create form part of a landscape which is either motivating or alienating for the various people who inhabit it.[11] And if, as both Hal Foster and (the far more sanguine) Harvey Molotch have recently argued, the experience-able world is an increasingly designed place (Foster 2002, Molotch 2003), then the implications here are far-reaching.

One way to see where these implications lead is to ask questions about the ways that the relationship between Who and What has become ossified in so much design research, about how this relationship has been instrumentalized. That is, we should ask questions about our research tools, where they come from and how they operate. The concept of the user is one of the most powerful tools we have, although it is rarely recognized as such. All the more reason to take a look.

Most companies, especially those in the business of new technology development, now feel that they need to know something about "their users," or at least, to claim that they care about knowing something, whatever their commitments actually are to that knowing. IDEO, for example, is one of the world's most successful design firms. Their website declares that "user observations are the starting point for every design program.…We engage end users throughout the design process to evaluate the desirability of new ideas and possible solutions" (http://www.ideo.com/about/methods/info.asp?x=1, accessed June 2004). Sapient's own website states that "Our deep understanding of technology — and the people who use it — combined with industry, design, and program management expertise, produces unparalleled value for our clients" (http://www.Sapient.com/about/about_us.htm, accessed June 2004).[12] So, there is a job description here which is perhaps not new to the business world, but

which has certainly been re-newed: the job is understanding people. The reason that most of us have our jobs is that anthropologists and sociologists (and some others) can claim, credibly and with authority, to know something about how to know something about people. So it is hardly a stretch to claim to know something about people-as-users; if anything, "users" is a narrower concept.[13] This notion of people-as-users has helped to created the very ground on which academic researchers have been able to collaborate with designers. The idea of people-as-users renders the social-cultural world as a series of use-instances, wherein people interact with consumer goods, and so the world starts to look like a place where designers and anthropologists should be collaborating. They appear to need each other.

Feminist scholars of technology like Donna Haraway (1988) and Karen Barad (2003) have urged us to be alert to, and wary of, technology's little ways of inhabiting people. We use technology, they say, but are also used by it; adapt it to our own uses but are in turn adapted by it. I would say that the term "user," itself, registers the presence of such transformations. "User" is, in other words, more than a word; it is a concept which establishes some of the material, social and cultural conditions for our research. It cannot, therefore, be dispatched simply by replacing it with gentler synonyms. It is, in this sense, one of the most significant adaptations that theory has made to its new environment, its new culture. It is also part of the reason why, in these migratory spaces, Whos and Whats are so predictably wedded to one another.

"User" is the name we have given to a perspective which understands people as consumers, or views people always through the lens of their consumer habits. The reductiveness of this term is something which has been attacked before, although it is no less popular for it.[14] But there is a specific sense, relevant to my interests here, in which I think the concept is problematic. It is a problem of instrumentality.

Some of Lucy Suchman's early work on human-machine interaction helps me make the point. Here is Suchman: "Every human tool relies upon, and reifies, some underlying conception of the activity that it is designed to support" (1987:3). If the concept of the user is a human tool in Suchman's sense, then the activity that it supports is design research (and from a broader perspective—one which sees design research as an established part of design—it supports the work of design, *in toto).* That should be obvious. What we need to search for a little bit—precisely because, as Suchman says, it has been reified or obscured by common practice—is the "underlying conception" of design research that the notion of the user supports.

In order to get at some of these underlying conceptions, let's think for a minute about what user research is meant to do. Practiced one way, user research looks for insights (social, cultural, theoretical) which will *inspire* the design of new products. Practiced another way, it doesn't inspire new products so much as *justify* them, after the fact of their making. In both cases, people become users at the moment when research sets out to study people-as-past-and-future-buyers. In usability testing, a form of user research which lies more towards justification than inspiration, people are rendered as users in a slightly different manner. Usability testing presents prototype products to people as a way of testing whether the products work, whether or not they can be easily learned, whether or not they entice. This abstracted process renders people as users by ignoring most of the parts of people that don't come

obviously into play when the person is using—literally handling—the product.[15] But in all the forms of research that I've mentioned, whatever their role in the design process, user research and users become part of a process of financial investment on the back end, and part of a marketing push on the front or public relations end. *Our users asked for it. This product was inspired by our users*. Users, therefore, are not just creative tools, they are economic and political ones.

Because "users" refers both to actual people (*the ones we interviewed*) and to a kind of abstraction or methodological fiction (*inspired by our users*), they (and I mean both the notion and the actual people it abstracts) can be used diversely, pressed easily into service as research methods, marketing tools, advertising slogans. They are an exemplary human tool, in Suchman's sense: highly reified (obscured) and themselves part of a larger process which reifies the theories, methods and projects with which they are associated. Who do we talk about when we talk about users?

Homeless mothers rarely get to be users in any of these senses. Does this matter? Would they even want to be? These questions, however important in theory, are moot in practice because both "user" (and via negation "non-user") are determinations always made by someone else. Young homeless mothers are not users because most companies don't design new technologies with the homeless in mind. And they do not for all of the obvious, pragmatic, sensible economic reasons. But these reasons are both undergirded and operationalized by the particular way in which companies pair up the Who and the What of their work. Common sense does the soft work of exclusion, but research theory and methodology, adapted to their new environment, do the hard work, the heavy lifting. And this, of course, influences the way that products function out in the world, and who they function for. In shaping products, imbuing them with an imagined user, design research helps to shape the worlds we *all* inhabit.

The most obvious problem with "users" is that it draws on a narrow band of people (the people who might buy, and who have bought in the past) in order to design the landscape of possibility for a far broader set of people. We all have to live with products which are designed with a small set of people in mind. To take a slightly prosaic example: Left-handers and right-handers alike have to live with right-handed products. I also think, somewhat more allegorically, of television sets in bars. The important point being, we all have to *live* (and I mean this in a particular, but meaningful sense; I mean it in the sense of public life) with the products of the design industry, whether or not we are able to, or would want to purchase them. Which leads us to the bigger problem with "users," this the parent of the first: "users," as a concept, occludes most of the ways in which people interact with things, and with each other.

This is what I mean: various sorts of meaningful connections get established between different groups of people (e.g. demographic groups) which, by the very fact of their existence as stable and distinct groups, imply that no such connections exist. Put bluntly, the social and cultural life of, say, mobile phones, is far more heteroclite than a study of just mobile phone users allows or admits. Moreover, the interests, activities, and problems of homeless women, whether or not they use mobile phones, WILL establish relations (of force, influence, opposition; relations which are symbolic or material or affective) with even the most stereotypical of high-end mobile phone users. Which is to say (again), the relationship between Who and What is one of the most important (methodologically important, and therefore, politically important) subjects for our research; much happens here that is important, constitutive and studiously ignored. I've been discussing some of the reasons why, or the

means by which, this relationship gets ignored in design research. But what does it look like when studies take this relationship seriously? What do we learn?

BAD OBJECT CHOICES[16]

In her book *Flexible Bodies*, an ethnography of immunity and the immune system in the U.S., Emily Martin spent time with ACT UP, an AIDS activist group. She is quite deliberate about this choice (the Who of her study), explaining that: "My fieldwork has made clear to me that the categories of social analysis that we once found so useful to describe our lives—gender, race, class, work, home, family, community, state and nation, science and religion—are no longer sufficient to describe, let alone analyze, the phenomena of the contemporary metropolis…" (1994:xvi). Elaborating on the Who of her research, in relation to her project's What, Martin says that her "experiences with ACT UP allowed [her] to see the politics of AIDS from a particular point of view" (1994:xv).

But Martin's choice of ACT UP strikes me as important not just for the particularity of its point of view, what Suchman might call its "located accountability" (2000), but for the unexpected quality of its Who—which is to say, its politics. We can, without too much difficulty, see AIDS as thematically related to issues of immunity and the immune system, given that AIDS has forced a consciousness of the immune system more than any phenomenon in recent memory. But the choice of an AIDS activist organization, an organization whose existence and motivation is political more than biological or medical, and which is run by people who are living with HIV but also by people who are not, is probably not quite who common sense tells us to ask about primary experiences of the immune system. Certainly they are not who most companies would have in mind as primary "users" of, say, health care services—they are either too marginal (not a *real* market) or too niche (not *our* market). Martin's inclusion of ACT UP in her sample honors the role played by marginalized others (ill, queer, radical) in the broader social-cultural experience of something like the immune system.

This is not simply a matter of making a choice which is surprising for the sake of being surprising or extreme. A lot of companies tout their studies of "extreme users"—people who are said to exist at some demographic or behavioral extreme of "normal" practice—as a more eccentric way to link up Whos and Whats. But extreme users still exist well within the logics of the marketplace, where all people are users. It is the marketplace itself, and the categories which stabilize the market, which code their behaviors as extreme in the first place. Better, then, to try to get behind what our methods, and the institutional settings for those methods, reify and occlude. Dick Hebdige's famous book *Subculture* (1979) is not a study of subcultures so much as a study of the culture industry at large. But Hebdige's insight was that subcultures are an occluded other (perhaps *the* occluded other) of more visible (more marketable) cultures. In other words, he showed how the coherence of one relies intimately upon the other, and relies specifically on the ritual occlusion of the other.

Bruno Latour, in his study 'The 'Pédofil' of Boa Vista. A photo-philosophical montage" (1995), followed scientists to the Boa Vista forest in order to better understand how laboratories function in scientific practice. He did so not because scientists who work in forests are extreme users of scientific tools and technologies, but because he understood that laboratory practice is fundamentally

exclusionary, and Latour wanted to see, first-hand, what had to be excluded for laboratories to work like laboratories.[17] Go to the forest to find the laboratory; ask the punks about the straights.

Attention to such constitutive exclusions animates much of the research that has been done on sexuality. It has shown how straight cultures and queer cultures are not, as most research on consumer culture assumes, mono-valent, distinct or isolated from one another. Rather, they are "elastic alliances, involving dispersed and contradictory strategies for self-maintenance and reproduction" (Berlant and Warner 1993:358. See also Eribon 2004, Sedgwick 2003, 1988). It is not just that queer and straight are inter-relational; there is an intimate commerce between them, and indeed, between any categories which show up in discourse or politics or user research as opposed, as even just distinct. What, then, are we able to know about family, home, or work—common topics for user research, and places where sexuality and discourses of sexuality play a strong role—if we do not apprehend them within some framework of sexuality? I don't mean to imply that all research, no matter the subject or time frame or client interests, must consider sexuality, or any of the various oppositions which stabilize its Whos and Whats; I do mean to imply that projects which do not are proceeding on the basis of exclusions which are resident in concepts (our theories and methods) but manifest in designs (in the products we create). Sometimes those exclusions will hardly matter; sometimes they will matter a great deal.

Better, then, to call these exemplary linkings of Who and What political than to call them surprising or unexpected. Howard Becker discusses the need for these sorts of politically motivated research tactics when he describes the standards (of rigor, of objectivity, of common sense) which tend to limit and homogenize our ideas about appropriate research samples. He calls these standards the "hierarchy of credibility" and goes on to say that "[t]he hierarchy of credibility has, as a corollary, that certain people or organizations aren't really worth studying at all" (1998:93). The researchers involved in the projects I've described above may not choose to describe their work as political in intention or outcome, but whatever their avowed goals, their projects have political effects because they include the people whose elision, in most forms of common discourse, serves to stabilize our favorite categories (of experience, identity, etc.). It might behoove us, then—following the theme of the conference—to include within our definitions of sociality those people and practices which are ritually excluded, which must be excluded, for the more demographically visible, marketable, definable "user" types to function so reliably.

The conceptual feature of our research which I am describing—viz. the intimate commerce between seemingly discrete categories—is characteristic not only of extrinsic relationships (between people, and between people and things), but also of intrinsic relationships, relationships and movements *within* individual bodies: that is, the workings of the self. Women who draw income and housing support do not live their lives, in any simple sense, through social services or through their consciousness of them. They create boundaries and distinctions. Here is an example. Many of the women in the West London house I studied do not take visitors, not even close friends, for the 12-15 months that they are having their baby, learning to care for their baby, and living in the shelter. By this strategy, they do not *appear* to their friends (to, that is, the world which matters most to them) as residents of a homeless shelter, even though they make no secret of the fact in a broader sense. This is one way in which mobile phones become so important to the women in the house: they allow them, in a sense, to take visitors (via their phones), to maintain close relations, without embodying themselves as recipients of social services. Their mobile phones (and they all had one during the time I spent there) help them create some vital boundaries. Which goes to show, at the very least, that the women

in the house are never simply "users" of social services. Wherever possible—and it's in no way always possible—they choose the frames in which they appear to other people, and "social service user" is an identity that some of them work hard to efface. To study them, therefore, *as* users of social services, or alternatively, *as* mobile phone users, requires us to know something about the people who aren't on benefits and who loom large in the imaginations of those who are—to learn, in other words, about people's tactics for *not* being users.

LANDSCAPES OF POSSIBILITY

Let me draw on recent work by Sarah Jain to come at this from another direction. In her forthcoming book *Injury: Design, Inequity and Litigation in the United States* (in press: n.d.). Jain very carefully, very usefully extracts the products of design from two sorts of reductive accounts, both of which are common, and both of which ignore certain features and motivations of the design process: one describes products as things which are simply and unproblematically put to use by the people who we can readily identify as their users; the other figures products of design as the final step in a rational design process which is governed predominantly by creative principals, by simple, unpredictable, unaccountable afflatus (and here, design research is usually included as part of that creative process). These characterizations of design, Jain argues, each ignore the radical unpredictability of the relationship between a product and the people who encounter it, and do so in order to maintain an image of design as, ultimately, a creative rather than a political process, one which makes products and not worlds. Jain asserts that what we call design is actually a congeries of decisions, made in boardrooms and design shops and ethnographic field sites and even courts of law, all of which inevitably, if invisibly, inhabit and motivate the final product. Taken together, these are decisions about how *certain* people should (and therefore sometimes do, or must) use those products. This is what I meant before when I said that products imagine their uses, their users, and their sites of use. When products suffer from failures of imagination (and failure in this regard is not always a bad thing), the results are sometimes liberating (who could have predicted the popularity of text messaging) and sometimes deadly (airbags, cigarettes, fast food, cars which still run on carbon-based fuels). By examining injuries that result from spectacular design failures, and the litigation which tries to account for those failures in juridical terms, Jain shows us quite graphically how people are anticipated or envisioned by the products they use; how products come to literally embody the various decisions and exclusions which buoy them up; and how, in the cases of design failure which she tracks, products (and the various mechanisms that produce them) sometimes materialize these landscapes of possibility with gruesome results for the people they exclude.

In this light, studies which begin with a user linked stably to a theme, a Who to a What, leave unconsidered one of the most critical questions they could be asking: *what relationships of filiation or force get established between the object under investigation, the people who use it, and the people who, in not using it or in peripherally experiencing it, are also affected by it?*

Perhaps the problem lies with ethnography as a favored method in design research. In business and design, ethnography has become the generic name for almost all forms of design research, whatever their relation to the history and practice of what has been called ethnography.[18] We can speculate on why this might be the case. Ethnography, historically, is rooted in the observation of

behavior in context. Ethnography watches, and asks. In order, therefore, to prepare for a study which employs ethnography, we need to identify something to watch, someone to ask questions, and this, in turn, often involves specifying a Who and a What, because once you have both, and they are stably linked, you know where to go to do your fieldwork. And if you only have two weeks and a limited budget, it's useful for all sorts of reasons (most of them about economies of time and money) to know where you're going to do your fieldwork.[19] This dynamic of watcher-watched, which sutures a Who to a What in advance of any actual looking or analysis, seems hard-wired into the very practice of ethnography, as a method which was designed for studying people in non-western contexts (where the context itself seemed to code for the Who of ethnographic study: we go *there* to study *them*). It is little wonder, then, that this version (and it is only one version) of ethnography has been adopted so eagerly by business, design and industry generally, fields with an abiding economic interest in pre-defining their market, their Who. Most business practice requires an identifiable, quantifiable Who before it will spend one cent on research, let alone development or manufacture. Because, in the end, there must be someone who will buy.

It's 2005, it's 1998, it's 1971. Howard Becker asks: "How do we go about finding cases that don't fit?" (Becker 1998:94). He means cases which don't fit the conventional categories for who counts as a legitimate or expected or familiar or marketable research sample. And Becker cites Everett C. Hughes who writes: "We need to give full and comparative attention to the not-yets, the didn't quite-make-its, the not quite respectable, the unremarked and the openly 'anti' goings-on in our society" (Becker 1998:94). So these are not new ideas, but then, what's new is, itself, always new, always being re-made and becoming newly relevant. In a migratory context, where all of the old ways are, perforce, remade as a consequence of being transplanted, this is even more so the case. In many design settings—sites of migration, home of a new culture for research—the exigencies of the market trump many of these old lessons of social research. This is probably inevitable, and maybe not undesirable, but we *can* be careful about which lessons get trumped and which should not be.

When we say, for instance, that we want contextual observations of mobile phone users to tell us something about how mobile phones are operating socially, culturally, commercially, etcetera, or that we want these observations to inform the design of the next generation of mobile phones, we flatten out our possible understandings of both users and mobile phones. We assume that mobile phones attain their intelligibility and their significance from the people who most visibly use them, and that people who use mobile phones attain their own intelligibility and significance (at least for the purposes of our study) from their interactions with mobile phones.

How can any study which bases its research plan on these hermetic formulations ever account for a world in which certain people are ignored by products, are excluded from visibility, are alienated by popular forms? How can such a study ever do more, that is, than replicate that very world, with all of its constitutive blind spots? Is it any wonder, then, that women and racial minorities and queer sexualities and disabled bodies often find it difficult to be comfortable in a world which so many people (the world's "users") inhabit with grace and ease?

My desire here is *not* to see the market democratized, to expose an ever greater range of people to consumerism and design—that is happening on its own. What I am trying to do is foster design research which operates through means other than exclusion. And I am doing so because I believe that

products are (or are embodiments of) social models which impact the lives of a great many people, those who can afford them and those who want them as well as those who can't and those who don't. In other words, the products of design contribute significantly to landscapes of possibility which we all inhabit, and it is in this sense that their exclusions matter a great deal.

A final sweeping thought about design research is that perhaps something is wrong at the level of the field's aspirations. Perhaps the goal of studying users in order to design better products for them was well suited to the *instigation* of a new field, providing the means to draw together design, engineering, computing, the social sciences and the humanities. But perhaps this conceptualization of design research is poorly suited to the task of motivating the field to *develop over time*, theoretically, methodologically…politically. I think that a far more radical ambition would be to try to support the design of what I have been calling landscapes of possibility. And that requires formulating and conducting our research within those same expansive, political landscapes.

DESIGNING FOR PUBLICS

So far, I've been long on analysis and short on practical suggestions. Although, I think a few suggestions have been implicit in the analysis. Namely:

- the logics of the marketplace (e.g. demographics), even though our products are often destined for the market, are not adequate as conceptual or theoretical grounds for research;

- people are many things, including users of consumer goods, but that is NEVER all that they are, and so that single aspect of their selves, divorced from all others and raised to the level of a determining factor, is also not an adequate model upon which to ground one's research;

- the purpose of research—and I would think especially research in a commercial setting—is to explore the various ways in which people create and are formed within their relationships with stuff, with other people, with groups of people and with networks of technologies, *not* to prefigure those relationships in our research plans;

- design research which predicates its investigations on the idea that in order to understand X, one looks at the use of X by particular people in particular settings, and which does so to the exclusion of a great number of overlooked, seemingly unprofitable people and places, is at least partially destined to endlessly produce and reproduce that hermetic world of its own imagining.

In the process of making these points, I have talked about research methods as tools, a perspective which implies that our methods and theories are products of a design process just as toothbrushes and cars are. And so, if one goal of social research in industry is to make better tools, then shouldn't our research methods themselves be subject to adaptation and revision, based on what we learn from our research? Like toothbrushes and cars, they too have users and uses, affordances and limitations of use, embedded politics of inclusion and exclusion. They too are subject to creative and instructive mis-use. It is this trajectory of thought that lands me at the feet of a few suggestions.

I have recently been studying online photography, and looking especially at the kinds of photographs which might have spent their lives in family albums but which now cover the Internet.[20] In this project, I've talked to both bloggers and to people who use the popular website flickr (www.flickr.com). This has become less a study of photography, per se, and more a study of publics, of how people who put their photographs online illuminate and potentially expand the way that public life functions today.

I think it is worth taking a minute to think about the things we design as public, and design itself, including design research, as a process which participates in the creation and recreation of publics. I don't mean to ignore the fact that design's products also have a role to play in the experience of what is private. What I am trying to name, and thus get some traction on, is something which I think design research wants to apprehend, but at this point often mishandles: namely, the ways that products circulate materially and symbolically, the ways they contribute to what I've been calling landscapes of possibility. One way to encompass these modes of circulation is to talk about design as a part of public life.

Places which are "public," in the word's ordinary language use, are typically conceived as the opposite of places which are private, and so the word tends to imply open, available, and non-intimate. Because "public" is almost always linked to a form of physical space, these qualities then become characteristics of that space. Thus, we get the phrase: "the public sphere." But this is not quite what I mean when I say that products are public. In my reading of the literature, I find three qualities of publics which I think are useful for design research as it moves forward (each slightly, and I hope usefully counter-intuitive, if your point of reference is the ordinary language use of "public").[21]

1. publics are less usefully described as things (spaces or spheres) with the quality of publicness than as the processes by which something comes into being precisely *as* public (Warner 2000:12 and elsewhere);

2. publics are born of activity done in the constant presence of others (Arendt 1958:23); they rely on what Michael Warner calls "stranger relationality" (76);

3. public action is always unpredictable; it generates what Hannah Arendt calls "startling unexpectedness" (178).

The first quality of publics, their verb nature, helps us to see public actions not as events which emerge into an existing space, defined *a priori* as public, but as themselves factors in the formation of publics. Consumer products, we could say, are the design industry's public actions. The products we design don't become public when they arrive in stores; their emergence into stores, but more importantly, into homes and offices and cars and even bedrooms, transforms publicness itself. Think about the experience of riding on a train; how did Walkmans (which never quite privatized their sound or the experience of listening to it as the product appeared to promise) transform a train car. Think about how trains have been transformed by mobile phones. These are only the most obvious examples, but all products have a role to play in the definition of publics. This is exactly the sense in which I have been talking about landscapes of possibility, and how the products of design contribute to those landscapes.

The second quality of publics, the fact that the most meaningful public actions happen in the constant presence of others (and here we have to allow for forms of presence which Arendt, writing in 1958, could hardly have envisaged) might help us to broaden who we include when we do user research and lead to better ideas about how we include them. When we say that user research involves users in the design process, one thing we mean is that user research makes users *present* in that process. And in this way, the user—manifest as a set of preferences or a behavior model or a segmentation or a thick description or a scenario—becomes present in our designs. But from the perspective of a theory of publics, these ways of representing the user, and the very idea of a "user" as such, are relatively weak forms of presence. One way to enliven this presence is to pursue the "cases that don't fit," to mobilize the relationships we create between Who and What, to see people within a more expansive set of relations than "user" could ever acknowledge. Another way, and I'll come back to this shortly, is to diversify the way that design, and design research become public.

The third quality of public action, its unpredictability, helps us to notice how reliant our current practice is on predictability. Business tends to view unpredictability as a necessary evil, but one which can be held at bay by good preparation, clever analysts, and maybe some rigorous futurology. In this prophylaxis against the unpredictable, design researchers are hired as sentinels, an advance guard. *We don't know what people actually do with our products...we'd like you to tell us.* This is how most design research begins. If companies could predict what happens to their products once they're bought and used, they wouldn't need user research. Unpredictability, then, is precisely what user research is in the business of tracking and describing—the crazy things that happen when stuff enters the world. But the goal that user research participates in, ultimately, is to mitigate the unpredictability of user behavior by helping to create products which can, in a sense, predict how they will be used (predict, first, *that* they will be used, and then *how*). And ethnographic methods are, themselves, beholden to predictability. Whos get sutured to Whats because we need to be able to plan for, schedule, and budget our research. But the most careful preparations notwithstanding, unpredictability is more than an ever-looming possibility, it is quite simply and ineluctably the case. Given so, mitigation starts to seem like a foolish goal. Especially if we start to think, as Arendt advises us to, that unpredictability is a public's greatest asset, the source of its power. Better, then, to find ways to work with it.

All products (the ones we come to know as products) eventually go *public* (in the senses elaborated above). Design research, we could say, is the process whereby researchers observe and document the manifold, fascinating, and (yes) profitable activities of the publics in which people interact with products (even the home becomes public in certain moments, and more and more, it does so as an effect of the products we bring into the home). As ethnographers know well, these sorts of publics are fascinating and dizzyingly generative. And I think we'd probably all agree that design research has contributed positively to those publics by creating products which are, so to speak, more aware of their surroundings, their conditions of use.

But my experiment, wherein we think about design as a discipline which creates publics, has a corrective purpose, not just a descriptive one. The first corrective is this: if we think about products as public, then I think we are forced to recognize that they impact more than just the people who purchase them (more than just their "users," which is exactly why the concept of the "user" is deficient). From an environmental perspective, for instance, one which thinks about manufacturing and pollution as part of a product's design and use, I think design's widespread impact is easy to see. But I think this is also true of the circulation and use of the products themselves, whether or not they pollute the environment. Think, for instance, of the implications for most women or for all children of

the fact that airbags were designed with an average-sized male as their user (Jain, in press: n.d.). In other words, products contribute to landscapes of possibility that we all inhabit. And these landscapes of possibility are something that design research might study. This, however, would require that we are more thoughtful, more expansive, and in some cases more subversive about how we link the Who of our research to the What.

The second corrective I have in mind (for more outlandish, far more impractical, and so, a perfect ending) is this: what if, as a way of more deeply honoring the obligations that publicness seems to confer on design, and of taking better advantage of the strange, productive promiscuity of publics (of unpredictability, of stranger relationality), we made the design process *itself* more public. What if, for instance, all of our research, all of our prototypes, all of our working models, all of those ideas about technology and usability and mobility and etcetera were made broadly available, just as the products they help create are eventually made available. I'm saying that products are not the only thing of value we produce as design researchers. I'm also saying that the process of making something public is far more risky, complex and exciting than at first it might appear. It sounds simplistic and naive until one realizes that things don't become meaningfully or productively public simply because they are dropped on a street corner or website. Meaningful publics must be carefully created, which is to say, carefully designed.

Right now, we can look around at other fields and see this process of becoming public in action. I think the practices of photographers and bloggers and del.icio.us users and audioscrobbler users *inter alia* show us, in pretty dramatic fashion, the generative potential—the design potential—of people pursuing their projects in public. When the materials of one person's project are made public, they have the chance to become material and motivation for another person's project. Or, those two people, similarly motivated and newly acquainted, might begin to work together. EPIC is important as one effort to make design research more public, to draw it out into the world just a little bit (into the world of its practitioners—a big step for design research), to lay bare not just its products but its processes, and to see what happens.

I propose this action of "making public" as one kind of corrective to the problems of user research that I've been describing. There are, of course, other forms of redress. "Ethical" or "green" design is one way to more carefully design landscapes of possibility, to understand that products impact a far broader and more diverse landscape than a single home, family or person. Call these direct methods. The methods I've intimated here are more circuitous, more risky, less predictable. But I think they are powerful for the range of interests they potentially serve—well beyond the interests of the companies currently making products and commissioning research. My suggestions are motivated by the recognition that our work, our design research, might have interest and value beyond the products that it might one day (if we're lucky and if all the research is accepted and if the company doesn't get bought or lose interest or decide to build something else) release into a public. And motivated, further, by the recognition that there is occurring, right now, all over the world, a struggle whose territory is cultural production (landscapes of possibility), whose borders are defined by intellectual property and copyright laws, whose stakes are the future possibility of creative production *in toto*, and whose most powerful weapon is the status of publics themselves—what is allowed and what is disallowed as a part of public life.[22]

I think that to see our work as a part of this larger struggle would have some drastic effects on how we conduct our research, how we connect Who to What, how we present our findings, and even on what counts *as* a valuable finding. In order to see if this is true, we have to "suffer," as Arendt says, the consequences of doing our work in public. And this requires finding effective ways to make our work—its processes, methods, theories, interim steps, as well as its results, its products—public. Flickr, it turns out, is an effective way to publicize photographs, to make them available for use by others, to nurture what copyleft lawyer Lawrence Lessig calls a "remix culture" (2004) and what political philosopher Hannah Arendt calls "action" (1958) and what media theorist McKenzie Wark (2003) calls "abstraction"—each a different name for radically creative forces potentialized within public action. In other words, conceiving effective ways to make our work public is a design problem. And we should be good at solving those.

NOTES

Acknowledgments – I'd like to thank Rick Robinson, Katrina Jungnickel and Nina Wakeford for the criticism (which is to say, encouragement) they each provided as I wrote this paper.

[1] With thanks and apologies to Raymond Carver (1989) who, I'm sure, had little or no interest in user research.

[2] I'm thinking here about anthropology primarily, but only because that is the discipline with the largest number of emigrants. There are also sociologists, English and literature students, linguists, historians, performers and performance studies students, and others. My own point of entrée was Art History.

[3] And others.

[4] Design, of course, has a long history, but it's possible that the same could not have been said of design before the anthropologists and sociologists and design researchers *et al* arrived—this, however, we'll never know, because there's no way to reverse migration.

[5] E-Lab, where I worked after completing an MA in Art History, was founded by three people: Mary Beth McCarthy, an accountant, John Cain, a graphic designer, and Rick Robinson, whose training was in social/developmental psychology, and whose own introduction to anthropology came in the course of a study done with Xerox PARC. My training in social and cultural (aka ethnographic) research methods, because it occurred at E-Lab, was practically inseparable from my training in design. Which is not to say that I became a designer any more than I became an anthropologist. But insofar as I picked up either field, I learned them as a hybrid. One result of my training is that, even now, I tend to think of design as a form of social science (i.e. social theories or social models embodied as products) and social science as a form of design (i.e. theories with affordances, usability, etc., embodied as word+image).

[6] For more on INCITE, see www.soc.surrey.ac.uk/incite

[7] I should, however, have taken comfort from the composition of the advisory board, all present in the meeting that day. They included Dr. Sarah Jain (Cultural Anthropology, Stanford University), Dr. Christena Nippert-Eng (Sociology, Illinois Institute of Technology), Dr. Nina Wakeford (Director

INCITE, University of Surrey), Dr. Elizabeth Anderson, one of the four directors of London offices of Sapient, and Prof. Lucy Suchman (Sociology, Lancaster University, UK), who joined by telephone. All of the academics present had spent time working within or consulting for industry. I think the composition of the advisory board makes it very clear that if there are meaningful differences here, they are not going to be found in Us versus Them accounts.

[8] I'm assuming here that my reasons for worry are obvious, and if they are not obvious...well, I'm surprised, but pleasantly so.

[9] A note here on what I mean and don't mean by the terms Who and What, as used throughout the paper. They are a kind of shorthand and so are reductive both by definition and by design. I acknowledge that, insofar as "What" roughly designates the themes of our research and "Who" the subjects of that research, Whats are not always things, nor Whos always people. The point of the shorthand is to signal that, in ordinary practice, especially in commercial settings, our research tends to be conceived (reductively) in just these ways. It is based not in a desire to simplify, or elide important specific qualities of how we formulate our research, but rather to name the kinds of elisions and simplifications that I think already inhabit design research practice.

[10] All of the women in the house have a mobile phone. All of them use it, I would say, passionately. For many, it was their lifeline to far-flung friends and family, who are made to feel more far-flung by the hard exigencies of public housing in London, which forces women to live where there is housing and not where they want to live. Plus, they live in a shared house with no private phone lines. All of the women I've met use Pay As You Go plans; they do this to control costs—they live on approximately £103 per week in benefits. Even so, they all say that they spend far too much money on their mobile phone. So they seem like pretty good "users," although, as I will argue, this is not the only way we can challenge the assumption that studying homeless women is not a good way to study mobility. I should also note, before leaving this footnote, that the advisory board eventually agreed to my proposal and I went on to spend the next 2.5 years working in the shelter.

[11] Alienating…even dangerous. Jain (in press: n.d.) describes how automotive airbags are not intrinsically or innocently or incidentally dangerous to children; they are dangerous precisely because airbags are designed with a statistically average American man in mind as their user, and so the resulting product codes for or envisions the average American man.

[12] Some other examples of the "know your user" dictum can be found on the following diverse sites: http://www.lib.umd.edu/itd/web/bestpractices/know_your_user.html
http://www.uie.com/events/roadshow/know_your_users/
http://geekswithblogs.net/ajohns/archive/2004/02/03/1708.aspx
http://revolution.byu.edu/design/InterfaceDes.html
http://www.iw.com/magazine.php?inc=121500/12.15.00webobservatory.html
For examples of companies which employ ethnographic methods, see:
http://www.intel.com/research/exploratory/papr/,
http://www.nop.co.uk/news.asp?go=news_item&key=119,
http://research.microsoft.com/research/detail.aspx?id=6.
For published literature on these methods, see also: Blomberg et al 2003, Squires and Byrne 2002, Salvador et al 1999.

[13] "User" has also become lingua franca in academic books about the social science of technology, for example, *Perpetual Contact* (Katz and Aakhus 2002) and *Wireless World* (Brown et al 2001).

[14] Because apologists can always claim that it is only a word and then suggest to the critic that they just choose something else, any word they like.

[15] www.teksci.com/teksci/dictiona.asp defines "usability testing" as: "testing the ease with which users can learn and use a product." A search for "usability testing" in Google.com will provide several other definitions. The key differentiator of usability testing over the broader user research is that in usability testing, the product already exists, if only in prototype. Research in this context means watching users interact with an existing product. And by radically simplifying what anthropology calls "context," just as scientific laboratories radically simplify the messiness of the world (Latour 1995), usability testing reduces people to "users."

[16] The book *How Do I Look? Queer Film and Video*, published in 1991, was edited by a group who called themselves Bad Object Choices. This name highlights, at once, their collective investment in the work of queering film and video studies, but also the ineluctably political and politicized nature of that work, the fact that a choice of object (or topic), and a particular perspective on that object, renders the objects of their study—and via a reflexive turn, themselves—as "bad object choices."

[17] Here, we could also cite Xerox PARC's ethnography of people in airports, conducted in order to support a larger study on workplace practice (Xerox Palo Alto Research Center (PARC), http://www.parc.xerox.com/); Intel PAPR's ethnography of salmon trawlers in Alaska to support a larger study on mobility (Intel's People and Practices Research (PAPR), http://www.intel.com/research/exploratory/papr/); or the INCITE-Intel collaborative ethnography of London's 73 bus route, conducted in order to support a larger study on ubiquitous computing (Incubator for Critical Inquiry into Technology and Ethnography (INCITE), www.soc.surrey.ac.uk/incite. For documentation of the bus project itself, see http://www.73urbanjourneys.com/). Given the rules of propriety and competition, the projects above tend not to have citations, so I am limited in most cases to a mere mention of the work, although each of the projects undoubtedly deserves more extensive treatment.

[18] This is something that many anthropologists complain about, shrilly.

[19] The issue of time deserves at least a footnote. Questions of timing are critically important in commercial settings, where perhaps the greatest shift for sociological and especially for anthropological practice (the greatest migratory distance traveled) is in relation to time (i.e. duration of study). Time and development, on their own, deserve much more thought as forces giving shape to current design research.

[20] The project is called "Photos Leave Home." It has been funded by the Economic and Social Research Council (ESRC), and was conducted in the United Kingdom between the Autumns of 2004 and 2005. You can read more about the project here: http://www.photosleavehome.blogspot.com and in several forthcoming papers (Cohen in press; n.d. and Cohen n.d.)

[21] Here, I am drawing mostly on Hannah Arendt (1958) and Michael Warner (2000), but also on some writers who don't address publics by name, but who I think describe significant features of how

publics function *today*. Namely: Lawrence Lessig (2004), McKenzie Wark (2004) and Eric Raymond (1999).

[22] Copyright cases concerning digital music and software licensing are two of the more publicized, recent examples of this struggle.

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3

GRASS ROOTS CAMPAIGNING AS ELECTIVE SOCIALITY (OR MAFFESOLI MEETS 'SOCIAL SOFTWARE'): LESSONS FROM THE BBC iCAN PROJECT

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This paper is based on ethnographic research during the development phase of the BBC iCan website. It discusses how we defined the object of study that would become the focus of the site – "grass roots campaigning" and how following two stages of research we found the site's early planning (influenced by the 'social software' movement) needed to recognize the deeply contextual nature of this practice – and avoid attempting to mediate the majority of a campaign online. Working with Maffesoli's theories of 'sociality'; we understood grass roots campaigns to be rooted in experiential "being together" and less 'individual' and 'political; than commonly perceived.

PROLOGUE

I begin here as we began our project:

> *We look for the political in the wrong places, on the wrong pages of the newspaper…Those decision-making areas which had been protected by 'the political' in industrial capitalism – the private sector, business, science, towns, everyday life and so on – are now caught in the storms of political conflict…It is no exaggeration to say that citizen-initiative groups have taken power politically.* (Beck, 1994:18).

These words evoke epochal change. Since they import a necessary tone of drama into proceedings, they were good for kicking-off a project. Every client wants their job to feel more exciting; and we consultants, even ethnographers, usually oblige.

The quote comes from by Ulrich Beck; German sociologist, proponent of the 'Risk Society', and arch-theorist of reflexive modernization. His work and particularly his essay 'The Reinvention of Politics' seemed to thoroughly encapsulate what the iCan project (about grassroots action) was all about. Beck's transformational vision describes the center of politics migrating away from institutions toward the people - its force not declining as commonly believed - but transmuted and re-distributed elsewhere in the social structure. As time went on I came to find this set of ideas limited; good for starting with, but providing little grist for later analysis and interpretation. So the history of this project is also for me the history of a displacement; one theoretical mentor for another (Maffesoli for Beck).

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This paper concerns reflections emerging from ethnographic research during the 'participative development' phase of the BBC iCan website. It is the story of my struggle to describe the nature of popular activism and wrestle this understanding into useful praxis. The paper illustrates: how theory is 'lived' (and avoided) on projects where it is not a focal point; and how theory can get replaced by other forms of knowledge. It is also my contention that this research when joined with the sociological theory of Michel Maffesoli provides the rudiments for a critique of the overly political and functional accounts of grass roots activism that predominate in this area.

From the onset of the project I realized the very object of study had to be rescued from the congeries of meaning that surrounded it. Before deciding the precise grass roots thing we would research, I had to maneuver through entrenched positions of thought that saturated it. On the one hand, the 'spontaneous sociology' of lay opinion understood campaigning as the province of heroic acts and people who were dramatically transformed in the process of standing up for what they believed in. This view of campaigning (distantly derived from Carlyle's 'Great Man' thesis by way of Erin Brokovich) – runs that campaigning, like history, gets its motive force from the extraordinary individuals who push it along. On the other hand, the phenomenon had to be detached from the academics and activists who cast a collectivized version of 'the people' as another kind of hero engaged in realigning the political system from top to bottom. Finally my clients, BBC designers and planners, had their own ideas of this activity deriving from new media 'best practice' and emerging discourses (such as 'social software') within their industry.

Throughout the research work two themes emerged: uncovering the object (of enquiry) and recovering the real (how to work with and address the activity without illusions). This paper explores these themes in relation to grass roots campaigning and concludes with some reflections on praxis.

UNCOVERING THE OBJECT

In research we always begin with objects already defined by others (partially or seemingly exhaustively). A practitioner can accept the pre-given definitions of a focus of study and let this standpoint drive a project's questions, hypotheses, and inevitably condition its conclusions. At its worst, if this stance forecloses any new insights, it can lead to what one business commentator has christened the GIGO effect (Garbage IN, Garbage OUT) (Shapiro, 1998: 125). Or, the researcher can challenge, refine and re-define (mildly or completely) the focus of study hopefully breaking new ground and revealing counter-intuitive findings along the way. This is a strategy philosophers of science have long referred to as 'building the ship while sailing on the sea'. Neither approach has the monopoly on successful research; and most practitioners split somewhere down the middle.

For thinkers like Heidegger, however, the most productive part of any enquiry is the definitional stage at the onset where one carves out the object of study (Heidegger, 1927:41). For him this is because here is where ontology gets implicated: an understanding of *being* is approached through the *beings* we construct to populate the world. In more humble terms, we can say that this is where important things that should be considered get overlooked - there are often not enough 'objects' dreamt in the researcher's philosophy. Here is also where a lot of 'theoretical' decisions get made before anyone thinks they are 'doing theory'. (This recalls another paper in this conference where the author discusses how organizations often decide their 'users' in advance of any research taking place to determine them (see Cohen, 2005, this volume).

On this project, as all commercial ones, we were not engaged with any chimera like 'pure research'. Our stated purpose was knowledge-seeking in support of a web service aimed at an area that had only been partly decided. After registering a decline of interest in national politics and failing to respond quickly to some important local stories; the BBC was planning a new online resource (with the working title iCan) that would be both a listening post allowing it to tap into significant grass roots events (solving their problem) as well as a site for encouraging and enabling 'citizens' wanting to learn or do something about such issues (to address their own needs).

Defining its goals further; the BBC wanted the site to fill a gap and help people in resolving more specific issues than those covered by national broad-based campaign groups; (Friends of the Earth, Greenpeace). In other words, they wanted the site to aim at issues people could notice and start doing something about themselves. *It should be biased toward action.*

So there was a product concept (the iCan service) and a target before there was an object of study; and it was left to the team from Lodestar (the social and cultural consultancy commissioned by the BBC to carry out development research) to further define and substantiate what this should be. We did set sail, while still building our ship; but there was a tangible focus to guide us - local issues and the people engaging with them. We found that the 'space' at which the project planners wanted to aim did exist and was full of activity. Working with BBC journalists and researchers we isolated a list of 12 issues that would give us a wide range of viewpoints into this phenomenon. We were going to focus on the following:

> *Controlling the placement of mobile phone masts, stopping planning permissions for unwanted development, the impact of second homes on affordable housing, the location of refugee asylum centres, roadside speed cameras, building new airports, accountability in NHS healthcare, prostitutes in neighborhood streets, traffic diversion, gated communities, and car congestion charging*

All of which had local 'campaigns' organized around them. But what kind of thing are these? And how are they distinguished from other varieties of participation?

If, defined ideal typically 'politics' is about directing the ongoing maintenance of power or control; then 'campaigning, in contrast, is aimed at effecting outcomes aimed at what are frequently called 'single issues' rather than the structure of power. Large national campaigns on topics like the environment or nuclear power steal the limelight here. If the province of the political is often considered national; which then devolves its institutions down to local levels; 'grass roots politics' is concerned with mobilizing for national parties at a local level or getting people into positions of authority there. This still leaves a space which describes the activities of people, not officials, trying to address issues and remedies still more discrete in time and space.

As we began investigating; we could now define this as our object of study: 'grass roots campaigning' and the web service's key target audiences as 'grass roots campaigners' and would-be campaigners. The BBC was right to realize this was an overlooked and underserved area. And as we found out also poorly understood: the 'third term' or intermediate space between the more glamorous 'national campaigning' and 'grass roots politics'.

We engaged in ethnographic research with the 12 campaign groups in England, Wales and Scotland; choosing them carefully to ensure breadth and diversity: some campaigns had to be complete, some in process, some successes, some failures, etc.

We visited each campaigner or co-campaigners in the place they lived and campaigned; our focus was to capture whole cycles and we actually called our methodology "collecting oral histories of campaigns". A centerpiece of each research encounter was creating with participants a very granular timeline - charting their earliest awareness of an issue, through to the decision to campaign, and all the events that followed that until the campaign's completion (if it was finished). From this we would generate the campaign process model. 1

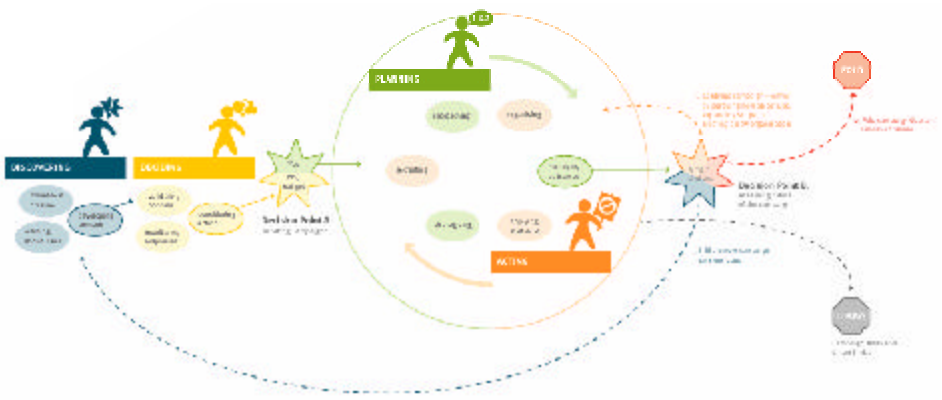

Figure 1 Grass Roots Campaign Process Model (design: Seigfried Herrnreiter)

This research lived up to its expectations; it was received as fascinating and informative by our clients. It had many insights into micro-level local activism. A key one emerging form analyzing the intricacy of the campaigns we modeled was that campaigning is more about late nights in front of a spread sheet or coffee mornings with the neighbors, than manning the barricades. Just as every battle or event has its unsung heroes who are said to tip the balance; the majority of the work of campaigning is accomplished during such 'unsung moments' (literally - ever heard a protest song about typing up a petition?).

As researchers we were exhilarated because we seemed to be getting at the 'reality' of grass roots campaigning. A reality almost eclipsed beneath the romanticization of protests and 'direct action'. For their part the BBC design team was expecting detail so they were especially pleased with the 'campaign process model'. One of their earliest research questions for us was to find out what campaigner's need at various stages of their campaigns. The model with its 4 main stages and multiple sub-stages, fulfilled this request and we knew the new media team planned to design *with* it as a cornerstone of their process.

Not long afterwards we saw the 'system conceptual model' the design team had prepared.. This was one of those crystallizing moments - when the whole ethos behind a project gets revealed at once.

A statement in the diagram read : “What is the journey between being an active user and a passive user?” It appears, the early iCan was re-conceived as a kind of machine to turn timid citizens with vague concerns into full-fledged campaigners - via the alchemy of a host of new media tools. 2

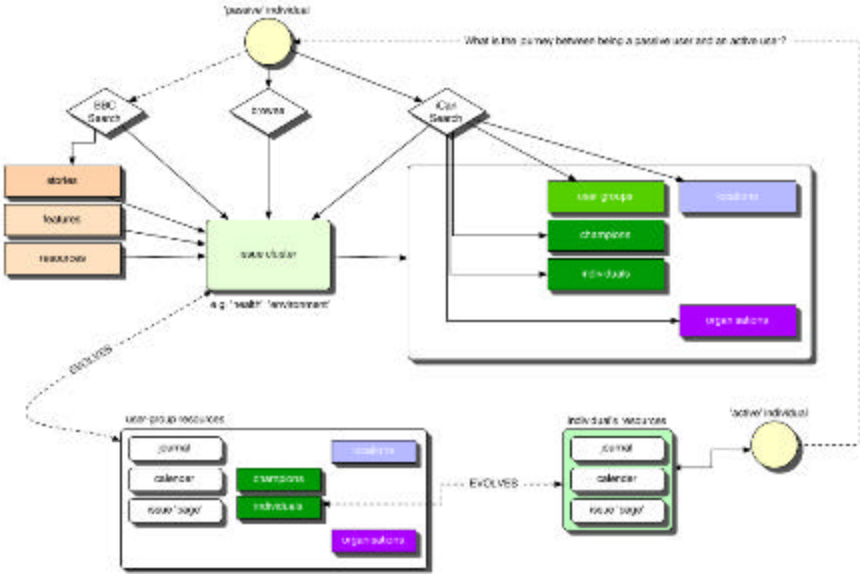

Figure 2. iCan System conceptual model

In the prototype that eventuated from this model, the iCan concept had morphed from its initial modest goals into a one-stop shop “For all your campaigning needs” (as the slogan ran across the homepage). It was to function as a ‘campaign management hub’ where campaigns were created, staffed and directed online. To signal its intention for action the site had a prominent button labeled ‘start campaigning’ on its homepage

When we tested it with users in a second stage of research there was strong interest in the subject of campaigning but almost unanimous rejection of the way the site portrayed and engaged with the practice. One participant said “This site for me would generate constant R.T.F.M. errors (Read The F..king Manual)”. While another dismissed many of its features as “Self-reflexive bollocks”. Almost all the experienced campaigners said the concept site made campaigning look *more* difficult than it actually was. ‘Start campaigning’ and the empty form that followed it was met with universal confusion.

How could research artifacts that were to us so accurate lead our client in the wrong direction for its prototyping? There are clearly some lessons for praxis here which we will reflect on later. In short, the BBC team had interpreted the predominance of ‘unsung moments’ - the daily organizational grind of campaigning - to mean this was the place they should focus the site for users. So they had added core features like organizing meetings online, sending emails and posting strategy ideas to fellow campaigners through the iCan system.

Building on the research from Lodestar, the designers had implicitly assumed a tight correspondence between the detailed stages of the campaign process model and the design of the website; and in addition to the initial bias toward action, we had helped them unknowingly assume a new bias toward comprehensiveness. Together we had made a powerful theoretical decision, without even knowing we had been theorizing.

The prospective 'users' of iCan had also mad e an equally powerful decision: a rejection of the site concept's tendency to totalize the life of a campaign as a new media experience, and as something to be wholly mediated through iCan. In contrast they desired to employ a website only as a sometime resource - favoring to keep the brunt of their campaigning more firmly anchored in the local world that gave birth to it. They equally wished their campaigns to be fed and filtered through multiple sources and means, not dominated by a single conduit.

Yet the research input was not solely responsible for the site concept becoming too overweening and ambitious. Some of this can be attributed to the usual new media 'scope creep', but there was another powerful actor here: Winter 2003 was the rising tide of the 'social software' movement and members of the project team had attended seminars at the BBC with Clay Shirky himself. The ideology of this movement would have communicated that campaigning was just the kind of human activity that could be migrated online and the proposed functions and features list would have been influenced by this climate (as the system conceptual model already showed its hallmark goal of building 'active' online community members). Therefore this 'zeitgeist', should be seen alongside 'data' and 'theory' as another force directing action and decisions in this context, as in so many others.

RECOVERING THE REAL

Campaigning as elective sociality

Two scenes:

January 2002,
Gatley, Stockport
Greater Manchester, England

> *Almost the whole road seems to be in the streets still though it was dusk hours ago.There are people sitting in the road manning the petition table and others milling around waiting. There have been vigils every night this week; but today the surveyors from the phone company came to prepare the site and the residents blocked them from getting through. A lot of people think they may come back tonight, so they're ready. It's cold but they're taking it in turns and the people outside before are now making soup for those still there. Lisa's practically had an open house since she became the campaign HQ. They say the only phone*

> *mast going up around here is that phony one they built over there…*(Informant data, Lodestar research 2003)

August 2002,
Church Lawford,
Near Rugby, England

> *It's another Village Fête day and everyone seems to be having a good time. There are stalls selling jam and cakes. There is a woman turning the tombola for a chance to win a prize and a man selling chances to guess the weight of the cake. Next to him is a table with a big sign behind it with the face of Alistair Darling (the Transport Secretary) and some airplanes saying: Unless YOU Stop Him This Man Will: Destroy our homes, Contaminate our air, Concrete over our heritage…And if they don't stop him there won't be a village left to have the fête next year.*
> (Informant data, Lodestar research 2003)

The preceding sketches are illuminating – they show a glimpse of the everyday world from which campaigns grow. But these scenes could also be misleading. They may suggest grass roots campaigning only emerges from 'traditional communities'; from places where people know all their neighbors and the bonds are very tight. You might believe the phenomenon we are examining is a throwback to the past.

Figure 3 Lisa (far right) and the Gatley anti-pone mast campaigners

But the truth is both Lisa in Gatley and Mark in Church Lawford said they had not known their neighbors very well. "Before there was some small talk in the streets, but now we really know each other afterwards", said Lisa. So grass roots campaigning seems to have contributed more to 'community' in than being a result of it. We must look elsewhere for the forces that brought these people together.

The French sociologist, Michel Maffesoli has created an elaborate narrative about the decline of individualism and the return of a culture of tribes Embedded inside this epochal framework are provocative sociological insights from Weberian and phenomenological perspectives.

> *Society thus understood cannot be summarized by any old rational mechanism; it is experienced and organized, in the strongest sense of the term, through encounters, situations, experiences within various groups to which each individual belongs.* (Maffesoli, 1996: 88)

For Maffesoli "the essential problem of social reality is relationism" (Maffesoli, 1996: 86), not how society as a whole is created – but how the innumerable micro-groups that are continually forming and dissolving come together. Just as 'Time' and 'History' were for Heidegger (1926) abstractions built from the more primordial human experiences of 'temporality' and 'historicality', so for Maffesoli 'society' is a structural abstraction out of our primary experience of 'being-together' with others, which he calls "sociality" and believes is the key attraction of motivating contemporary group formations. But unlike the strong 'social' obligations in traditional community, group allegiance here is a ultimately a matter of choice, hence it is called 'elective sociality'. In this it follows the "logic of the network" rather than that of the clan group.

These ideas resonate with the reactions we received to the iCan prototype. They were getting something out of it beyond the drive toward reaching campaign goals. I started to think that the power of "being-together" could be the charismatic core of these kinds of groups - even if they were ostensibly devoted to serious, 'political' action. Maffesoli had borrowed the term 'syntony' (mutual tuning in relationship) from the phenomenological philosopher Schutz to describe the quality of moments when,

"...individuals in interaction are epiphanized in a vivid presence." (Maffesoli, 1996: 73)

This description matched with stories of group experiences we had collected within the twelve campaigns; about late nights after work spent brainstorming together; about breakthrough moments where the whole strategy of a campaign shifted because a committee of four people seemed to think as one. Sue and Dionne (who campaigned against a refugee center planned for their town) were ten years apart, had never met before; but became fast friends spending nearly every night together: smoking, answering queries on the phone; mobilizing support and planning their next move. Throughout our interview they spoke as one. We as researchers were someone else to win to the cause (like the reporters before us) and they presented a united front, which seemed to go far beyond a presentation.

If this was the quality of collaboration on even some of these campaign, it goes some way toward explaining why the new media campaign experience we were proposing evoked a visceral rejection especially in the more experienced campaigners.

Although Maffesoli does not himself address the issue of local activism; it is my contention that grassroots campaigns are a species of group generated from sociality; and that this description better accounts for the phenomenon than rival 'political' explanations.

The situation in Gatley outside Manchester was that a mobile phone company planned to put a phone mast at the end of a quiet street overlooking a playground. Lisa, our informant noticed the planning application on a lamp post and began to tell other residents of the plans and research the possible effects of living near masts (headaches, sleeplessness) They were especially concerned of any effects on children. She had never been involved in politics or much interested in it .But throughout 15 months of campaigning she acted as defacto leader. There were countless informal meetings at Lisa's house and larger ones in pubs (with 120 attendees); there were newsletters, a march in the street, petition tables and week-long vigils in the road (as described above). All told a lot of 'being-together'. Most of the road's resident's elected to enact their sociality by supporting the campaign. They were united by a common repulsion against the idea of the phone mast.

The issue at hand was even more unequivocal for the residents of Church Lawford. According to government plans to build a new airport outside Rugby, their village would be under a runway (hence the reference to concrete). Obviously this had a mobilizing effect; most locals were appalled at the idea of the destruction of their village. So most supported the campaign with committee members like Mark playing a larger role as its media spokesman. (He had been totally non-political and in his earlier life and was a police officer who required special permission to participate). On call all day every day for the campaign, if the airport plans are shelved, Mark said "He will hang up his campaign hat and *get his life back*."

So we see that campaigning can be a time of collective upheaval but also one of classical" collective effervescence" (Durkheim, [1912] 2001). All the more so for revolving around something more taxing than hobbies, sports or leisure (as Maffesoli's examples tend to run). For the people exposed to this special form of "experiencing the other" (Maffesoli, 1996: 73) campaigning was truly a means of constructing community. All by way of encounters, situations, and experiences within a group to which they would not normally belong. As Lisa attested, life in the neighborhood was not the same afterward.

Maffesoli has a further concept that illuminates the nature of grass roots activism. He believes sociality brings with it a sense a *communalized empathy*" (Maffesoli, 1996[2]:136). I realize this kind of empathy was a catalyst running through the campaigns we studied. Empathy that was no longer person to person, but had become generalized across a locality. Empathy for a common fate that a nearby other is enduring alongside you. Empathy for having to move out of your village, for having a mobile phone mast placed in your road, for having traffic routed into your quiet residential street. If this kind of active empathy is achieved within a collective, it solves a host of traditional political problems at a single stroke (without resorting to the usual 'political' means): from the communitarian dilemma (Sennett, 1998) of how to motivate civic action (by instilling an ethic) to Rousseau's ([1762] 1969) panacea of transforming the 'will of all' into the 'general will' (by suppressing individual self-interest). But such empathy is usually only effective for a very proximate area (within which people can share common experiences) and for one issue (or 'object') at a time. Ergo, this mobilized will cannot be relied upon as the basis for an ongoing political project.

Consequently, for many reasons, I want to question if what we observe in grass roots campaigning is best described as politics at all. Where Beck was correct, in the statement I began this paper with, was in identifying business, science, towns, and everyday life as current hot spots. But he was wrong to proclaim that what motivates people toward such 'sub-politics' – is the desire to get involved in "shaping society from below" or taking part in "the substantive technification and industrialization process" (Beck, 1994:23). This sounds like the objectivist viewpoint of a systems theorist who cannot imagine any motivation outside of the logic of the system. These groups may have encroached on the political sphere because their voices can act as limiting factors on others' decision-making power. But grass roots campaign groups do not want to run or control business, science, towns, or everyday life. Rather they want to defend and respond, or protect and guide certain outcomes in these areas - when the normal political system seems to have impacted on the way they want to pursue their everyday lives. Thus, grass roots campaigning is not so much an extension of the political; of politics reaching down into a subterranean zone as Beck would have it, but a natural extension of the process of 'sociality'; of it electing to reach out and safeguard something it values– be it quiet streets, an uncluttered seafront, or an affordable house in the town where one grew up.

Moreover, this reaching out is something well within sociality's grasp. Neighbors who can plan street parties and raise money for the PTA can likewise organize to prevent a mobile network operator placing a phone mast next to their playground. Viewed this way grass roots campaigning is more like a slight re-focusing of the 'logic of the network' (its members now aggregating to play a new role as campaigners) than any kind of bold transformation of the socio-political structure. The energy that gives rise to it has not migrated from elsewhere a la Beck; and certainly does not entail the people becoming like' little politicians'. Likewise, totally unlike the classic discourse on political activism (Marcuse, 1969), (Huenefeld, 1970); grass roots campaigning does not require its members to be 'politicized', get emancipated, and 'raise their consciousness' (in a jargon which already sounds quaint); or, in short, become a different sort of person than they already are. (This underlines Maffesoli's emphasis on 'roles' rather than essentialised 'identities'). In our understanding of grass roots mobilization– based on attraction, repulsion, and empathy – ideology had little part.

Furthermore, this activity fails to satisfy classic definitions of politics – with a focus on gaining and maintaining power, office or control. (Johnson, 1995) Power in itself is not a goal, and almost never referred to here; although campaigners often want to direct some of its "effects" (Foucault, 1980) towards their areas of concern (or more frequently to block or re-direct some its effects). So to sum it up in Maffesoli's terms,: grass roots campaigning is a phenomenon whereby certain members of the 'mass' that he describes as having "in a quasi-intentional sort of way, as its sole project, its perdurability in existence" join with other members to take the actions they see as necessary (temporarily acting in a *quite intentional* sort of way) to protect the quality of that existence. And then more often than not they return to 'everyday life'. (As Mark talked about hanging up his hat). Additionally, through involvement in a campaigns people reap the benefits of connecting to those around them through "shared sentiments; deepening their ties to 'community' and their chosen sense of identity within it.

This is a description that I believe accounts for the motivations and dynamics of the twelve campaigns we studied in the iCan project. However, Leo from Cornwall (a hairdresser and 31 year old campaigner for affordable housing) who best expresses the ethos of grass roots campaigning when he says, "I never desired to start a group or be part of a movement…I just see things around where I live that need sorting out, and I try to sort them." (Lodestar/BBC, 2003:3)

MAFFESOLI MEETS SOCIAL SOFTWARE

If before we were guilty of somewhat reducing the nature of grassroots campaigning to process we always had the data (as above) to open us to another point of view. Sustained theoretical reflection with Maffesoli had made us realize that while understanding the intricacies of campaigns through our models, we had neglecting to sufficiently take account of their context. Now we were in a better position to 'explain' or at least describe why the iCan concept had been so soundly rejected. This theoretical engagement also furnished us with material to arm our client against the temptations of the zeitgeist that had shaped their approach to this website.

Comparing theory to practice, a striking early discovery was how closely some of Maffesoli's descriptions of sociality overlap with the standard discourse on internet communities (Jones, 1995) (even down to a preference for the network metaphor) (Kelley, 1997). Yet living in this context left our informants unwilling to participate in their campaigning activities through iCan**.** In part, this is to some degree inevitable when translating a group of practices from the 'real world' to 'online' (especially ones that, we found, rely heavily on the charisma of face-to-face interaction.) However, I believe this loss of context was only part of the issue why test users rejected the early iCan prototype. They also seemed to be reacting to a deeper miss-alignment in the very concept of the site.

While seeking to understand the experiences of these differing 'parallel worlds' – campaigning as presented on iCan, versus campaigning as we saw it in localities, I arrived at the following scenarios:

> 'The Social': the campaigner, as an 'individual', is free, contracting, and joining in egalitarian relationships. This forms the basis for the project of politics = iCan campaigning
>
> 'Sociality': the campaigner, as a 'person', is dependant on others, accepts a social context and joins an organic whole = local campaigning

This is adapted from Maffesoli (1994:66, 6) explaining the epochal shift from 'individual' to e 'person' but amounts to a categorical description of the phenomenology of these two approaches to campaigning. The miss-alignment can be explained thus; our users had been acting under the second scenario; then encountered a website built according to the first. In the terms above, the iCan site really was a kind of '*social*' software, whereas what we should have been designing was software for sociality.

For there is no doubt about it, the early iCan concept embodied its own social theory (or 'mental model') based on the first scenario above: free individuals who are registered members were to be given rights on the site; allowed to communicate with others, as long as certain rules are obeyed - no flames – supervised by online moderators. Every member was to be encouraged to create a campaign which had to be founded by someone who was then its 'owner'; who could continue to manage the campaign online; documenting its progress, and communicating with other members.

In the twelve cases we studied, campaigning as we came to understand it, was not only not this direct, coherent or individualized, but was often socially emergent. The campaign duos were compelling evidence of this. Leo and Luke, who had been separately monitoring the housing issue in their town for a year, took letters to the editor of the local newspaper on the same day, expressing

outrage about the lack of affordable housing for young people. They were soon introduced by the paper's reporters and decided to campaign together immediately. Sue and Dionne's collaboration began in similar circumstances, and there were many stories of whole campaign committees that just seemed to organically coalesce.

In 'reality', it is not heroic lone Erin Brockovichs who create the majority of campaigns, but a case where there is an immanent want woven into the situation of a place and time; and several 'persons' or a group (usually on the margins of being in direct relationship) rise out of 'the mass' to take on campaigning as a 'role'.

So the major problem with the iCan concept was not merely that it required doing things through the internet; but that its mechanisms and tone were too functionalist (meaning both too individualistic and too directly political - it made the users feel they would be removing this activity from 'the logic of the network' (e.g. *their* network, Maffesolian definition), and its basis in sociality. While tapping into a 'network effect' (new media definition) was considered of secondary importance, if helpful at a latter stage (many of the campaigns did have their own websites, used for campaign PR not management).

Social software "can be loosely defined as software which supports, extends, or derives added value from, human social behavior - message-boards, musical taste-sharing, photo-sharing, instant messaging, mailing lists, social networking." (Coates, 2005). And it main proponent says succinctly "It's software that supports group interaction." (Shirky, 2003).

Despite all its talk of 'the social', groups, and networks; 'social software' as a body of practices still posits the sovereign individual (as alone as one of Leibniz's monads) ([1712 1965) sitting at the keyboard picking and choosing who to engage with. Social software projects an individualist fantasy of the social, or what one could refer to as 'the social sublime' as it places the torrent of social life safely at a distance - on the terms of the 'user', subject to his control and escape. While promoting the idea of making connections with others, social software, also functions to protect its users *from* the social. To be 'within' sociality is not to have such distance. It's to share common sentiments, empathy or even a common fate with those around you. You may choose whether or not to get involved – but once you are 'in' a group, involved with it you are. Disengaging from sociality is not like getting rid of someone you chatted with on IM, grew bored with, can delete from your addresses and block from messaging you.

In contrast to the social theory conveyed by the prototype site, I believe it was this more embedded experience of 'sociality' (outlined above) that our participant campaigners were embodying when we showed them the ideas for iCan (how you could start a campaign online; or create an 'e-poll' to test your support in advance) and their response was simply to mutter – "But it's not like that…its not like that". For them starting a campaign online would feel like a making a public speech act with no one listening.

The social/political model also introduces a rationalized culture into grass roots campaigns (which are as unlike the traditional 'Robert's Rules of Order' type of association as any group could be). Yet when imagining it transferred to iCan (which like many web services mediates human action through forms, processes and hierarchical procedures) this is what is conjured up and the charisma of campaigning dissipates under it spell.

We must emphasize that for Maffesoli the two scenarios above are not neutral choices. From society to sociality there is historical directionality – our cultures and collectivities, he believes, are moving away from the era of rational individualism toward person-centered absorption into locality and the mass. There is no doubt the internet and new technologies play a large role here (in connecting and consolidating tribes as well as providing them with 'objects' (such as photos or jokes) to coalesce around (Engstrom, 2005) But in the iCan concept we had tapped into the web's most rationalistic potentialities to envisage a service more reflecting the ersatz politics of 'society', than the grass roots 'sociality' of the present. And we had done so despite using the 'latest' technology and being part of one of the newest web crazes of the time (at least for the serious-minded). This uneven development is not surprising when understanding technology change is simple compared to adapting it to how people change (Norman, 1998).

To put it more bluntly, the technology of social software may be state of the art but its social theory is a bit naïve, even untheoretical. Here I am in accord with Paul Dourish, who in a recent paper opined that this movement employs a "highly positivist interpretation of social phenomenon – a sort of social science, perhaps, uniquely attractive to engineers." (Dourish, 2005:1). I think this raises the question of how frequently the people who make appeals to the social in the new technology actually draw on social science (as we practice it) at all. 3

The most cited new works on social networking in the blogsphere; Linked (Barabasi, 2003) and Nexus (Buchanan, 2002) are both by theoretical physicists. These works are significant within our industry context; they fire people up, spawn brainstorm ideas; stimulate new market propositions and later provide a rhetoric of justification for them. This is just part of a long history of technology bypassing our space for the 'harder' sciences. So why are we as ethnographers or interpretive social scientist so often left out of these 'conversations'? Is it simply because our representations of sociality are often too indeterminate, nuanced and troublesome to serve as a rhetoric of justification? Just as on this project *Linked* might have told you the full-strength iCan was a great idea; ethnographic research plus Maffesoli revealed the irritating old-fashioned preference for embodied contexts and face-to face interaction. (Maybe projects cannot bear too much reality?). Yet if we are gaining a greater share of voice in these conversations we must wait see how influential it can become. (EPIC will help the cause).

As well as partaking in the newly-coined zeitgeist of social software, I believe the iCan concept ultimately rested (for its conception of the social) on one of new media's hoariest theoretical fundaments – the perennially under analyzed concept of the "community of interest". For those who have reputedly undergone this 'shift' the activity in question would be just another interest group to take part in. But I believe I have shown the natural ground from which grass roots campaigning arises is 'elective sociality' which in its characteristics occupies an intermediate space between traditional communities and the 'community of interest' (as so described). Therefore a site concept built on the presuppositions of the later would not be expected to complement this practice. In particular such campaigns usually require longer gestation periods; more shared experience, and closer bonds as a threshold compared to many online interest-groups. Again, theory gets done where you least expect it; and while we were seeking to newly define our object, pre-set parameters held sway.

For some e-democracy propositions (such as FaxYourMP.com or Political Compass) the goal of streamlining interaction online with the state political system is the way to go. (This is a classic example of 'National Politics').But we had extended this same approach to an area of very local campaigning that does not conform to the usual definitions of political action or benefit from the same optimizing

logic. Of course there have also been other successful online 'campaigns' as well, such as the celebrated case of Charlene Blake and Chrysler. 5 But this was a patently a campaign that does work via a one-dimensional 'community of interest' (Chrysler owners with defective brakes) and being distributed across the country was key to creating the kind of aggregated power that here effected change (an example of a 'National Campaign' rather than a grass roots one).

Social software may be the best existing technology for building active *online* communities. Instant messaging, photo-sharing, and forums bring the pulse of sociality especially when introduced to a static or editorial-based website. And the movement has helped engender a "resurgence" in this area of technology (Boyd, 2005:1). But as we belatedly realized, the early iCan concept was trying to work in the opposite direction – not adding an element of 'social life' to an online community but extracting a practice that normally grew in embedded contexts and trying to grow it online instead; or transplant existing campaigns there. For *grass roots* campaigning, this would represent not an *intensification* of social activity but a *diminishment* of it.

Once you realize this, it makes the very idea of iCan as a one-stop campaigning hub seem a kind of category error (or misuse of the technology). The realization also begs us to re-consider the idea of tools to support group interaction. To fully recover the real here we have to ask not only what 'supports' an activity; but what supports it most successfully. For grass roots campaigning, we must come to the conclusion that it's not any *software* that best supports it; but rather embodied human *practices* that don't need to involve it - weekly committee meetings, planning brainstorms, vigils. And they support it best in two senses: practically; in achieving campaign goals - they are the most direct and unmediated ways of deciding on issues and making decisions, and morally; they are the kind of practices which consistently generate 'group solidarity'.

This is crucial because the main way campaigns fail is by petering out before reaching goals - owing to members losing motivation. So the most important sense in which you can 'support' a campaign is by *sustaining* it. These practices fulfill this: meet ings, brainstorms, vigils - being together, thinking together, being resolved together. The power of 'vivid presence' and acting in harmony appear the best means to preserve the "communalized empathy" upon which campaigns rest and through which the reciprocal will to continue are reinforced. As much as we love online tools we may have to admit forms of being-together are more 'optimal' for this task. As Maffesoli continually argues against his rationalist colleagues, much of social existence can not be explained by instrumental rationality. (1996, 21). Despite engaging in seemingly goal-oriented, purposeful action, the people involved in grass roots campaigning need to rely on other bases to maintain the effort (the kind of bases only an awkward social scientist might point out when faced with an innovative new technology concept).

It is probable that a grass roots campaign may be an intensely local object that can only have limited parts of it mediated remotely. In this, according to Maffesoli it would simply be partaking in "the spirit of the times" whereby "it is the 'local', the territorial and proxemic that determines the life of our societies" (Maffesoli, 1996, 57). Viewed this way you could say the industry zeitgeist of social software had fallen prey to a far wider one.

REFLECTIONS ON PRAXIS

If it now seems surprising that the iCan concept was working with social software in the reverse direction, viewed from within the new media world it was a perfectly logical extension to make - just

applying the available tools to a new area. Starting from the knowledge that people (who may be isolated or disconnected) have concerns, it seemed to follow that by using the web the BBC could connect them to information, other activists, and give them a online forum to wage their campaign. Nothing could be more simply and efficient.

What explains how this unlikely proposition came to seem so promising is the power of the logic of practice within this design community. The history of this project can be understood more generally as a case where one community of practice imposed its logic on another; even as it was attempting to serve it. If practices have been defined traditionally as skills or habitual tasks vs. a newer conception of them as more creative, construct ive, or improvisational, (Knorr Cetina, 2001: 175) then this difference can be said to correspond to the variations between building a website and conducting a grass roots campaign. The one has its standards, rhythms and routines (including firm timelines and deadlines) as well as its trained professionals. The other is emergent even as a 'community of practice', often populated by first timers trying to learn as they go along and engage their issue for as long as it takes to win. Surprisingly, in this framework the grass roots campaign, not the web design project would be considered the more creative 'epistemic object'; characterized by its open-endedness and 'incompleteness'. Maybe for this reason the attempt to remake it would be inevitable.

For it appears such practices are not equal; the one with more established sense of order seems better able to apply its procedures onto the other. So, for a time the practice of grass roots campaigning was itself re-designed to fit the logic of the web design and tools planned to support it. By the crucial means of the campaign process model that was our prime output we, the researchers, had reshaped one community of practice to fit the functionalist assumptions of the other. There was a messy real-world campaign all laid out in linear stages. Now wonder the team thought all they had to do was design some features to support them all. An outcome Knorr Cetina anticipates when she says "Research work seems to be particular in that the definition of things, the consciousness of problems, etc is deliberately looped through objects and the reaction granted by them" (Knorr Cetina, 2001: 175).

We researchers were so embedded in the new media process that there was a failure of praxis at this point. Rather we were contributing to the process what it wanted of us. Likewise the social logic of grass roots campaigning (which we had studied in depth and context) was initially overlooked allowing it to more closely (if tacitly) fit the prevailing 'community of interest' idea for which the social software tools would have been adequate.

How had these oversights occurred while still delivering work that appeared first class?

We were too focused on our own methodology and techniques

The BBC new media team had requested something like a process model and we knew how to deliver it. In the complexity of projects it is seductive to have an approach that feels cut and dry. But here is a cautionary y tale – to beware methods not grounded in theory and to keep seeing research artifacts in relation to the context out of which they are created. By not doing so, we turned grass roots campaigning into the functionalist object the design process wanted; but not one its 'users' would have embraced.

We paid to much attention to what our clients wanted

Rather than, in classic consultant speak, giving them what they really *needed*. We knew they desired a deep anatomization of the grass roots campaign to flesh out their design and it felt good to be so fully 'looped' into the project. If we had interrogated our data more deeply (in addition to model-

building) after the ethnographic stage we could have offered more designer 'tough love', "But campaigns really don't work like that…" Instead we confirmed their bent toward an all-encompassing solution. (Ultimately amended after prototype testing).

We did not have enough theory

We also did not initially have the right theory (or enough theories) to challenge the power of the new media design process; or to direct us to look in more depth at the places we had forgotten to pay attention - the embedded situations from which campaigns arise. We also had never explicitly theorized the relationship between the research models and the design. I had believed it necessary to employ a 'high theory' to describe grass roots activism (Beck) but had neglected to develop another kind (a human-centered design theory) to explain how to apply our research to the site concept and design.

> *Good training in theory and acquaintance with its latest results is not identical to being burdened with 'preconceived ideas'. If a man sets out on an expedition, determined to prove certain hypotheses, if he is incapable of changing his views constantly and casting them off ungrudgingly under the pressure of evidence, needless to say his work will be worthless. But the more problems he brings with him into the field, the more he is in the habit of moulding his theories according to the facts, and of seeing facts in their bearing upon theory, the better equipped he is for the work. Preconceived ideas are pernicious in any scientific work, but foreshadowed problems are the main endowment of the scientific thinker, and these problems are first revealed to the observer by his theoretical studies.* (Malinowski, 1922).

Despite Malinowski's 1920s diction and touching faith in science, I think this is still the best statement on the conditions for meaningful praxis in ethnographic fieldwork. It also diagnoses my situation on the iCan project. Like many ethnographers meeting the timescales of commercial work, I had not brought enough 'foreshadowed problems' into the field. Especially not enough to resist being sucked into the maelstrom of social software 'best practice'. But I had enough respect for empiricism, that when the prototype concepts tested badly, I went looking for explanations and turned to theory. Discovering Maffesoli helped us go back and see the 'facts' we had gathered on grassroots campaigns in a new light that shaped the practical recommendations we would make for the website.

Our interpretation of grass roots campaigns: as emergent in nature; as rooted in experiential being together; and as human projects driven by affect and effervescence as much as efficiency and purposiveness lead to a different concept of iCan. The site is now more ad hoc and modular, works to a user's own timescale and agenda; it acts as a resource to feed their efforts; instead of the platform for staging an 'online campaign'. There is no longer a 'start campaigning' button (mandating action) on its homepage. The BBC showed amazing responsiveness in scaling back the proposition we had all arrived at.

If it had taken an outright user rejection for praxis to be achieved, this can significantly be attributed to another force which complemented and intensified the usual new media logic of practice. I said earlier that iCan's early concept was influenced by the zeitgeist of social software. And Martin Vogel, the iCan Project Leader, confirms that the team saw themselves as "Definitely part of the social

software movement" (Vogel, 2005). Zeitgeist is a powerful force in the new media and technology industry; and it is operationalised by the almost universal practice of benchmarking (looking at choice selection of products doing similar things to your area and learning from them; borrowing and improving ideas and approaches). What this technique also does is spread the influence of a zeitgeist; creating norms of what is right to do; what would be the old hat approach; or what would be innovative and 'cutting edge'. Furthermore, because these objects are out there and tangible, for a time their very existence says more about their essential rightness than any evidence could prove or disprove.

Why is this an issue for praxis? Because zeitgeist gets in its way; becomes an obstacle in the necessary dialectic between theory and data and our attempts to resolve their encounter into an outcome we can apply in practice.

Zeitgeist often has more charisma (in the Weberian sense of authority derived from a non-rational basis) than either theory or data; and therefore has more power to influence. On this project we were all swept along by the social software zeitgeist for a time. We all believed that drawing from it we had invented the ultimate linear, rational, solution "for all your campaigning needs" (whoever you were). The iCan project leader offered a further explanation for the hub concept "We got a bit too fixated on that from a research point of view, and even design point of view, probably because it was the most innovative part of the proposition". (Vogel, 2005). The desire to be innovative within one's community of practice is a strong motivation whatever your community, and its significant to note how this can lead one away from the perspective of the 'end user'. This project also reveals how on top of customary habits and ways of doing things every such community is swept by ongoing revolutions in practice that may carry equal weight.

All until the shock of the prototype testing results brought us out of the social software zeitgeist and back into alignment with our users - the local campaigners and would-be campaigners already immersed in their networks of sociality. They did not want iCan to run their campaigns; just contribute to them.

Sometimes research works; it really does restore a sense of the real. You can't put too much importance on being empirical or theoretical; and for this project you can't overemphasize the importance of two stages of work. Achieving knowledge on this project was cyclical. But the latter stage of the project devoted to 'practice' - using prototype screens almost as a projective technique - proved more fundamental for opening up and developing our ideas about grass roots campaigning than the ethnographic research phase would have on it own. It was also through responding to this crisis and drawing on Maffesoli; that we were able to 'do theory' on this grass roots phenomenon rather than simply 'using' it.

Therefore, I want to suggest polemically and certainly seemingly self-servingly, that commercial ethnographers can achieve a profound knowledge of social phenomena while pursuing their commissioned work. And furthermore, that in many ways they are in an equally good position to those in academia to do this. Structurally, Bourdieu (1988) has shown how academic career trajectories are advanced through point scoring by critiquing the texts of the previous generation of luminaries. While this endows them with many 'foreshadowed problems' (pace Malinowski) it also means that in many ways an academic career is oriented practically toward the past – possibly drawing on present research just enough to complete the critique of the elders.

Now in client work, one of the drawbacks has always been that we are hardly free to pursue our own research agendas (which of course creates difficulties in amassing cumulative knowledge). But it also means we are often guided (more like thrusted) by our clients into hot spots of emerging phenomena – especially as concerns the socio-technical nexus. In blundering into a fresh area where we, or maybe no one has previous expertise; but by being empirically-led as well as theoretically informed, we can break new ground (even without research grants). As I believe we did by extending Maffesoli's theory to grass roots campaigning.

Having a wealth of past references ready-to-hand is not enough alone to guarantee you are in a position to grasp social reality. One can apply in-depth theory at a latter stage when it can still shape the outcome. So I want to suggest that Malinowski's quote can be read both ways: not only - contra applied or commercial ethnography - that we may not be in the position to take as many theories or 'foreshadowed problems' into the field. But also - contra academia - that by having more theories (and being more professionally invested in them) academics may be more likely to hold on to them – and not refine them in light of empirical evidence. Having too few or too many pre-conceived ideas *can* spoil the account – and thereby omit the unexpected but essential. Sometimes it may be best to observe naively, or at least uncommittedly, as long as you don't stay naïve for too long. As much of the SST (Sociology of Science and Technology) work has shown, we are not all as heroic as Malinowski – professional thinkers often stick by their 'pre-conceived ideas' and don't much enjoy seeing them falsified. (Latour, 1988).

To combat this I certainly am not advocating naïve empiricism; or letting the 'facts' speak for themselves. One could never interpret this paper as downplaying the significance of theory – Maffesoli is after all the *deus ex machina* of this story. My point is simply that in Malinowski's formula: 'moulding theories according to the facts' vs. 'seeing facts in their bearing upon theory', this must be a balanced equation. And on balance I suggest people on projects usually major in the former and minor in the later; while for academics the relationship often runs in reverse. This gives both camps an equal chance at getting at the 'truth' of phenomena. But, I believe, gives project people an edge in effecting praxis.

Finally, in interpreting this paper, I want to offer a warning and an encouragement. In this discussion of an early project concept that scaled back on its plans to facilitate grass roots campaigning through a website, I am not trying to re-circulate any simple distinctions about the value of the 'real' vs. the 'online' world. As devices and systems become more ubiquitous and more usable these worlds are certainly becoming more continuous. Instead my message is that we must always remember the importance of context and that adoption and adaptations only ever occur within contexts; and will do so at variable paces and degrees of success for different human practices – with no inevitability that all practices will get there in the end. We do ourselves (and our clients) a disservice to overestimate the rapidity or desirability of all such change, because real innovation in technology design will only take place by delineating, as a baseline, exactly what people feel does gets lost in the transformation. If anyone can and should do this, it is we ethnographers. There is no other professional group who can both look so deeply into (and out from) a user's internal (emic) perspective while remaining resistant enough to the lure of technological determinism to say the content of these perspectives still matters. Therefore I see our prime role as double; as defenders of the real and the best chroniclers of context.

I guess my only enemy here is the zeitgeist; because I believe swallowing a zeitgeist leads to hubris and over-extension of the preferred means at hand; while sustained theoretical reflection leads to humility and more appropriate praxis. The warning is serious; there are zeitgeist everywhere (industry and theoretical ones too) that condition and direct human action to such an extent that we often do

not know we are making their choices. And there is something like the whole bias in our culture towards two magic words called 'new technology' that even we as critical ethnographers don't always manage to escape.

So while not being naïve either about any transcendental value in theory or data; I think we should everywhere be on guard against the unthought assumptions of the zeitgeist - in whatever guise it may take – including our own professional hubris.

NOTES

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[1] It was an 'experience model' of the type pioneered at E-lab by Rick Robinson, who describes them as "tools to think with". The method was to take the detailed data about events within each of our of twelve grass roots campaigns (forming a broad sample) then aggregate it into a general form which would be a visual and descriptive representation of a wide range of present and future campaigns.

[2] Steps in the transformation would include - using an: online campaign journal, calendar, and 'issue page'; as well as interacting with: user groups, champions, and individuals; in different: locations and organizations.

[3] Shirky (2003) Draws fascinating conclusions from WR Bion – the pioneer of group psychotherapy - to illuminate the nature of 'individual' / 'group' interaction. But this is still a decontextualised study from an ethnographic standpoint.

[4] Without sounding overly schematic, 'elective sociality' can be seen as occupying the same intermediate or 3rd space in the evolution of more recognized 'social' forms; that grass roots campaigning occupies for the 'political'; which is why it seems to follow that the one gives rise to the other.

[5] In an early example of an internet campaign, Charlene Blake posted her comments on defective brakes on her Chrysler to a newsgroup evoking responses from hundreds and leading to a class action suit against the automaker and recall of 350,000 vehicles

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4

ETHNOGRAPHY, OPERATIONS, AND OBJECTUAL PRACTICE

TIM PLOWMAN
Cheskin

This paper raises issues around commercial ethnographic praxis and its relationship to social and cultural theory. Michel de Certeau's theories around everyday life practices and Karin Knorr Cetina's concept of postsocial objectual practice are juxtaposed in order to explore how commercial ethnographic practice might seriously engage with theory and transcend some of the assumptions that currently constrain its application within industry.

INTRODUCTION

In thinking about products and services in relation to people there appears to be much at stake that lies outside the traditional scope of design research – gender, macroeconomic, and ecological factors come to mind. From an ethnographically-based perspective, further considerations necessarily come into play when critically assessing the contribution of design research beyond that of a highly under theorized, emergent subset of business practices. This paper is an initial attempt at a theoretical intervention into the relatively field of commercial ethnographic practice. Ultimately, it is simply an effort to start a conversation among its practitioners by problematizing the scope and approach of this new application of social science. This interrogation of commercial ethnographic practice is facilitated through an examination of the common theoretical ground between the social historian Michel de Certeau's (1984; 1998) notion of operations and cultural anthropologist Karin Knorr Cetina's (1997; 1999; 2000, 2001) theory of objectual practice. The points of convergence that emerge between the work of these two theorists provides the field of commercial ethnographic research with a perspective into everyday, object-centered sociality that is intended to provide one example of how this type of ethnographic work can be theoretically informed.

De Certeau's contribution is in his refusal to endorse the opposition of high culture versus popular culture and by extension the dichotomy of creative art versus mass production. Over the course of his research, he analyzed what he called operations or silent procedures that people perform with designed artifacts, institutions, language, and other cultural objects. What was at stake for him is the very particular ways people use some readymade objects and engage in common practices, the way they organize their private space, their workplace, the way they practice their environment and the space available to them such as kitchens, malls, streets, airports, train stations, and neighborhoods. His choice of research sites allowed de Certeau to focus his theorizing on the ordinary practices of people's everyday life experiences. He replaced the presupposition of passive mass consumption of objects and products with the notion of large-scale, anonymous creativity by ordinary people. For de Certeau every person can be regarded as a *producer* through the art of recycling objects, adapting and transforming Heidegger (1962) calls "ready-at-hand" instruments or products, objects that become extensions of ourselves.

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On its face, Knorr Cetina's work appears quite different. She begins with proposition that this century has seen the rise and increased centrality in social practice of what she calls knowledge objects – objects that deeply engage their subjects. The onset of modernity resulted in an initial buoying of state sponsored sociality with the development and expansions of the welfare state. This enhanced sociality was accompanied by an increased reliance on social explanations (for example collective causes for accidents and the rise of class action lawsuits). Recent decades, however, have seen the retraction of welfare state, the emptying out of primordial social relations, and the narrowing of social imagination and the onset of postsocial relations. While the Internet and to a lesser extent ubiquitous computing has supported minor new forms of sociality, this void has been increasingly filled with a growing reliance on objects in the social world.

According to Knorr Cetina, objects are increasingly replacing and mediating human relationships, especially in what she calls knowledge societies. While much of her work focuses on the behaviors in and around the objects central to experimental laboratories and financial markets and other complex phenomena, Knorr Cetina's nuanced treatment of object relations has obvious implications for the commodities, services, and experiences that we consume. For Knorr Cetina, social relations appear to have become less social in the original sense of the term. A close examination of her work in this area reveals this loss of the social as a less pessimistic position than it might appear at first blush.

Commercial ethnography, as a sub-discipline if one can call it that, is for the most part theoretically unproblematized and perhaps that is the way it should remain. The discussions that do take place tend to focus on methods and ethics. Maybe theory should not have a place of prominence in this emerging enterprise – perhaps not even middle range theory that modestly starts its theorizing with delimited and empirically-based aspects of social phenomena, as advocated by Robert K. Merton (1968). Theory in social science tends not to be "results oriented" in ways that meet the expectations of capitalism for return on investment. This is not a complaint. It is simply a matter of what I see as fact. But as practitioners, we all know that theory stealthily creeps it way back into our work, sometimes uninvited, edging our interpretations this way, pushing us towards that conclusion, even if it is often invisible to colleagues and clients alike. I believe that if the enterprise of commercial ethnography is to grow, mature, and advance beyond a research method (like focus groups) used by economic and political concerns, a wider range of theory will have to be more explicitly and consistently integrated into what we do.

For commercial ethnographic research to result in better products, services, experiences, and systems it is not enough to produce better versions of these things in the conventional sense. For social scientists (and designers, developers, product managers, and senior management) the question should become: how can we produce artifacts so they radiate the degrees of freedom necessary to enhance the self-invention that de Certeau observed given the increased mediation of sociality through objects? Or phrased slightly differently; how can an object cultivate a social relationship with the consumer that goes beyond attributes like affordances, brand, and interactivity? Over the years I have put this only slightly ridiculous question in various forms to a number of unfortunate fellow practitioners only to hear replies like "that's a trick question" or better, "are you insane?"

But I find this proposition too compelling to drop and think it's important to attempt to re-articulate the logic of operations and objectual practice through a commercial, ethnographic frame of reference that takes recent developments in cultural and social theory seriously as opposed to treating them as a strictly academic, unrelated, and irrelevant endeavor. The increased importance of design within business planning and strategy and the rapid appropriation of ethnography by design and

market research provides a unique historical moment – one that is already closing – for commercial ethnographers to articulate the importance of theories that explore areas of human behavior usually glossed over by conventional research currently carried out in the for-profit world by interests in the United States and Western Europe. This paper tries to take a small piece of this larger and more complex set of structural relations by asking if the cross pollination between the two seemingly unrelated theories of de Certeau and Knorr Cetina can provide commercial ethnographers with the beginning of a basis for enhancing adoption and adaptability in design or at least thinking about its role differently? Perhaps more critically, it is important to ask whether this direction comport with the business imperatives that tend to favor hyper-consumption and individuation, apparently inherent in the production of goods and development of services?

DE CERTEAU

The first section of this paper explores a small number of de Certeau's ideas about cultural consumers. Specifically, I examine his understanding of current socio-economic conditions, the idea of productive consumption, strategy and tactics, and embodied behaviors like walking. The latter serves as a model for thinking about what consumers' make and do with objects and ideas they encounter. These concepts provide the context for thinking about opportunities for leaving consumers space for sociality and self invention as well as frame the relevant work of Knorr Cetina in the subsequent section.

In his discussion of everyday practices performed by people as they move through their lives, de Certeau methodically peels back layers of assumptions and beliefs about the modern conditions of our existence in order to set the stage for analyzing how we get by in the world – that is, the swarm of acts that comprise our being in the world. This is critical move is central to his project; nothing about our understanding of human thought, activities, and behavior is taken for granted in his analysis. Exactly the same process needs to occur in commercial ethnography. Needless to say, this constitutes a radical departure from the place where most discussions and opinions on commercial ethnography are situated. Rather than compartmentalizing this epistemic rupture as beyond the legitimate scope of commercial ethnography, a more productive move is to leverage his critique of established modes of knowledge production in our own field. This seems a necessary, if difficult first step.

In his introduction to everyday practices, de Certeau argues that our lives are dominated by production that is "…rationalized, expansionist and at the same time centralized, clamorous, and spectacular" (1984: xii). It is important to note that this is not a value judgment on his part. These, he argues, are the objective conditions in which we live. While his language lacks precision, the general thrust of this claim is undeniably true, especially for developed and emerging economies. It is apparent in things as diverse as recipes, arms negotiations, product manuals, and car designs. As consumer-citizens we reap vast benefits from this mode of socio-economic organization. But this trade-off, while worth noting, is not his primary concern. De Certeau wants to understand what people *make* and *do* when interacting with objects, places, representations, and environments in spite of (and because of) this particular mode of production. This is a starting point that precedes current heuristics of commercial ethnography such as user needs, emotion, meaning, and experience.

To help us move beyond the heuristics of commercial ethnographic practice, it is worth reemphasizing a point that de Certeau, as have other social scientists, makes about individual experience:

> Analysis shows that a relation (always social) determines its terms and not the reverse, and that each individual is a locus in which an incoherent (and often contradictory) plurality of such relational determinations interact (1984: xi).

The basic but necessary point is that our experience of the world is essentially contingent, relational, and disjointed. The terms of our relations play out in concert with our interactions with other people like our co-workers, institutions like supermarkets, spaces like city streets, and texts such as television which are cast in a particular socio-economic context. In articulating the idea of a person as a locus of things, processes, and representations, de Certeau privileges our acts of productive consumption or secondary production – a far less spectacular and clamorous form of production. What is the delta between what has been produced and the frequently unselfconscious secondary production hidden in the process of its consumption? What can we learn from that secondary production's logic? What can this secondary production tells us about our relationship with objects, environments, and services that goes beyond the impoverished imagination of design research? The concept of secondary production or productive consumption within a rationalist socio-economic order that is often experienced incoherently implies an unusual user perspective but one worth exploring.

De Certeau's idea of productive consumption is different from what some social scientists have labeled agency in theorizing how people consume things – the basic idea that people reappropriate and reinterpret objects for their own purposes. Giddens (1984) developed his version of the analytic category of agency in his theory of structuration. In his formulation he distinguishes between discursive and practical knowledge, recognizing actors (or agents) as knowledgeable and reflexive. That is, they know what they are doing and orient their behavior in relation to structures. Moreover, their knowledge is reflexive and situated and habitual use or practice (e.g., consumption) becomes institutionalized. In this conception agency is articulated in relation to structures. Michel Foucault's and Pierre Bourdieu's ostensible contributions to the theory of everyday practice are also found wanting. Foucault analysis of micropolitical techniques stops just at the point where de Certeau's interest begins; what people make or do when confronted by the instruments and effects of power. Similarly, de Certeau finds Bourdieu's efforts to frame practice in relation to socioeconomic rationality and as largely unconscious unconvincing.

De Certeau's approach is more nuanced and explicit. For example, he is interested in consumption at the level of behavior (watching television) and representation (the meanings or "legends" communicated through television) but more importantly he is interested in what the cultural consumer "'…makes' or 'does' during this time and with these images" (1984: xii). It is doubtful that most of the practices that people make or do become institutionalized in the way that Giddens imagines – they are often unselfconscious (as opposed to unconscious) , too muted, too fleeting, a murmuring of the crowd, a remainder with regard to where design research typically places with emphasis. These operations are a way of thinking fused with a way of acting: "…an art of combination which cannot be dissociated from an art of using" (1984: xv). This embodied, multi-modal way of operating in the world that I will call "thinking-doing" is usually what commercial ethnographers are called upon to try to parse, decontextualizing elements and linking them back to business assumptions about relevant aspects of human thought and behavior.

Situating our fused thinking-doing in a society that is overwhelmingly functionalist, productivist, and over-determined beyond our comprehension – a world of our own making but exterior to us nonetheless – means that we exist in conditions that are primarily defined and occupied by systems of production. When consumers use, do, and make, it is within the contexts of multiple, defining systems

that are not specifically designed to leave consumers any "…place in which they can indicate what they make or do with the products of these systems (1984: xii). The absence of degrees of freedom is marginalizing and yet all of us engage in acts of creative appropriation at the very point where practices ceases to have their own language – where they are unnamed, falling outside the scope of our rationalist grid or understanding and ordering. Reliance on this rationalist grid has an undeniable logic when it comes to efficiently or not so efficiently designing products but it unreflexively forecloses any potential infusion of the language and art of consumption into our closed systems.

Two concepts developed de Certeau can help us think more deeply and critically about how we might break open the closed circuit of design research. The first of these is what he refers to as a strategy.

> A strategy is "…the calculus of force-relationships which becomes possible when a subject…(a proprietor, an enterprise, a city, a scientific institution) can be isolated from an 'environment.' A strategy assumes a place both conceptual and physical that can be circumscribed as proper…serv[ing] as the basis for generating relations with an exterior distinct from it (competitors, adversaries, 'clienteles,' 'targets,' or 'objects' of research) (1984: xix).

Strategic things have carved a temporal and cognitive place out for themselves and generally set the terms of their own conditions and interactions. Designed artifacts, objects (commodities), and spaces are the material and congealed instantiations of strategies, having both a physical and conceptual place. But isn't this precisely the object of design research and commercial ethnography, to render the strategic in terms that are more user friendly? This is not to suggest that products should not have a strategy, simply that strategies might be less totalizing in character. Given de Certeau's account of strategy in our experience of social relations, the implications of designed user friendliness now seem more like a filter that captures isolated fragments of our making and doing and distills them back into a strategy through the productivist logic of place.

Tactics stand in contrast (and alignment) to strategies in that they are dependent. A tactic is also a calculus, an interaction that has no place in and of itself.

> The place of a tactic belongs to the other. A tactic insinuates itself into the other's place, fragmentarily, without taking it over in its entirety, without being able to keep it at a distance. It has no base where it can capitalize on its advantages, prepare its expansions, and secure independence with respect to circumstances (1984: xix).

Despite exhibiting continuity and permanence in some cases, a tactic exists only in relation to the operational conditions of existence provided by a strategic place and tends to be the purview of the consumer. Everyday practices like reading, shopping, walking, cooking, and brushing teeth are tactical. Translated into the language of commercial ethnography, tactics are in some respects equivalent to user behavior or needs seen from a very different vantage point. In contrast, strategic places are frequently muting and totalizing simply because of the necessary conditions of their own existence – they emerge into the world from the stronghold of a place expressed institutionally, socially, and culturally and thus organize tactical expression.

According to de Certeau we have not always lived in a world dominated by tactics and strategy but it is important not to mistake his assertion as a form of nostalgia. He dispassionately describes the breakdown of local stabilities, changes in the way people read (1984: 166-169), and the receding practice of orality (1984:132-139) as markers for the emergence of strategy and tactics through the centuries. These and other factors have contributed our current conditions in which society is "…no longer fixed by a circumscribed community, [where] tactics wander out of orbit, making consumers into immigrants in a system too vast to be their own, too tightly woven for them to escape from it" (1984:xx). The language of this passage goes to the heart of the matter. Consumers are immigrants. We visit, touch, cling to, but never really own or are liberated by objects, processes, and spaces in ways that transcend rationalist consumption. But this is not to say that we are passive. And although immigrants, our tactical practices with regard to reading, for example, facilitate "…the drift across the page…the improvisation and expectation of meanings inferred from a few words, leaps of written spaces in an ephemeral dance" (1984: xi).

Enunciating Bodies

Relying heavily on linguistics – Wittgenstein provides the philosophical blueprint for his enterprise – in articulating his theory of tactics, de Certeau uses the metaphor of enunciations occurring in different registers and pragmatics to emphasize the temporal and context dependent character of everyday practice. He theorizes walking as a form of rhetoric and indeed most forms of user interaction can be theorized as rhetorical expressions: "[t]he art of 'turning phrases finds an equivalent in an art of composing a path" (1984:100). Walking, as is true with other everyday practices, forms a kind of poem – in this case on the sidewalk, around the corners, up the stairs. Walking is neither foreign to the spatial organization of an urban environment nor is it fixating conformity. In a key example, de Certeau again resorts to language to describe how walking enunciates. He use the concept of synecdoche and asyndeton to describe how walking manipulates spatial organizations. Synecdoche consists in representing a larger entity with a piece or fragment of that entity (e.g., head is taken for cattle). Asyndeton is the omission linking words like conjunctions and adjectives (e.g., be one of the few, the proud, the marines). Both these processes are present in a range of consumer interaction and use and should be designed for. They create a sort of swelling and shrinking phrasing on the part of consumers as they engage with objects, processes, and spaces; "[a] space treated in this way and shaped by practices is transformed into enlarged singularities and separate islands" (1984:101). Stand in any plaza and watch how people use the space, observe someone as their hands move across a mobile phone, watch a child pretending to comfort a doll and you will see the tactical oscillation between these two poles articulated in various ways.

But if walking is a manipulative act on the part of the user, it is also an expression of a system of urban planning, a normative discourse. Similarly, lip gloss, the automobile, processed food, glasses, and clothes can be regarded as "…instruments through which a social law maintains its holds on [our] bodies…regulat[ing] them and exercis[ing] them" (1984:147). The process of correcting, removing from, and adding bodies appears to be an endless means "…by which a society represents itself in living beings and makes them its representations" (1984:147). Our bodies and interactions are telling a code, we believe and we act, we are practitioners of the "real." Despite the commonsensical and self-fulfilling character about how we decide what are the right things to do or have done to our bodies, our behavior, according to de Certeau, is not speech that "does not know" what it says. That turn of phrase, speech or behavior that "does not know" what is says, is trope that he returns to again and again in order to destroy the idea that our interactions with objects and systems are thoughtless and unarticulated as opposed to rationalist, technocratic discourse. They are articulated in the doing, in the

moment, without a discourse to create a strategic place and constitute a vast reserve of social and self expression.

In this section I have examined a very limited number of concepts from de Certeau's work in order to provoke thinking around the implications of everyday practices for design and commercial ethnographic research. While the concepts of fused thinking-doing, strategies, tactics, place, and rhetoric provide us with different ways for thinking through the contributions of commercial ethnographic research, they are also incomplete in important ways. But after all de Certeau's intention in writing "The Practice of Everyday Life" was simply to create the possibility of discussion and not produce the definitive account of these concepts. To complicate matters even further, in discussing the key concept of tactics, de Certeau suggestively remarks that "[t]hese ways of reappropriating the product-system, ways created by consumers, have as their goal a therapeutics for deteriorating social relations and make use of techniques of re-employment in which we can recognize the procedures of everyday practice" (1984: xxiv). Although not explicitly stated in terms of a "therapeutics" Karin Knorr Cetina's work on objects, sociality, and the postsocial condition provides insight that sheds light on this very suggestion of compensatory transformation by de Certeau.

KNORR CETINA

More sociologically than linguistically inclined, a starting point for Knorr Cetina's work on objects and sociality stems from the observation that practice theory has tended to neglect creative and constructive practice in favor of an idea of practice as recurrent, specifiable, and schematized preferences and prescriptions. Here she joins in de Certeau's critique of Bourdieu, by arguing that the focus on habitus and rule-governed behavior obscures important aspects of the social life of artifacts, objects, and systems. Like de Certeau's everyday practices, object-centered sociality is an active process in which people insinuate themselves into object worlds through specific practices. While many other authors have sought to analyze aspects of sociality with respect to objects, particularly in terms of consumption and identity (Lury 1997, 1998; Slater 1997; Appadurai 1986), Knorr Cetina's work stands out for its empirically grounded elaboration of the processes and mechanisms of object-center sociality. This is precisely the piece missing from de Certeau's account of tactical reappropriation by consumers.

Like de Certeau, Knorr Cetina argues that the ways that we inhabit the world and conceive of our relation to it have changed radically over time. The emergence of a multiplicity of practices associated with literacy, science, medicine, globalization, and other institutions has emptied out previous forms of sociality, creating a "postsocial" society. The existence of postsocial relations does not mean that sociality has completely receded from our lives. On the contrary, current articulations of social principles and structures have simply broadened to include objects. For Knorr Cetina the receding of more local (human) social relations has produced two parallel processes: increased individuation and an "expansion of object-centered environments which situate and stabilize selves, define individual identity just as much as communities or families used to, and which promote forms of sociality that feed on and supplement the human forms of sociality" (1997:1). Objects that act as sources of the self and provide relational intimacy are simply another form of sociality.

It is important to note at the outset that much of Knorr Cetina's focus on object-centered-sociality is in the context of what she identifies as expert or epistemic cultures. Epistemic cultures are "…amalgams of arrangements and mechanisms…in a given field [that] make up how we know what we know…[and] create and warrant knowledge" (2001:1). She argues that we live in a world "of increased reflexivity mediated by expert systems…[and] that today's individuals engage with the wider

environment and with themselves through information produced by specialists" (2001:177). The expert systems are usually productivist in character and have a proper place from which to capitalize on their gains. It hardly matters whether we are talking about physicists refining hypotheses like supersymmetry or Dr. Phil dispensing advice to the emotionally troubled. The products of epistemic cultures, like computer science, are less important than the processes and knowledge-related forms of embeddedness themselves. This represents a departure from de Certeau's view of everyday practices constituted by the cultural consumer making-doing and tactically renting space in the congealed objects, systems, and environments of strategic knowledge, be it a skyscraper, written language, or a Dodge Dart. The discharge of knowledge relations into society "…has become constitutive of social relations" (Knorr Cetina 1997:8). It is, increasingly, how we are social.

Knorr Cetina's focus on creative practice and de Certeau's concern with fused thinking-doing raises an obvious question: to what degree can practice in relationship to designed objects, processes, and environments support transformation and invention? De Certeau is not decisive on the point of transformation – for him the consumer's interaction is almost a running battle as much as it is a poem. In discussing reading as a kind of poaching, he forcefully argues that it is incorrect to assume that "…consumers settle down…[that] the only freedom supposed to be left to the masses is that of grazing on the ration of simulacra the system distributes to each individual" (1984:165-166). But are the enlarged singularities and separate islands that characterize what people make or do an example of transformative practice? Does the wandering through a mall as one might wander through a text change either? Again, these tactics employed by cultural consumers are temporal, lacking a proper place. By de Certeau's own logic the practitioner cannot "…keep what it acquires, or does so poorly" (1984:174). Perhaps transformation does not depend on the literal acquisition of cultural resources and that the character or mode of the relations themselves can support transformation and invention.

Knorr Cetina analyzes object-centered sociality from the perspective of "…characterization[s] of practice [that] might make the notion more dynamic and include within it the potential for change" (2001:175). In order to do this she explores the role of objects in epistemic or expert cultures with the goal of unpacking the idea of an expert and technical competence. The primary characteristic of object relations in these cultures is dissociation, forcing the subject (self) to stand apart from the object (work) through modes of relating that are characterized by interruption, reflection, and abstraction. Paradoxically this standing apart is precisely what allows the subject to deliberately and reflexively loop her cognition and consciousness through the object. The loop is reflexive in that this form of immersive relation permits the object to "speak" back to the subject – to reveal itself. This kind of creative or constructive interaction occurs when practice becomes non-habitual or non-routine and both subject and object are potentially modified, articulated, and constructed.

This emphasis on the creativity of non-routine practice appears to stand in contrast to analyses of interactions with commodities and instruments that simultaneously posit more performative and unselfconscious but nonetheless goal-directed activity. This emphasis seems to deviate from de Certeau's thinking in that it is precisely the normative, routine-inducing systems and structures that serve as incubators for the cultural consumer's silent procedures. Do objects, processes, or spaces that are instrumental and/or commoditized fail to provoke the kind of sustained or productive reflection required for sociality? According to Heidegger instrumental objects are "ready-to-hand," meaning they have "…the tendency to disappear while…using [them]" (Knorr Cetina 2001: 180). They become an unproblematized and uncreative extension of us. In contrast to Knorr Cetina dismissal of instruments and commodities from the realm of creative practice, some authors like Celia Lury speculate modern conditions of production and technology have redrawn the lines between self and object and that

commoditized objects define us by what we do with them in the form of "performative self understandings" and "experimental individualities" (Lury 1998). This view of the prosthetic extension of the self into society although new to cultural studies has it origins in the work of Emile Durkheim and Marcel Mauss and it further augments de Certeau's characterizations of cultural consumption.

In fully understanding the centripetal force objects can play, a number of different dimensions of the phenomena merit investigation including, "…personal object ties, object-centered traditions and collectives, and object-created emotional worlds" (Knorr Cetina 1997: 9). While a range of objects might amenable to this analytic framework within the context of non-routine practice, the scope of Knorr Cetina's analysis is limited to object relations centered around what she calls knowledge objects. She argues that these sorts of objects are fundamentally different from commodities and instruments – an argument that I do not find completely convincing and which she qualifies in her most recent articulation of objectual practice (2001: 187). A strict view of knowledge objects associates them with scientific investigation (e.g., protein synthesis) but recent work has extended the notion of what a knowledge object is. For example, Ewenstein and White (2005/06) examine the role of visual representations of an epistemic object in the form of an architectural project to create a new Herbarium in the Royal Botanic Gardens at Kew. Winroth (2003) looks at the securities market in the Stockholm Stock Exchange. Knorr Cetina herself, in a more recent work, analyzes foreign exchange market activity at a Swiss bank (Knorr Cetina & Bruegger 2000). Granted these types of knowledge objects are deeply complex but the concept can also be profitably extended to computers, software, and the Internet (Turkle 1984, 1995) and, as I will argue at the conclusion of this paper, other less complex objects.

An essential characteristic of knowledge objects, Knorr Cetina argues, is their lack of completeness and instability (1997:10). These objects present their subjects with an ontology that is continually unfolding as they are explored, mapped, watched, and analyzed. They are processes and projections as much as they are things and possess a characteristic non-identity with themselves; they are never fully attained. Provisional representations, partial understandings, or stand-ins provide a presence and pathways into the object world but there always remain significant absences – in a sense they are present and absent by turns. This instability of these objects is the inverse to the fleeting and rhetorical relationships of de Certeau's unselfconscious cultural consumers. But this is only one side of the equation, the mode of engagement on the part of the subject is equally important. Knorr Cetina employs Lacan's account of the mirror stage to argue that interactions with knowledge objects are accompanied by a "structure of wanting" on the part of the subject which in turn engenders mutuality or reflexivity. For Lacan, the mirror stage establishes the ego as fundamentally dependent upon external objects, on an other.

Knorr Cetina takes this idea and argues that structures of wanting are always directed at an empirical object that can never be adequately describe through language. Thus rather than eliminating lack, languages and models tend to exacerbate it. This is true of knowledge objects: "[i]n one sense one could say that objects of knowledge structure desire, or provide for the continuation of the structure of wanting" (1997:13). By introducing Lacanian theory, part of what Knorr Cetina is trying to do (aside from asserting that the libido plays a role in object-centered sociality) is foreground the robust, reflexive, and experientially-based object relations that can occur around a knowledge object. This goes well beyond mere positive emotional associations in that object-centered sociality is open dynamic and allows for a "…variety of conceptions and implementations" as well as a mix of sustaining attachments to an object (1997:14).

This section has explored several concepts of Knorr Cetina's work, specifically the notions of postsociality, knowledge objects, and structures of wanting. I have also tried to demonstrate that concepts represent productive points of contact between her ideas and de Certeau's. In this final section I want to raise some practical implications provoked by the comparison of these two theorists particularly in areas where they diverge. This divergence, however, is more suggestive of complimentarity than of incommensurability.

IMPLICATIONS FOR COMMERCIAL ETHNOGRAPHIC RESEARCH

An initial point of difference between the two theorists, with implications for theorizing around sociality and ethnographic praxis, concerns their differing accounts of the consequences for the breakdown of local stabilities and local socialites. For Knorr Cetina the flip side of increased individualization resulting from the emptying out social relations is growing practices around object centered sociality. These practices tend to occur within epistemic, white collar cultures although not exclusively so. De Certeau casts his net into a different social stratum, concerning himself with the ordinary man, "a ubiquitous character, walking in countless, thousands on the streets" (1984: dedication). This has implications for the modes through which the two groups develop objects relations and the relevance of sociality to their respective practices.

Knorr Cetina's subjects tend to lose themselves in knowledge objects through a mutuality of wants on the part of the subject and lacks in the object but it is something the subjects have to work to create, there is frequently significant labor involved in producing that mutuality (1997: 17). Can we attribute the same investment of labor on the part of the desiring subject when it comes to commoditized objects? I think it can be argued that to a lesser degree this is true. There is often an expenditure of labor leading up to, during, and after the act of consuming what Knorr Cetina generally regards as commodities or instruments. For example, researching the purchase, mastering the interface, and conspicuously using a cutting edge mobile communication device can involve significant labor. Granted such a device might be classified as an instrument or even a commodity but when acknowledging the work done in affective computing, Knorr Cetina also admits that emerging technologies are producing objects of such complexity that they may be amenable to the analytic framework she proposes.

The question of sociality in epistemic cultures and everyday practices is one of degrees as acknowledge by Knorr Cetina (2001:187). The increasing availability of complex commodities along with the development of continually changing versions has created a class of products she calls "epistemic everyday things" (2001:187). Examples might, include sophisticated sports equipment, software, computers, mobile computing devices, video and computer games, and the Internet. While this list does not contain reading and cooking, it is evidence that object-centered sociality exists outside of expert cultures. The difference between traditional epistemic objects and epistemic everyday things perhaps has less to do with their complexity than with the ability of the object to key into a structure of wanting on the part of the consumer and provide some absences or blank spaces.

De Certeau's murmuring masses also tend to lose themselves – but tend to do so literally in systems in which they are immigrants. Yet through their acquiescence they take possession and create changes and impressions. De Certeau compares consumers to renters who drive interaction with "…their acts and memories…their own history: as do pedestrians, in the streets they fill with the forest of their desires and goals" (1984: xxi). The difference here is, to some extent, the relative power granted to the subject in a high energy physics lab as opposed to a city street or at home opening a new

cell phone. In the physics lab the subject might be said to have far more points of contact with their object, and perhaps, ultimately, more control over it. But I would argue that there are sufficient avenues of control available to cultural consumers in the context where everyday practices play out. After all would so many consumers throw away or never read the instruction manuals that accompany the products they purchase or refuse to read a map on the cusp of getting lost while driving if that were the case? One could argue that de Certeau's silent procedures are simply the slightly impoverished, commodity-oriented version of Knorr Cetina's object-centered sociality. But to argue that, in fact, no sociality inheres in everyday practices would be a mistake.

De Certeau talks about the tactical way cultural consumers insinuate themselves into objects, forms, and environments. In some respects, a classic confirming example of this may be William Whyte's study of small urban spaces (1980). Whyte employed classic ethnographic techniques and time lapse photography in an exploration of how people use urban space, particularly plazas in New York City. In delineating the essential elements for the successful use of urban space (sitting space, light, trees, water, elevation, proximity to foot traffic, etc.) he discovered that 101st street in East Harlem functioned as well as the highly frequented Seagram's Plaza in Manhattan. The obvious difference between the two examples is that the Seagram's Plaza was designed and 101st Street emerged from silent procedures. The key point here is that a number of Whyte's recommendations based on how people appropriate public space made it into the City's zoning and development codes, thus transforming both subjects and objects. This is a diachronic example of mutuality by proxy – the reflexive feedback loop that is so critical to Knorr Cetina's notion of object-centered sociality is there, it simply occurs at a social level, through generations, and across space.

Whyte's observations or people's use of small urban spaces also revealed the specific modes of consumption, the ways in which they unselfconsciously looped themselves through aspects of an environment (public space). De Certeau's model of enlarged singularities and separate islands (1984: 101) is precisely mirrored in Whyte's analysis: the concentration of people on the corners of steps, the avoidance of the center of large open spaces for encounters, the myriad groupings formed by its visitors. Using Whyte's work as an empirical example of de Certeau's theories also highlights an aspect of Knorr Cetina's theory that is underdeveloped. The absence of a meaningful discussion of physical and sensuous components of object-centered sociality, how the body might be engaged in conjunction with the mind potentially extends the range of ontological unfolding, especially for epistemic everyday things.

In trying to more explicitly apply some of de Certeau's and Knorr Cetina's thinking to the concerns of commercial ethnography, a few problems begin to emerge: can reappropriation and insinuation be designed into a product and how much engagement is too much? For a great number of product designers and developers transparent instrumentality is a highly desirable goal – the gold standard. Push the object into the background, lower the cognitive burden, and make things as simple as possible to interact with. In this regard, most products have followed the path of Heidegger's ready-to-hand concept as a model to be aspired to. Commodities are also expected to be stable and complete as opposed to possessing lack or continually unfolding. Can you sell things to consumers and develop services and environments that are never quite themselves while balancing the demand for reliability and predictability in other areas? Moreover, can the potential for meaningful reappropriation be designed into these things or is that a self-defeating act?

Ultimately, reappropriation will occur in any event and may even be designed into the next iteration a product at a simple level through the observation and distillation of unintended uses.

Consumers will always "turn phrases" within systems as readily as they will rigidly follow instructions. The scope of inclusion in a product's next iteration for unanticipated uses of things may increase as competition for resources grows. The degrees of freedom built into a product or experience has as much to do with its required resources as the way it is presented to the cultural consumer – the way it is conceived of in a productive, rationalist framework. The flipside of the question of reappropriation is the issue of product stability and is perhaps even more complex.

The British sculptor Richard Wentworth has explored the stability of objects through his film "Making Do and Getting By" and photographs (1987). For Wentworth "…[t]he real object…will not remain stable; even the most commonplace bucket, broom, chair or table, will conduct meanings that lie beyond appearance" (Warner 1994:13). In contrast, many products and services, at least the ones that commercial ethnographers are involved in, are essentially instrumental in character – they are a means to an end or ends. The addition of a lack or instability into a product or service would, from the perspective of human factors, usability, etc., seem to doom it to failure or worse, conceptual art. Products and services have a built-in elasticity in a way similar to Knorr Cetina's epistemic objects. A key characteristic of these objects is that they exist in multiple representative forms such as models, calculations, on-screen representations feeding into structures of wanting. The same could be said of objects in the world of commerce and labor. Plans, organizational charts, advertising, messaging, branding, and corporate identity are also simulacra that, to some extent, lead an existence independent from their object. It could be argued that the branding of commodities fulfills a similar role to the one Knorr Cetina's ascribes to sustained or "theoretical reflection" (1997: 10). In thinking about design, this implies the requirement for some level of cohesiveness in terms of how a subject understands interdependent representations but it also allows for flexibility (instability) if the object is designed at the level of experience, going beyond instrumental and/or cognitive dimensions.

In light of the demands for relative stability in many classes of products and experiences, designing in the capacity for non-routine behavior is arguably what sets some products and experiences apart in a very crowded marketplace. If de Certeau is to be believed, then commoditized objects, experiences, and environments are often the site of non-routine but logical behavior. That logic simultaneously conforms to the system in which it is articulated and lies outside or on top of it. Again, this behavior may not possess the systematic purposefulness of the lab environment, lacking a place, but the tactics employed can, as William Whyte's work demonstrates, produce transformation.

As noted at the outset, the goal of this paper is to raise a series of questions with regard to infusing theory into commercial ethnographic practice around issues of object-centered sociality. I have used the work of de Certeau and Knorr Cetina to explore the notion of cultural consumption that both appropriates and reflexively loops through everyday epistemic objects in order to think about the current limits of product, service, and environment design as well as the ways in which commercial ethnographic practice might transcend those limits. The recent focus by businesses on designing experiences as opposed to products or services represents a somewhat constrained effort in this direction. The effort is constrained precisely because the work, when supported by ethnographic data, generally organizes its thinking through congealed models like a hierarchy of needs, human factors, values, market segmentations, and user requirements. It fails to examine the subtleties of the particular modes through which we engage objects and spaces. This effort also ignores the accompanying productivist, socio-economic context creating the conditions of those personal but fundamentally social experiences. If ethnographic praxis is to having a truly meaningful impact at the level industry and consumption it cannot afford to engage in practices that create the same omissions.

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THE COMING OF AGE OF HYBRIDS: NOTES ON ETHNOGRAPHIC PRAXIS

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It has been nearly 15 years since Donna Haraway wrote in Simians, Cyborgs and Women that, "In so far as we know ourselves in both formal discourse and in daily practice we find ourselves to be cyborgs, hybrids, mosaics, chimeras." While Haraway's referent was not the community of practitioners, scholars and change agents assembled for the EPIC conference, her attention to the particular arrangements of material goods, human labor and social relations in processes and histories that have consequences for people's lives resonates with the themes addressed in the workshops and with concerns that bring many of us to this conference. In this talk I will explore how ethnographic praxis is constituted in and through our focus on the here and now of everyday practice by which logical divisions and dualism such as material – social, virtual – real, local –global, spiritual – secular are unmasked. Recognizing both our hybrid subjects and our hybrid identities, I will suggest we turn the analytic lens on ourselves and acknowledge and celebrate our pragmatism, our lives on the margins, and our commitment to the ephemeral, situated orderliness of everyday practice.

INTRODUCTION

The EPIC Workshop track was designed to allow conference participants the opportunity to discuss topics of interest in a small group context. The conference organizers thought it would be useful to create a forum where people from throughout the EPIC community (e.g. workplace studies, technology design, marketing, business) could share their experiences and perspectives, and begin to map out the scope and depth of our intellectual heritage. Judging from the comments after the workshops, these objectives were accomplished. It might not be too much of a stretch to suggest that the future of our EPIC community rests on our ability to talk across our differences and create frameworks where our diversity is also our strength. The EPIC workshops were a step in this direction.

My comments here are intended to provide one view onto the workshop topics, highlighting the *relational* questions addressed by many of the workshops. That is, relations between the *social* and *material*, between *online* and *offline* interactions, between *global* agendas and *local* problems, and between our *spiritual* selves and our *secular* workplaces. It seems that the notion of hybrids, both as a way to

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conceptualize the *subjects* of our research and as a way to describe our design, research, applied, academic, corporate, citizen *identities* might be a good way to organize my remarks.

HYBRIDS

The notion of hybrids is offered as an alternative formulation to the dualisms that permeate much of our discourse and to the misplaced concreteness that attends to naturalized categories like real and virtual, local and global, material and social. Donna Haraway (1991:177) writing in Cyborgs, Simians and Women offered, "In so far as we know ourselves in both formal discourse and in daily practice we find ourselves to be cyborgs, hybrids, mosaics, chimeras." What I want to note in this quote is the reference to the knowing that comes from an examination of *daily practice.* In my view dualisms, pure categories lose their explanatory power when examined in relation to the complexity and particularity of everyday practice.

The inadequacy of these dualisms to account for everyday practice was made vividly clear to me in a study I embarked upon soon after I arrived at Xerox PARC over 20 years ago (Blomberg 1987, 1988). My project then was to understand how a new computer-mediated design tool, Trillium, would change the working practices of the interface designers at Xerox (hopefully for the better). In addition, my study would help the developers of Trillium iterate on the design, making Trillium better suited to the practices of the designers. In those days, as is the case today, the taken for granted categories of technical and social permeated the discourse. The formulation that most expected from my study was that the introduction of Trillium would result in or cause the designers to work in a particular new way. Although time does not allow me to go into detail on this study, what I found instead was that Trillium was taken up in very different ways by two different groups of designers. For one group it was used mainly to represent the visual elements of the interface and some of the interface behavior. For another group the underlying Trillium code was translated into the computer code that ran on the target machine. In other words, the interface designers were writing machine executable code, not just using Trillium to design the user interface. What I concluded back then was that Trillium could not be defined or known in isolation from the practices of the designers who used it. The tool acquired its very materiality in and through its use. The social and the technical were inextricable linked. While I didn't have the language of hybrids at the time, our understanding of Trillium arose out of the particular arrangements of material goods, human labor and social relations and these were not fixed.

HYBRID SUBJECTS

The focus of our ethnographic inquiries and our interventionist agendas require that we view our subjects as collections and arrangements of people, things and relationships. Out subjects are never unitary, but are situated in networks of relations whose significance and orderliness can be understood through an examination of everyday practice. The notion of hybrid subjects points to this characteristic of our ethnographic project.

SOCIAL – MATERIAL

Two of the EPIC workshops have taken up the question of the relation between the social and material (technical) from two different perspectives. The workshop *Defining the Impact of Physical Spaces on Social Interactions* asks how the physical environments in which live shape the possibilities for social interaction. Although not specifically called out in the workshop description, it seems that the flip side of this question, namely how our social interactions define the physical spaces we move through, is

also at play. In this regard I am reminded of one of my first experiences working at Sapient, a business and technology consulting company, in early 2000. As the nominal head of Sapient's San Francisco Experience Modeling group, I was in the position of having to argue for the value of Project Rooms as an essential tool of our practice. Project Rooms were physical spaces where our research teams collaborated in the collection and analysis of the materials from our ethnographic studies. I ran into a lot of resistance from people who didn't understand why Project Rooms were required for our work. I had become convinced in my brief time with the Experience Modeling group that these spaces were critical to our work. But it wasn't the rooms or the physical spaces themselves that were so powerful, it was the practices that had emerged at E-Lab and later adopted by the Experience Modeling group that defined these spaces. The value of project rooms was in the way they were used, in the particular arrangements of material goods, human labor and social relations, and these were not fixed.

Exploring the question of the relation between the social and material from a somewhat different perspective the workshop, *Object Sociality: Researching Living Things* examines the place of things in everyday practice. Material objects are conceptualized as full fledged actors in webs of relations that constitute our practices. This is topic that has occupied the thinking of one of my close collaborators of many years, Lucy Suchman. As Suchman (2004) writes, "Given an ontology of separate things that need to be joined together [the material – social dualism], machines must in some sense be granted agency in order to be reunited with us. …. But what if our ontology comprises configurations of always already inter-related, re-iterated sociomaterial practices?" In considering the relation between people and things, Suchman is pointing to the difficulty of conceptualizing one without reference to the other – their inextricable links.

An example that articulates the inseparability of material objects from how we see and know as social actors is a study by Chuck Goodwin (1994). Goodwin's study of the professional practices of archaeologists carefully details how archaeologists are able to recognize meaningful differences in samples of dirt excavated from archaeological sites with the aid of the Munsell color chart of universal color categories. This chart along with bureaucratic coding forms provides archaeologists with the professional vision to see samples of dirt as evidence of particular kinds of past human activity. As Goodwin (1994: 609) writes, "Of all the possible ways that the earth could be looked at, the perceptual work of students using this form is focused on determining the exact color of a minute sample of dirt. They engage in active cognitive work, but the parameters of that work have been established by the system that is organizing their perception." The system referenced is the particular arrangements of material goods, human labor and social relations which in this case have evolved over years through the practices of archaeologists. It is through these arrangements that archaeological knowledge is constituted in everyday practice.

VIRTUAL – REAL

The workshop, *Studying Distributed Sociality – Online and/or Offline?,* directly addresses the relation between the virtual and the real, the online and the offline. While the workshop's specific topic focuses on designing ethnographic research that can move between real, offline physical spaces and virtual, online spaces, the prior question – how to conceptualize differences between real and virtual spaces remains. The places where we live and act are not easily differentiated as either virtual or real, online or offline. These places are always, already constituted at the intersections, connections and relations between and among the multiple ways we are situated. It is relatively easy to find somewhat dramatic examples where place is defined at the intersection of the virtual and the real, in other words, hybrid places. For example, many of us have seen the image of the digital receptionist greeting her guests in a

hotel lobby. The receptionist could be anywhere connected through a video link with access to the guest register and other relevant databases. Or there is the example from medicine of a remote, digitally connected pediatrician treating her patient with assistance from a combination of human and technical assistants at the patient's side. In these examples the boundary between the real and the virtual is blurred and the place of interaction must be located in the practices of guests, receptionists, patients and doctors.

A more mundane example of this blurring of the real and virtual is the ubiquitous corporate conference call. My colleague, Melissa Cefkin and I have been involved in a study of globally distributed sales teams where meetings are one of the key organizing devices for the work of the sellers. Meetings bring together various configurations of the selling team to discuss opportunities, coordinate activities, and respond to the bureaucratic demands of the corporation. Let me describe one such meeting. Gathered in a conference room at the company's Manhattan office building are four US based members of the sales team. Among those assembled in the conference room is a team member from Germany who happens to be visiting on that day. There are two other team members who are physically located in the Manhattan office building, but have chosen to stay at their desks in the temporary workspaces they have claimed for the day. They will be dialing in to the conference call from there. Then there is another contingent of three team members assembled in a conference room in the company's Frankfurt, Germany office building. A handful of team members are dialing in from various individual locations, one from a train while commuting into Manhattan, one from a home office, another from a client site. Supporting this interaction is a computer screen sharing tool, although not everyone has logged onto this meeting tool. Some folks are following the discussion using documents circulated in advance of the meeting via email for those unable to log onto to the intranet, including the person on the train. It's hard to maintain the purity of categories like real and virtual, online and offline when confronted with the mundane, everydayness of this situation. The space of interaction is defined through the particular arrangements of material goods, human labor and social relations which cannot be defined as either online or offline.

LOCAL – GLOBAL

The global reach of western capitalism and how local, communities might help define change agendas are explored in the workshop on *Business Ethnography for the Bottom of the Pyramid*. Exploiting local knowledge for the benefit of culturally and economically distinct communities not currently served by design agendas delineated in the developed west, points to the interdependencies of global capital, local markets, and particular social configurations. Our ethnographic praxis suggests that a fruitful way to explore these interdependencies is by focusing on the everyday practices occurring in family kitchens, on school playgrounds, in corporate board rooms, on financial trading floors, and at offshore workplaces. In these places the local and the global are in reflexively constituted.

By calling attention the everyday practices of those caught up in these webs of relation – and here I'm referring to the local practices of powerful corporate actors as well as those we often think of as most affected by globalization – we can get insights into how these global networks are constituted. My concern that we pay attention to the everyday practices of our increasingly interconnected economy led me to organize a conference in 2004 at IBM on *Work in the Era of the Global Extensible Enterprise*, where I hoped to call attention to the human labor, the particular social and material relations, so often rendered invisible in the flow diagrams and value exchange calculators that often define this new terrain (http://www.almaden.ibm.com/institute/2004/). Here again the hybrid, at

once both local and global, seems a better analytical construct for characterizing these shifts and (re)arrangements in global markets that we are witnessing.

SPIRITUAL – SECULAR

Another dualism, the division of the secular and the spiritual, is explored in the workshop, *Holy Hanging Out: Exploring Spirituality and Religion in the Corporate Environment*. Here the topic concerns both the way the spiritual shapes how we construct and conduct ourselves in the workplace and the rationale that sustains religion as a taboo topic in corporate research. Those of us trained as anthropologists before the critical, self reflexive 80s were taught that "primitive" societies didn't separate the religious or spiritual aspects of their lives from the domestic or economic. The spiritual could be seen in the practices of the economy and the interactions in the home. While we have since come to question this other dualism, the primitive – modern; in corporate ethnography, the spiritual and religious have rarely figured in our analysis. The utilitarian and instrumental still dominate our conceptual frameworks even as our hybrid subjects connect us with contexts where it is more difficult to maintain the separation of the spiritual and the secular.

However to advocate a hybrid, mixing of the spiritual and secular is not without risks. Management books exist that tout the value of spirituality in guiding corporate decision making. The authors of the book *Church on Sunday, Work on Monday* claim that, "Spirituality – however defined – is now a popular resource for business needs whether for sparking creativity or for being a better person on the job" (Nash and McLennan 2001). And the authors of a recent book, *Capturing the Heart of Leadership Spirituality and Community in the New American Workplace* (Fairhorn 1997), proclaim that their book "…seeks to promote a new spiritual approach to organizational leadership that goes beyond visionary management to a new focus on the spiritual for both leader and led." Living in Silicon Valley I can not help but note that the Dalai Lama is sighted at gatherings of the powerful digerati of the Valley.

The place of religious imagery in corporate settings recently surfaced at a diversity day event at IBM in San Jose. The speakers for the day had been talking about the importance for IBM of valuing diversity for IBM and had pointed to the various IBM clubs that supported diversity (e.g. Hispanic, Native American, Women, Gay Lesbian, Bisexual, and Transgender, etc.). During the question – answer period, an Asian woman, who prefaced her question by saying she wasn't religious, wondered why she wasn't allowed to send Christmas greetings to her colleagues. Instead she had been instructed to send only Holiday greetings. She wondered what was so different about the Christmas holiday from all those other markers of identity and belonging that were being celebrated that day. The people on the stage were a bit dumb founded and unable to come up with a response. Then someone from human relations department stood up and tried to make the case. She distinguished between markers of identity that had a religious origin and those that did not. Since the Christmas holiday was connected to Christianity it would not be appropriate to send a religious greeting to our workplace colleagues. Later that day, as part of the diversity celebration, we were all treated to a performance of the Chinese dragon dance. Although not explicitly noted, this dance has connections to Chinese belief systems that date back centuries. No absolute distinction can be claimed between the historical, spiritual roots of the dragon dance and the religious connections between Christianity and various Christmas holiday traditions. But we shouldn't be looking for absolutes.

As we argue for hybridity as an analytic construct, we must examine, understand and evaluate the particular histories and arrangements. As Haraway cautions hybridity –in this case the joining, mixing of the spiritual and the secular – offers possible dangers and potential advantages alike.

HYBRID IDENTITIES

The notion of hybridity is also related to our identities as researchers, designers, practitioners, academics, and employees. Simple labels often confuse and obscure the multiplicity of positions and locations to our subjects that many of us occupy.

RESEARCHER – DESIGNER

Two of the EPIC workshops, *Collaborating Across Social, Organizational and Disciplinary Distance* and *The Sociality of Fieldwork*, have taken up the topic of the connections, flows and boundary spanning activities that characterizes our work. We often find ourselves participants in multidisciplinary activities where our contributions must intersect with the work and interests of others. Our research and our locations inside corporate settings make it increasing difficult to maintain single identities. Our collaborators are often extremely diverse and broad, including researchers from other disciplines, developers, consultants, strategists, designers, project managers, customers and a host of others.

My own views on adopting a hybrid identity have gone through a transition over the years. When I first arrived at Xerox PARC in the early 80s, the anthropologists at PARC resisted requests from our fellow researchers and corporate interlocutors to provide design implications or design guidelines based on our research findings. We withdrew behind a somewhat narrow definition of who we were – anthropologists and ethnographers. Our colleagues marked this separation somewhat affectionately by remarking as we passed them in the halls, "Here come the anthroids." But we soon realized that our research would not have the impact we desired if we didn't engage more directly with others who could help turn our research findings into tangible outcomes, whether technologies, organizational processes, learning interventions, or business strategies. To be successful we had to be willing venture into terrain that was unfamiliar.

In many ways things have changed rather dramatically since my early days at PARC. Now many of us move back and forth between researcher and designer without really noting the shift. And others readily collaborate with designers and developers in research planning, in analyses and opportunity identification. However, these hybrid identities also have provoked controversy and attempts to police the boundaries of our practice. Who can claim to be an ethnographer? Whose design recommendations should be listened to? Some of us worry that our expertise as ethnographers will be undervalued if those with little or no training are allowed to lay claim to ethnography. And others are concerned that their expertise as designers will be undermined if researchers are allowed to drive design agendas. While I understand the impulse to control who can legitimately profess expertise in ethnography or design, I think we will do more to advance our agenda by demonstrating the value of our work and expanding our practice than by policing our borders.

The breakdown in the divide between researcher and designer is related to other projects underway that fundamentally question the value of maintaining strict disciplinary silos in the academy and elsewhere. Our hybrid subjects (e.g. sociotechnical) often require a blurring of academic disciplines and expertise. For example, I've been involved in two initiatives in inter-, multi-, or trans-

disciplinarily. One involves developing a curriculum for the new design school at Stanford, where researchers and practitioners from a number of backgrounds including design, business, social science, engineering and architecture are exploring what it would mean to place design at the center of a new study area (http://www.stanford.edu/group/dschool/big_picture/our_vision.html). In another initiative, I'm witnessing attempts at IBM to "birth" a new hybrid field focused on services that draws on perspectives, theories and methods from computer science, engineering, management, social science, economics, and business (http://www.research.ibm.com/ssme/). Those who are driving this effort are convinced that the traditional academic silos do not allow for the cross fertilization needed to understand the shift to services in the global economy and its implication for business. There seems to be a growing recognition that our hybrid subjects require hybrid identities.

ACADEMIC – APPLIED

The applied – academic distinction in some ways has been the subtext of the EPIC conference. While not named explicitly in the workshop descriptions, many of us feel deeply that our hybrid identities span the academic – applied divide. The division between the applied and academic sciences has been contentious in a number of disciplines for many years, but perhaps no more so than in anthropology (Baba 2005). Our current admonition to recognize the hybrid nature of all research (applied and academic, practical and theoretical) was echoed by one of the key figures in American Anthropology, Bronislaw Malinowski, who wrote, "Unfortunately, there is still a strong but erroneous opinion in some circles that practical anthropology is fundamentally different from theoretical or academic anthropology. The truth is that science begins with application…" (Malinowski, 1961). While our research may be funded by corporations, non profit organizations, or for profit institutes; our research already and always has the potential of contributing to both theory and practice – to ethnographic praxis. Moreover, we will do a great deal to legitimize our practice as corporate ethnographers by contributing to the theoretical and methodological debates current in academic research spheres, whether in anthropology, sociology, critical studies, feminist studies, design theory, HCI or computer science. One of the goals of the EPIC conference is to provide a forum where practicing and academic researchers can advance the field of corporate ethnographic praxis and by so doing clearly and proudly claim our hybrid identity.

Related to the recognition that our research can have multiple outcomes, from contributing to academic debates, to developing new methods, to designing workplace-appropriate technologies, to uncovering new business strategies, is the question of whose interests we serve. The workshop on *Framing Ethnographic Praxis for Innovation*, with its focus change, must necessarily ask, innovation of what, by whom and for whom. Here our sometimes conflicting, interdependent identities as corporate actors and world citizens may collide. These questions are complex and must be answered by each one of us in the particular contexts in which we work. I raise them here to suggest are one lens through which we must consider and define our ethnographic praxis and our relation to it.

CONCLUSION

Ethnographic praxis points us to the power of everyday practice to challenge the assumed concreteness reflected in categories like social – material, real – virtual, local – global, secular – spiritual. We should celebrate a commitment to the ephemeral, situated, orderliness of everyday practice and to our complex, situated identities.

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6

CRAFT, VALUE, AND THE FETISHISM OF METHOD

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In order to set the scene for the panel on methods, I will be drawing on C Wright Mills' injunction to avoid the fetishism of method. Mills urges us to think about our methods in terms of a process of craft production. I want to explore what key elements of this craft might be, beyond the usual focus on actual techniques such as interviewing or ethnographically informed data collection. Foregrounding the papers in the session, I will examine ideas of value, temporality and transformation (and perhaps even transgression).

The focus of this conference on sociality is offers us a provocation to question a forceful trend in ethnographic work that is conducted in commercial contexts: a concentration on the individual consumer, and the nature of personhood as singular. Thinking about the importance of methods to the work that happens amongst our diverse set of practices, my initial question is: how do particular methods render sociality visible or invisible? And, following on from this, whose sociality is made visible or left out? Do we include the sociality of participants to the exclusion of our own? Do our social networks, the form by which we enact sociality at conferences, in meetings, in hiring each other, transform our methods? How should we understand and theorise these multiple sets of social relationships?

The conference theme has prompted interesting theoretical possibilities for reshaping the work of 'ethnographic praxis in industry contexts' (which I originally misunderstood as the words fitting the EPIC acronym). What follows are several provocations which might prompt us to get beyond the discussion of particular techniques, and to move towards a more elaborate and reflexive discussion of the connections between our techniques, the way we relate them to our theoretical concerns, and how all of this work is based on specific assumptions (implicit or explicit) about what can be known, and how knowledge becomes legitimate knowledge. So this is a call for a discussion not only of method, but also of methodology and epistemology. I think we need to make a sense of value stick to these discussions, perhaps by considering the ways in which our methodological practices could be thought of as craftwork rather than following recipes.

My aim is not to develop a philosophical treatise about EPIC methods. Instead I have organised my thoughts around the issues of what we have to do to survive as ethnographers in industry contexts, how this effects our ways of knowing and doing, and lastly, given some methods (such as video ethnography) have become fetishised, which fetishes we might want to keep and which we are willing to abandon. What is recognised by all the method papers is that even in discussing techniques, an exploration of method needs to go beyond 'war stories' with clients to establish some wider framework for further exploration.

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THEORY/METHOD: US/THEM

It is impossible to consider methods without looking at their theoretical heritage. This sounds like a basic methods course soundbite, but in fact it is very hard to hold on to this knowledge in the course of doing typical EPIC projects. There is a danger that we isolate 'method' and separate it from 'theory'. We are comfortable having our two paragraph summary of the 'ethnographic approach' in a pitch. However most of us would feel far more uncomfortable about a two paragraph summary of our theoretical approach – either because we don't want to have one, don't think we need one, or think that it was so sophisticated it couldn't *possibly* be summed up in two paragraphs. This theory/method division is a trap. I suggest that we look at the theory/method divide as Edmund Leach might have done in *Culture and Communication* (1976), and seek out the interesting bit: the liminal zone in the middle. Which brings me to rabbits. Culturally constructed as neither completely wild nor completely tame, they inhabit one such liminal zone. In Leach's terms what follows is that they become the subject of anxiety, taboo, humour or (most famously for the Australians) extermination. Liminal zones are often highly political and politicised. They may suggest transitions, but they also can reinforce social structures. First provocation: what kind of liminal between theory and method space do we want?

There is another classificatory urge that crosscuts this theory/method division. It is more likely to happen in the corridor talk of our professional encounters than in formal discussions, but also has an impact on our methodologies and epistemologies. This is the construction of categories of 'us' and 'them' separating academics and industry researchers, or industry practitioners. This division reverberates through our methodological and even epistemological practices as much as it does our actual techniques – let alone presentation styles. The classificatory urge operates in at least two ways. First, there is what we might call the 'KoolAid continuum'[i]. Progression along this continuum is anecdotal, assessed by others, and depends on how much corporate 'KoolAid' the practitioner is supposed to have drunk. The expectation is that amount of KoolAid is inversely proportionate to the willingness to advocate for 'real' research. This construction itself acts to police EPIC work, and I'm profoundly disturbed by the way in which we might perpetuate it ourselves. It also operates with a kind of double standards. Interestingly the continuum is not invoked in terms of academic corporate culture, a factor which I think all those paid by public sector institutions might pause and consider, given that their workplaces, at least in the UK, are increasingly run by figures plucked from 'real' corporations. Second provocation: to continue to innovate methodologically we need a more detailed understanding of the ways in which methodological knowledge is impacted by different aspects of corporate culture.

The second 'us-ing' and 'them-ing' practice, as it relates to methodologies in EPIC work, is much more basic, and that is a simple bifucation held apart by an idea which can be roughly captured in some concept of luxury. In this classfication 'academics' have the luxury of time, no clients, etc, whereas industry folk have the pressure of impossible deadlines, impossible demands. Or, from the other side, industrial researchers have the luxury of expense accounts, nice kit, no teaching load to juggle, and academics suffer because of educational bureaucracies, insufficient funding, heavy teaching loads etc. Again, I think this system of classification merely results in new ways to control our own work, including our methods. Industry researchers *appear to* be able to carry out more expensive fieldwork – better incentives to participants, nicer video cameras, sometimes more staff to project. Academic researchers *seem to* have more freedom to justify methodologies and styles of analysis that would not be tolerated in industry. Without denying some of the real structural differences between working for different kinds of institutions, our identities in EPIC work are always hybrid. Most practitioners have

entered the field from graduate level course and have some kind of commitment to some kind of discipline. Most academics work in institutions underpinned by corporate sponsorship or donations. We are all rabbits now. Provocation three: we also need more sophisticated understandings of methodologies that arise from such hybridities.

METHODS AND ANECDOTE

I've stated that I don't want frame the discussions on methods by presenting a philosophical treatise. Nevertheless I do want to make quite clear my conviction that epistemology and methodology should be discussed alongside methods. Recalling Sandra Harding, I'm thinking of epistemology as about ways of knowing, what can be known, who can know, and what counts as legitimate knowledge. Methodology, according to Harding, is theory and analysis of how research does or should proceed. Methods are the technique (Harding, 1987).

Here is a scenario that I've seen played out several times. What follows is an ideal typical version. Researchers are trying to do some exploratory work on topic x. They present their ethnographic research plan to clients or internal sponsors. Clients or internal sponsors listen to presentation. Then one senior person says, "No, I don't think that is interesting. My wife/child/mother doesn't do x". Other clients and internal sponsors join in and say "Yeah, that's right. Neither does mine". Meeting derailed. Revision in research plan often follows.

What is happening in such situations is not a problem so much of method as of epistemology. At stake what tests beliefs must pass in order to be legitimated as knowledge, and what kinds of things can be known, i.e. subjective truths can count as knowledge. Or perhaps, subjective truths *plus* financial or organisational power.

Methods, Susan Leigh Star has suggested, are ways of surviving experience (1994). They can be our life rafts as we reassure ourselves that we are not doing a focus group, we are doing an ethnographic interview. At EPIC perhaps this might be rephrased: methods are ways of surviving 'experience models', and a corporate environment that seems to expect that data is output through some process of experience modelling (or whatever the local nomenclature for these expected output frameworks might be). Star's motto is a reminder that methods are not just with or for others. They are also resources upon which we ourselves can draw, personally and professionally. Provocation four: the reason to discuss methodologies and epistemologies in our liminal zone is that we can begin to engage with different ways in which we can create and talk about legitimate knowledge, even if this is unpopular, subversive, or to rephrase Kris Cohen (see conference paper), smuggled knowledge.

Yet of course those classification practices are at work in this zone also. The word 'ethnography' is in Star's terms a boundary object (Star & Grisemer, 1989). In other words it is a way in which otherwise separate communities may be able to negotiate a temporary and strategic way to communicate. I think we could all agree that at least being able to mobilize the word 'ethnography' can prove strategically useful in talking to those who do not understand the notion of local culture, or how it might be investigated outside the realm of actually asking direct questions about it of the target sample. However trying to define what is 'real' or 'traditional' ethnography seems to me to pose entirely the wrong question. 'Traditional ethnography' always makes me shudder because it reminds me of phrases like 'traditional family values'. Provocation five: let us use this conference to mark a point where the phrase 'traditional ethnography' is no longer necessary as a foil against which EPIC work is

measured, but without silencing debates about the epistemological assumptions of current EPIC practices.

THE VALUE OF 'GOD-TRICKS'

In order to expand our discussions from method to methodology and epistemology, we also need to start discussing the consequences of our own locations, or locatedness, in forums such as this conference. What are the consequences of the resources and frameworks in our professional and personal locations that begin to define what is legitimate knowledge? Provocation six: we should begin to develop a more fully and open reflexive methodological praxis, one where some of the more difficult encounters between EPIC folk and 'traditional' academic professional associations becomes integrated into discussions epistemological discussion.

The consequence of not bringing this discussion upfront is to reinforce the kind of dislocatedness which Donna Haraway refers to as the 'god-trick' - 'promising vision from everywhere and nowhere' – a kind of false objectivity (1991) . Haraway talks about this perspective in terms of the workings of scientific endeavors. I think it might also spill over into EPIC work from engineering culture in which, as the Silicon Valley Cultures Project showed so well for that area, social issues and mundane experiences are translated into the logics of engineering problem-solving (English-Lueck, 2002).

Thinking sociologically we could also explain some of this investment in producing god-trick knowledge as a consequence of the social structures in which EPIC work is embedded. Ethnography tends to be treated as a method not a methodology by clients and sponsors. In addition there are moments in our own practices that need to be interrogated a bit more closely. The 'value' attached to the knowledge that we produce tends to stick to whole stories rather than incomplete or fragmented narratives. Can we begin to talk about disputed or marginal knowledges as resources for methodology – rather than just as part of a user segmentation? Kris Cohen suggested that more radical conceptions of the user would lead us to different engagements with theory. What I'm suggesting is that we extend this adventure through the full range of methodological and epistemological discussions right back to the actual techniques we use.

FETISH AND CRAFT

In *The Sociological Imagination*, C Wright Mills urges sociologists to avoid fetishism in research methods (1959, p224). He suggests that instead we think of our methodological practice as involving craft-like investigations. He advocates the 'rehabilitation' of crafts practice in intellectual practice. So bringing this to EPIC work, what fetishes have already emerged? There is perhaps a certain fetishism around video taping user experiences. Clients often want the tapes - the objects with magical properties to reveal user experiences – and it is often difficult to present such data as constructed, partial, contingent knowledge. The idea of ethnography itself, even when used strategically as a boundary object, sometimes seems to acquire fetishistic properties. Being drawn into a situation where ethnography is presented as extendable to virtually any inquiry is almost inevitably seductive. Maybe we could begin discussing its limits and boundaries, again in terms of a more nuanced discussion of value.

The proposal to think about methodological work as craftwork is a useful way to shift the framework of discussions. Thinking about methodologies as craft, we need to consider what are the materials and what are the processes of our work. Are the methods our materials, or is it the data itself? The culture of craft is that ideas emerge through an intimate engagement with materials. In EPIC practice usually we seem to work through our data to to get to frameworks, models, deliverables. But perhaps we should explore a different kind of craftwork, which begins with engagements with theoretical perspectives out of which emerge new and challenging methodologies and methods. This is my final provocation.

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[i] In 1978 cult leader Jim Jones persuaded his followers to drink grape-flavoured KoolAid laced with cyanide and tranquilizers in what has become known as the Jonestown Massacre (for earliest use in common parlance see http://www.wordspy.com/words/drinktheKool-Aid.asp)

7

FIELDWORK AND ETHNOGRAPHY: A PERSPECTIVE FROM CSCW

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PREAMBLE

What value does 'ethnography' have in the design of organizational and technological change? We ask this question in light of the fact that ethnography, whatever it might mean or entail, has been a key component of systems and organization design research for some time and has become—seemingly unproblematically—almost the *sine qua non* of contemporary practice in Computer Supported Collaborative Work (CSCW), the area in which we have plied our trade. Indeed, one can plausibly claim that CSCW was the first (and conceivably remains the only) interdisciplinary perspective in which some version of fieldwork, namely of an ethnographic kind, has become the default mechanism for intervening in design.

On the face of it, however, the dominance of 'ethnography' as this default fieldwork approach in CSCW sits rather uneasily beside the contested nature of ethnography, and particularly the examination of the reflexive relationship between fieldworker, subject and field, that preoccupies contemporary anthropology and sociology. If, for CSCW, ethnography is almost a taken for granted approach, in these other disciplines, almost nothing about 'it' (or even that it is an 'it') is taken for granted (1). Associated with this indifference, we suggest, are both benefits and costs. The benefits have to do with the way in which a 'body of practices' have quickly grown up and been maintained in CSCW. These were adapted to a set of interdisciplinary research problems that, during the 1990s especially, were core issues for CSCW (See Anderson, 1994). As it happens—though as we shall see this is significant—these were not and indeed are not the same as those that confront anthropology or sociology or other non-design related disciplines. Whatever the concerns of these disciplines however, we would argue that CSCW did quite well out of this adaptation, applying various fieldwork-based techniques to solve particular design questions. Nevertheless, and as we say, it came at a cost; one that CSCW practitioners are only now paying for. The costs are to do with the suitability of that 'body of practices' for engaging in a set of new and arguably more complex research problems: whereas, in the 1990's the issues that confronted CSCW were related to understanding 'plans' as descriptors and aides in organisational action (in the work of Suchman and especially Schmidt, 1997), the role of 'local knowledge' in large scale organizational processes (in our work and numerous others), and so on, now CSCW researchers are perplexed by such things as the 'moral order' of home settings (See Harper, Ed, 2003), the social

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organization of information production and use in public spaces (See O'Hara, et al, 2003), and the intersection of novel technologies in each.

Now without wanting to explain why there has been this shift in topic or why the body of practices developed in the 1990's does not provide all that is necessary to orient to these problems, we raise this issue now since it seems to us that an exploration of how CSCW might solve this current dilemma can be a pretext for saying something of relevance to the community that will read this paper. Whether our view is right or wrong, it seems to us that an answer to CSCW's current dilemmas might be produced by considering the distinction between disciplinary assumptions about *method*, substantive disciplinary *concerns*, and disciplinary *sensibilities.* More particularly, we contend that to undertake ethnographic fieldwork for the home or for public spaces (and other new or in some ways perplexing domains) and attending to the potentialities of new technologies requires a particular open-mindedness about method, a thoughtful selection of concerns, and an artful refinement of disciplinary, particularly design-oriented sensibilities. These cannot be taken lock, stock and barrel from other disciplines (say anthropology or, within sociology, ethnomethodology), it seems to us, since they have to be 'worked out', 'worked at' and 'tested' through trial and error. This does not mean that they have to be created from scratch, either. It seems to us that they can be built on what has been learnt about adapting ethnographic fieldwork practices to design concerns over the past decade or more of CSCW research, and indeed from what has been learnt about the use of fieldwork techniques in other design oriented disciplines too, such as human factors and human computer interaction. The problems that confront CSCW are new, we know; and their solution will be new, also, we believe. But it is our view that their solution will partly come out of what has been crafted in the trials and errors and testings of fieldwork in numerous disciplines, including CSCW, in the past. These can be combined with some new thinking, some new trials, some new testings. The result, we hope, will be a new body or practices, ones central to the concerns of CSCW and possibly–but only possibly–for other design oriented disciplines as they start to deal with new issues at the start of the twenty-first century.

ASSUMPTIONS AND ISSUES

In CSCW, the ethnographic turn from the late eighties onwards was (and still largely is) predicated on a–more or less–ethnomethodological version of what ethnography entailed or might be. This approach claimed (its practitioners still do, as we shall see) to be unique in its concern for understanding the subtleties of work as it occurs *in situ*. This claim was made despite the fact that some version of fieldwork was then and is now common in other design oriented disciplines; indeed, it is a concern that has at times preoccupied Scandinavian and German 'work science'; French 'cognitive ergonomics', human factors, computer machine interaction, and even, most recently, the industrial design communities.

We want to start our discussions by asking, then, what assumptions are contained in any of the versions of fieldwork that typically get practiced in design? It's not that we want to say that any approach is right or wrong, or that this approach is ethnographic and that is not; we want to start by saying, hold on, just what do we mean when we say fieldwork in design? Is it all of a muchness? Or is there a difference when it comes to design between, say ethnographic styles or other styles? Certainly, we know there is a difference in ethnographic research when practiced in anthropology and let us say, ethnography when practiced by archaeologists just as there is a difference between these and ethnomethodological ethnography–but that is not our concern. The issue is what is special when 'it' (whatever it is) is applied or linked to design?

We propose to get there by exploring, albeit briefly, different texts reporting on what fieldwork might entail. The texts in question are *Cognitive Work Analysis* (Vicente, 1999), *Contextual Inquiry* (Beyer and Holzblatt, 1998) and *Designing Collaborative Systems* (Crabtree, 2003). These are all concerned, in one way or another, with design and fieldwork. They represent what we will call (no doubt unfairly) a human factors (with a cognitive science slant) viewpoint; a practical Human Computer Interaction (HCI) approach; and an ethnomethodological one, respectively. Unfortunately, they don't tell us all that we need to think about as we confront the new problems for CSCW. For this we will turn to some one primarily concerned with fieldwork in anthropology, which of course means ethnography. Given what we have said about the extent of fieldwork and design it might seem odd to look here, but if the reader can bear with us till the end of our paper then perhaps they will see the merits of doing so. In any event, Marcus's *Ethnography through Thick and Thin* (1998) offers what one might call a post-'postmodern' view, but addresses a question that is fundamental to any and all fieldwork practices: how the analytic foci of those practices determine the nature of the enterprise itself. When consideration of this is combined with the insights and learnings we can take from the other more design oriented books, we think that we will then have enough to propose some solutions to what fieldwork in design might be, most especially of the ethnographic kind and, perhaps, some directions for how we might solve some of CSCW's current concerns.

A SET OF COMPETING VIEWS

As we noted from the first, fieldwork for design has existed outside of the sociological and anthropological traditions for some time. Vicente's book *Cognitive Work Analysis* represents a fairly typical view of how it might work from within human factors and related disciplines. The book reports methods intended to help specify socio-technical systems that go well beyond current systems. Designing for the future, he claims, requires understanding of the constraints that shape an environment. These constraints are both human and technical. To get to these requires a mixture of data gathering techniques, one of which is the task of fieldwork. Fieldwork is useful, according to Vicente, since it provides evidence about how workers actually do their work and the skills that are necessary in that real work. Fieldwork data may be contrasted with data produced by studies that tend to over-idealise work, especially those which use laboratory data-gathering tools alone. These Vicente calls 'normative'. But in addition to the use of the fieldwork 'technique', Vicente also thinks one needs evidence from studies of cognition, ergonomic models of body movement, and much else beside.

Now, though we don't want to go in to too much detail about the human factors approach here, it is worth delving in to it a little bit if only to provide some evidence for later discussions. It can also provide us a chance to draw the readers attention to the fact that some of the claims made in anthropology and sociology about the need for ethnography (and the nature of that 'need') are pretty common in other disciplines too, even when they don't use the term ethnography themselves, preferring the more neutral and all encompassing term, fieldwork, as does Vicente.

More particularly, Vicente claims that fieldwork of his kind (and we are wanting to claim it could be others' kinds too) is intended to uncover five characteristics or issues. They are: first, that it should describe and explain how individuals deal with what Vicente calls context variability, with everyday contingency. It should describe how work is social and socially organised, even in those situations where work might seem at first glance solitary. It should show how workers have developed tools that enable them to reduce the mental work that a task might demand, tools that reduce the strains on what

Vicente calls cognitive processing capacities (this is beginning to get away from the social and more towards the cognitive sciences). These tools may take many forms: lists and memoranda, wall charts, displays and so on. It should demonstrate how historical factors have influenced the current organization of the work (irrespective of whether those influences are good or bad). Finally, fieldwork should present the techniques that workers devise to get work done despite the contingencies that arise and also within severe time constraints.

These claims are not controversial–they would be recognizable to many ethnomethodologists, for example (leaving aside the issue of a quite distinctive disciplinary language) as 'stuff worth looking at'. The point Vicente is making is that these analytic interests enable certain kinds of design related work to be done–conceptualizing the information and skills needed to deal with contingency, for example, along with more generic sets of skill that enable the 'right' work to be done: these are concerns when judging what a newly designed system might do. Taken as a whole, knowledge of these sorts of skills, tasks and strategies enable the specification of the competencies that workers need, and this enables a system to be designed that ensures that these competencies are still sufficient to make the work interesting and rich enough to ensure worker motivation.

We should point out that there is much here we find problematic but we do, nevertheless, want to highlight three particular features. These have to do with the unique claims of this approach, rather than the gross similarities with other approaches (some of which we have alluded to). To begin with, it clearly invokes fieldwork as a corrective to other methods, and vice versa. As we shall see, this is something of a leitmotif in all the texts we deal with, irrespective of the particular view they have on fieldwork. Second he views fieldwork data as necessary for design but is not sufficient for design. The next two books we deal with have different views on this: Beyer and Holzblatt saying that 'it' (fieldwork data) is adequate given that it is rendered for design from the outset; Crabtree because 'it' is the 'right stuff' since it is ethnomethodological. Third, Vicente's argument is specifically applied to what, for convenience, one can call 'command and control' systems. That is, to tightly coupled, time- and safety critical work. As we shall see, there has been a tendency to focus on these in CSCW too, especially when the discipline first developed, but now it is trying to deal with other less tightly focused domains and this is causing it problems. The limits one might see in Vicente's work, then, might be reflected in the limits of CSCW's historical expertise with this move to other places. Fourth, for Vicente, fieldwork is pretty much unproblematically a matter of recording data. It therefore requires no particular skill or expertise beyond a training in the 'method' (which often has to do with such matters as how to point a video camera and index the resulting data). The serious analytic work is done within the modeling process.

It should be clear then Vicente is not deluded as to the nature of work practices and seeks to avoid over idealising them. He is arguing for an understanding of contingency, nuanced skills and complexity which is broadly similar to the kinds of understanding we might associate with various positions in sociology, for example (about which we shall say something shortly), though with some obviously significant, not to say stark, differences in respect of the role of modeling. What is curious to us, however, is that–given all this–his approach seems to imply that data collection in the field hinges (merely) on fieldwork method, for which one imagines only a little training is required. Yet it seems to us that this is to avoid questions (bigger ones in our mind) about what one might look for. After all is it certain or so clear what is, say, a work skill or a control task might be? Can a fieldworker simply go and gather data about them? How are these to be distinguished, described and clarified? All of these questions, it seems to us, need to be dealt with before the modeling starts. Of course such concerns will sound familiar to those in the anthropological tradition where the question of what one sees and

how it is bound up with how one sees (ones' subjectivity if you will) has long been a bug bear. We shall come back to this.

In any event, another concern we want to raise has to do with how Vicente holds that fieldwork data is in need of a remedy. Getting the raw stuff is easy, he implies, but he then claims it requires correction: in particular, it needs abstraction. At this point, of course, the 'ethnographic practitioners' from anthropology might be getting queasy, thinking that such modeling would mean losing the sense of verisimilitude, of situatedness, that makes their kind of fieldwork 'real'. We don't think this kind of contrast is quite as clear cut as it seems. As with some other issues, we shall come back to this but suffice it to say that, the need to model, to abstract, raises an issue of comparison that we consider deeply relevant to the problems of design we discuss later. For us, when it comes to design, comparison is at least as important as verisimilitude; for others, less interested in design, it is this verisimilitude that matters above all else (though as it happens this does not mean that these others are agreed as to what this verisimilitude might be or how to attain it). This does not obviate treating the issue of what kind of comparative analysis might turn out to be relevant to design seriously. Vicente thinks modeling and abstraction as a way forward; we think comparison of various kinds and of various current contexts can offer a way forward too, perhaps even a better way to see what the future might hold when one is thinking about how to design for it.

In any case, another concern we want to note has to do what one might call problems of method. If we have noted that for Vicente, fieldwork is merely descriptive and hence requires relatively little, if any, disciplinary training, we ought to note too that in contrast, design is based on a kind of imaginative exercise, worked through but extending well beyond the 'here and now' concerns evoked by fieldwork data. In other words, *contra* the claim by some disciplines that engineering–like disciplines such as human factors are empiricist, positivistic and even 'scientistic', Vicente is arguing the exact reverse: he offers a way to transcend the data, if you like (though quite how much imagination comes in to play as a result is another issue, one that we will comeback to). We do not want to take sides here, but it is certainly worth noting. Besides, it is our view that much of this debate (about empiricism of one sort or another), which sometimes invades discussion about fieldwork and design, is miscast.

Be that as it may, let us now turn to Beyer and Holzblatt's *Contextual Inquiry.* This is now the most popular textbook used to teach the requirements capture for interactive systems design in North America. It is intended to be used as a reference guide, a book of rules-of-thumb and maxims of conduct, for students and practitioners in industrial settings. In part its success has to do with how it lays out some of the practical tools and techniques that need to be used in any design process in ways that can be easily understood, in part because in the process it strips out any reference to how these rules of thumb have emerged through the prior (and canonical) research traditions to be found in anthropology, sociology, HCI and our own patch, CSCW.

Whatever their disciplinary roots, Beyer and Holzblatt treat the business of data collection as a matter of method, where fieldwork data is a remedy for other, less accomplished methods. Unlike Vicente, though, Contextual Inquiry has a view about labour or rather about how many people supply it. In anthropology, researchers can and all too often do squabble over which of them has rights to report particular places, as if ethnography was essentially a monopolistic, sole trader enterprise. In Contextual Inquiry, by contrast, it is a team business. Indeed, much play is made of multi-person data gathering mechanisms, all of which are oriented to producing evidence that can be agreed by everyone. Affinity analyses, for example, entail placing Post-It notes on whiteboards to represent different skills and elements in a work process, and by having more than one person interview participants within any

process, the consistency with which these Post-It notes are arranged can be used as a tool to force reconsideration and examination until there is some consistency of view. Affinity analyses are not about highlighting dissimilarity and specificity, but commonality and unity of interpretation.

Though collaborative agreement about the significance of data is a goal in Contextual Inquiry, the fieldwork as a whole is not data driven but design driven. Contextual Inquiry treats fieldwork as only one part, the first, in a three-step set of actions: fieldwork, design and implementation. And as a consequence of this, what fieldwork entails, how it is organised and documented, is always construed in reference to the larger purpose at hand: design. There is no sense in which fieldwork can be done by, let us say, one individual or team and then handed over to the next team who undertake subsequent tasks. Though there is a clear distinction between the skills used at different points in a Contextual Inquiry—interface design in particular having an especial role—the process of undertaking fieldwork is structured from the outset around the needs of design and the practical process of implementation.

Space precludes saying much more about Contextual Inquiry at the moment. Suffice to say that we can delineate some notable similarities and differences between this approach and the Vicente's. Firstly, both concur in viewing the merits of fieldwork in its superiority as a method. It is contrasted with the failure of other 'methods', interviewing being a notable example. In the latter, the tacit nature of some knowledge remains just that, tacit, and hence the data resulting from this method will be impoverished. A difference between them is that for Contextual Inquiry fieldwork is not merely a matter of data collection but of a structured orientation to design questions. A further difference is that in Contextual Inquiry data analysis is a team-based activity in which uncertainty is progressively eliminated by a move towards consensus. Put another way, data needs to be 'worked up' in and through a process of collaborative design reflection.

We want to say something about what we can usefully appropriate and what we find more problematic. We would like to use the word 'sensibility' here to give a clue as to our view on this. We are unimpressed with any argument that proposes that say, ethnographic fieldwork as superior by dint of its status as a method. In our view, no single form of methodical form of data collection, and hence the data thereby produced, can, in and of itself, be regarded as superior to another, except in respect of particular purposes. The purposes in question are, in our business CSCW, related to design. Another related issue we want to note has to do how there are some subtle but important differences between Vicente and Beyer and Holzblatt on this issue. The former sets out circumstances where he feels fieldwork data are inadequate to the design of the future–at least in some critical system cases; Beyer and Holzblatt assert the superiority of fieldwork data over other data for design of the future, at very least in terms of future interface design. We want to suggest that both might be right *and* wrong, depending on what kind of design problem is at hand. At the same time, Beyer and Holzblatt hit on something very important (if slightly controversial) in their insistence that data must orient in some way to design. What to look for, how to look for it, and how to assess its significance are, from the outset, design-related matters. Contrast this with Vicente's approach to fieldwork: that it's just a matter of collecting data and that, given some basic training almost anyone could do it. Part of the difference, of course relates to the fact that the fieldworker in Contextual Inquiry is part of a team though it is more than that. Through an iterative process, this team reflects on the data and offers suggestions as to how to go and look again. Seeing leads to understanding and in turn to demands for seeing differently. That is, what might be relevant, what is worth looking for, what might be documented and shared, is not clear beforehand but must emerge. It seems to us that this sensitivity to the problem of data is right: the problem is not one of how to capture it, it is *what* to capture (2).

As suggested above, however, the key here is the different treatment of the future. For Vicente, future design–design beyond minor iterations–cannot be based solely on knowledge of current practice, no matter how sophisticated. For Beyer and Holzblatt it can, as long as the data is subjected to a rigorous and consensual treatment by an assemblage of experts in a team. Now, it seems to us that whatever one might think about the relationship between how things are done now and how they might be done tomorrow, the real issue is one of imagination: how to imagine what the future might be. Here, there is a substantive difference. For Vicente the future can be imagined without creative thought, paradoxically without imagination: simply through the rendering of the constraints, and other factors in graphical forms. He outlines a tool kit for presenting systems in ever reducing hierarchical orders so that the mind's eye can see what might be. Process produces imagination. In contrast, it seems to us that Beyer and Holzblatt offer rules of thumbs and lists of topics to look at as if they were sufficient to ensure that the fieldworker can imaginatively see what might be, even as they look at what is. If one offers a technique for mechanical imagination post-fieldwork then the other offers tools that ensure that imagination is deployed in a manageable way in the field.

We are skeptical of both, though for different reasons. As to Contextual Inquiry, one of the problems we see here is that when Beyer and Holstblatt offer lists of such things as artifacts and 'work arounds' and so forth as topics to be looked at in the field they don't explore how these topics have arisen as being the-kinds-of-things-worth-looking-at in the first place. Now of course, they might not mention these histories because theirs is a textbook and cannot encompass scholarly reflections of prior research. But it seems to us that in eschewing any such discussion their book gives the impression that one only needs a list to be imaginative. In contrast, we think that being imaginative is cultivated through becoming familiar with the subtle ways in which prior studies have uncovered and explored issues; oftentimes through what one refer to as the sensibility of the particular discipline in question: from ethnomethodology, for example, or through anthropology, or indeed CSCW. Whatever these sensibilities might be (we shall come back to them), what we want to suggest now is that 'success in the field' depends, in large part, on understanding prior studies: their pitfalls as well as their successes. Knowing this does not guarantee imagination, as if scholarship and creativity were the same thing. It does however, it seems to us, provide the basis–and so we can now point to the sensibility we are sketching–that allows creative looking in *ever more complex or differentiated domains*. Our own (sometimes bitter) experience of undertaking, managing and assessing fieldwork prompts this view. We will discuss some of these experiences below.

Thus we find ourselves sympathetic to Vicente's claim that looking at the present is not sufficient to imagine a future. But oddly enough we don't think that one only needs to bring in materials from other sciences and make graphical, abstracting schemas that let the mind's eye wonder–more lookings at more places is, we think, also a fairly good way of getting to see what might be. Bearing in mind other ways of thinking about socio-technical systems can bear fruit, we nevertheless think that the future can also be seen around us if only we had the wherewithal, historical, comparative, to see it. Let us put this another way: it seems to us that one of these books allows a designer's sensibility to suffuse fieldwork, but somehow makes that sensibility appear mechanical and this, in our view, renders fieldwork devoid of what is sometimes called the ethnographic imagination. The other book, in seeking to produce formal apparatus that can foster design imagination, makes the process of design mechanical. Neither, we believe, is satisfactory, certainly for what fieldwork-for-design might do, all the more so as CSCW, as our concern here, moves to address new domains and technologies.

The next book we want to consider, *Designing Collaborative Systems*, we deal with because of its explicitly ethnomethodological orientation, as we have noted one common in the CSCW literature.

Crabtree restates, as does Suchman before him, the distinctive quality of ethnomethodological work. He explains that the ethnomethodological programme of inquiries was developed as a way of rejecting and moving on from some of the more commonplace approaches to studies of work within sociology in the 1960s. These tended to look at work not with an interest in how it is done so much as with a focus on how traditional sociological topics, gender, power, and so forth, were manifest in that work. These concerns may result, Crabtree reports, in neglect of many other aspects of the work. More strongly, he argues, if one lets the data speak for itself, rather than expecting to see certain kinds of things, then it may be the case that those expected concerns would be reduced in significance. When he says data speaks for itself, he means that members' 'understandings' speak for themselves. By this is meant how people in any situation see that situation is how that situation should be reported and described. Such members' understandings can be captured through finely detailed studies of the ways in which those understandings are accomplished.

Now, what is being highlighted here is that Crabtree is claiming that ethnomethodological studies of work are empirical in a specific way, one that is a contrastive with other sociological approaches. Put simply, ethnomethodological studies produce evidence that traditional sociologists miss. The problem we have with this, though, is that what may be an empirical 'remedy' to other sociological methods may not *always* be corrective for design; indeed, the idea of offering a corrective may be misleading. Crabtree's view is that the empirical materials his approach produces is the kind of material that designers might want and need *because it is ethnomethodological material.* In other words, the close attention to the processual-character-of-egologically-organized-work (as it would be put in the correct argot) is what designers need, and it is what they do not get from other traditions. The task of the ethnomethodological fieldworker is to produce this data, and subsequently participate in a process (of whatever kind) that one might call, 'design'. In this sense, the fieldwork produces 'descriptive' evidence rather than analytic; stuff which can, as it were, be handed over. None of this precludes the changing of hats so as to participate in the design process. When the ethnographer becomes a designer, Crabtree suggests that they act like a 'bricoleur', shifting around the various elements that are used to construct the context in question. He goes further and suggests that the way this bricolage may be undertaken can be oriented towards various ideas of patterning and invokes Alexander's work on this topic (Alexander, et al: 1977).

Space precludes saying a great deal more on *Designing Collaborative Systems*. For our purposes, what arises from Crabtree's book are two main things: first, the assumption that relevant data is simply there waiting to be captured, and it takes the form of organized, sequenced activity. This may be contrasted with Beyer and Holzblatt's view that what one might look for, how one might render the world in ways pertinent to design, is itself a matter for ongoing and team-based decisions about how to *orient* to design. In contrast, for Crabtreee, detailed data pertaining to the processual character of work is not only necessary, it would seem to be sufficient (in no need of any remedy) for design purposes.

Close attention to the processual character of work and its real world contingencies is something that all the writers we have looked at so far have in common—despite their evident differences. One would go further and state, unequivocally, that it is ethnomethodology that has engendered the focus, detail, and interest in the processual character of work we find now in all CSCW-related work. Only Vicente, oddly, starts with the assumption that data gathered in this way might not necessarily be relevant, appropriate, adequate or pertinent. We would like to claim that it might be, or it might not be, depending on what kinds of design question are at stake. As we noted above, we want to suggest that far too little attention is paid to what the design questions might be. These decisions–when to design, where to design, how to design– it seems to us, cannot be made *post hoc*. Or rather we ought to say they

should not be made only then. It seems to us that they should be dealt with before, during and as well as after the process of fieldwork.

This leads us back to imagination or sensibility, the ability to see the field in ways that lets possibilities arise, that makes fieldwork more than a mere mechanical looking. One of the concerns we had with Beyer and Holzblatt was the lack of discussion and reference to prior research, and the possible implication that followed that the fieldworker only needed lists of things to look for and things to see to ensure that fieldwork is productive. Crabtree, by way of contrast, spends some time referring to other studies of work, elucidating as he does some of the insights that one expects of ethnomethodologists. But we get little insight in to what *designers* might need to see that is of concern neither to ethnomethodologists or sociologists. It seems to us that this has all sorts of consequences. Not only might it restrict what might be noted through contrast, metaphor, distorting illumination and occlusion; it even restricts the frame of reference that might drive a design oriented fieldwork. For Crabtree, anywhere is a good a place to start as anywhere else since it is not design considerations that determine what the fieldworker should capture, it is members' understandings. Thus, one cannot start with the hope that a different future might be made possible by, say, the deployment of some new technology if that technology is not within the gestalt of the members.

SOME OF OUR STUDIES

None of this should be read as being especially critical of the books we have discussed for they all, in their different ways, make broadly sensible remarks about a specific set of fieldwork and design relationships even if we have reservations about some of their assumptions and particularities. But a larger problem we have is that some part of the research we have undertaken does not really fit with the specified set of relationships described in the books above, and we feel that less and less of it will do so in the future. What we have in mind is, for instance, the issue of the 'coupling' of ethnographic work with specific design issues, although the possible set of problems is actually much greater than that, as we shall see. Our concern lies in the degree to which any and all of these approaches either helps or does not help with a new and more complex set of design-related problems. We turn now to some studies we have conducted to illustrate some of these complexities. We do so not because we think they are exemplary or canonical, but because we know them best and can be self-critical about them. Our primary concern is to try to disentangle some of these issues of method, analysis and disciplinary concerns and, from this, examine why we consider none of the above approaches to be entirely adequate for dealing with the next generation of design problems, at least not on their own.

We begin with reflections on one of the earliest ethnographic studies conducted in CSCW, that of an Air Traffic Control room (see e.g. Harper et al, 1991; Bentley, et al, 1992; Hughes et al, 1992; Hughes et al, 1994). It is not entirely coincidental that this study was rigorously ethnomethodological. It focused heavily on the processual character of work. But at the same time this slant reflected another fact, not merely our own prejudice for ethnomethodological inquiries (of that time). The 'stuff we looked' was of a tightly-bounded domain, and this was well suited to the kinds of approach we wanted to deploy (as it happens very much the kind of domain that Vicente is interested in). The work was also tightly coupled with a specific design problem: the replication of paper-based information under 'glass'. Its main conclusions had to do with the cooperative nature of ATC work and the very public exchange of information that takes place in ATC settings that ensures that the work is done effectively and safely. The research made a series of fairly general recommendations concerning the kind of information that needed to be conveyed, the constraints (at that time) and affordances of 'glass' as

opposed to paper, the importance of collaborative error-finding rather than error omission, the actual nature of the skills deployed, and so on (for an overview see Sellen and Harper, 2002: 110-18).

Nevertheless, we should point to some features of this work that were left largely unremarked at that time. Firstly, it traded on a very considerable amount of background knowledge on the nature of ATC work that was derived from interviewing and general chit chat away from the work site; in other words without abiding by the imperative to see work *in situ*. Hence the 'data' included something of the character of 'interviews' which some of the authors above feel is ill-advised. Secondly, part of its analytic work involved the evocation of a 'culture': some typical 'doings' that suggest some limited generalizing about 'the-ways-things-are-done-around-here'. These ways of doing could be compared as different 'cultures'. Here the cultures we had in mind related differences between military and civilian ATC, cultures which existed (as it were) in side-by-side rooms (quite literally: the two sides of ATC occupying the same building near London Airport). There are two points we are alluding to here: first, the ambiguity of data collection methods: in our view, it's not the method that might matter, it's the data. It was the data we were after in the ATC project, irrespective of the method. Second, we are wanting to allude to how we saw and sifted through and indeed oriented ourselves to what data might be relevant: in this case through comparison of very similar things.

The second study we want to mention is research we conducted into retail finance services (see Harper et al, 2000). Here, we want to point to the very different set of problems entailed. Firstly, the retail finance institutions we looked at were large, complex organizations (and there was more than one of them) and secondly, the range of technologies in question were many and varied–they included, for instance, database technologies, video-conferencing, e-mail applications and expert systems. Some points we might make about this work include that we were, for the most part, feeling our way as regards what to look for in the context, since there were, to begin with, no clearly defined design problems for us to look at and no existing studies of the kind we intended to do, of socio-technical systems design for large, complex organizations (though needless to say there were many other studies: in socio-technics, for example, as well as in management studies that we could learn from). The kinds of conclusions that came out of work had to do with such things as the peculiar time-criticality of customer-facing work in retail finance and how the design of existing database technology and proposed video conferencing systems paid little or no account of it; and the role of 'local knowledges' commonly used in branch-based organisations and how these 'knowledges' might progressively erode in more large scale centers to the detriment of service provision and worker engagement. These research outcomes were driven by comparison of different institutions and different contexts within the institutions, and reflected our desire to say something that was to a degree more generic than we had attempted in the ATC study.

If the ATC and the banking study were different in the scope and degree of comparison they entailed, there were differences in the 'methods' too. Some of our fieldwork was done over a long period of time entailed attention to the processual character of the work, some to trying to understand the kinds of 'knowledge' that workers seemed to have, and some to the more general kinds of organizational problems they have to orient to. Some of it involved brief visits, 'interviews' with individuals, etc., whilst other parts involved very close attention to interactional processes. Yet very few if any of the 'problems' we encountered (leaving aside the trivial) were problems of method. They were much more often problems of analysis and how to say something useful and insightful in a context were there was no close coupling between fieldwork and design. There was, nevertheless, an expectation that we would say *something* on this topic. In ATC the relationship between user and system cried out for exploration; in retail finance it was altogether much more attenuated. We found

our way by evolving as analytic instruments a series of tropes, many of which were borrowed from ethnomethodological thinking. We used these to conceptualise problems of what 'working' in retail finance might mean and entail in relation to a range of technologies, some of which we listed above. There is no space to discuss the use of these tropes in any detail, but they included such matters as the 'ecology of the workplace'; the 'egological' organization of work activity, 'the 'social' organization of work, the nature of 'skill' and 'knowledge', and so on. These turned out to evolve some of those we had developed in the ATC project, where the egological organisation of controller's work had been a useful device, for example (3). Two things strike us about the tropes we deployed in the bank study. Firstly, some of them bear a striking resemblance to concepts deployed by others under very different intellectual auspices (as we have seen, above in the work of Vicente and Beyer and Holzblatt). Secondly, these tropes have turned out, in varying degrees, to have a recurrent value in that they continued to make some sense across a variety of domains even now: the lesson is not that they are always relevant however, it is how some tropes have more power than others to do work, analytical work. We shall come back to this.

The next example of our research is of a study conducted in a non-work setting (at least in the conventional sense); in this case, in to a so-called 'smart home' (see Harper, 2003, especially Randall pp227-246). This study entailed looking at family life in an existing (but constantly evolving) smart home. The location, in a sense, was not a 'home' to anyone insofar as families were there for relatively brief periods of time. But the point we want to highlight was that, as before, the brief was quite vague and our research ranged over the usefulness of various control systems for domestic life (lighting, heating, security) and the adoption or otherwise of trans-domestic technologies, such as for internet shopping and health monitoring. Our methods in this context involved relatively brief visits (typically two, lasting on average a day each) where something that looked like 'in situ' interviewing (of the 'show me' type) was undertaken at the beginning and end of each family's stay in the house, accompanied by ongoing video recording of family activity in the house (with two cameras in each downstairs room). We adopted these methods not out of any conviction concerning whether they were 'right' or 'wrong' but because they seemed to us to be 'do-able' in the light of the kinds of design-related problems we were dealing with. Some part of what we were doing clearly involved the evaluation of specific technologies with a view to further design (such as with some of the control systems), but other aspects were more speculative (such as related to medical monitoring).

That is, there was what we can only describe as a 'variable' coupling between the ethnographic work we were doing and design activity. Analytically, one of the things we found most interesting was the extent and limitations of the tropes we had found useful in our prior analysis of work settings. Some could be deployed, though not mechanically, in a domestic setting; but in many respects the domestic domain and the technologies we were dealing with were recalcitrant to our previously developed tropes and techniques. It turned out that the domestic space was step too far for the expertise we had developed in CSCW studies of work (hitherto the primary concern of the field). The sorts of new matters that we had to define new ways of dealing with (and by that we mean dealing with them analytically and not in terms of method: we could get data about them) were such things as the 'rhythms' of family life, rhythms which were as much about symbolic as practical matters, ephemeral in some ways yet robustly oriented to in others; and relatedly the 'moral order' of the home, where patterns of appropriate behaviour were bound to age, status, time, activity, economy and accountability. At the same time, the technologies that could effect these and other aspects of home life were—are—diverse, with different affordances and consequences on the nature of behaviour in question. All this and more perplexed us when it came to design consideration, not in the sense of

coming up with design solutions, but in the sense of how we could get them to fit in to a design 'space': an arena for thinking sensitively about design possibilities.

BACK TO THE GENERAL ARGUMENT

We could, of course, make reference to other examples of our own work or indeed to any amount of exemplary work carried out by others. It would add little of any consequence to our argument. Our purpose in raising these studies is twofold: to show that these studies do not quite fit the picture of fieldwork and ethnography and its relation to design offered in the three books mentioned above. Somehow our experiences were much less tidy. Second, to point toward what we earlier called the corpus of reasoning on fieldwork and design constitutive of the body politic of CSCW. What we have seen here is that this body politic has a history, a past; but also this body politic needs to evolve and shift as the discipline (CSCW) moves to new design concerns. This leads us on to the last book we want to deal with, Marcus's *Ethnography through Thick and Thin.*

It seems to us ironic, to say the least, that Marcus, who has nothing whatsoever to say about design, encapsulates what we think is the problem of how fieldwork is to meet with design. He is an anthropologist who has been at the forefront of thinking about the nature of ethnography, the way in which ethnographic materials are presented or conveyed, and what 'usages' ethnography can be put to for some time (4). The key link between this book and the others we have considered is, for us, how it asserts the view that fieldwork, and in his case he means ethnography, needs to be understood as *always being driven by particular analytic foci*.

He gets to this issue indirectly. He gets there by exploring another question: how 'ethnographies' in anthropology are typically the product of solitary individuals, sole researchers on voyages of observation. He suggests that these voyages, like Sinbad's, produce stories, and that this begs all sorts of questions about fact versus fiction, about narrative structure and the problem of evidence, because they *are* stories. These 'limited views' from 'the particular', as Geertz would have it (Geertz, 1999), are, in Marcus' post-postmodern view, less and less authoritative. Now, we do not want to explore anthropology's agonies here, but this loss of authority that Marcus highlights relates not only to the individual's voice but also to anthropology's chosen topics. Marcus is adamant that anthropology's future lies in interdisciplinary concerns and that interdisciplinarity carries with it its own tropes which might be well removed from the story telling praxis of traditional anthropology. The importance of this, in our view, cannot be overstressed. It moves the argument about what ethnography might be away from a concern with reflexivity and narrative, for example, towards other matters. It explicitly relates to what we above distinguished as disciplinary methods, concerns and sensibilities.

Before we list what these might be, let us note that Marcus proposes as a way forward. He outlines a so-called 'multi-sited' approach to ethnography. This represents, he thinks, a return to comparative ethnography, but in a different way:

> *'… comparison emerges from putting questions to an emergent object of study whose contours, sites and relationships are not known beforehand, but are themselves a contribution of making an account which has different, complexly connected real-world sites of investigation …. In the form of juxtapositions of phenomena that have conventionally appeared to be 'worlds apart'* (pp86).

In our understanding, 'multi-sitedness' is explicitly not a matter of visiting more than one place (although it may well include that). It is, rather, about the problem of making analytic *connections*. Now, we have to be careful about what we make of this here since, as we have stated, Marcus is not concerned with design and, in point of fact, much of what he describes as 'interdisciplinary' doesn't appear to us to be very interdisciplinary at all. Indeed, his notion of interdisciplinarity is world's removed from the kinds of interest we have, but his arguments provoke us to ask what contribution 'multi-sitedness' (or new forms of comparison) can make. The most important feature of this argument, it seems to us, is that the problems of interdisciplinary engagement are not problems of method. Multi-sitedness implies an eclectic approach to 'method', and thus there are not and cannot (in any simplistic way) be about remedying the failure of other 'methods'. Nor are they problems of substantive disciplinary specific concerns because the contemporary crisis in those concerns is precisely what leads him to interdisciplinarity. The 'multi-sited' view of interdisciplinarity, then, leads us to reflect on problems of *empirical* relevance, of *conceptual* orientation, and of the role of *comparison*.

The questions that are entailed in considering these things include the following sorts of queries: what kind of site constitutes an appropriate site for investigation?; to what extent can detailed and local results typically resulting from ethnographic enquiry produce results which are generalizable?; and for what purpose are these generalizations being sought? These questions entail some engagement with the relationship between evidence and purpose, an analogue, though hardly ever put this way, of fieldwork and design: in this case the fieldwork being the 'data', the design the 'theory'. It seems to us that there is often a failure to answer these questions amongst those who are newly arrived in the ethnography/fieldwork and design domain, and even in CSCW, where there can be no excuse for being naive, there is too sometimes a neglect of them. Within CSCW, even for those who claim some scholarship, another problem, one that concerns us, is that as CSCW tries to address new domains of inquiry the just what of relationship between fieldwork and design is different. Hence, the pattern of that relationship we have somehow come to map out and learn does not now help us as we would wish.

CONCLUSIONS

We have been arguing across a range of topics, theoretical, conceptual, empirical, and historical; we have also been alluding to what disciplinary perspectives and sensibilities might be, especially in a context where different disciplines are competing with one another, and one discipline in particular (CSCW) has reached a point where it might need to adjust itself.

Many of these concerns are grand and the solutions to them equally likely to be grand, but not all of the matters we have dealt with turn out to be about very grand problems. We have wanted to imply that much of what passes for a discussion of fieldwork in design, for example, turns out to be musings about how to how to collect and use fieldwork data. These musings have been raised in a context of a largely restricted set of domain and technology problems, where the main issue has turned out to be which method needs to be remedied by which other method. These musings, as we call them, are not illegitimate concerns. We do not wish to suggest otherwise, though do wonder whether they should be the main substance of what is discussed at conferences like EPIC. Nevertheless, from the perspective of CSCW, we consider the historical relationship between ethnography and design to have been a broadly happy and useful one, albeit serendipitous. We are, however, also wanting to state, explicitly, that this success cannot be relied on as CSCW tries to encompass new topics, domains,

technologies and concerns. These are serious matters, and talk of data gathering are somewhat distracting, we think. There are major problems inherent in fieldwork for design as we see it evolving and these are more than methods related.

To begin with, we think the importance of design as an analytic focus of what CSCW has been and what it will become needs to be made greater, more central, more of a focus, if you will. We should at least begin to set out a stall here. The special quality of ethnography in design we assert and hope for does not lie in the superiority of one 'method' over another, nor in the privileging of the local and contingent over the formal, the ideal for example, nor in focusing on the tacit as against the explicit; and so on: all the stuff that seemed to perplex the first three authors we considered. In our view it lies in the deployment of a specific 'ethnographic sensibility' and its relationship to a complex assemblage of activities one can call design and, related to that, a 'design sensibility'. The marriage of these 'sensibilities' ought not to treat data as freestanding and separate from design, as if fieldwork material were an object that stands for the skills, artefacts, processes and culture in question like a painting; nor should it treat design as a straightforward fitting of solutions to problems. It should not do so for this somewhat paradoxical reason: *the relationship between fieldwork and design cannot be determinate*.

We say paradoxical because we have been claiming throughout that there is a relationship between the two; now we are saying that it is not fixed. We do not think this unreasonable to claim this: the more important issue is that one recognizes that there is a relationship, complex, changing, fraught, and attenuated though it may be. Once this is recognized, the fact that the range of possible design-related questions that fieldwork data might contribute to is increasing should be then thought of as natural consequence of this relationship; even a measure of success. But this increase will make things more difficult, not less, even if we can pat ourselves on the back for getting in to this position.

We have tried to show the reasons for and the character of this increasing complexity, albeit briefly, by alluding to some of the problems we have had to deal with in our own studies. These experiences point to a growing range of relevant issues. Fieldwork practices have to develop that deal with, for instance, areas where there might be no clear idea what the design problem might be. It might be that fieldwork is funded to address the potentiality of future technologies that have, as yet, no clear application domain in view. Besides, it might even be that social mobilities are such that the location at which any technology might be used are so dynamic that it is difficult to know where to look in the first place.

Very little of the discussions of fieldwork and design have, it seems to us, begun to grapple with these sorts of problems, which are, in our view, practical and conceptual, analytical and empirical. The books we have reviewed have dealt with some of the issues, but not adequately. Fieldwork itself, for example, may have to take place in organisational contexts where design methodologies might be increasingly complex, and design teams may increase in size; fieldwork may take place in conjunction with various change management or project management methodologies which also might be changing. Design possibilities may range from incremental development of known applications to radical re-design. No set of recommendations concerning the relationship between fieldwork and design as yet has dealt with all these different possibilities. The point here is that no standard or orthodox approach from anthropology or sociology or more likely, even ethnomethodology with its focus on members' methods, or even from CSCW itself, could help us determine such things as which members are the relevant ones for design, or what particular work processes are important. Our feeling, to complicate matters but based on our own experience, is that organizations and their representatives sometimes have only fairly vague ideas about what the purpose of any given research

relationship may be. Even more problematically, functional divisions within the organization might mean that assumptions about purpose may vary from department to department, or role to role: whose member's understandings count really is sometimes entirely unclear, almost unfathomable.

Thus we are back to the beginning of our arguments, to the idea that the purposes of fieldwork for design depend on the interdisciplinary audiences to which they are addressed if by that is meant not just academics and scientists but the people, the users, who end up with designed technologies. Collaborative endeavour is indicated, but we are not a great deal closer to defining what that collaboration might entail than we were at the outset. Certainly, the answer does not lie in what one might call disciplinary-specific concerns for method, nor the great themes that constitute a disciplinary 'body politic'. We think that, when it comes to CSCW, these disciplinary themes—given by anthropology, sociology, psychology and ethnomethodology—were merely a starting point a long time ago and that now, some twenty years after the term Computer Supported Collaborative Work was first coined, what counts as evidence, what is relevant, how it is to be assessed, what are the ways in which the evidence can be used imaginatively and so on, are all part of a new body politic, the one of CSCW itself. In this view, anthropologists can't do fieldwork for design if by the term 'anthropologist' they mean professionals concerned with the body politic of anthropology (and of course the same applies to any other set of disciplinary interests). But beyond this, it also means that the themes that have merged in CSCW are ones that have a history and, presumably, a future: the trouble, though, and as we say, is tat what worked in the past might not work in the future.

As it happens, and leaving aside the future for the moment, we think that it is possible to list many of these themes or tropes that have worked to date. We have already mentioned some. Of course, space precludes a detailed rehearsal, and in any case we hope it is clear from our discussion of aspects of our own research history, we would be absolutely against any suggestion that these tropes can be used in a 'cookbook' fashion. Rather, we have been at pains to point to the way in which disciplinary 'sensibilities'–which in our case where historically ethnomethodological but are now distinctly CSCW–may allow us to produce a set of broad, 'sensitising' or 'illuminating' concepts, starting points that can serve as reminders that some kinds of thing are often to be found whilst not diverting us from our equally powerful interest in what is uniquely 'situated' about what we are studying. In short, they can provide us with a 'way of looking'.

The kinds of things we have in mind, and we put them in a language that Marcus would be familiar with, though we could equally well express them in other terms, are as follows:

a. Follow the plan: Investigate the documentation, the procedures and the rules as formally stated and how people orient to them. Appreciate the 'information life cycle'- its birth, life, and death.
b. Follow the job/trade/business relationship: Understand what various 'stakeholders' expect, anticipate and want from new technological and organizational forms.
c. Follow the skills: Understand in detail what skill is actually being deployed rather than assume that any externally imposed description (e.g., the organizational chart; theories of flexible specialisation) is adequate.
d. Follow the knowledges: Find out what people actually need to know in order to do the things they are trying to do, and understand the ways in which these knowledges might be 'local' or 'contextual' and intertwinned.

e. Follow the use: The particular things that people do with technology, and the ways in which they do it.
f. Follow the ecology: establish the ways in which people elegantly integrate their purposes with a material environment which they in part organise.
g. Follow the 'troubles': Investigate the 'false starts', 'glitches', 'diversions', 'distractions', 'interruptions', and 'digressions' which are aspects of all activities and not noise to be eliminated from the data.

These topics and tropes are not–they cannot be–exhaustive. That is not their purpose. They will evolve (or more properly, co-evolve) as they are compared, juxtaposed, re-emphasised and viewed in ways that echo Vicente's juggling of abstractions (without endorsing assumptions about their formal properties) across the huge potential range of domains we could feasibly be interested in, and in the context of the varying relationships between ethnographic investigation and design problem we indicate above. New tropes are needed to figure out, to make 'workable' fieldw ork for the design of smart home applications, for various mobile devices, for activities supported in novel ways in public space, ad so forth. These new tropes won't abandon the old ones. Nor will they abandon the robust conviction that the material captured and juggled thereby reflects a persistent concern for 'situatedness', because part of their use will always be to explore what is unique about the phenomenon under investigation and what design requires in terms of 'patterns', 'typicalities', 'generalisations' (or whatever one might want to call them).

Whatever the nature of this mix of the already cultivated and new tropes, it is our view that they will allow imaginative explorations of the future and thus empirically grounded comparisons of what could be with what currently is. Capturing what might be for these exercises will entail thoughtful comparisons of the present and the future. It won't be a mechanical thoughtfulness; it won't be an empiricist one; but it will be a grounded thoughtfulness almost certainly undertaken by teams of individuals where the link between fieldwork and design is diverse and often changeable. We have sued this paper to delineate why we think these (and other) features we be common.

In a roundabout way this brings us back to the original remit of anthropology: Otherness. What we have been wanting to say is that Otherness of and in the future can be foreseen through observation of, comparisons within, and studies of the arrangement of, artifacts, processes, technologies and human endeavour in any and all of the places around us, even those places we have not looked at before. Fieldwork for design, a certain kind of fieldwork oriented to, married with, suffused by design, will do this. Wherever this fieldwork leads one to look, we think the shadows and ghostly intimations of the future will be found if only the fieldworker has the requisite sensibility to see them. This sensibility owes much to disciplinary origins, to disciplinary history and auspices. But to be successful as a sensibility it needs to be more than that. It requires a genuine engagement across the various fault lines we have mentioned and needs to involve the respectful combination of a *design imagination* and an *ethnographic imagination*. This mixture, we think, will produce a fieldwork for design that can work in the future. It has produced fieldwork for design in the past for CSCW, and some success can be seen here. The question we have posed in this paper is how this sensibility can evolve so as to maintain the link between fieldwork and design despite different domains, topics, auspices and technologies. We know how the past has got us here; we can see that getting to the future will entail some trouble, some effort. Our case is that effort will be required because it that future is unknown. Yet it seems to us that surely that is what should excite us. We know it's going to be different: it's going

to be different in terms of what we look at and what we can design for. This paper has set out some arguments for how we can bind the looking in to the future with the design of that very same future.

NOTES

[1] For a thoughtful review of the same from the perspective of anthropology, see Geertz, C. (1999) *Available Light*, especially 'The state of the Art', chapter V, pp89-144.

[2] There are similarities between the naivety about data gathering of Vicente and the early claims of the ethnomethodological ethnographers in CSCW. See Hughes et al, 1994.

[3] Though some of the tropes from ATC seemed to have little provenance elsewhere though were much celebrated n the literature. One such was the idea that a division of labour could create a cocooning effect in ATC.

[4] Perhaps most famously for his book, *Writing Cultures*, written with J. Clifford, in 1986.

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8

THE BAKER'S DOZEN: THE PRESENCE OF THE GIFT IN SERVICE ENCOUNTERS

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This paper explores whether or not Marcel Mauss's concept of the gift is applicable to understanding the diverse roles that ethnographers assume in corporate environments. Kneading together the themes of gift exchange from anthropological literature on the one hand and "Representations" from the participatory design research community on the other; we suggest that the artifacts we create and share with customers actually evoke the presence of the gift in customer interactions. We argue that specific types of representations - a key component in our methodological toolkit - may be likened to the thirteenth loaf in the baker's dozen; given to the customer to demonstrate equitable partnerships, enhance communication and garner trust in a perpetually changing marketplace. Using case studies, we examine how these objects illuminate the complexity of our own sociality in professional settings and furthermore, help to deepen or transform customer service engagements.

INTRODUCTION

In part, this paper describes our experience of a paradox in business: companies wish to create the spirit of durable relationships with customers based on a series of transitory exchanges within a dynamic and competitive marketplace. We take our premise from Michael Thompson's playful foray into "rubbish theory", through which he explores what it is that determines people's perceptions about value. He suggests that transitory goods are those that decrease in value over time, while durable goods increase in value and thus have "infinite life spans". What makes the premise interesting is its interpretative and temporal aspect: what one considers a second-hand car, another deems an antique (Thompson 1979). We will focus not as much on interpreting the value of goods per se, but on how a sense of durability gets infused into a landscape of transient interactions with customers through the use of graphical representations.

We discuss the relational aspects of service engagements and how our focus as practicing ethnographers is towards building relationships with our customers and amplifying the relevance of people's work. The specific way in which we do this is through sharing with customers the artifacts that have been created from our interviews and observations with them in the field. Graphical representations are a significant component of our methodology: we discuss the use of representations

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as well as a toolkit of graphical icons called "the Blue Guys" to explore themes in the discourse around "the gift" (Mauss 1990). We suggest also that the representation itself inherently embodies our sociality, that is, our professional roles, interactions and connections with others. Hence, discussions around representations evoke a response from customers that feeds our mutual relationship and extends beyond it. Finally, we examine the dichotomy in the role that the artifact plays within a global services organization.

This paper is also a reflexive one. Ethnographic and ethnomethodological literature in the domain of Computer-Supported Cooperative Work accentuates how critical it is to make context visible for truly useful understandings of people's work. Suchman (1987) for instance, questions some fundamental assumptions behind the design of intelligent devices and cautions against the building of static, abstract models or ideal-state representations of work, emphasizing instead that one examine actual contingencies in operational plans. Similarly Bowers, Button and Sharrock (1995) call attention to the fact that people working in a production print environment can anticipate problems in advance and respond to situations as they occur in ways that a job scheduling system simply cannot. In essence, ethnomethodologists caution against the privileging of work process over work practice.[2] What we hope to demonstrate in this paper is how we frame contingencies within our representations and use them not only to design and implement technology but to build stronger customer relationships. Representations reveal the grittiness and dynamism of the problems uncovered in the field by contrasting what was expected to happen with what actually happens. It is not, however, only the accuracy of the representation, the contextual minutiae or the deconstruction of work that is significant, but how the creation and use of these representations enable crucial conversations. Our emphasis is on the *interactions* that socially embodied artifacts afford and how representations invoke far deeper discussions with collaborators and more immediate and powerful ways in which to engage.

THE SPIRIT OF ETHNOGRAPHY IN BUSINESS

Our data reflect our participation on projects with customers of a Fortune 500 company. Over the last fifteen years, the company made significant changes in how it markets its offerings, to some extent moving from the exclusive production of objects ("selling devices") to the evoking of relationships ("selling services").[3] Anthropologists, ethnomethodologists and designers played no small part in supporting the shift towards services by exploring the application of ethnographic methods in service contexts, which included constructing methodological training workshops and participating with consultants and content delivery teams in the new service based organization. According to the Wikipedia, the open-source, online encyclopedia, the service sector is distinguishable by its "focus…on people interacting with people and serving the customer rather than transforming physical goods", while the service economy "refers to a model wherein as much economic activity as possible is treated as a service."[4]

Don Slater points out that the distinction between goods and services appears slight when considering the wider material, symbolic and social practices that influence each. What is more critical he says is to focus on "the changing conditions under which social objects are brought into being in the first place" (2001:14). While agreeing with Slater, we have also found that the difference in strategic emphasis between products and services in the corporation has in turn transformed the scope and definition of our work, some of which will be covered in this paper.

Earlier, our work as ethnographers in this corporation focused primarily on product innovation or addressed issues around organizational practices *within* the institution. Whenever possible, we used a participatory design approach (cf. PDC '94 for a wide range of studies advocating these methods). We worked closely with engineers, computer scientists and graphic designers to create robust prototypes and new features for products that were grounded in understandings of the work practices of our research partners and the internal needs of our organizations. Our roles in customer engagements were directed at research and development, and the timeframes for projects were longer than in consulting engagements. Typically, we conducted a series of open-ended interviews and observations at customer sites, using audio-video equipment to record the interactions for future analysis. From this we produced a number of "deliverables". Our objectives were not to create a checklist of technical specifications, but to demonstrate the nuanced context and varied perspectives in the doing of the work (Button and Dourish 1996) that might transcend pre-conceived notions about how the new prototype or product might be used.

We continue as before to conduct fieldwork at customers' sites. What has changed over the last seven or so years is that we are now actively invited to participate in the sales cycle.[5] We work on service engagements with a focus on new business process offerings that employ emerging technologies, saturating whenever possible, technology and service offerings with understandings of customer practices. Similar to other companies that have leveraged the skills of social scientists and designers (cf. case studies in Squires and Byron 2002), the new global services organization has incorporated ethnographic approaches at its point-of-sale. For the sales force, relationship building is crucial and ethnographers and designers can provide special attention to clients. First, the expectation is that we might help to expand the conversation from the sale of solutions and devices to the elicitation of the "real" problems faced by the customer. As one Account Manager noted wryly, "They trust researchers because they know you aren't trying to sell them hardware." We are asked to deepen the communication during sales calls over the telephone or in person by describing the variety of perspectives that the company can offer their clients – from new themes in research to socio-technical approaches to organizational change, and challenges in the adoption of new technology.

Secondly, the sales process now involves multidisciplinary teams that blend technical and business components with work practice analysis and Lean Six Sigma; a multi-dimensional approach that is well advertised through case studies on the company's public web site. The objective is to engage with customers around the specific problems they face and to develop a tailored solution that has wider applicability in the market. Prior to the development of the services organization, business account managers sometimes offered their time and our skills - data gathering, analysis and design - gratis to customers[6] in order to build the relationship.[7] Nowadays, we charge differently in different situations, sometimes only for travel and local expenses and in other cases our services are built into the costs to the client. The point is that the company is investing strategically in this work, whether or not customers explicitly pay for it. Thus we still maintain old approaches with development and research teams and have also been experimenting with understanding and developing new ways to intercept services business models within the global services organization.

REPRESENTATIONS AS PARTIAL CONNECTIONS

Much has been written in the last ten years about the nature of work and representations in participatory design and science and technology studies, ranging from issues associated with the specific rendering of work practices (Wall and Mosher 1994; Brun-Cottan and Wall 1995) and the

voicing of work (Suchman 1995), to the inherent negotiation between invisible/visible work (Star and Strauss 1999), and abstractions and particularities of ethnomethodology and design (Button and Dourish 1996).[8] More recently, researchers have resurrected the concept of the gift in socio-technical domains, such as open source software (Zeitlyn 2003), and file sharing practices (McGee and Skageby 2004).

Rather than focus on the democratization of design, we turn our gaze inwards to the representation itself, bringing to light how the artifact deeply embodies our own sociality as ethnographers and our "partial connections" (Strathern 2004). We refer to partial connections not as techno-biological entities, but as Marilyn Strathern so brilliantly argues, as "a series of perspectives… [that interact] as a constellation of elements [where] each position generates a further elaboration with an enlarging and diminishing effect on the constellations of the previous position" (2004:108).[9] In other words, representations capture what we see, hear, discuss, absorb, feel and observe but also contain the distillation of our analysis and partial worldviews, with issues shifting in focus from the foreground to the background and back again depending on our assigned or adoptive roles as archaeologists of the workplace, scouts for innovative design, and flag-bearers for the invisible.

We use Wall and Mosher's definition of representations as illustrations that consist of "sketches, video-stills, photographs, copies of notes or documents or combinations of these" that are "typically used to characterize some aspect of a site, a work activity or a proposed design" (1994:90). Over the years, we have created a portfolio of representational methods and tools that allow us to visually display our findings and analysis. This includes a graphics toolkit that consists of a library of icons that help us to depict the variety of every day objects used or exchanged in the environments that we study, such as computers, printers, tables, telephones, documents, files and filing cabinets, email, faxes and software applications. Appealing figures bring alive the interactions at work; with people seated at conference tables during meetings, puzzling over printers, handing each other documents of specific kinds in a particular sequence, or staring at computer screens on desks that are spilling over with books, papers, and emptied mugs of coffee. There is even an icon of the ethnographer as a videographer, later adopted as part of an emblem for the work practice and design team. It adorned the wall near our laboratory and was placed within the footer of our slide packs which are circulated within the research organization and business units. Then again, the icon library, although often updated to incorporate new icons based on new field studies, does not contain images of stethoscopes, vehicles, ATM machines, clocks, art objects or scientific instruments. Instead, the flurry of phenomena we might uncover in different "vertical industries" is captured through video stills and photographs.

At times representations are printed out on 11"x17" paper and pinned up in the laboratory. They are used to illustrate our methodology to internal visitors or in discussions with external customers. These are visually compelling pieces and in part, symbolic of the group that created them. The icons too, have grown a life of their own, affectionately referred to as "Blue Guys" by those in the know and over time, the term has been adopted by others further removed from their creation. As team members left the group to work elsewhere, they asked for copies of the Blue Guys to take with them. Recently they were adopted by another group of researchers working on a large consulting assignment overseas. They have become widely requested, and the toolkit has been made available on a company-wide repository. The Blue Guys are currently being used by the global services organization as well as product development, sales and research groups. This is not mere clip art generated for the general public but has come to represent the identity of the group in which it was created, its methods and its particular purview of design that privileges the customer. The representations embody the shadow space between client and supplier, a porthole to work that some will never see in person.

Representations re-situate the multiplicity of tasks, sites and interpretations of the workplace. They turn indexical and polyphonic; marking with situational interactions the places where work gets done. Representations point to elements in a customer's work place to which we wish to draw attention. At the same time, we maintain that these are snapshots in dynamic environments where people's roles are not clear-cut, but change according to need and circumstance. If representations are abstractions of primary research and grapple with what is understood from a field site and what is offered for design, they also present a topology of how people's work practices interleave complex activities at various points in time. The views toggle across "place" (the different digital and physical realms in which activities happen), time, and conceptual or analytical "space". On the one hand, the views capture problems that people experience when moving information from numerous, password-protected databases to an Excel spreadsheet; coordination work with sheaves of paper moving between hallway printers, staff mailboxes and the individual's desk; the distilling down of hundreds of images between the library at work and the projector at home, and on the other hand, they encompass data revealed through observations at customer sites and suggestions made during product design sessions.

To sum up, the icons used in the representations serve to catalog both the work practices and material objects that we seek to portray. Yet, while the imprint of the ethnographer permeates our representations, the ethnographic voice still persists as narrator rather than *in* the narrative. Rarely is the grist of our evolving relationship with customers made explicit in the illustrations, except to frame the methods that we use, the phases of the project, or how the data are analyzed. The ethnographer must choose what to reveal to the immediate audience while keeping in mind that the sub-text of the work may be disassociated from the reports circulated to senior management. For instance, will internal political negotiations at customer sites be important to our product team if our engagement is only a short one? How do we describe work that "falls through the cracks" without blame being personally attributed to employees? How do we apply to service engagements the position that representations disclose certain aspects while leaving others out? Consulting projects are phased along tight schedules: risks are high, the clock face marks time by Quarterly results, and "closing a deal" involves the difficult act of persuading customers to close the door on your competitors. What the corporation wishes to understand are newly revealed perceptions and practices that will help keep or attract *purchasing* customers.[10] Elicitations of people's work are of interest to the extent that they extend business alliances or inform opportunities for innovation.[11] In these cases, it is the intended audience that will determine the framing and content of our analysis. We create many versions of the same analysis for different audiences; use cases for engineers or software architects with information on how customers interacted with systems; graphical representations for customers, business development, heads of research or C-level personnel, and reports whose content range from understandings of current state to future scenarios and challenges.

Those in sales best understand how critical it is to build personal relationships in a marketplace crowded by choice. Workshops are organized for customers; account executives make one-on-one calls to clients; and business groups and Lean Six Sigma teams, following the decade of Knowledge Management, use intranet portals to record and communicate "lessons learned" and "best practices". Any foothold that might further a relationship is explored. Unexpectedly, we found that the pragmatism that ethnographers and designers bring to the table is a welcome addition to discussions with customers wary of purchasing and adopting emerging technologies.

REPRESENTATIONS AS GIFTS

It is the nebulous, shifting nature of gift exchange that so fascinates in the context of service engagements. As Mark Osteen points out, "Certainly we do not give gifts to everyone whom we encounter, and most gift theorists recognize a distinction between the domains of market and gift". (Osteen 2004:2). Chris Gregory builds upon Mauss's initial proposition that certain types of gifts might exemplify the original form of exchange between people or groups, but distances himself from the spirit of Mauss's writings. He proposes that the mode of exchange renders gifts and commodities distinct, wherein the focus lies on objects that are exchanged and whether they are "alienable" or "inalienable". Osteen summarizes Gregory's work succinctly, "In a gift economy, objects are personified; in a market economy, persons are objectified" (2004: 233). Keith Hart (2001) however, reminds us of the depth of sociality in exchange, "Mauss held that there are two prerequisites for being human: we each have to learn to be self-reliant to a high degree and we have to belong to others in order to survive, merging our identities in a bewildering variety of social relationships." The glue that holds social groups together is "the idea that exchange must be two-sided", in other words, that reciprocity is induced by the obligation to give, receive and return over time. Hart (2001) continues, "In every way modern markets deny this premise, separating individuals from the object world and from each other, banishing the spirituality and social substance from exchange. Yet our humanity inserts the spirit of the gift into market economy in profound ways." These are precisely the tensions explored in this paper, and we use the following examples to illustrate how the thread of reciprocity surfaces through various interactions with customers.

CASE STUDIES

Let us ground the discussion based on three case studies. The first case study involved a project in the transportation industry aimed to ensure 100% compliance in meeting regulatory standards with respect to distributing and reading updated bulletins of equipment maintenance documentation. In all, 3500 maintenance technicians had to read and sign that they read the new bulletins within twenty days of publication: a challenging proposition. The solution had to integrate with the sites where documentation was stored, in addition to new software that they deployed worldwide. Two ethnographers, one with a technical background, participated as part of the services consulting team in fieldwork and design sessions with the customer. Sets of representations were constructed, based on detailed descriptions of the current practices and the compilation of multiple viewpoints: (a) corporate management responsible for ensuring compliance, (b) the IT department implementing the tracking system, (c) middle managers covering site level compliance and (d) supervisors and technicians responsible for maintenance. (See Figure 1) Representations captured procedural, physical and cultural aspects of the work as well as some underlying problems. For example, how time-consuming and frustrating it was for technicians to interrupt pressing work in the bays in order to read on PC's and sign off on documents in binders stored in break rooms. Or, the binders tended to float to different points in the bay, so the chances of finding them were marginal. The second set of representations captured the discussion around proposed changes and implications for software integration with their current system. Finally, a third set of documents illustrated the anticipated impact of the solution, uncovering metrics to measure impact on existing practices and noting what would change for each one of the stakeholders. These greatly helped the customer envision the look and feel of the new solution. Ethnographers invested heavily in eliciting multiple perspectives and spent a substantial amount of effort sharing what was learned and refining the solution with the infrastructure vendor and the services team responsible for the solution. Obtaining a comprehensive view of the entire process

revealed points of interception for the solution further upstream than anticipated. The representations illustrated to the customer that the global services team had a good understanding of the work: in fact, the technical solution followed very closely the initial proposal created by the ethnographer. Months after implementation, the feedback from the client was very positive. The attention paid to all perspectives and in particular that of the technicians was deeply appreciated: a supervisor wrote saying it was his belief that the solution was successful because it addressed concerns of those responsible for compliance at every level. Beyond the customer relationship, the analysis embedded in the representations and final solution was used to develop a case study, published on the customer's website. The illustrations were also utilized as training materials for global services consultants.

In the second case study, a research team was sponsored by a business unit to work with a state-affiliated organization that supplied educational and technical infrastructure to school districts. This was not a service engagement per se as it took place before there was a global services group. But it embodied many of the characteristics of a service project; primarily, to exploit existing technology to improve current processes and to uncover other potential applications, in this case for a new scanning and web-based repository for administrative and academic work. Over several months, two ethnographers and a designer interviewed administrators and faculty members at particular sites and observed the hurdles faced by K-12 schools in making state educational standards transparent. The study had interesting outcomes. New applications were found in unanticipated places, such as a music teacher wanting to create student folders that held digital sheet music (enabling students to practice their instruments by reading music off their computers), lessons, music schedules, videotaped segments of student recitals, and written critiques. We also identified a few opportunities to streamline paper intensive work. On the whole, our findings highlighted just how difficult it was to compile and put standardized educational materials into electronic form for web access by the school and wider community. One of the schools then used the representations to justify creating a new contract position to help overworked teachers move material online. Moreover, our interactions were pivotal in raising a number of issues for school districts; among them that security and access needed to be addressed at a district policy level before the solution could be implemented.[12]

A third example in higher education used work practice analysis to develop a web-based prototype application to integrate new digital image collections with the library's academic collection. The team, unlike those in consulting engagements, was composed entirely of researchers. The project was not bound by severe time constraints and the fieldwork, software architecture and prototype development took more than a year. The technology was designed based upon ongoing and lively interactions with faculty, librarians, curators, technical support, administrative staff and management. The ways in which faculty members organized, sorted, presented and archived 35 mm slides had a significant impact on our design.[13] Ultimately, the design of the prototype was based on metaphors

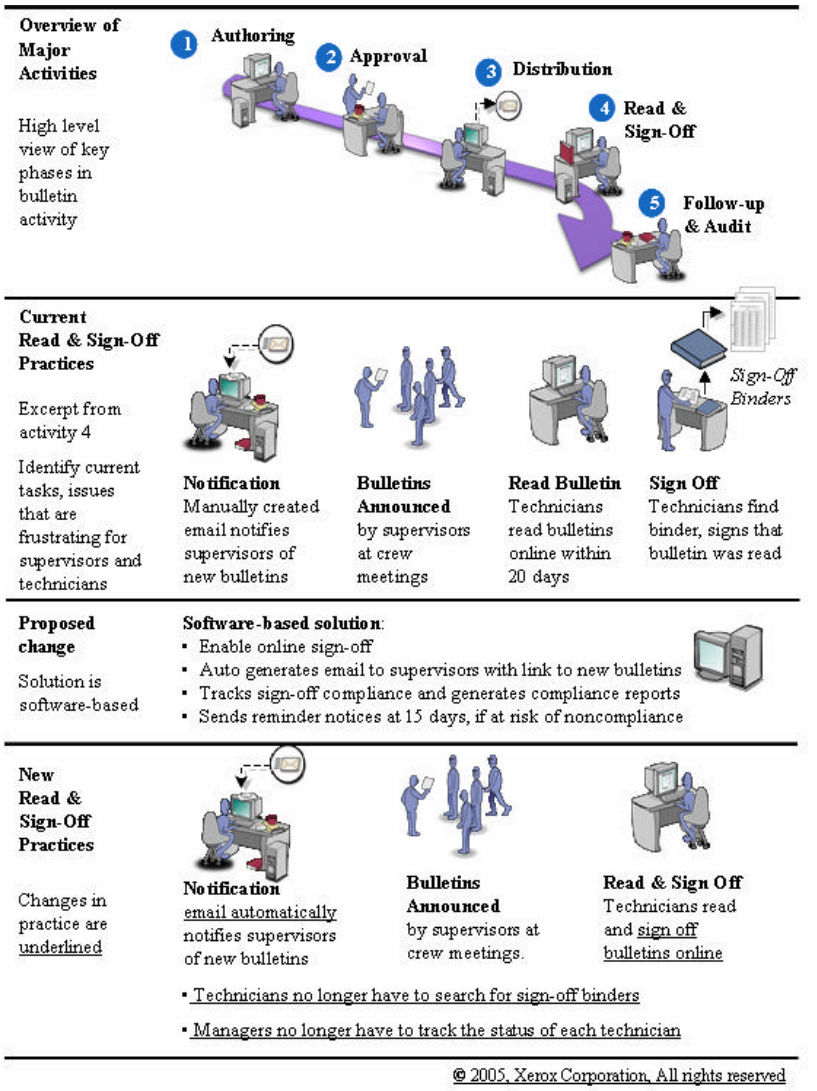

FIGURE 1 Evolution of representations over the course of a service engagement
Representations capture different stages in a service engagement focused on developing a solution to support maintenance bulletin compliance. An overview provides a high level view of key phases in the compliance activity. From here we capture the detailed work practices within and across the phases. As we understand the complexities and issues with the current practices, we begin to overlay various solutions that may address the issues. Finally, representations capture the envisioned solution and the impact on the work.

gleaned from our interviews and observations of objects that people used, such as slides, light tables, carousels and albums. At this time, we also developed an early version of a graphics toolkit, a language in pictures, that enabled those of us who thought in visual terms to reflect this orientation and describe our findings graphically. Early on, representations were used to verify whether we had accurately captured people's work practices. We spent several wonderful hours with an architecture professor, for instance, who enthusiastically marked up the data with his corrections. Representations also captured the technical and organizational challenges that we observed while people were converting and cataloguing images. On one occasion, a joint meeting was set up with the departments concerned to discuss what we had found. Considering that the data were sensitive as they highlighted where things went wrong, the outcome of the meeting was remarkable, with the technical staff openly voicing their concerns. As a result of this, a decision was made to buy digital (photographic) equipment that saved time and prevented re-work. Thus, as initial representations were developed and shared with the library staff the ensuing discussions would give rise to others on campus that would lend additional perspectives on issues and lead to actionable decisions.

We return to the central problem raised by Thompson; what is it that creates or destroys value? What we hope these examples demonstrate is how representations compound value; building, layer upon layer, mutual comprehension, trust, joint engagement, buy-in and commitment. These are critical for the service engagements we encounter. We recognize that it takes a long time to build emerging technologies that will integrate effectively into customer environments; one can rarely anticipate how well they will be received. Nevertheless, to a large extent, customer after customer gifts to us their time and a patient revealing of their worldviews. The sharing of representations with them is not merely to verify and expand our findings or give voice to the dexterity of labor so frequently lost in the din of daily work, but to recognize the risks that they take in deferring value. Representations help to bring immediacy back into the relationship, where customers need not wait for the new solution to be built in order to determine whether or not their risks in the partnership paid off. By visually and tangibly acknowledging the problems they share with us and the richness and complexity of their practice, we return it to the forefront. These are the thirteenth loaf in the Baker's Dozen: prestations[14] within pre-existing formal engagements that nevertheless demonstrate our efforts towards reciprocity. We do not expect customers to pay for these graphical instantiations, nor do we expect a return in kind. Instead, we anticipate that our analysis will be thoroughly dissected or discussed with our clients, and hope that it might trigger discussions within and across their organizations. Often we find that discussions around representations provoke a different level of communication between teams, where people are momentarily free to reflect on the intricacies of their own practice and visualize the intersections of work across the whole organization. There is a higher probability that our service to them may in turn strengthen our mutual engagement as customers grow personally invested in the joint solution. While it remains questionable as to whether customers explicitly acknowledge representations as gifts, it is clear that representations inevitably induce deeper connections and generate a panoply of conversations, minimize misunderstandings and erect a scaffolding of trust between us. We argue that these prestations to customers are distinct from deliverables to our business partners: the former recognizes the construction of relations inherent in our immediate interactions, while the latter emphasizes the sharing of information where in the long run, the roles that people play are deemed more significant than the individuals that fill them.

THE ABSTRACTION OF KNOWLEDGE WORK

Next, we will explore an issue raised by Lucy Suchman that remains relevant not only to participatory design but our work within the services organization. “It is problematic,” she writes, when "normative representations" are removed from the sites where work takes place and "used in place of working knowledges" (1995:61). In addition, Brun-Cottan warns that ethnographers and designers must “keep the analytic grounding of our work visible” lest the value of our contributions remain tacit and risk becoming invisible (2004:8). This is symptomatic of the tension in our role as corporate ethnographers. We investigate, analyze and recommend. If we get lucky we are even able to “implement” in meaningful ways. But an uneasy symbiosis exists between “inalienable representations”,[15] that is, objects that partly embody the ethnographer, and “deliverables”, which take a life of their own when for instance, a senior vice president emails our reports to others who are less aware of and less interested in their sub-text. How do you convey the value of labor in situations where it is seen as a cost? The challenge remains very real: can the ethnographer truly address the issue that the metadata sometimes becomes the data itself, a mere descriptor of information used for purposes other than which it was intended? Is it indeed possible to avert the specter of shadows cast in Plato’s Cave, and infuse discussions about the customer back into two dimensional representations skimmed over by harried executives?

The next example demonstrates how complex the positioning of work can be, and perhaps raises more questions than can be answered. We were involved a highly visible potential strategic alliance between two Fortune 500 firms. Hardware and software from both companies would be used to build a “workflow solution” for a financial institution that wished to streamline the processing of large amounts of files and papers. The third company, the customer, was keen to cut costs. Onsite, we interviewed those who processed the data as well as their managers and the Information Technology team, tracking, among other things, who did what to which files as documents were circulated, and trying to better understand the ways in which people sifted and retrieved information. We noticed that different employees spent a considerable proportion of time checking and re-checking the same financial data, which caused our Lean Six Sigma and IT colleagues to consider automating that function. On closer examination of the video-tapes, the ethnographers realized that not all financial information was complete at the beginning of the process. People double and triple-checked the figures to ensure that new information was recorded. New data appeared at random times on databases, in emails and hardcopy. Financial mistakes could mean a significant loss of business.

When the representations were shared with the client, the manager, John,[16] and other employees said that they were amazed at the “accuracy” of our insights, considering that we had been onsite for less than a week. An electronic UI mock-up had been created on the basis of use cases and work practices captured in the representations. The ethnographer visited the site a second time to review the user interface and potential problems that might emerge in the new solution. The customer responded positively to the participatory aspect of the project and worked closely with us to define the parameters, features, and fields for the software. Issues around automation also changed. John recognized that Jane’s work could not be replaced with a software system because she “still needed to check the figures and set up the remaining bells and whistles that can’t be automated.” For technical and monetary reasons, however, the project had to be modified. During a conference call to discuss this, John remarked, “I really liked when [the ethnographer] came and we discussed the layout of the screen. I want to be able to see some of that [on the new workflow system], so if we want to change direction, I’m excited about all of that.” Shortly afterwards, there were organizational changes in the company. New personnel were introduced and additional costs surfaced that had not been anticipated

by either side. The relationship too was altered, moving from discussions about how current practice might map to the future solution, to insurmountable technical challenges. Eventually, both parties agreed to withdraw from the contract.

In the meanwhile, several representations had been extracted from reports and incorporated with Lean Six Sigma workflow diagrams into slides for a "CEO Summit", a joint meeting between the CEOs of companies participating in the alliance. Only, the work practice insights had been framed rather differently. What was identified as important at the client site, that is, the employees' meticulous double-checking of financial figures from disparate electronic and hardcopy sources, was positioned as redundant work and a case created to cut the costs of labor through automation.

This example should not suggest a deliberate misrepresentation of data but how the project took a life of its own and the nuances of work were lost in its re-framing in order to meet strategic needs. Similarly, there was a concern that the Blue Guys, now available on the corporate web site, might be utilized simply for straight descriptions of work in flowcharts that reflect static process rather than dynamic practice. Would the icons being used to represent work become mere tokens or also embody the invisible nature of the work? More recently, we are in fact seeing new and interesting ways in which the Blue Guys are being portrayed by others in the company. They are being added to descriptions about work processes. The net effect is that people are given a more prominent role in technology discussions and are being placed back into the story. It has become far easier to discuss and portray people's work instead of focusing entirely on a techno-centric view of the world.

CONCLUSION

If one wishes to infuse some aspects of an ethnographic approach, or even determine if they are desirable and feasible, it is necessary to understand the work practices, constraints and objectives of the organization and teams responsible for the consulting engagements. Part of our research agenda is to explore if ethnographic methods would be useful in services engagements and if so, what kinds of tools and training could be provided to the consulting teams. At the same time, ethnographers and designers are invested in doing right by the individuals with whom we work, whether internal colleagues and managers, or external collaborators. These partial connections might merge, run in parallel or diverge. What we hope to have demonstrated is how representations embody those tensions, and how Mauss's concept of the gift discloses relational nuances even further. This in turn, leads us to discuss the changing nature of our work and to reflect on its impact. Finally, the question inherent in this paper is how one might integrate and draw upon different disciplines as a resource to help comprehend our own ethical practices in pragmatic ways.[17] In the end, the objective is straight-forward and asks simply for us to reveal our own "mundane practice" within sight of our complex but mundane sociality.

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[1] See Jordan (1996) for ways in which to elicit different kinds of analyses from a workplace, creating records that focus on people, objects and settings.

[3] This, incidentally, sometimes involves "hardware drag", or the sale of devices in addition to consulting services.

[4] The source of this information is: http://en.wikipedia.org/wiki/Service_sector

[5] Many of our interactions with the services business resemble Squires' description of rapid ethnography for product development (2002:107-108).

[6] At times to our dismay as it appeared to undervalue what we delivered to our management, especially when it was difficult to quantify contributions to company's "bottom line".

[7] Internally, our activities were funded by the research organization.

[8] We would like to acknowledge the pervasive influence of long-standing debates about ethnographic representation in anthropology. This paper however, will not specifically address those issues.

[9] One might see two dimensional instantiations of this concept in software tools such as Grokker and the hyberbolic browser.

[10] We often study sites where the client is not a corporate customer.

[11] See Suchman (1995: 62-63) for a discussion on how representations might best be used to induce a dialog between actors and ethnographers rather than an elucidation of difference.

[12] Cf. Brun-Cottan (2004) for further information on the "conflicting interests" between teachers, manufacturers, organizations and ethnographers in this case.

[13] See Marshall (1998) for a more detailed analysis of this project in the context of metadata creation for online image collections.

[14] For Mauss (1990), a "total prestation" represented a multi-layered gift, one that embodied social, economic, political and even religious properties. Not all these properties are relevant in our case. However, it is clear that representations reflect and indeed further the weaving together of the social fabric of relationships with customers.

[15] For a fuller discussion on Gregory's distinction between gifts and commodities and alienable or inalienable objects, see Osteen (2004).

[16] This is a pseudonym.

[15] Many thanks to Nina Wakeford for this insight.

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9

CONFIGURING LIVING LABS FOR A 'THICK' UNDERSTANDING OF INNOVATION

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The paper examines the living lab as an approach within communication studies for examining the naturalistic involvement of users in ICT design, based on ethnographic principles. First a more precise definition of the living lab is presented, indicating the epistemological background. Next the different phases of our living lab configuration are elaborated, illustrated by a research project on a handheld electronic reading device (e-paper). Finally we discuss the value of this approach for companies involved in ICT research and design. In the conclusion also the advantages on product development level and social level are indicated.

INTRODUCTION

Understanding and involving users in (broadband) innovation has become a central issue in private and public research and policy on information and communications technologies (ICT) in Europe and elsewhere. Furthering design and development of future technologies and services that are well adapted to potential users will lower the threshold for social acceptance and increase the potential for e-inclusion. In addition it can help to understand why certain innovations can be 'disruptive' for existing technology paths. For companies comprehending the (potential) user more thoroughly can help in minimising risks of technology introduction and possibly shorten the time-to-market.

Our basic idea is that for thoroughly understanding users:

> *'technologies should be studied in situ and in use, as part of socio-technical arrangements of humans and machines joined together in action and embedded in social contexts.' (Ratto, 2000)*

This highlights the rejection of technological determinism and the naturalising influences of functionalist ideas of technologies, while seeing technology as practice. Therefore they can only be assessed in their relations to the sites of their production and use (Suchman, 1987).

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Research Focus

These considerations fit in the living lab method, which entails studying underlying social processes that drive the use and development of communication artefacts. The main question in the paper is how the living lab setting, based on ethnographic principles, can be seen as an optimal way of optimally enabling users to contribute in the early stages of innovation processes.

Our approach relates to the field of Social Shaping of Technology (SST). SST research:

> *'(I)nvestigates the ways in which social, institutional, economic and cultural factors have shaped: 1. the direction as well as the rate of innovation; 2. the form of technology: the content of technological artefacts and practices; 3. the outcomes of technological change for different groups in society.' (Williams & Edge, 1996: 868)*

The overall living lab approach and the different phases as presented in this paper were developed and finalised by experiences within different projects. In order to answer the research question in the paper first a more precise definition of the living lab is elaborated, indicating the epistemological background. Next the different phases of our living lab configuration are elaborated, illustrated by a case study on a handheld electronic reading device (e-Paper). Finally we discuss the value this ethnographic approach has for companies involved in ICT research and design. In the conclusion also the advantages on product development level and social level are indicated.

Defining living lab

The 'living lab' is a specific type of test and experimentation platform (TEP). The latter indicates all facilities and environments for (joint) innovation including testing, prototyping and confronting technology with usage situations (Ballon, Pierson, & Delaere, 2005). Living labs refer to facilities for designing, developing, testing and evaluating communication technologies and services in early stages of the innovation process by involving (early) users, in line with the SST research. These can take the form of some sort of lab (like a house), in order to test and experience (uses of) technologies. This corresponds to the smart house idea, which has been around since the eighties. However our focus is on living labs that have a broader geographical reach, covering a range of (mobile) people, several households, a specific area (e.g. campus), a community, a neighbourhood or even a town. These kinds of facilities can be set up and managed by one company. They can also be configured as open and innovation-oriented platforms that involve various technology and service providers as well as users in different stages of technology design, development and testing.

The living lab aims at using the natural user environment as much as possible, hence enabling ethnographic research on the participants in the living lab. One the first scholars to use this notion of living lab was William Mitchell from MIT (Boston), Medialab and School of Architecture and city planning.

> *'Living Labs is a research methodology for sensing, prototyping, validating and refining complex solutions in multiple and evolving real life contexts.'*[1]

In a rare systematic overview of joint innovation facilities, Niitamo describes the living labs as:

> *'A broad regional development program where testing, developing and validating new products and services indicate future systemic innovation needs.'*[2]

This kind of living lab is based on the notion of user as co-producers of ICT, which refers to the idea that the user is never an 'end user' but re-interprets technological artefacts within his social context once they are adopted (Bergman & Frissen, 1997). This point of view needs to be situated within the theoretical notions of 'social construction of technology' (Bijker, Hughes, & Pinch, 1987). The living lab is thus characterised by confronting (potential) users with (ideas, prototypes or demonstrators of) technology early on in the innovation process. Within the (new) product development and innovation management literature this is also typified as the 'fuzzy front-end', as dubbed by Smith and Reinertsen (1991). This refers to all activities preceding the start of formal product development process (Khurana & Rosenthal, 1997).

Epistemological background of the living lab approach

The account of a living lab in this paper is based on our disciplinary background in communication studies, as an interdisciplinary study of the relation between media, society and people. We view media and technologies as a form of social mediation (Garnham, 2000). The communication studies historically have been focussing on the content side of media. In our research we start from the vision of media as technological tools, being the basis for human culture. In alliance with authors like Silverstone, Haddon and Morley we look at ICT from a domestication perspective, which has its roots in cultural studies (Berker, Hartmann, Punie, & Ward, In press; Morley, 2003; Silverstone & Haddon, 1996; Silverstone & Hirsch, 1992). One of the basic goals is:

> *'(...) to frame the analyses (...) within a broader framework of the role of various media in articulating the private and public spheres, which (hopefully) allows us to articulate these micro-analyses to broader perspectives on macro-social issues of politics, power and culture.' (Morley, 1992: 40)*

In order to clarify the epistemological background of our living lab approach within communication and technology studies, we refer to the dichotomy in sociological approaches of the technological innovation process: a factor approach - which explain social phenomena through an identification of factors - versus an actor approach - where the perception and experience of the actors is the point of departure. The division between factor and actor approach corresponds with similar epistemological classifications, like 'objectivist' versus 'subjectivist' (Willmott, 1993) or 'positivistic' versus 'interpretative' (Servaes & Frissen, 1997).[3]

Ethnography in general and the living lab approach in particular fit in mainly with the actor or interpretative approach. Nevertheless to operationalize living lab research we choose for a 'multimethodological' approach (Mingers, 1997). The prevalent qualitative part enables us to identify the meanings and experiences of the actors, while the quantitative research plays a supportive role. It is the combination of both, that will enable a more elaborated triangulation of findings, based on

methodological or systematic pluralism (Ford, 1990; Roth, 1987). Before positivism reached its high point in the 1930s and 1940s this pluralism was generally accepted in sociology and social psychology (Hammersley & Atkinson, 1995: 3)

Living lab configuration

Our living lab configuration is set up as a meta-methodology for analysing the confrontation between ICT and users from an ethnographic perspective. The overall configuration and the underlying research methods - that are further elaborated in this paper - are partly founded on an analysis of other international living lab cases.[4] Yet for the most part they are built upon hands-on experiences in three projects we were or are involved in: '*Vlaanderen Interactief*'[5], '*e-Paper*'[6] and '*i-City*'[7].

The living lab research cycle presented below is in fact a methodological synopsis of our experiences in setting up and conducting living labs in these projects. The innovative character of our living lab approach does not so much stems from the individual - often familiar - (ethnographic) methods applied, but from the specific configuration of (qualitative and quantitative) methods and the way these different methodological components are geared to one another. This means that our living lab configuration is still work-in-progress. At present we identify four different phases: contextualisation, concretisation, implementation and feedback. These phases are illustrated by elements from the e-Paper project.

The e-Paper project is a case of how users can be actively involved in technology development and large-scale testing of a specific handheld electronic reading device, based on the e-ink technology applied for an online newspaper. In order to develop and implement the device, standing up to the needs and expectations of the user, 200 test devices will be distributed for a period of three month among a test sample. The socio-technological research framework is based upon the living lab configuration (from contextualisation until feedback). E-paper is public-private sponsored research project, initiated by the Interdisciplinary Institute for BroadBand Technology (IBBT).

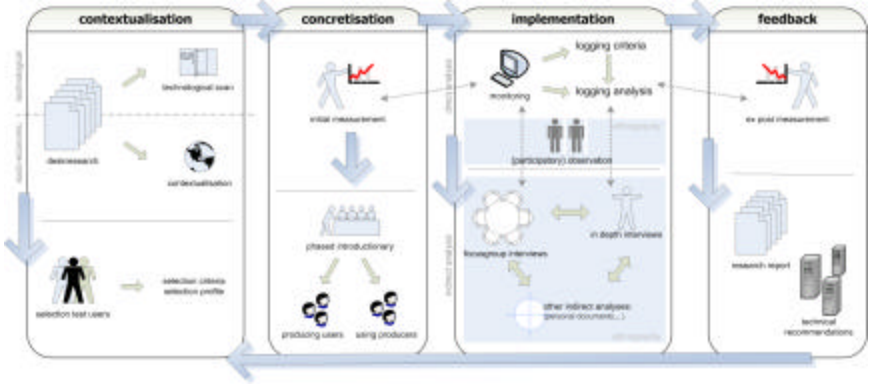

Figure 1. Overview of living lab research cycle

Contextualisation

The contextualisation phase is an explorative phase. This phase resembles the exploration phase in grounded theory (Glaser & Strauss, 1967). A theoretical basis has to be expounded in order to (1) define the research framework and (2) to identify eligible respondents. In this phase different research methods are being applied in order to provide the required background and insights. Starting from the technological challenges as well as - if applicable - the defined use cases specific desk research is performed on two levels: technological and social. This enables a first confrontation on an abstract level of what is technologically possible with what is socially feasible and vice versa.

Contextualisation methods - In order to situate the technology or service under investigation, first a technological scan is performed. The purpose of this analysis is not only to give a(n) (quick) overview of current and future technologies but also to map the specific functionalities and characteristics related to these technologies. Second a (state-of-the-art) study is deployed in order to determine the (socio-economic) contextualisation of the research focus (research framework as well as research topic). Complementary, other methods can be applied like environmental scanning (Gordon & Glenn, 2003). This serves as input for phenomenal variation in purposeful sampling (see below).

Case description
Within the e-Paper project different assumptions were made, based on the contextualisation research (on technological and social level) that had an impact on the composition of the test panel. E.g. some newspaper readers are subscribed to some (interactive) e-services directly related to the newspapers. One of these services is an alert service that notifies the reader (by e-mail or SMS) on important news facts. Within the test panel a group of those users were selected based on the assumption that these users are more likely going to use such services on the e-paper device and therefore could provide useful insights and feedback.

The outcome of these different steps - which are not always strictly separated from each other - does not only help in setting out but also in the validation of the qualitative research framework. In order to identify potential users or user groups, this research phase allows us to stipulate the selection criteria and therefore the selection profile.

Selection - Crucial in our living lab configuration is the formalization of the selection of respondents. For our selection we apply sampling procedures from qualitative research. The selection is based on non-probability sampling, which means that not everyone has an equal chance of being selected. We choose our respondents in the living lab purposefully. Sandelowski discerns three kinds of purposeful sampling: maximum variation, phenomenal variation and theoretical variation (Coyne, 1997). The objective of maximum variation is analytical diversity. The kind(s) of variation need(s) to be made explicit as well as when to maximize each kind. This refers for example to socio-demographic variation in gender, age or education. Respondents that differ on these variables are useful in the living lab research because of their analytic value, not because of generalization. People of a particular age are selected because, by the virtue of their age, they can provide certain kinds of information. Phenomenal variation aims for variation of the target phenomenon under study. The decision to seek this variation is 'often made a priori in order to have representative coverage of variables likely to be important in understanding how diverse factors configure as a whole' (Sandelowski, 1995: 182). This is also referred to as 'selective or criterion sampling', where sampling decisions are made beforehand on 'reasonable'

grounds. A third type is the theoretical variation. This is a variation on a theoretical construct that is being developed on analytic grounds in the course of the study (Golden, 2000). In this way the selection of test users is based on the evolving theoretical relevance of concepts, which ties in with the notion of 'theoretical sampling' typical for grounded theory studies (Strauss & Corbin, 1990).

In the living lab research cycle there are two instances of sampling. In the contextualisation phase we select the main living lab participants. In our living lab configuration we apply maximum variation and phenomenal variation for our purposeful sampling procedure. More in particular the selection criteria are based on data from the preceding technological scan and the socio-economic contextualisation study (as described above). In order to check the identified criteria and to determine the definitive selection profile or 'screener',[8] explorative focus groups - based on these criteria - are organised. Besides finalising the selection profile, this generates a first glance at the prospective sample within the living lab configuration. Later on, in the implementation phase, we select the people that will be monitored and examined more closely by way of (ethnographic) observation and qualitative interviewing. This selection is mainly based on phenomenal variation and theoretical variation. The quantitative logging analysis serves as guideline for identifying the most information-rich cases.

Case description
In the e-Paper case purposeful sampling (by quota) is used. First an analysis of the socio-demographical composition is made in order to determine the basic subgroups: residential versus professional use, male versus female and whether or not using online services. For the phenomenal variation the additional criteria focus on the research goal. We identify four main categories on the fact whether people read newspapers -and if so if they do this on- or offline- or not. Remaining selection criteria in the screener are based on their mobility (are they often on the road), their usage (do they download online newspaper, do they use interactive services (offered by the traditional newspaper)) and which portable/handheld device (like pda) they possess.

Concretisation

Once the recruiting of the test-users - according to defined selection profile - has been completed, the next step is to get a thorough description of the current characteristics and everyday life behaviour and the perceptions of the selected test users regarding the research focus. Therefore an initial measurement of the sample is made before the technology or service is introduced or before the test panel becomes active in the living lab.

In this initial measurement we look at specific characteristics on the level of the user (socio-demographic and economic) as well their relation towards (the introduced) technology or service. The methodology to perform this snapshot depends on the size of the test panel. Usually a(n) (online) survey is used. Through a (semi) structured questionnaire this method enables us to gather an extensive data-stream in a quick way from a large audience. Depending on the scale of the sample, this method can be supported by qualitative methods like in-depth interviews. This measure point enables us to perform an evaluation at the end of the living lab project.

In the initial measurement we distinguish two types of components: a fixed and a variable component. The fixed component looks at data that can be applied in all living lab settings. In this component we first look at socio-demographic and economic characteristics of the user regarding his/her family profile, personal profile and professional profile. Second we look at the general media-profile (access, usage,…). The variable component exists of a number of questions that are case-specific. Depending on the research topic this could also mean a more in-depth questioning of the fixed component.

Case description
In the project the variable components focuses on the newspaper-profile. Besides these thematically oriented questions, also a list of questions related to the services and applications of the technology in question (e.g. e-paper device, possession of mobile devices,..) is included in the variable component.

The initial measurement provides the necessary information to start with the living lab. This start up can occur in two ways: simultaneous or phased. The way of phasing depends not only on the goals and objectives of the living labs but also on the level of applications and services that has to be tested and/or is available. Depending on how the latter is offered, different categories within the sample can be introduced in the test and experimentation environment.

Implementation

The implementation phase is actually the operationally running test phase. From a user-oriented and ethnographic research viewpoint we distinguish two major research methods: direct and indirect analysis.

For the direct analysis remote data collection techniques and strategies are used (Blomberg et al., 2003). In our projects technological monitoring is implemented. On the device level (e.g. pda, mobile phone or digital television) as well as on the platform/network level (if applicable) software logging tools are employed. This tool registers, on an individual level, all the relevant user actions on the technology/device level. In order to organise this logging efficiently, we determine in advance the logging framework, including for example how detailed the logging needs to be done. The data processing based on this logging provides an insight in the usage and is triangulated with other research findings. Besides logging analysis, in this research phase an appeal is made on the basic ethnographic methods like (participatory) observation.

Case description
In order to be able to analyse real time behaviour, within the e-Paper project all the actions on the e-reader device are registered. Due to the unpredictability of usage, it is necessary that we log - and analyse - all usage, in order to detect expected and unexpected user behaviour. The activities will be logged on three levels: device, services and applications (on demand services in particular) and content. At the device level it includes e.g. activities on how many times the device is switched on, linked to the (geographical) location. To what extent people are activating and using certain (personalised) services as well as updating their user profile is monitored on the service and application level. Finally at the content level not only the effective usage on which articles people read and for how long is being monitored, but also their navigation within this content.

Constant analyses of these loggings make it possible to set up thematically focus groups or to conduct in-depth interviews during the test phase. This enables us to identify emerging topics on a

short notice as well as to identify, in a convenient way, the eligible respondents for these interviews. These analyses provided also a tool for the industrial partners to check new introduced services or applications and - if necessary - respond in a quick and appropriate way in the early stage of the development. This created a very dynamic and iterative process between developers and the living lab users.

From here on indirect analyses are being applied to investigate the meaning and motivation for behaviour. For the indirect analysis, different research methodologies can be used like (thematically organised) focus group interviews, in-depth interviews and self-reporting techniques like diaries. The themes of the focus groups depends on the logging analysis, e.g. around some applications which are heavily used by a certain type of user.

All these direct and indirect analyses within the implementation phase are complementary and contribute in gathering information on adoption, usage, meaning, motivation and possible influence.

Feedback

The feedback phase at the end of the living lab project consist of two research steps. First an ex post measurement is conducted. Based on the questionnaire of the initial measurement a closing survey is administered on the whole test sample. For this ex post measurement the survey method is used. The goal is to check if there is any evolution in the perception and attitude towards the introduced technology or service, to assess changes over time in everyday life in relation to technology use and to detect transitions of usage over time. The results of this measurement are compared with the insights from the qualitative research in the previous phase.

The second research step is to infer technological recommendations from the analysed data, gathered during the implementation phase. This refers in the first place to the findings based on the direct and indirect analyses in the former phase. This generates among others characteristic user profiles and user patterns, elaborated in the research report. The outcome of the feedback phase can be used as the starting point for a new research cycle within a living lab project. In this way the iterative feature of our research cycle can be made operational in the living lab configuration.

Value of the living lab

Based on the process overview of our living lab configuration, we now specify the particular usefulness. For this we make a difference between living labs that offer a platform for the co-operation between different (sometimes competing) companies and living labs by one company only.

Living lab by an individual company

For an individual company the living lab offers a number of advantages in comparison to other methodologies. We identify four major benefits.

First our living lab configuration offers assistance for designing and conducting (large-scale) naturalistic multimethodological research around a particular technology, device or service, based on ethnographic principles. It gives the opportunity to embed complex product ideas and prototypes in a environment that resembles as much as possible the context and everyday life setting of the people that

(could) use the object of research in real-life. This kind of 'new product ethnography' is helpful in structuring the 'fuzzy front-end' investigation of new product development (Cagan & Vogel, 2002: 183-188). The living lab is thereby especially useful in a multi-platform environment. Through digitalisation services and applications are no longer dependant on one platform or device. In the living lab setting different devices can be tested simultaneously among (potential) users in the front-end stage.

The value of this living lab approach can also be found in the particular composition of the different methodological elements. The composition includes both qualitative and quantitative methods, enabling a more diversified triangulation of findings. The objective is to enrich the description and the evolution of behaviour, motives, attitudes and knowledge of the persons involved in the living lab, during the research period. Yet these findings can to some extent be transposed to people with similar profiles, inside and outside the living lab.[9]

Third the living lab is uniquely positioned to involve sociality in the real-life testing of network technologies and services. Through this kind of test and experimentation environment we configure a setting where people socially interact with each other in an everyday context. This enables a thorough investigation of the mutual shaping process between sociality and the use of ICT. It is this domestication process which can generate unpredictable uses.

Finally the living lab is suitable for investigating the use of ICT in three spheres: work, home and elsewhere. Because this method often covers a larger geographical space, it is especially helpful for ethnographic research of people being mobile and people in public spaces. This refers for example to the use of mobile technologies in the bus, on the road, at the shopping mall, in a queue, on the bicycle, in public spaces, while doing sports etc. These situations have received less attention in the literature than work- or home-related ICT use.

Living lab by a cluster of companies

The living lab configuration as presented here, becomes even more valuable when it integrates different companies and stakeholders. This is a typical case of an open innovation platform in a pre-competitive setting, where different organisations join forces to test out early ideas and to see for example how sociality can form new and unexpected technologies and services. In the current techno-economic landscape these kinds of test and experimentation platforms can overcome a number of systemic failures in the innovation process (Edquist, 2001).

Test and experimentation platforms like these living labs focus on stimulating interactions, creating institutional support for innovation and accelerating the emergence of new technological systems in order to reduce innovation failures. This implies that they should be open to various business stakeholders, actively building trust, allowing business model experimentation and promoting the formation of clusters. The openness can also engender more creativity, because ideas can flow more freely due to less concern for competition misuse in this protected environment.

Conclusion

Understanding and involving users in the design and development of ICT is essential for industry. Too many technologies and services are still being developed and designed from a technological point

of view. The living lab method presented in this paper offers an alternative approach. However living lab research still lacks clear procedural guidelines, which makes it hard to use for industry practices on consumer and user research. In this paper we presented a framework for conducting a living lab as a test and experimentation platform enabling (early) user involvement in technology and service innovation. More in particular our focus was on ways to establish the living lab as a 'field' where ICT can be tested in real life test environment, based on ethnographic principles.

The paper provided an initial impetus for formalising the living lab approach. Four general phases in the living lab configuration were identified: contextualisation, concretisation, implementation and feedback. The strength of this configuration is the integration of different (qualitative and quantitative) methods. This multi-methodological structure reinforces the triangulation, which is valuable for an ethnographic approach. In addition our communication studies approach merges views from different disciplines dealing with the involvement of users in technology design (CSCW, economics, sociology and anthropology,…). While this living configuration has been empirically tested, more research is required to additionally validate the method.

The advantages of this living lab approach are situated on the technological (system) level and on the social level. On the technological level it helps in developing more social context-specific insights on ICT use in relation to technological design. In the field of design this fits in with the shift from 'user-centred' design to 'people-centred' design. Instead of on task-centric users, the emphasis is on people in their social context as the fundamental source of innovation (Wakeford, 2004). This entails the reconfiguration of the relationship between ethnography and design from a 'product-oriented' model to a 'socio-technical model' of research. In the latter ethnography is employed as a research tool for exploring the social aspects of innovative technologies:

> *'(…) the aim is to explore the sociality of novel design spaces opened up through the deployment of radical technology configurations in real world situations of use.' (Crabtree & Rodden, 2002).*

The outcome from a living lab is not in the first place aimed at user requirement specifications. This kind of socio-technical approach is mainly meant for developing and elaborating sensitizing concepts that draw attention to central characteristics of sociality implicated in ICT usage. The latter may then be further explored through continued design. In this way - underscoring the notion of mutual shaping - ICT becomes a vehicle for social research, the results of which in turn drive design.

On the social level these experiments inform us about possible conditions for stimulating the societal and economic embedding of technology. Embedding technology in real life situations also generates images of potential societal impacts of innovation (Frissen & van Lieshout, 2004). Ideally the living lab is created as an 'experimental field' within a socio-technological scope with specific goals and with a structure, but simultaneously dealing with the uncontrollable dynamics of everyday life.

To conclude, the relevance to people-centred innovation of test environments like living labs, arguably lies in the extent of user participation that is made possible, the establishment of an experimental setting that resembles real-life situations as closely as possible and the enabling and support of non-linear, mutual shaping innovation processes. In this way the construct of different methods that form our living lab configuration is an attempt to bridge the divide between sociological studies and technology design.

NOTES

[1] http://www.sric-bi.com/LoD/meetings/2005-06-08/VPNiitamo.ppt; accessed 25 September 2005

[2] http://www.sric-bi.com/LoD/meetings/2005-06-08/VPNiitamo.ppt; accessed 25 September 2005

[3] In this sense Crabtree (2001: 159-172) discusses the shift in systems design from 'human factors' to 'human actors'.

[4] See Ballon, Pierson & Delaere (2005) for a description of some international living lab cases and other test and experimentation platforms (TEP).

[5] The living lab research in the project 'Vlaanderen Interactive' (Flanders Interactive) looked at the design and use of interactive digital television in a domestic setting (http://www.vlaandereninteractief.be). The initiative was subsidised by the Flemish government. The project ran from October 2003 until October 2004. It included the following partners: Flemish Government, Telenet, Interkabel, VRT, VMMa, VT4. The user research was subcontracted by Telenet and financed by IWT (Institute for the Promotion of Innovation by Science and Technology in Flanders). The partners in the user research were the University of Ghent (UGent), Catholic University of Leuven (KULeuven) and Free University of Brussels (VUB). Due to NDA disclosures not all the details or results can be given at this time.

[6] The e-Paper project (https://epaper.ibbt.be) is carried out by a consortium of the following companies: Philips, De Tijd, Belgacom, Hypervision and I-Merge in co-operation with the following IBBT research groups: SMIT from the Free University of Brussels, CUO and Distrinet from the Catholic University of Leuven, MICT and INTEC from the University of Ghent and IMEC. The project runs from January 2005 until June 2006. In this project SMIT is responsible for setting up the trial as a living lab configuration. Due to NDA disclosures not all the details or results can be given at this time.

[7] The i-City project (http://www.i-city.be) is a wireless experimentation environment in a medium-sized Belgian city (Hasselt). i-City is a participative consortium, which officially started on July 13, 2004, consisting of Microsoft bvba, Telenet Operations NV, Siemens NV and Concentra Media NV with the support of the regional and local governments. At present, SMIT (Free University of Brussels) is advising i-City on selecting and analysing the living lab test users.

[8] The 'screener' is the instrument designed to identify the characteristics that are appropriate for the project, based on sampling by quota (Blomberg, Burrell, & Guest, 2003: 968).

[9] This is indicated as idiographic, holographic, naturalistic or analytic generalization in contrast to statistical generalization (Sandelowski, 1995) (Peters, 1995).

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10

USING PHOTOGRAPHIC DATA TO BUILD A LARGE-SCALE GLOBAL COMPARATIVE VISUAL ETHNOGRAPHY OF DOMESTIC SPACES: CAN A LIMITED DATA SET CAPTURE THE COMPLEXITIES OF 'SOCIALITY'?

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This paper describes an innovative attempt to construct a large-scale, global comparative visual ethnography of domestic spaces, and uses the notion of 'sociality' to interrogate the ability of such a broad but relatively thin data set to do justice to ethnography's potential to capture and communicate the salience of the socially co-constructed contexts of people's lives. Whilst noting the risks inherent in using data sets that exclude information about social context and meanings, it argues that working within the confines of these deficiencies can be turned to positive account to drive both theoretical innovation and analytical rigor.

INTRODUCTION

This paper looks at the implications of a research approach that might appear to be un- or anti-ethnographic in some of its characteristics, and uses the conference's focus on 'sociality' as a way of evaluating the potential of this approach for delivering what we expect from ethnography.

The research approach in question is a database of photographs of twenty households in each of twelve countries. It was developed to be part of a suite of mainly quantitative global consumer trends research products offered by global market research company GfK NOP. It is called the Visual Survey of Domestic Space, or VSDS.

The paper goes into some detail to describe the genesis of the VSDS. The business context for the development, sale and application of this approach frames up its potential vices and virtues. It then considers a couple of ways in which the VSDS has been used in order to look at how the 'limited' nature of the data set can play a role in capturing the complexities of sociality.

SOCIALITY: THE NET WE CAST

As the theme of this conference, 'sociality' is well chosen to strike towards both the heart of our theoretical roots and our best intentions for the effects of our practice. The fact that we are a broad community, drawing on a wide range of theoretical traditions and embracing an equally wide range of situations for practice, presents challenges for how we conduct our conversations in this academic form. Not only do we not necessarily share theoretical points of reference, but for many of us the point of being professionally situated where we are is that we have embraced the fact that we can know

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stuff about the world without necessarily having to find the appropriate place for that knowledge within an academic discourse. As a community of practice, we have a different relationship to theory than within the academy. We always have half an eye on our close theoretical kin, our fellow practicing ethnographers, and half an eye on the light that our theoretical conceits can shed on the preoccupations of others with whom we work who come from quite different intellectual traditions, or from none.

Being not entirely of the academy may free us somewhat from what Marilyn Strathern calls the "doubly snared perplexities" (Strathern 1988: 342) of sociological epistemology, but out in the world our role is often to convince those with whom and for whom we work that parts of the world that matter to them are more complex, and more rewardingly difficult; and that their ends will benefit from applying more of the special kind of attention that we bring. In our work we aim to ensnare the imagination of clients and collaborators, drawing on our theoretical roots for our nets.

Figurative nets, or actual 'networks', are probably the most prevalent metaphor that as social scientists we use in the wider world to give people a sense of what our view of the world is all about – its 'sociality'. So for the past several years I, a dry British social anthropologist rather than a Geerzian, have used oft quoted words from *The Interpretation of Cultures* to communicate to clients where it is that our practice is coming from (and where it is going):

> *Believing, with Max Weber, that man is an animal suspended in webs of significance he himself has spun, I take culture to be those webs, and the analysis of it to be therefore not an experimental science in search of law but an interpretive one in search of meaning.* (Geertz 1973: 5)

I owe my use of this quotation in the context of my work as a practicing ethnographer to my colleague Rick Robinson - in so far as I have stepped into the telling of a story that he started at E-Lab and that has run on through Sapient's Experience Modeling discipline to our current practice of ethnography within the global market research company, GfK NOP. I cite this is an example of one type of relationship to theory that we have as practitioners of ethnography within the business world. We make use of bits and pieces provisionally. We share tit bits with one another. Whereas within the academy we might have a far stronger sense of where our colleagues sit in relation to certain discourses, and engage them on the specifics of those positions, in our broader community of practitioners we more often offer up gifts: "you might find this useful".

Our relationship to theory is often thus more about positioning ourselves with each other, and in the world in which we work, than about advancing discourses. Our shared canon of theory is more likely to be used to show what we know about the world than, as in the academy, to test the robustness of that knowledge. In this vein we may draw on Mauss (Mauss 1990) to emphasize the shared construction of meaning through exchange, or Bourdieu (Bourdieu 1984) for the social work involved in maintaining people's identities and differences, or Miller (Miller 1987, 1994) to point to the transformative agency of consumers. As a young and as yet only partly 'professionalized' profession we have a strong interest in origin stories; tales of where we have come from and where we have been along our way. So works that chart the history and evolution of our practice such as Byrne and Squires' collection of anthropology and design collaboration stories (Byrne and Squires 2002) have a visceral appeal.

Our interest in the situations in which we as a community of practice have operated is apt, because it is in the sociality of the contexts for our work that our theories and our methods have life. And thus this paper is partly about what flows from the decision to use a particular method – predominantly visual data collection – within a particular context for the practice of business ethnography – global 'consumer trends' market research.

BUSINESS CONTEXT OF THE VSDS: ETHNOGRAPHY AND MARKET RESEARCH

The team of business ethnographers with whom I work has recently moved from the application of ethnographic research in technology design consultancy to the world of market research. There are broad differences in these contexts that are relevant to the application of the method described in this paper. In the former situation, as is often the case in design research, the ethnographers were almost invariably the only researchers involved in the process, meaning that the ethnography that they practiced was synonymous with research. In the team's current, market research, context several different research methods co-exist, the main lines being drawn between quantitative and qualitative research, with ethnography playing a not entirely comfortable role as the exotic cousin within the qualitative fold. Ethnography is a more recent entrant to the world of market research than it is to product and service design or work within organizations, and though there are certainly examples of its thoughtful and appropriate incorporation within the repertoire of qualitative market research techniques (Mariampolski 2001), it is often proposed merely as a method for delivering 'consumer closeness' rather than for its analytical strengths.[xviii]

It was partly in recognition of the fact that the structural and analytical center of gravity of the market research world lies in large scale comparative quantitative studies (and partly from the having experienced the frustration of the difficulties of effectively integrating ethnographic and quantitative research when working in a design research context) that the newly arrived ethnographic practice at GfK NOP embraced the opportunity to develop a market research product that would attempt an unprecedented integration of ethnographic and large scale quantitative approaches.

Working closely with researchers operating within quite different paradigms and epistemologies begs questions of who controls the eventual interpretation of the range of research methodologies applied to a given problem; questions that are less pressing when ethnographers are the only researchers around. In market research, as can be the case in ethnographic studies of organizations (in mental health contexts, for example) ethnographers do not have the monopoly as professional describers and theorizers of social life and personhood. (Pulman-Jones 2001) Turned to unintended ends, our methods may undo the best intentions that we have towards capturing 'sociality'. The danger when integrating an ethnographic approach with a large-scale global quantitative research approach is that data produced by the ethnographic methods will get applied within the quantitative paradigm.

Global consumer trends experts at GfK NOP map differences in the 'values profile' of consumers in the global consumer trends subscription product, Roper Reports Worldwide, which surveys a thousand people in each of thirty countries, annually. Value profiles are arrived at through a questionnaire that asks people to rank a standard set of over sixty-five value statements, either as personal priorities or as associations with a range of global brands. Responses are mapped to a classic consulting two-by-two with corners that oppose 'People' and 'Power' on one axis and 'Fun' and 'Tradition' on the other. The data can be cut many ways to demonstrate broad differences by country or by consumer profile, revealing, for example, that consumers of a particular brand tend to prioritize creative and altruistic values. For global marketing and product design strategists this tool provides a

powerful high level scan of their market terrain, identifying specific populations for further investigation. It also provides a commentary on trends in consumer values, the 2005 data showing, for example, that the values profile of consumers in rapidly growing emerging economies such as China is, as might be expected, shifting from an emphasis on Tradition to an emphasis on Striving. (Striving is defined by prioritization of wealth, power and status.)

Powerful as this tool is, from our perspective as ethnographers interested in the sociality of local contexts it might be seen as collapsing sociality into individuals, with the complexities of local sociality rendered as globally referenced attributes of individuals as, for example, in reported incidence of 'traditional values' noted above. As the ethnographic component of Roper Reports Worldwide, the Visual Survey of Domestic Space, was thus conceived as an appropriate complement to the quantitative study, providing a lens from the global comparative level of the quantitative data sets back into the particularity of local contexts.

However, applied outside the guiding intentions of the ethnographers, might the visual data set be a double-edge sword? Rather than revealing the complexities and connections that comprise sociality, might it be put to the service of the effacing of social relatedness that Strathern has lamented as characteristic of late-modern life?

> *The individual… would vanish quite simply from the exercise of its individuality. The repository of choices: what we shall see if we look will be the choices, the experiences that evince 'individualism'. Individual style living! Prescriptive individualism displaces the individuality of the person. We are already there of course. . .*
> (Strathern 1992:149)

"What we shall see if we look will be the choices, the experiences that evince 'individualism'." On the face of it this sentence seems a particularly apt description of a photographic market research study of domestic interiors. Within the context of global comparative qualitative market research, could not a visual data set of domestic spaces that allows the material fabric and contents of a home to be identified be used precisely to define individuals through inventorying their "repository of choices"?

We will now discuss in more detail the context in which the VSDS was conceived and look at some examples of its use to consider the balance of benefits and pitfalls when applying in a market research context, and with ethnographic intentions, a method that can only deliver a limited part of the whole within which ethnography usually aims to ensnare 'sociality'.

GENESIS OF THE VISUAL SURVEY OF DOMESTIC SPACE

The VSDS was developed as a direct response to a specific business problem. The business problem, and the market research agency / client relationship in which it was communicated and framed up, were pivotal in defining this new research approach.

The business problem was identified in the course of ongoing quantitative customer satisfaction research for one of GfK NOP's global clients. An important attribute of this company's products for the home is style. GfK NOP's research had identified an interesting group of people who have a positive attitude to the company, like the experience of shopping in its stores, but nonetheless purchase

very little because, “I couldn’t find anything that matches my style”. On the face of it, here was a straightforward opportunity for the company to increase its sales. A group of customers are already well disposed to it: it merely needs to work out how to provide them with style options that meet their requirements. But what do people mean when they refer to, “my style”? And what is the relationship between these potential customers’ sense of their own ‘style’ and the repertoire of styles presented by the company in its stores?

Style definition, and style preferences had proven difficult to define through quantitative market research. The company had developed a framework for categorizing the styles of its own products, which drew on a small set of seemingly fundamental oppositions such as modern vs. traditional. They had arrived at a set of core style groups, each with several sub-types. They also specified a set of ways in which these style groups could be combined with each other. It wasn’t a simple matter for the company to come up with a framework for defining and categorizing styles that would usefully enable co-ordination of product development and design groups. But defining a set of style categories as a part of company practice and policy was a quite different problem than the problem of understanding how ‘style’ played a role in the experience and understanding of the company’s customers who, whilst they might have no notion of how to define their own style if asked, could still know with certainty, “that’s not for me”.

Discussions about this problem between the company and the account team at GfK NOP coincided with the launch of the Observational & Ethnographic Practice within GfK NOP, prompting the idea that ethnographic research might provide an appropriate approach. This use of ethnography thus has to be understood both in the context of the client’s problem and that of furthering GfK NOP’s relationship with the client through adding value to and integrating with the ongoing pieces of research GfK NOP conducted as part of the relationship with the client.

The range of benefits perceived within GfK NOP for this ethnographic addition to an existing quantitative research product is instructive, and echoes longstanding debates within anthropology about the status of visual data as illustration. As in other areas of professional business services such as technology consulting to which ethnography has been added, ethnographic research can be positioned as a novel differentiator that provides rhetorical evidence of ‘added value’ in the services marketplace. The first and most straightforward way in which this value could be seen as being added was through the ability to *illustrate* the quantitative trends analysis. Where, for example, a consumer trend towards regarding the home as a ‘haven’ had been identified, photographs would be available to illustrate the incidence of homes-as-havens, by country, or by a given consumer profile, lending rhetorical weight to the quantitative findings. By contrast, for the ethnographic team within GfK the appeal of the new approach lay not so much in the ability to provide illustrations to support quantitative research findings, but in the prospect of new analytical possibilities, the challenge of integrating the visual data with the quantitative data, and the potential for subverting conventional assumptions about the nature of ethnographic data sets.

The anthropological debates about the status of visual data on which the role of the VSDS touches are nicely framed up by Glenn Bowman as sitting between a position articulated by Margaret Mead, arguing for the irreducibility of images to text (Mead 1975: 3-12) and Kirsten Harstrup (1992: 8-25) arguing that the image, “is merely the surface level of phenomena which cannot 'speak' for themselves but must be ferreted out through textual contextualization and exegesis”. (Bowman 2001) Against these two poles of the debate, Bowman proposes an alternative possibility for the role of visual

data in anthropology that, as we shall see, characterizes well the best practice application of the VSDS data:

> *the photograph in anthropology is as much a means of discovering information as it is of presenting that which has been found… a locus for dialoguing rather than as a source of information in itself. The value of the image in ethnographic fieldwork is here precisely in its indeterminacy insofar as that allows processes of interpretation… to go on around it.* (Bowman 2001)

The methodology of the VSDS does not allow for discussion of the visual data by informants, but the VSDS photographs serve a similar function for hypothesis generation and analysis amongst the researchers. Bowman's diagnosis of the value of the mute foil of the indeterminacy of the photograph is borne out by our experience with the VSDS data.

EVOLUTION FROM METHODOLOGY TO MARKET RESEARCH PRODUCT

Two over-riding requirements determined the way in which it was decided to provide ethnographic input to address the identified problem of style preference: the fact that style preferences needed to be understood across a global market presence; and the fact that in order to be actionable the ethnographic component needed to be effectively integrated with GfK NOP's existing suite of global quantitative consumer trends research products.

On the one hand this presented itself as a straightforward case for the application of an ethnographic approach: a comparative exploration of the role of style in people's everyday lives – both in terms of their mental models and the material artifacts through which style is expressed. But on the other, there were constraints that meant that an approach based on conventional in-home ethnographic studies would not be viable, being too costly, taking too long to execute, and generating too much data to allow straightforward integration with the existing qualitative data sets.

As large as possible a sub-sample of the 30 countries surveyed by Roper Reports Worldwide was desired for the study. A conventional in-home ethnographic study in a large enough number of homes in ten or more countries would be prohibitively costly. In order to achieve the required scale cost-effectively, data collection would need to be simplified in order to limit fieldwork time and complexity.

The options for simplifying data collection were through using brief, tightly-focused in-home interviews, focusing on how style is understood: or through direct visual documentation of variations in the ways in which people's style manifested itself in their homes. A conventional ethnographic study would, of course, encompass both of these components, but the need to make the study cost-effective and scalable meant that to do both was not an option.

A tightly defined set of photographic data documenting the home was chosen because it offered the potential for close integration with the quantitative data through a relational database, and because it would allow a simple research design that could be executed by researchers with a wide range of skill and experience levels, as might be required if the data set was to be extended to larger sample sizes in a growing set of countries in subsequent years. (In the event we were able to use experienced ethnographic researchers in each of the twelve countries in which we conducted the first round of the study in 2005, through our global ethnographic network partner Social Solutions.)

The problem of 'style preference' had been the catalyst for this new research approach, but in order to match the broad consumer trends scope of the main quantitative study the VSDS needed to be extended to be about 'domestic life' more broadly. The impetus for using photographs as the primary data source had come from one company's business problem: style preference – but the study itself needed to comprehend more than the 'manifestations of style' that had been the initial scope around which the concept was developed. A 'limited' research method (or form of data collection) needed to be optimized to provide input to as wide as possible a range of consumer research issues.

What then needed to be photographed in order to provide useful data on 'domestic life'? At the most basic level it was clear that when designing a study that could be undertaken anywhere in the world, the guide for the researchers could not rely on specifying photographs to be taken in named rooms, given variations in how domestic space is divided up and named. So the visual data needed not to be structured around specific rooms or spaces, but on the areas within the home in which a core set of domestic functions are carried out, or 'functional areas'. Eventually nine 'functional areas' were defined:

- Food preparation
- Food consumption
- Relaxation
- Home office
- Hygiene
- Sleeping
- Service and maintenance
- Where vehicles are kept
- Center

Researchers would ask participants to show them the areas within the house where the activities above take place, and then take a set of photographs of each area. The ninth category allows participants to indicate the area that they considered to be the 'center' of their home. The intention is that participants are not included in the photographs.

Having started out with the aim of creating a consciously 'limited' data set, we have tried as far as possible to stick to that discipline. The participant designated 'centers' have provided a valuable single narrative component to the data set, albeit an oblique one. But otherwise the data set remains resolutely *not* a participant-centered ethnographic narrative in the conventional sense. Rather, the structure implies a permanent set of hypotheses inviting testing: that a set of core functions of domestic life can best capture fundamental variations in the material stuff of domestic life; and that visual evidence of variations in how those core functions are executed and combined in the home will provide useful data about differences and changes in domestic life and consumption.

The challenge of embracing the limited nature of the data set is reinforced by practical considerations. For example, the aim is that fieldwork time in each home is as short as possible, conversations with participants being limited to what is necessary to get the photographs. However, researchers do write up brief notes on the information that they happen to gather about the participants, along with any ethnographic insights. These are captured in free form notes in the database, and inevitably prove extremely useful in deciphering some of the enigmas of the photo sets.

However, the fact that the research visits are definitely not interviews means that the researchers' notes are of widely varying extent and focus. Despite their apparent usefulness in facilitating some interpretations of the photographs, the fact is that there is no prospect of gathering enough narrative interview data to provide a thorough deciphering of all aspects of the visual data. Recognition of this limitation means being thrown back on Glen Bowman's model of the photographs as mute foils for the elicitation of meaning.

WHAT POTENTIAL DOES THIS APPROACH HAVE FOR CAPTURING SOCIALITY?

The risks inherent in this approach stem from the fact that it can assist in the pulling of individuals out of their local context to be lined up for comparison across a global set, for the aggregation of trends in individual attributes. The material culture of homes (the visible stuff) might be reduced to 'possessions' – tokens of the consumers, mutually identifying them as owners of a given product or brand, or falsely attributing intention and choice to the incidence of objects within the home. At it worst the material contents of the home become mere consumer inventory.

On the other hand, the limitations of the data set compared to a conventional ethnographic data set (one that provides a holistic view of a context for experience) bring with them potential benefits. The fact that the data set does not pretend to provide a total account of a social context forces an approach that recognizes the limitations of any one data collection method and requires a hypothesis driven approach. Sometimes it is useful to be freed from the tyranny of the completeness of ethnographic data sets, whose very completeness – the fact that seemingly any question can be answered by the data – can tempt away from theory and analysis towards mere reportage. (Not to mention the fact that the data sets of 'rapid' commercial ethnographies are rarely as 'holistic' as we wish them to be.)

In addition, in the context of market research, it may be a radicalizing move to remove the individual consumer from the picture and force a focus on the material culture that surrounds them and insists upon the fact that they inhabit shared contexts.

How does the balance of these benefits and risks play out in practice?

STYLE PREFERENCE

On the face of it there are a significant components missing from the data set for an appropriate understanding of the role of style in people's lives. The visual data only show what is in the interior of the home. The only data gathered from the participants is in the course of getting them to indicate where each of the nine domestic functions is carried out within the home. The visual data itself provides no direct understanding of why items are there. Are they chosen, inherited, found, tolerated, cherished, or chalked for removal at the earliest opportunity? Are they shared, or doggedly owned and protected by just one member of the household? Are they ironic, or sincere?

But it is the very fact of what we know that is 'missing' from the data that forces us to develop different perspectives on style and new hypotheses for understanding how it manifests itself in people's homes.

Awareness of what we are not able to identify from the visual data led us to focus on fundamental aspects that we *might* be able to identify. Given the difficulty of providing a single consistent set of style categories by which our household data could be organized, such as 'modern', 'country', etc., we focused on the basic characteristics of domestic space that provide the backdrop and context for style.

In the absence of a direct route to characterizing style preference through the VSDS data, we attempted to establish other attributes reflecting care and intention in relation to the domestic environment. Looking at photographs from different homes it was clear that several of us might be able to agree that a particular room was 'messy', but that messiness was a complex category that in many cases became a subject of debate: one person's 'messy' is another person's 'lived-in'. But further analysis did lead us to realize that we could reliably identify some attributes of the way that domestic space is disposed. We discovered that it was possible reliably to categorize spaces in terms of their level of order, and in terms of object density. Spaces with high object density and high order were 'neat'; whilst spaces with high object density and low order were 'messy'. These were the first steps in a process of developing a framework for classifying variations between the disposition of different domestic spaces, and in building up to higher order classifications such as degree of 'style intention'.

Whilst there was no escaping what the VSDS lacked of what would be expected of an ethnographic data set for the understanding of style preference, in this case necessity *was* the mother of invention, spurring us to identify relevant dimensions of care and intention in domestic interiors that we might otherwise have missed.

HOUSEHOLD TYPES

Another example of the paradoxical advantages of the limitations of the visual data set is provided by its application to interrogate the validity and effectiveness of a global household segmentation model built on a combination of household composition and lifestage for understanding the differences between household interiors.

The segmentation model in questions has rhetorical plausibility, given that it is reasonable to expect that the primary way in which household interiors would differ would be according to the composition and lifestages of the households occupying them. However, reliance on the solely visual nature of the data in the VSDS allowed a radical interrogation and reframing of the assumptions on which the segmentation had been built. Being able to talk with members of households in our data set about their lives in their homes might have reinforced the segmentation's assumption that household composition is a primary and defining characteristic; that the home is the way it is because of the activities of household members and the dynamics between them.

What became apparent when the images themselves were interrogated was the that apart from evidence of the presence of children of different ages in the home, the contents, style and disposition of space in homes differed less by household composition and lifestage than by socioeconomic status, country, region and other cultural factors.

In this case the primacy of the visual data enforced by the VSDS led to recognition of fundamental aspects of sociality (material conditions; material culture) that might have received less emphasis in a data set that was dominated by the personal narratives of individual participants. Rather

than focusing on meanings articulated in participant narratives, and on the consciously co-constructed aspects of the home, as would have been likely with conventional 'rapid' commercial ethnographic interviews, analysis had no choice but to focus on material aspects of the home that are often overlooked in studies that privilege the narrative articulation of participants. In this case there was a clear benefit from the enforced separation of visuals from participant narratives in the ethnographic data set.

CONCLUSION

This paper has described the development and application of an ethnographic research approach that runs counter to conventional ethnographic practice in that it uses a highly focused and selective data collection method to construct a data set consisting solely of photographs of domestic interiors.

Such a data set forces recognition of its limitations, and as such can provide a useful means for generating new perspectives and hypotheses on research problems. A forced emphasis on material culture can surface aspects of sociality that are sometimes under-emphasized in conventional ethnographic data sets dominated by participant narratives.

Whilst this paper has noted that visual data sets such as the VSDS could potentially be used to support accounts that remove individuals, as repositories of consumer choices, from their social contexts, in closing it is interesting to observe the extent to which in practice the visual data set is acknowledged to be the sole domain and property of the ethnographic research team.

As soon as the first sets of photographs started to come in from the field the ethnographic team made rapid forays through the data to pull out brief picture sets that could provide a taster of what the database might eventually provide. These were somewhat speculative stories about how domestic functions as defined by the VSDS 'functional areas' overlapped differently within homes in different parts of the world. One of the first quick-and-dirty stories that emerged was of laundry experiences differently inflected through overlapping with student study in a shared student house in the UK, with wine-storage in the backstage provisioning of the pleasures of a bourgeois home in northern Italy, and with purification of drinking water in a kitchen in Mumbai. These visual stories proved to be simple and compelling ways of bringing to life the basic proposition of the VSDS with clients. Yet despite the fact that the stories told around the pictures were simple and easily remembered for re-telling, the quantitative consumer trends experts with whom the ethnographic team worked were reluctant to use the stories in client-meetings when members of the ethnographic team could not be present, insisting that they couldn't tell the story the way the ethnographers could.

Whilst as straightforward illustrations the value of the photographs might appear to non-ethnographers to be self-evident and readily available, in combination for the construction of narratives or propositions they become complex puzzles, emphasizing rather than masking the ethnographer's skill and labor. Armed with an understanding of what can be told via the pictures' underlying 'functional area' structure, for the ethnographer the photographs may thus become "technologies of enchantment" as defined by Alfred Gell – like the ornate prow configurations of Trobriand Island canoes used for Kula expeditions, at once simple and beguilingly complex:

> *I am impressed by works of art in the extent to which I have difficulty … in mentally encompassing their coming-into-being as objects in*

> *the world accessible to me by a technical process which, since it transcends my understanding, I am forced to construe as magical* (Gell 1992: 49)

The final paradox of this 'limited' form of ethnographic data may thus be that, compared to participant narratives that we serve up, or especially to illustrative video clips, those with whom and for whom we work may be less likely inappropriately to co-opt the photographic data set to define people in ways that are insufficiently grounded in the complexities of the sociality of their lives.

NOTES

I would like to thank Nina Wakeford for her comments on earlier drafts of this paper.

[1] Dominated by the psychological paradigm of depth and interiority, conventional qualitative market research does not have easy affinities with social and cultural anthropology's attention to networked complexities of meaning and structure. The author has described a categorical lack of affinity based in the same epistemological opposition, between the psychiatric team and the continuous care team in a therapeutic school unit for children with emotional and behavioral difficulties. (Pulman-Jones 2001: 130)

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CELEBRATING THE CUTTING EDGE

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This paper examines and celebrates the notion of the "cutting edge" as it applies to ethnographic praxis in industry. First of all, EPIC is the first-ever business anthropology conference. Secondly, the conference is just one example of the growth and mainstreaming of the field of design anthropology. Thirdly, the field of design is, after all, all about innovation, and anthropologists who work in this area can provide examples of leading practices to anthropologists working in other domains of application. At the same time, design anthropologists can also learn from more mature varieties of practice. For instance, thoughtful practitioners in other fields have come to regard themselves as "scholar-practitioners," rejecting the dichotomization of scholarship and practice. Adopting such an identity would serve those design anthropologists well who are engaged in branding efforts to highlight the importance of their analytical training and skills.

THREE VANTAGE POINTS ON THE CUTTING EDGE

What does "cutting edge" mean? There is more than one way to answer this question, but to start with, an important part of what we mean by this phrase is the concept of innovation and newness. The papers in this session all displayed innovations, whether in the authors' work practices, the domains to which they applied their expertise, or the theoretical frameworks that they brought to bear on their topics.

In this short introduction, I would like to examine and celebrate the idea of the "cutting edge," because the idea was so relevant to all of the participants of this conference at a number of different levels, and it is no doubt equally so to many readers of the proceedings as well.

The first way in which I would like to consider the "cutting edge" in our field is to note that the Ethnographic Praxis in Industry Conference was the first ever in this area. The conference was about many things, but one important element was the opportunity to build community. On the one hand, the theme of the conference was sociality – and at the same time, conference participants were also enacting sociality in their interactions with each other! By spending several days together, chatting during the long breaks and social events, the practitioners of this rather diverse field had the opportunity to develop a stronger and more cohesive sense of community.

Secondly, the conference is just one example of a broader trend, namely the growth and mainstreaming of the field of design anthropology. (In this section and the following ones, I will frame

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my remarks around the discipline of anthropology, since that is my own background. However, I recognize that our field is an interdisciplinary one, and hope that readers will be able to substitute their own fields as they read what follows.) Indications of the growth of design anthropology include the increase in the membership of the anthrodesign email list, numbering 618 as of early November 2005, and the Danish government's plans to start up a research institute dedicated to "applied business anthropology" with a focus on innovation and design (Kontrapunkt 2005). This research institute is expected to train Master's and Ph.D. students, and will provide a space for projects that are driven by both theoretical concerns and client needs (Kontrapunkt 2005).

We can also look at innovation in ethnographic praxis in industry from a third vantage point. The field of design is, of course, all about innovation. The Industrial Designers Society of America defines industrial design in part as "the professional service of creating and developing concepts" (Industrial Designers Society of America 2005). Looking at this field with an ethnographer's eye, it appears to me that the non-designers working in it tend to adopt a culture of innovation. They valorize change and novelty. In general, I see their focus on innovation as a strength, and I believe that this is an area in which design anthropology can contribute to other varieties of practicing anthropology. Sure, all domains of ethnographic application are continually evolving. But design anthropologists have been trained in an atmosphere that inculcates a disposition to experiment with novel ways of doing things, and that valorizes creativity (Wasson 2000). Their work – for instance, the eight case studies presented in this part of the conference – can provide inspiration for practitioners working in other applied fields.

SCHOLAR-PRACTITIONERS

The field of design-oriented ethnographic praxis in industry is quite young. Its roots go back perhaps twenty years, to the Xerox PARC researchers, and its rapid expansion began roughly ten years ago (although business anthropology in general has a much longer history) (Robinson 1993, Salvador et al. 1999, Wasson 2000). For instance, E-Lab, which played key role in the expansion of the field, was founded in 1994 (Wasson 2000). For this reason, the age demographics in our community are also weighted toward younger people. I think our collective youth, both as a field and as individuals, gives us energy and momentum. But at the same time, there is also a wisdom that comes from experience, and we can learn from applied anthropologists, and fields of applied anthropology, that are more mature.

I recently developed a NAPA *Bulletin* that brought together the life stories of eleven prominent practitioners who are women (Wasson 2006b). The Bulletin, published by the National Association for the Practice of Anthropology, is one of the main publishing venues for applied and practicing anthropology. Each issue is devoted to a particular topic. So this issue was devoted to the life stories of women practitioners; it was a project in autoethnography and an exploration of gender issues in the world of praxis. Although the contributors encompassed a range of ages, many of them were further along in their careers than many of the EPIC conference participants. And they worked in a diverse array of fields, most of which have a longer history than design anthropology does.

One of the clear themes that emerged across these women's stories was their identity as "scholar-practitioners." This theme was initially implicit, but during our group discussions, it was made explicit by Jacqueline Copeland-Carson when she coined this felicitous label (Copeland-Carson 2006). The women who were writing their stories had observed the widespread polarization between academia and practice, theory and application, and they rejected it. While they were practitioners, they rejected the

notion that they were "only" practitioners. They had moved beyond this dichotomy to construct an integrated sense of themselves as both contributing to scholarship in their field, and engaging in cutting-edge practice.

This notion of the scholar-practitioner is one that I think we could usefully adopt in design anthropology as well. Indeed, many of those who attended the conference have already moved beyond the practitioner/academic divide and maintain both aspects of this identity in our careers. I am putting forward this notion more because it responds to issues that some of us face, and because it articulates an important part of who some of us are and what we have to offer our clients.

In October and November 2005, the anthrodesign email list had a lively discussion about what makes "us" distinctive. Common questions included

- What do anthropologists offer the field of design, that is different from what others who are not anthropologists offer?
- What is the relationship between anthropology and ethnography?
- Can non-anthropologists do ethnographic work as well as anthropologists can?

These issues were not new. The anthrodesign discussion was revisiting a set of concerns that have been widely discussed within the field since its emergence (e.g. Squires and Byrne 2002). The issues are important to us because they influence our success in the marketplace. How do we position ourselves to clients? Why should a company hire us rather than a different kind of expert?

A common response that design anthropologists make to such questions involves the role of analysis. Anthropologists often note that the field of design tends to associate the term "ethnography" with data collection only. Yet, as they point out, this form of research also critically involves data analysis. So it appears that design anthropologists are branding themselves as the analysis experts, among other things.

Yet analytical expertise requires a knowledge of scholarship and an ability to apply theory to solve practical problems. Here, then, is where the "scholar-practitioner" identity could be useful. If we want to brand ourselves as not just the data collection experts, but also the analysis experts, then we need to emphasize that we are remaining in dialogue with current scholarship in our field.

Furthermore, even an awareness of new methods is related to scholarship. We may think of ethnographic journal articles as being mainly about theory, but actually they can be about data collection and analysis techniques as well. For instance, I have a paper in the *Journal of Linguistic Anthropology* where I talk about how I transcribed and analyzed videotapes of virtual meetings (Wasson 2006a). I developed a whole new approach involving a series of software programs and a new set of transcription conventions. One of my reasons for publishing this information was to help other people who might be facing similar challenges.

In addition, Nina Wakeford's insights about the importance of exploring our positionality are based on theoretical frameworks such as feminist and post-colonial studies.

My comments about scholar-practitioners should not be taken to mean that I wish to draw lines between those of us trained in anthropology, and those of us who come to the field with other backgrounds. Our community benefits greatly from the interdisciplinary nature of its members. Some members may find it useful to think of themselves as scholar-practitioners, other may frame their

identity differently. Indeed, I am not arguing that all design anthropologists should think of themselves as scholar-practitioners. Rather, I think that many of us already have an implicit, tacit sense of ourselves as occupying this space, and that making the implicit explicit can be useful, because it is so in tune with the branding efforts that seem to be underway in our field.

Finally, it is worth pointing out that this conference was itself a fine example of how scholarship and practice can be integrated. EPIC was about many things; it helped to build community among us in the sense of sociality; but hopefully it also contributed to the development of an intellectual community where we can learn from each other and push the cutting edge one step forward.

ALWAYS IN MOTION

To return to the question of what the "cutting edge" means… the notion also implies a sense of time passing. "Cutting edge" practices are the latest, the most current, the leading practices. Since time keeps moving on, that means that the cutting edge itself is constantly in motion; today's cutting edge is tomorrow's old hat. Of course, the old hat may remain a highly valued headpiece, still used regularly. For instance, one of the first contributions of anthropologists looking at design issues was their recognition of the role of sociality. That early insight is still foundational to much of the work we do today; that is why it was chosen as the conference theme.

What insights are bubbling up in our community now, that we may come to regard as foundational ten years later? The eight case studies presented in this section provide snapshots of some of the areas of innovation that are currently emerging. We selected these papers to illustrate innovation in diverse aspects of ethnographic praxis in industry, including work practices, domains of application, types of product, and theories.

The papers are grouped into two parts, those that focus more on cutting edge "how to's", and those that focus more on cutting edge thinking. In the first group, Dan Bruner describes research on a cutting edge type of product, "rugged clothing," which occupies an interesting intersection between apparel and work tools. Ame Elliot describe some very creative work practices with regard to her group's use of physical artifacts to assist in design efforts that involve collaboration between speakers of two different languages. Wendy March and Constance Fleuriot provide an entertaining example of a novel data collection technique: they asked teenage girls to describe the worst technology they could imagine. Finally, Alexandra Mack and Dina Mehta discuss the work practices they developed in order to conduct collaborative research across geographic distance, in particular their use of blogs and Skype.

The second set of papers starts with a contribution from Ari Shapiro, describing his insights into the cutting edge domain of health and medicine. Next, Elizabeth Churchill and Jack Whalen describe the new work practices that they needed to develop in order to manage a complex project involving two nation cultures, two organizational cultures, and several functional cultures. Scott Mainwaring and Allison Woodruff examine another cutting edge domain of application, namely technology in the home, specifically "great rooms." And lastly, Keri Brondo, Marietta Baba, Sengun Yeniyurt, and Janell Townsend present an innovative theoretical finding that contradicts previous assumptions in organization studies: they discover a plant where what appears to be the workers' loyalty to their employer is really the workers' loyalty to their local communities and rural way of life.

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12

ACCELERATING COLLABORATION WITH SOCIAL TOOLS

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As more and more corporate ethnographic work is crossing international borders, we are increasingly collaborating with teams that are spread across the globe. As a result, we need tools that enable us to work across boundaries. Since early 2004, the authors have been collaborating on a research project developed by an American company seeking to develop solutions specific to the Indian market. One of us, an Indian sociologist, led a team of ethnographers in India, while the other, an American anthropologist, managed research and analysis for concept development in the US. While all of the US-based team members spent time in the field in India during the project, integrating the teams into the same "brainspace" was a challenge. This paper describes how we used social tools to enable each set of team members to understand the work being done on the other side of the world.

INTRODUCTION

An emerging issue for Ethnographic Praxis in Industry is the fact that corporations are become more and more international. Working with, and developing products for, other countries means we are increasingly working across international boundaries, and creating teams in different locations and time zones. When the basis of the work is ethnographic, and collaboration is the key to innovation, finding ways to communicate with and create a coherent team is crucial. This paper describes our experiences using social tools to communicate and collaborate during a long-term ethnographic research project in India sponsored by an American corporation.

Kerr (2004) has noted that distance collaboration stimulates both innovation and productivity. Recent work on distance collaboration has focused on education, or non co-located teams within a single company (Mark, et. al. 2003; Nooteboom and Gilsing 2004; Blomquist, et. al 2005; Nurmi and Marttiin 2003), but there has not been much attention paid to industry related ethnographic work. Likewise, while blogs and other social tools are becoming more common in the workplace (Porcaro 2004, Cass, et. al. 2005, Gahran 2004), they are only recently becoming a core of data collection for ethnographic projects, as seen in the research of March and Fleuriot in this volume.

While blogs have been around for about 10 years, they have taken off in popularity in the last few years. Walker (2003) defines them as "a frequently updated website consisting of dated entries arranged in reverse chronological order so the most recent post appears first. Typically, weblogs are

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published by individuals and their style is personal and informal. …Once at a weblog, readers can read on in various orders: chronologically, thematically, by following links between entries or by searching for keywords."

With their basic programming interface, Wikis have been used as social tools well before blogs. Wikipedia, currently the best known example defines them as "web applications that allows users to add content, as on an Internet forum, but also allows anyone to edit the content." Other social tools include presence and communication tools such as instant messaging (IM) and Voice over Internet Protocol (VoIP). Features built into all of these tools that work for sociability in collaboration are that they are light, they are used in real-time, they allow publishing and co-creation of documents, and they can be archived.

While Martin Wattenberg suggests that blogs and wikis play opposite roles because one is individual focused and one is group focused (Delio 2005), our experience on this project shows that the different social tools in fact play complementary roles.

PROJECT BACKGROUND

The project was based in the Advanced Concepts and Technology (AC&T) group of Pitney Bowes, the Research and Development arm of a global mail and document services company. The company was interested in expanding into emerging markets, and in particular, creating products that would specifically meet the needs of, and provide value to those markets. In order to do this, AC&T deployed a Concept Studio team to begin an engagement in India. The Concept Studio is a group of anthropologists, engineers, designers and MBA's who work on customer centered innovation teams. Typical engagements begin with a business unit sponsor, who sets a broad strategic question for a project. Until this engagement began, all of our projects had been United States based. This engagement was not only situated halfway around the world, but had a very broad strategic question, basically asking how Pitney Bowes could create value in postal and financial institutions in rural and urban India.

The Concept Studio method of working is an iterative process based in extensive customer observation, brainstorming, and concept prototyping. We wanted to maintain this process for a project in India as well. However, even though one team member had previous field experience in India, and it was intended that all team members would spend some time in India, we knew from the beginning that we wanted the majority of the fieldwork to be conducted by researchers in India — people on the ground who were closer to the culture and able to understand and gain insight that Americans would not. At the same time, it was crucial that Pitney Bowes team members all spent time in India, both to see and understand the country, and because outsiders can see things and gain insight that insiders do not.

In order to conduct the research, Pitney Bowes hired Explore Research and Consultancy, based in Mumbai. Explore is a firm specializing in qualitative and ethnographic research and consultancy across product categories and regions in India.

At the beginning of the project, we met in person, and did field work together, to ensure the consultants understood how Concept Studio worked and what the expectations were from the research. We also established protocols for sending weekly written reports and pictures, and had a

weekly hour long conference call. In between, there were occasional emails and an intermittent phone call.

CHANGES IN COMMUNICATION MEDIATED BY SOCIAL TOOLS

We established the project blog after the project had been running for a year (Figure 1). The delay was primarily due to security concerns. The blog was password protected and each of the team members had access to a site. It did not replace other forms of communication, but enhanced them. We still used e-mails but not for much, and FTP for the numerous pictures. While the blog did not replace these media, it did displace them. Because we could use the blog for quick information about site visits and questions, we were able to use our weekly phone calls more productively to discuss site visit learnings in more detail, ideas, and future directions, rather than asking clarifying questions. The blog enabled our weekly discussions to be about higher level research issues such as what are we learning, what do we need to know, what are we doing with what we are learning, and planning.

The more frequent blog communication enabled us to reach the same wavelength faster. Current thinking could be posted, so we had a way to keep track of ideas and thoughts. The researchers in India answered questions quickly and it was easy to ask for more clarification. All this got recorded as comments at a specific blogpost, allowing for easier archiving and searchability. One of the most useful aspects of the blog was for the researchers to post quick highlights of site visits immediately, so the United States team knew what they saw in the field and could guide further work, well before full reports were written.

Figure1. Screen shot of project blog.

New entries could be posted and edited by the author, and anyone could add comments on entries easily at the blog site. We found that the blog quickly became a method which allowed us to communicate with more immediacy and informality, as it was easy to post and comment. While email also can provide immediacy and informality, we posted items to the blog that we wouldn't have emailed. The blog is a "place" and conceptually different than email. This not only provided a location to store all the information together, it enabled conversation in a different way than email. Unlike email, the blog holds different strands of conversation in one, easily viewable place. We were also more likely to use it to post small comments and immediate thought that we wanted to share and keep, but that we might not have emailed because they weren't necessarily worthy of the interruption created by email.

Since communication between team members became less formal and more spontaneous and intuitive, it was also at times more chaotic and hence, creative. It is not like writing a report or spending time thinking through points to discuss at a conference call, conscious that every minute costs a lot—with these tools, we could move our expressions from "what we did" to a much more intuitive and spontaneous "what we feel" and "what we think."

Moreover, it allowed us to experience the wonder in creativity arising from the chaos that only a decentralized, self-organizing system can embody. Taxonomies were emergent rather than pre-structured or closed; points and counterpoints leading to synthesis, seeding ideas and seeing them take shape.

Our categories emerged as a result of the research and our interactions through which our archiving was created. This archive was used as a training ground for new team members, and allowed part-time participants to get up to speed in their own way. The categorization was embedded in the navigation, and included ideas, notes, project phase, sites, and status.

In addition to the blog, we used Skype, a VoIP tool, for immediate real time communications, and a wiki on which this paper has been written. Using Skype worked well, though it can also have time lag and duplex problems. Skype also allows chat, and since Dina spent a great deal of time online, we were often able to IM, despite the large time difference. We knew whether she was online or not through the presence indicators on the main window. Sometimes she would get a brief note: "Dina, u available for a 2 minute call?"

The beauty really was in the spontaneous chats and calls we often had in-between the conference calls. It's too expensive to just pick up the phone and call someone in India for a small clarification, and usually not warranted. Also, due to the time differences between India and the United States, there is hesitation in just picking up the phone and calling someone at say 11 pm at night. With Skype, we didn't worry about intruding into each others' spaces at odd hours, and we didn't need to wait to get clarifications.

As we have developed this paper, we have discussed other tools out there that could be used for projects such as ours. The two most immediate are tagging and photosharing. Tagging allows users to create and share their own taxonomies (Golder and Huberman 2005), and in a team project can allow team members to place structure and meaning on a mass of ethnographic data. Adding tags to pictures shared in the blog or on a linked photosharing space can remove the need to download, sort, categorize and manually archive them.

WEAKNESSES OF SOCIAL TOOLS FOR COLLABORATION

As with any collaboration tool, the blog does have its strengths and weaknesses. A blog is typically used as a means for the blog owner to post his or her own daily thoughts in a forum that encourages others to comment. The blog we used had some associated drawbacks such as lack of threading and not being readily print-friendly, and it was not initially set up well for archiving. We hired Stuart Henshall to redesign the blog and add some functionality that made it more usable such as a "recent posts" section, and an enhanced printing capability.

Unfortunately a blog alone, or any social tool alone-for that matter any of the commercial knowledge management or collaboration tools, does not do everything. Blogs work well as exchange spaces, while IM creates a conversation space. Wikis allow users to edit and negotiate content. Most of the existing knowledge management tools are best suited for archiving, which social tools do not do as well. Our ideal tool would be one where we bring all these features together into the same workspace, so we could go to one place to exchange, converse, build, brainstorm, collaborate, and archive.

IMPLICATIONS: MOBILITY, BOUNDARIES, AND "BEING THERE"

As we mentioned at the beginning, we considered it fundamental that all United States based team members spend some time in India. The use of social tools does not change how important that is, as best expressed by Darryl Rathbun, one of the Pitney Bowes team members. Although Darryl was on the project from the beginning, he did not visit India for a year. He reflected, "Before I went to India, I had a good mental picture of what India was like. Conversations with the people were not different--they went the way I expected them to go. The things that I found different were not related to the research itself, it was more the day to day life. It was what's it like to be on a road in India, what's it like to have so many people around you...I never had a mental picture of a truck driving alongside an overstuffed wagon pulled by oxen," (personal communication, September 6, 2005). While these things aren't directly related to the research, they are important for gaining a full sense of the culture and environment.

While nothing can replace the experience of being there physically, these tools can engage some senses that bring the distant space closer. Dina spent some time doing research in a village without electricity and telephones, but there was connectivity with cell phones. Through her mobile, she could upload pictures to the blog, and speak to people anywhere in the world on Skype. On our next round of fieldwork, we plan to take this further with webcams or video integrated with VoIP, allowing team members in the United States to see India in the moment, allowing all team members to be present in the field, guiding, probing, absorbing, and learning.

As we work further and further apart, presence and mobility become more important. Social tools enable this as well as speed and efficiency. The tools we used changed the nature of our collaboration. There was more spontaneity and immediacy in our interactions leading to better information and communication flows. The tools also allowed a more natural, real-life and emergent adaptation to chaotic conditions, which in turn allowed flexibility and facilitated a process of creativity that we might have lost otherwise.

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13

SOCIAL RELATIONSHIPS IN THE MODERN TRIBE: PRODUCT SELECTION AS SYMBOLIC MARKERS

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A manufacturer of work clothes wanted to learn how workers use and experience its products to enhance marketing and sales. After a multi-sited field study, I learned that more critical than individuals or persona were the social practices that emerged within particular social units. Different work crews established their own distinctive patterns of clothing use which served as symbolic markers of group identity. Workers adduced functional attributes to explain sociality-based choices.

The client—a manufacturer of rugged clothing for ranchers, construction crews, and work groups—wanted to better understand its customers and to know how individuals use and experience its products with the aim of utilizing the results in marketing and product development. Over four seasons in different regions, I employed the model of multi-sited (Marcus 1998) ethnographic research among persons in different occupations. The assignment was not distinctive, but the findings were unexpected. I learned that more critical than individuals or persona were the social practices established within particular social units as well as the emergent network of social relationships. Product selection was less dependent on individual preference or the intrinsic qualities of the clothing and more on the practices that had become established as symbolic markers of particular social units.

It is not surprising that occupational differences affect clothing selection, but our results went beyond that. Two construction crews or companies doing similar work in the same geographical region had different preferences, bought different products and had their own rationale for their choices. It was not a matter of Gemeinschaft as the group was not cohesive on other issues, and significantly, clothing choice was not entirely standardized as some persons insisted on their own individualized dress despite being hazed.

In our approach, we conducted more than 140 field interviews, observed the workers on the construction sites and ranches, did informal interviews, and were invited into homes and bars after work to continue the research and to socialize. Four sets of visits were conducted at each site over a nine-month period by our team of user experience consultants, returning seasonally to the same geographic locations. We visited job sites around the country in warm, cold and harsh environments. Our returning to the same field sites helped to build rapport, as our welcome increased with each repeat visit, providing a deeper understanding. As with all ethnography, we observed what people did, how they dressed and how they used the work wear products within their social environment.

The length and intensity of the study meant there was a vast amount of data collected, data that could easily create "analysis paralysis." Handling such data efficiently and intelligently was crucial to the success of the project. There were more than one thousand "nuggets" of data from the first set of

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visits alone. By nuggets I mean significant quotes, triggers and barriers to adoption and purchase, design ideas, and unanticipated responses. An inductive process was used to create an affinity: a natural bubbling up of the significant ideas and findings. Many images and video were reviewed for triangulation of the data. By looking beyond the spoken responses of participants, critical factors were revealed. Not just spoken words but gestures, emotional tone and other embodied practices were recorded. After each season's visits, the process was repeated.

I went into the study with some common-sense expectations regarding product choice. For example, choice might have been primarily dependent on the requirements of the job, and this was indeed a factor, or possibly the foreman or manager would decide what clothing would be used, or each person made an individual choice. We found, however, that we couldn't know how products were selected without understanding group patterns of social relationships. To overstate for emphasis, sociality trumped function, hierarchy or individuality.

For the work crews, their work wear served as markers of group identity, a fact we had not known before we started field observations. We learned that product selection set each group apart much like British social anthropologists' observations that African tribes in different communities distinguished themselves by minor variations in rituals. On the job site, one worker would start a trend with distinctive dress which others would follow. The groups were not homogeneous, however, as some individuals within a particular work crew refused to conform, thereby making their own individual statements and defining themselves differently.

Let me begin with a simple example. We would study two different work crews and observe that the members of each group wore different brands of jeans, for example, Levi's or Wranglers. Whatever brand was selected, almost all of the workers within that group wore the same brand, say Levi's, while workers in the other group wore Wranglers. When we inquired about why they wore a particular brand, the answers were essentially the same for both groups. They claimed their jeans fit better, weren't tight in certain areas, cost less, and wore better and longer. One worker claimed that in his brand "the crotch won't blow out." Another claim was that "the zippers last longer." Each work crew justified their selected jeans as better, and we began to realize that their work clothing was serving as a mark of identity for the social unit. Crew A wore Levi's while Crew B wore Wranglers.

During a visit to a large construction site, we began observing and interviewing a crew of rod busters. The team of eight was performing identical work in an identical environment: tying long rows of layered rebar with wire before the top layer of a bridge could be poured with cement. There appeared to be a "typical" uniform: boots, jeans, one or two t-shirt layers for protection, work gloves, a tool belt, and a hard hat. The tool belt had one unusual device for spooling out the wire for tying the rebar. Dykes, pliers and other hand tools also populated their tool belts. Their task was to pull some of the wire, place it at the intersections of the rebar, and wrap and tie the wire into place. The job required the workers to be bent at a 90-degree angle at the waist. We observed one experienced crew member take off his gloves, pinch them together at the cuff and adeptly hang them on the handle of the pliers protruding from his tool belt. He had cut holes in the wrist of each glove to create this convenience. He reported that it was a technique he had learned from an "old timer" about five years earlier. He even showed us prepunched gloves in his truck. The rod buster had told/shown some of the other crew members, and six of the eight were now using this method. This worker was what I call an "influencer."

A critical question emerges as to who are the influencers. In this case, it was an experienced worker but not the most senior member of the work crew. And most important, in terms of hierarchy, he was not the foreman or manager. He was, however, one of the more social members. For example, when one of the other workman's boot sole was peeling away and getting caught on the rebar, he was the first to make an amusing comment about it, and it became a topic of conversation within the group. Whereas some of the members of this work crew were more reticent, initiating fewer interactions and working more in isolation, this member was much more outgoing and was friendly with most of the other crew members.

We found this method of storing gloves used only within this particular crew, and the way they talked about it suggested that it was "their way," and they were proud of it. It was functional, but there were other optional ways of storing gloves—to put them in pockets, tuck them into the tool belt—but this method was distinctive of this crew, and they knew it.

Another work group wore red hooded sweatshirts whereas other crews commonly wore gray or blue. This was a distinctive marker for this group, but I was not there when the first person initiated this practice nor was I able to determine who the influencers were and how the practice spread through the group despite putting forth some effort into post-analyzing. This may be a limitation of this study or of the methodology employed. Because I did visits over time periods, I did not see how the adoption of red hooded sweatshirts emerged. But I know this particular group did it and other groups didn't. I also was not aware of the importance of sociality prior to initiating this project and initially didn't gather data on sociality. Nevertheless, the finding is unique and seeking the origin would be interesting.

Examining how recruits were socialized into a group provided additional insights. On a dangerously slick metal roof of a new strip mall in winter, four carpenters shot nail guns and used Sawzalls while snow blew by at 20 miles per hour. Two of the three carpenters were appropriately dressed with four layers (all zipped and well tucked in), tool belts full, hoods pulled on tight. The newest recruit was on his first day with this crew. The other workers ridiculed his dress when they spoke to us, noting that he had shown up for work without even a proper coat. They observed that his jacket was too short, his pants too tight, and his t-shirt was not long enough to remain tucked in. "You can always spot a rookie." The foreman even chimed in periodically shaking his head at the rookie's clothing choices. After borrowing a heavier jacket, he got to work. The rookie's response to us halfway through the day, as he was clearly cold and exasperated, was that he needed to go out and buy some proper clothes. His model would be the two others on that specific crew.

It became apparent midway through the study that workers were talking about their clothes as "tools" on the job, not the way you and I would. They talked about clothing the same way they talked about a power drill or saw. Buying a high-quality tool was a symbol of knowing what you are doing, and being skilled enough to know the difference. Having the right tools was critical to being a good construction worker. Many tools were passed down from a craftsman to his son. There was a continuity of family identity. Workers felt their craftsmanship and their occupational identity, and even their legitimacy as workers, were revealed through the quality of tools they used. Ethnographers know that clothing is more than something to wear, but clothing, as well as the tools used on the job, took on a symbolic overload. Proper clothing was a tool to get the job done in an efficient and safe manner, but it was also a mark of identity, showing others that they were experienced and competent workers and members of the crew.

Patterns of clothing use constitute a visual language that only those in the know are able to understand. When I began the study, I did not appreciate the subtleties of minor variations in clothing choice nor group influence on clothing selection. Work wear proved to be a social marker of belonging to a particular construction occupation as well as a particular crew. Ethnographers who have examined tool use have looked beyond functionality to the decorations, embellishments and distinctive in-group practices. I think it's the same with clothes that become symbolic markers.

The most uncommon observation during all of our visits was that of a woman on a cement crew. We observed this team pouring and planing sidewalks and curbs and subsequently interviewed the woman. She wore dark blue bib overalls and had multi-sized trowels pulling on the overalls' large back pockets. Like all the others on the crew. she carried a five-gallon bucket that held the rest of her tools. She professed a high level of confidence telling us of her decision to make more money by entering "a man's world." It was noted that her breast pocket had been cut out entirely, leaving a darker blue unfaded patch underneath. Her response was surprising. The overalls had belonged to her teen daughter. The breast pocket originally had large rhinestones that spelled out "QUEEN" across it. She had cut this off knowing that it would cause too great a disruption to the group. In other words, she would have received an onslaught of ribbing about this. It became apparent that she put on a tough appearance both physically and emotionally to overcome her femininity. She talked about her ambiguous status, as a woman in a man's world who desired to create her own personal style yet who also recognized the need to conform to established social norms.

Although clothing was clearly important to the workers, paradoxically, the task of purchasing clothing was delegated to a wife, girlfriend or mother. They were the ones doing the shopping. The women purchasers knew what their men wanted because the men had established the pattern—the type, style and brand to buy—and the women bought the clothing as replacements. If an item was not appropriate, it was returned. We learned this by being there and observing the process as it happened, utilizing the methods of ethnography. Many workmen never told anyone when to buy some new piece of clothing. Their clothes would get to the point that they had huge holes in them, were significantly stained, or smelled so bad that they were simply thrown out and replaced. The women knew what issues the men confronted, for example, if the men had put on weight, the wives knew this and would buy either the next size or two sizes larger.

Learning who bought the clothing, taken in isolation, say by survey methods, might lead to the erroneous conclusion that clothing choice was not important, as purchasing was delegated to others. The client might then advertise to women, thinking that they made the decisions, which would have been a reasonable but inaccurate conclusion. The importance of a workingman's clothing and the fact that it was regarded as a tool of the job were only revealed to us through the course of our field study. The finding could have been easily missed through less intensive research methods.

Another notable aspect of the research was the unusually high degree of access I was allowed. Not only were the participants sharing personal insights, they opened their private spaces to me for further inspection. Prior to the visits, contact had been made to begin the rapport-building process. Between visits, communication continued. My aim was to gain the trust of the workers; returning several months later enhanced our rapport. In one instance, as I interviewed an older cattle farmer, he told me stories about his son. It was apparent that I reminded him of his son based on how I acted and on my interests. Our discussions often strayed to family and deeper into raising cattle than I really needed to go. He was teaching me the business. Ethnographers in the field often take on the role of apprentices. Later in the day, he offered to take me to a baseball game in Kansas City. At the end of

the day, he pulled me aside and invited me to go drink some "Jack" and smoke cigars with him. I found myself entering the insider's world and engaging in discussions and activities that were more personal than the research seemed to call for but which proved to be invaluable.

There are other instances when I as a researcher became more like family than an outsider. After initial visits, I recognized birthdays and major family events, and this led to deeper relationships that allowed me access to non-traditional areas (vehicle glove boxes and trunks, spontaneous visits to homes including areas such as master bedroom closets to see wardrobe items). This would not have been possible without the repeat visits and keeping in touch for the entire nine months. Building rapport and relationships is a fundamental principle of ethnographic research, and our research was certainly enhanced by these personal relationships that went beyond more formal methods.

The implications of our research for the client were profound and suggested new lines of ethnographic inquiry. In moving from individuals to persona to sociality, the key would be to determine how these distinctive markers of affiliation and identity became established. Some members of the group, not necessarily the master craftsmen or those at the top of the hierarchy, seemed to set the tone for the others. These are our influencers. If the company could identify them and market to them, it would have vast consequences. Because we recognized the importance of sociality toward the end of the study, we were not able to probe as deeply as we would have liked, and we were left at the end with unanswered questions. We would have wanted to know more about the influencers and about the socialization processes within the group.

But it is true of all ethnographic inquiry that you always have unexpected findings and unanswered questions that suggest new lines of research. No ethnography is ever complete. The "unanswered" become the seeds of the next inquiry. A major conclusion to draw from this study is that the next one should take account of sociality from the beginning and build it into the research design.

Ethnography these days has long departed from its roots in Malinowski and Durkheim and has moved to a more postmodern perspective. Our findings, however, seem somewhere in between, a combination of social pressure to conform to tribal practice and a permitted expression of creativity, heterogeneity and individuality. But the answers, we feel, and the way to best serve the client, are to be found in the arena of sociality, within the workings of the collective.

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PHYSICAL ARTIFACTS FOR PROMOTING BILINGUAL COLLABORATIVE DESIGN

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Physical artifacts, such as sticky notes and mock-ups, are widely used in Human-Computer Interaction research for supporting the collaborative design of technology. Because these representations use channels of communication other than speaking and listening, they offer the potential to facilitate collaboration in bilingual groups working through an interpreter. This paper identifies challenges of bilingual design meetings based on technology development collaborations between Silicon Valley corporate research organizations and two different Japanese companies. Three of the most successful physical artifacts used in these meetings are described to illustrate ways of supporting bilingual collaboration. After discussion of the specific contributions of these artifacts, general recommendations for bilingual collaborative design meetings are discussed. The paper concludes with the recommendation that careful choreography of the work area is necessary to ensure every participant has access to the physical artifacts necessary for successful collaboration.

INTRODUCTION

Based on experience collaborating on various technology design projects with two different organizations that required bilingual meetings in English and Japanese, this paper identifies several obstacles to successful collaborative design meetings. The different projects exposed many challenges, but this paper focuses on how physical artifacts can enhance collaboration. The problems described, the examples of artifacts for addressing the problems, and the recommendations for supporting collaboration are all specifically concerned with bilingual design meetings, a narrow subset of all the meetings necessary for developing complex technology.

The recommendations in this paper are grounded in experience with meetings at Ricoh Innovations, the Silicon Valley research lab of the Japanese office automation company, and at PARC, in collaboration with an undisclosed Japanese company. In both cases the meetings were not only bilingual, but also cross-cultural. A cross-cultural meeting may be held in only one language, but opportunities for misunderstanding still exist based on different expectations about participants' non-verbal cues, such as seating location, gaze, and self-presentation style. Meetings with participants from Silicon Valley and Japan produce numerous opportunities for misunderstanding because each culture has different expectations about in-meeting behaviors, such as the appropriateness of periods of

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silence, authority to make binding decisions in real time, and the relationship between individuals' roles in the organizational hierarchy and the validity of their contributions (Brannen 2003:97-100). These expectations are particularly salient during collaborative design meetings, where one metric of a successful meeting is contribution from all the participants, a metric that might not be appropriate for evaluating the success of other kinds of meetings during the project lifecycle.

The participants expected collaborative design meetings to differ from other kinds of meetings in several key ways. First, all of the content of a design meeting cannot be prepared in advance because the participants work together to produce the output during the meeting. Second, the end-users of the technology, rather than participants' individual organizations are the client for a design meeting. Finally, design meeting are exploratory and the outcome is subject to revision. Because the organizations involved use iterative design methods, the collaborative design meetings were intended to provide an informal venue for examining alternative designs, with the understanding that modifications would happen later.

PROBLEMS USING INTERLEAVED TRANSLATION FOR DESIGN

Even though the participants agreed on the goals, many design meetings were still problematic, in part because of the communication problems arising from bilingual interleaved translation. When using interleaved translation, each participant's comments are paraphrased every few sentences by a single interpreter translating to and from English or Japanese. Interleaved translation is only one format for bilingual meetings; another example is simultaneous translation, where an interpreter talks over a speaker with a lag of a few seconds, repeating their words in a different language. All translation formats have advantages and disadvantages, but interleaved is generally a better fit for meetings that encourage informal discussion with impromptu comments in both languages. The possibility for opportunistic contributions was important, and during our collaborative design meetings, participation was roughly divided between English and Japanese speakers, and either could begin speaking at any break in the conversation. However, interleaved translation also placed several limitations on design meetings.

Loss of momentum – The biggest limitation of interleaved translation is that only one person's comments are translated at a time and the interpreter's attention is a scarce resource. Even though the Silicon Valley researchers originally came from many cultures and some were non-native speakers of English, all of them were accustomed to interrupting others to interject their thoughts into fast-paced discussions. As a group they had less experience participating in translated meetings and seemed to find the forced turn taking of interleaved translation more frustrating than the Japanese participants. An additional problem with repeating every idea in the second language was that some of the participants spoke both English and Japanese, but had to wait while a comment they understood in the original language was repeated a second time before they could respond. Maintaining enthusiasm was difficult when the meetings using interleaved translation took not twice, but three times longer than similar meetings held only in English.

Self-Censorship – One reason the loss of momentum frustrated some Silicon Valley participants was the perceived loss of spontaneity, a trait particularly valued in meetings during the early phase of the design process. Some of the collaborative design meetings were brainstorming sessions, and the loss of spontaneity was damaging because evaluation of an idea's worthiness should not take place during the brainstorming session. Unfortunately, brainstorming with interleaved translation produced

long pauses while people waited to speak, and these pauses encouraged self-censorship while participants decided if their idea was worth being repeated in another language.

ARTIFACTS FOR SUPPORTING BILINGUAL COLLABORATION

Physical artifacts, such as sticky notes and mock-ups, are well understood to promote collaboration for the design of new technologies because they help create a shared understanding between participants (Arias et al. 2003:349). Because they introduce additional channels of communication, artifacts are particularly useful for avoiding problems arising from interleaved translation. The primary advantages of the artifacts are allowing non-verbal ways to participate and encouraging candid exchange of ideas by relaxing participants. Making artifacts during collaborative design meetings helps avoid loss of momentum because participants have something to work on while they're waiting for translation. Visible artifacts also help with self-censorship because they allow other participants to ask questions about something they've seen someone work on, but not yet heard discussed.

Persona posters, hats and bags, and everyday objects are three different kinds of physical artifacts for supporting bilingual collaboration. These artifacts were particularly helpful during bilingual meetings, but the artifacts and their surrounding practices were derived from generally effective HCI brainstorming techniques (Buchenau and Fulton Suri 2000:424-433 and Oulasvirta et al, 2003:125-134). A brief description of how PARC used each kind of artifact during design meetings with a Japanese company follows.

Persona Posters **–** Cooper describes personas as amalgams of end-users that provide sufficiently detailed descriptions so that all the members of a project can visualize how the persona will respond to a design (Cooper 1999:124). For our collaborative design meetings, we divided the participants into teams of five, mixing English and Japanese speakers with an interpreter. Participants were told to imagine they were making a movie of their technology vision and that they should use images cut from stacks of Japanese magazines to share their vision. They used the magazines to communicate what their end-user looks like in terms of appearance, personal style, likes and dislikes, and daily activities. Each team made a single poster to explain their vision of an individual end-user.

The persona posters helped bilingual collaboration in several ways. By using Japanese language materials we reinforced that our end-users were Japanese and that during the process of designing for them speaking Japanese is normal and necessary. Even if participants needed to wait for the interpreter to describe why someone else had clipped a particular image, they could still negotiate with each other using their magazine clippings almost as trading cards without sharing a common spoken language.

Figure 1 Cutting photos from magazines to create persona posters.

Hats and Bags: A Day in the Life – Although both the end-users of our technology and some of the participants in the design meetings were Japanese, our participants differed in age, gender, and attitude from the typical end-users. To demonstrate their persona posters, people acted out a day in the life of their persona in front of the group. In preparing to take on the character of their persona, the actors picked out hats and bags from a large selection and used them as props to advertise that they were acting as a persona, not as themselves. The hats and bags helped the bilingual collaboration process because they encouraged everyone to relax and be silly by giving them permission to act as someone else. Physical humor exposed information about individual personalities to speakers of the other language that they would not get otherwise because interactions were mediated through the translator. Acting as someone else helped develop empathy for the end-users and helped the Japanese participants understand that even though the end-users were also Japanese, they were not designing for themselves.

Figure 2 Selecting hats and bags to act as a persona (some identities concealed).

Everyday Objects as Props – To get inspiration for technology ideas during a brainstorming session, we brought in everyday household objects, including toys, to use as prompts for the inspiration process. During this exercise, each participant selected items from boxes containing objects with interesting physical properties and used that object to tell a story about a technology use scenario, for example about how a child could interact with a stuffed animal to communicate with a parent far

away. The everyday objects helped bilingual collaborative design by allowing participants to avoid loss of momentum because they could keep reasoning with their object while waiting to be interpreted. Having objects on a table in front of each person also helped participants avoid censoring themselves because other people could see what they were working on and direct questions to them about what they were doing.

Figure 3 Using everyday objects to imagine device behaviors.

RECOMMENDATIONS FOR BILINGUAL DESIGN MEETINGS

Persona posters, hats and bags, and everyday objects are examples of physical artifacts that helped overcome some problems with interleaved translation during bilingual collaborative design. However, to use these artifact s successfully, they must be integrated into the overall meeting format. The following are general recommendations for facilitating successful bilingual collaborative design meetings:

Set ground rules for interpreting and then make everyone follow them – Talk to the interpreter before the meeting and stress that because everyone should be able to participate freely in the meeting, everyone's comments must be interpreted, even if they try to wave away interpretation. Also clarify to everyone that the interpreter will interrupt two people speaking to each other in the same language to translate. In addition to allowing everyone present full access to all discussions, these interruptions are important for counteracting two common hierarchy issues in Japanese organizations: subordinates talking only to their superiors instead of directly to the group and supervisors asking their subordinates (who often have better English skills) to address the group on their behalf. The ground rules should clarify everyone's comments will be translated by the interpreter, not by members of their own organization.

Normalize different behavior during the meetings – For a brainstorming meeting we asked participants not to wear ties and handed out team sweatshirts for everyone to wear, attire different from the suits they normally wore. Cues about how to behave succeeded in creating a new kind of meeting norm where people felt comfortable making outlandish suggestions. Wearing silly hats actually helped elicit candid comments by sending a message that this is a different kind of meeting and different kinds of rules applied. For some parts of the meeting we asked people to work seated on the floor, and the Japanese participants immediately took of their shoes and sat, with the Silicon Valley participants sheepishly taking off their shoes too.

Work in small groups with different combinations of people – Over the course of multi-day collaboration workshops, we explicitly formed and reformed small groups of different people into teams. By frequently changing around who works together, problems with inter-group dynamics, such as pairs talking only to each other without waiting for translation, can be avoided through new team assignments.

CONCLUSION: CHOREOGRAPH FOR COLLABORATION

By incorporating physical artifacts into bilingual design meetings following the above guidelines, PARC overcame some problems with interleaved translation. Persona posters, hats and bags, and everyday objects were effective in avoiding loss of momentum and self-censorship because they allowed non-verbal ways to participate and encouraged candid exchange of ideas by relaxing participants. However, the artifacts and the meeting management recommendations alone are unlikely to succeed without careful choreography of how participants use the workspace.

Choreography is necessary to make sure every person is in within arm's reach of all the necessary materials, a requirement for enabling participation. For example, during the early part of one design discussion, only one participant and the interpreter were standing within arm's reach of markers and a white board, while the other team members sat on the opposite side of the table. The two scribes could write in English or Japanese, depending on who was speaking, but only the two of them could record anything, which undermined the opportunity for others to communicate nonverbally through sketches or to make notes to themselves to remind them of important points to explain when their turn to be interpreted came. By stopping the discussion, removing the table, and asking everyone to stand by the whiteboard, more people participated in the discussion.

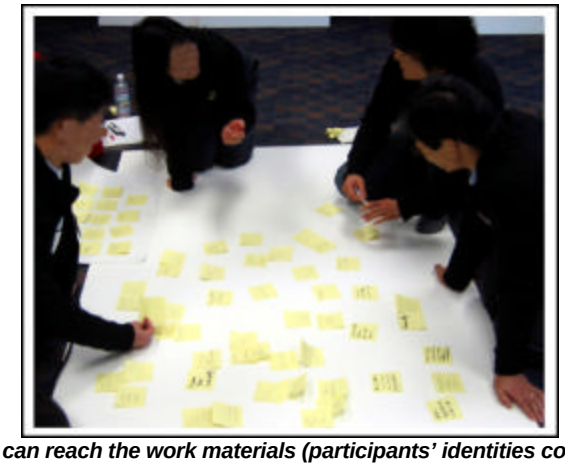

Figure 4 Everyone can reach the work materials (participants' identities concealed).

Choreography is also needed to insure that individuals don't escape the group and work completely independently. Over the course of multiple design meetings, one participant would listen to instructions and then go sit down away from his team to complete a version of the design task on his

own, making sketches and writing text to explain his ideas on his own paper. When finished, he would return to the group and commandeer the interpreter to go through each point of his complex ideas. Repeated requests for him to rejoin the group while he worked alone went ignored, but choreography succeeded in getting him to participate with other group members. Moving tables and chairs removed places to sit away from the group and drove the loner participant to work alongside others. Repeatedly placing sticky notes on top of his own paper while he wrote coerced the loner to record ideas in the shared format. Placing his notes on a wall alongside other sticky notes encouraged him to engage with other people's ideas because when asked if his notes should be placed next to sticky notes for one idea or another, he had to listen to the ideas to make that decision.

As these examples show, physical artifacts can overcome some of the problems with bilingual collaborative design meetings. However, the ways in which people interact with the artifacts, each other, and the workspace must be carefully choreographed to insure that the meetings succeed.

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THE WORST TECHNOLOGY FOR GIRLS?

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The aim of the research was to discover how teen girls use technology in relation to privacy practices in their everyday lives. Asking teenage girls to describe the worst technology they could imagine was a fruitful way of exploring their feelings towards location-awareness, tracking and surveillance in particular and served as inspiration for the design of concepts which embody many of their concerns.

INTRODUCTION

This study explored how teen girls currently understand and create both privacy and "publicness" in their everyday lives. We also wanted their view of the potential impact on their privacy of technologies such as location-aware computing. How do they feel about technologies that reveal their locations to their families and friends?

Prior studies on teenaged girls' use of SMS messaging (texting) or PC based instant messaging (IM) show that the silent nature of these two forms of communication makes them easy to use when other people are present, and a preferred means of private communication with friends for teenage girls living at home. Studies of Japanese girls and texting carried out by Ito and Okabe (in press; n.d.) suggest that phone based email or texting allows teenage girls in Japan to maintain their "intimate community" of friends. Studies of teen texting in the UK by Grinter and Eldridge (2001) also pointed to texting as a cheap and fast, but also silent, method of communication. Studies of IM use in the US by Grinter and Palen (2002) were undertaken when few US teens had cell phones, and suggested that IM was the preferred technology of US girls who want to carve out a personal and private space within the home.

The study described in this paper set out to explore the role that technologies play in creating, maintaining and preventing privacy in the lives of older teenage girls in the US and UK and whether the girls in the US still used IM rather than SMS. All the participants were living in a parental home and attending school full-time, and we studied how they used their cell phones, PCs and other forms of technology to enable private communications at a time in their lives when they desire to be in constant contact with their friends, and yet are subject to surveillance by parents and teachers.

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PARTICIPANTS

The participants were 24 teenage girls, aged between 17 and 18. Five were from Portland, OR, eleven from Seattle WA, in the US and eight from the UK; evenly divided between Brighton and Bristol. They were recruited in friendship groups, with the initial contact bringing one or two friends. They were paid for their participation, and consent forms were obtained from each teenager, together with a parent if they were under 18.

The girls were all in full-time education, and were all living with one or more parents, though 46% of the girls had divorced parents and either lived with just one parent or switched between the homes of both parents.

As a prerequisite for participation in the study, all of the teenagers had cell phones and access to a computer with internet access. Three of the UK girls had a computer but did not have internet access at home, and used school or friend's computers to participate in the study. They were all provided with a digital camera for data collection, and kept as part of their payment.

METHODOLOGY

Each group of two or three girls had an initial meeting with one of the researchers and then participated in a joint photo blog for two weeks. In-depth individual interviews were held at the end of the blogging period. Quotes and photographs from the blogs and interviews were then used to design concepts that expressed "the worst technology" for girls.

Blogs

The blogs were set up by the researchers and posed one question per day, which each participant answered with text and photographs. They were also asked to add journal entries to provide information about their days. We wanted to capture data about everyday experience, emotion and ephemera, and to gain an insight into teenage life from a distance.

Only one of the participants in the study had a blog prior to the study, using LiveJournal, but all were aware of them. In the first group of three girls we gave each an individual blog, as an attempt to provide privacy, but that negated the role of the friendship group. It seemed that if they shared a single blog they might communicate more, both with each other and with the researcher, so we gave all the subsequent groups a shared blog. Whether the girls saw each other during the day at college, and whether their blog partner was their best or less close friend affected their levels of openness when answering the questions. The most engagement with the blog came from close dyads that went to different schools and used the blog as a form of chat. The girls also reported during interviews that the blog provoked conversations with one another that would not have otherwise occurred.

One of the author's previous research studies with teenagers had involved giving them a journal and camera to record their thoughts and practices. This time blogging was used as a way of collecting their writing and photos, but with the additional bonus of being able to interrogate the girls further while their thoughts were still fresh. One of the girls had participated in both the studies and was asked to compare her experiences with the book and the blog.

> *With the book I was so much more creative, I could do so much more. I felt my personality really showed... But I couldn't see the book being so*

much fun if both L and I were doing it. It was better since we were friends and we could see what we'd both written without having to go over to her house and say, so, what did you write?

Interviews

At the end of the blogging period the researchers interviewed each of the girls individually. Interviews lasted 60 to 90 minutes and used mapping exercises to talk about daily living patterns. The girls drew maps to explain the layout of their houses, the places they went and their friendships. The house maps revealed information about how the siting of technology in combination with the layout of their home dictated their privacy practices. Maps of where they went were used to talk about what they told their parents about their location and maps of friends revealed more about how they kept in touch.

FINDINGS

Privacy as principle

Maybe not surprisingly, it was apparent from the blogs and interviews that privacy was extremely important to all the girls as a matter of principle. Privacy for teenage girls is about defining the self as an individual and an adult, and is part of an ongoing negotiation of boundaries.

Because I am living at home, I do realize that there is only so much privacy still aloud (sic), but my parents are really good about giving me that space. I feel privacy is something that you can have without having to always explain yourself and your actions and it is very important to me.

The fact that privacy is not necessarily about highly secret content was also a common theme:

It doesn't have to be anything big, I just don't want to tell them… I don't want to know where they're at all the time…it's something of my own, when I leave my house I'm doing something of my own, something on my own…it's more my time... Home life and outside of home life is different. It's a secret life, you know.

Finding privacy in the home

All of the girls saw the main living areas of the house as public rooms which were primarily a parental zone and not really their space. In order to create privacy for activities or conversation they either went to their bedrooms; moved within the home to somewhere they could not be overheard, or moved outside the home altogether to a car or public space. Acoustic privacy was far more important than we had supposed as the majority preferred voice communication and were not using texting and IM for silent communication in the ways that had been previously suggested by the studies of Ito and Grinter.

The only times they felt able to use the family spaces were when parents were out and they could take over, or if they had so many friends over at one time that their parents were forced into another part of the house. While drawing maps of where they go one of the most frequent destinations (especially in the US) was friends' houses. The most popular friends' houses were those with parents who were out at work until late, or were large enough to provide physical distance between the teens and the parents.

The majority regarded as their bedroom as a private space, including one of the three girls in the study who shared bedrooms, but only 70% said that a closed bedroom door gave them privacy. T describes her non-private bedroom:

> *Unfortunately there is no lock on the door so my Mom sporatically (sic) bursts in when i am changing or when I'm with my boyfriend. also the walls are very thin so even when i am having a private conversation on my cellphone i have to whisper so my brother can't hear.*

The inhibiting effect of the presence of others, usually parents but also siblings, was a recurrent theme for all girls. LI; "I'll leave, I never make a phone call or take a phone call in front of my mum. I don't know why, even if it's completely innocent I don't know why I just don't feel comfortable." Whether phone calls were private depended on the layout of the house and how far their bedroom was from their parents'. A described her careful considerations of whether her parents can hear her conversations when they're in their room, which is next to hers; "I've thought about that. If they're on the phone I've listened to see what I can hear, to work out how much they can hear…You can't hear much …at night, I just turn on my TV."

Despite the frequent mentioning of being overheard, it did not seem that texting and IM were being used to create privacy at home; instead the mobility of the phone was used to escape those who might be listening. The cordless phone became a surprisingly important piece of technology as it offered privacy and mobility within the home. All the girls in the UK had or had had a cordless phone that was on the family landline and which supplemented their cell phone (they were all using "Pay As You Go" phones which they funded themselves). In the US all but one of the girls were part of a "Family Plan", which provided free calling from their cell phone at evenings and weekends, so had less need of the cordless phone.

In order to achieve privacy in the home the phone, whether the family's (cordless) landline or a personal cell phone, was carried to the most private place in house: bedroom, car, street, sauna, bathroom, closet, garden, trampoline, the room most distant from their mother… were all listed as places to go to with a phone in order to have a private conversation. For instance K described phone calls in her bedroom closet; "if there's something I really don't want my parents to hear I'd go in my closet. I can go in there and sit down…to talk on the phone sometimes." There were several stories of parents overhearing their calls, and ensuing conversations about things they just didn't want to talk about. As has been previously stated this was not necessarily because the conversation was about highly sensitive topics, rather a sense of it being their own private life.

Texting and IM were not seen as more private because the content might be read by others. Texting was seen as useful mostly for quick, 'functional' communication, such as sending a text to check if a friend was at home before ringing on their landlines. This enabled late night or discreet calls

between friends who were primed to pick up the house phone on the first ring. It also meant the parents paid for the cost of the call. This agrees with Grinter and Eldridge's study in the UK (2001).

The sense of privacy when using the computer also depended on its location within the house and whether it is shared. Computers which sat in a shared space were most open to parental scrutiny, but any computer that was used by other family members did not feel private and one girl described how she had made herself the administrator so she could protect her creative writing from her mother. 44% of the girls in the US had a laptop or PC that was theirs, which gave them the ability to take the device, and the communication it contains, to a more private place such as their bedroom.

> *I have my own computer, …. It is a laptop and i love it; it's name is Poppy. i have it in my room so that is REALLY nice... i can use the computer for what i want and my parents don't get up in my business.*

The only girls who were frequently using IM were the US ones with access to a personal computer or laptop, and one UK girl with lots of family in Peru.

Privacy and mobility

The cell phone enabled all the girls to provide ad-hoc location updates to their parents, and renegotiate curfews as required. The very existence of the phone, and the knowledge that parents can call their daughters at any time had given them far more mobility outside of the home, and thus more privacy. Knowing that the girls would be carrying the phone often meant that there was very little prior communication about where they were going. At the same time the ability to call at any time meant that several of the girls had assigned special ring tones to parents in order to "censor" the environment before answering the phone, and at other times used the ambiguity of the technology to avoid unwanted communication with both family and friends.

The girls appeared less worried about location tracking than we expected. Only a couple of the girls were really worried about their parents knowing exactly where they were, and this seemed more of a concern in the US; maybe because the legal age for drinking alcohol is 21 and the places they were concerned about were the homes of friends who were known to have parties involving alcohol. Several girls felt it would be more of a problem with friends or boyfriends, as they carefully juggled social groups.

THE WORST TECHNOLOGIES

Asking teenage girls to describe the worst technology they could imagine provided an interesting approach to revealing their feelings towards location-aware, tracking and surveillance technologies. The girls were quick to describe their worst technologies as those that would reveal exactly what they were doing or saying, either through video, audio or text. Only one girl had a blog outside of the study, for which she carefully administered viewing rights after her parents read it, and only one girl had a paper journal which she carried with her at all times. Several noted that they were wary of email and instant messaging as text could be copied.

The following concepts were directly inspired by the girls' descriptions of worst technology. Each idea includes the quote that inspired it, plus a simple sketch and description of the imagined product. The ideas are meant to highlight their concerns, especially about revealing the everyday content of their lives. The concepts were posted to a blog and the girls were invited to provide their feedback.

Family Video

Family Video lets you stay part of your child's life, even as they grow up and spend more time out of the house. A small video camera attached to a flotation device acts as a personal CCTV which sends back a constant video stream to home. The video can be seen on any display in the house; special events could be watched on the big screen TV.

Response from L: "This is super awkward! I couldn't even imagine all the horrible things that would arise if that followed me around. This is probably the overprotective parent's dream!"

Constant Connection

Constant Connection provides a continuous open communication channel for parents and children. The connection can be maintained between two mobile devices, or the parents can use the home audio device, which is ideally suited for a kitchen counter.

Response from T: "the last thing a teenager wants is to bring their parent in their back pocket to the movies or to a party. Being a teen means your trying to separate from the nest and assert yourself as a confident, competent individual."

Ticker Text

Ticker Text converts all communication from designated cell phones into an easy to read text format. Each text message that is sent or received on the phone is printed out on a paper roll. Phone conversations are also now easy to read and archive using speech to text technology.

Response from L: "As for the phone conversations being written, another not so good idea. My sister was grounded for a while after she accidentally recorded one of her phone conversations and my parents found it."

Teen Monitor

Teen Monitor provides a simultaneous broadcast of all your teenager's conversations through an audio speaker in your home. A small and inconspicuous microphone picks up all their phone calls, and everyday conversations with friends, so you'll always know what's going on in their lives.

Response from C "Definitely up there on the creepy scale… the only time that I can see this device or any of the others for that matter, as justifiable is if it were used to monitor a teen that was otherwise uncontrollable and was endangering themselves or others with their behavior. Basically the extreme problem child."

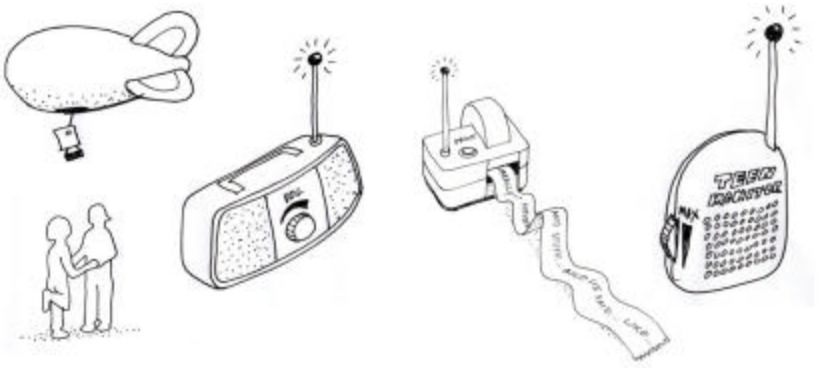

FIGURE 1 Sketches of worst technology concepts. Family Video, Constant Connection, Ticker Text, Teen Monitor.

CONCLUSION

The girls in the study were using the mobility of technology to create and maintain privacy. The cellphone meant they no longer had to risk being overheard because they either left the house entirely, or within the house the cellphone or the cordless phone allowed them to escape out of earshot.

The worst technology for girls is really something that invades their privacy, and discloses the content of their lives: what they are doing, saying and writing. Mylifebits and SenseCam from Microsoft research, Lifeblog from Nokia, and the Eyetap system from Steve Mann are all experiments in the kind of technology that these teens most worry about: collecting images, or a constant video stream, or audio recording, everything you write, your IM sessions, or emails. If combined with CCTV, then their worst nightmares would be in place. What they feared most was the combination of what Nack (2005) calls “see what I see” and “see me”.

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IRRATIONAL CHOICES, UNFATHOMABLE OUTCOMES: PATIENT ETHNOGRAPHIES IN PHARMACEUTICAL RESEARCH

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With the proliferation of oral therapies (in place of injectables) for many chronic conditions, the locus of medical treatment is shifting from the surveilled context of the clinic to the private space of the home. In the home, compliance and persistency – the extent to which patients take medications over time – have become pressing concerns. Non-compliance adversely affects health outcomes, and costs manufacturers millions. The medical community has difficulty understanding non-compliance, often relegating it to individual irrationality or dysfunction in the doctor-patient relationship. Ethnography opens up the issue by entering the private space of pill-taking to understand the beliefs, relationships, and activities that contribute to patient (non-)compliance.

INTRODUCTION: REBECCA HAS CANCER

In the hospital, the doctor told Rebecca to get her affairs in order. He'd found a tumor the size of a football nestled between her large intestine and her ovaries. There was no clean way to remove it, and – without a thorough pathology – no reliable way to tell what it was. The doctor assumed the worst.

Rebecca went home. An intensely social person, she refused calls from friends and family. She went online. She identified a surgeon who could remove the tumor. Two weeks later, he took out one of her kidneys, her spleen, and parts of her intestine, stomach and pancreas. The cancerous mass was gone.

Gastro-intestinal cancer is notoriously aggressive. Despite successful surgery, Rebecca's surgeon recommended further therapy. She went back to the Internet. The drug he suggested – the first of a new generation of so-called "targeted therapies" in oncology – is a simple, once-daily pill. Relative to old-school chemotherapy, its side effects are minimal. It promised to turn a once-fatal diagnosis into a mere chronic condition. Patients who took it were surviving for years without recurrence. But, Rebecca's insurance wouldn't cover it completely. Undeterred, she contacted the drug's manufacturer and arranged to receive it for free.

Up to that point, Rebecca was a model baby-boomer cancer patient: pro-active, informed, and determined to take control of her care. So, sitting in her living room in suburban New Jersey eight months later, it was hard to believe what she was telling me. After all she'd been through – after the shocking diagnosis, the invasive surgery, the exhausting battle with medical bureaucracy – Rebecca wasn't taking her medication.

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THE PROBLEM OF PATIENT NON-COMPLIANCE

In the course of product commercialization, the drug's manufacturer had talked with hundreds of physicians and nurses. Their enthusiasm was overwhelming, and their experience suggested that patients were exceedingly compliant with the therapy. None of the physicians or nurses seemed to know patients who stopped therapy of their own accord, or missed more than the very occasional dose. The manufacturer interviewed patients, who corroborated this story. But, by simple mathematics, the manufacturer knew otherwise. They compared the number of prescriptions written with the number of pills still sitting on warehouse shelves, and saw that something was off. A more complex calculation led to a statistical figure: only 77% of their patients were compliant with their life-saving therapy.[1]

This conclusion flabbergasted the pharmaceutical company. Their drug is efficacious, its side effects are "mild to moderate," and the medical community is fully behind it. There's no "rational" reason why patients should refuse it in such numbers. Patient non-compliance resulted in a three-pronged crisis for the manufacturer. First, by missing their prescribed dose, patients increased the likelihood that their cancer would recur. Second, by leaving their prescriptions unfilled, patients increased the likelihood that the marketing team would miss its annual revenue target. Third, by experiencing recurrence (and possibly premature death), the long-term market for the therapy was at serious risk of shrinking.

The company needed to act. In this case, action did not take the form of lab science and high-tech manufacturing --- the cornerstones of the pharmaceutical industry. It came through the vagaries of brand marketing, where drama and spin direct human impressionability. The advertising agency assigned to this drug believed that the drug's Direct-to-Patient communications were overly clinical, and did not sufficiently address patients' "emotional drivers." The providers of compliance programming felt that their offerings were missing important "behavioral cues" that lead to patient non-compliance. The marketing team worried that some people with cancer just have a death wish. How else could they make sense of their behavior?

The marketers convened several focus groups. They assessed patients' and physicians' knowledge of the disease and the drug. Their hypothesis – grounded in the tradition of health research on compliance – was that the more people know about their condition, the more likely they are to stay the course of therapy. And, the critical channel for knowledge of this sort is the patient/doctor interaction. What they found were knowledgeable, proactive patients like Rebecca, and relatively personable, committed physicians who were doing what they could to promote compliance. Patients and doctors were as surprised as the marketers to learn that people were not compliant. Everyone was stumped. The market research manager decided they needed a new approach. She commissioned an "ethnography," and I went to Rebecca's house.

A SOCIAL ANTHROPOLOGICAL APPROACH TO NON-COMPLIANCE

When I got there, I had every reason to believe that Rebecca was one of the compliant ones. We had spoken on the phone several times, and I knew how proactive she'd been in managing her illness and treatments. Over the phone, she told me she was taking the drug. Her doctor thought she was taking the drug. So did her nurse, her pharmacist, and her sister. But, her husband knew otherwise. After several hours of conversation, he told me that she didn't take the pill for several months after

receiving it, and then only took it a few times per week. He revealed this information without reproach. Her decisions were her business, he told me, and it's up to her to decide how to handle her disease.

Rebecca explained her situation more fully. After surgery, she was living in a fog. There were days when she would just sit and cry. Sometimes, she was afraid the cancer would come back. Other time, she was afraid the drugs would harm her more than the cancer itself. Most of the time, she was just afraid, and she really couldn't think of why. At that point, she wanted to wake up from the nightmare. But, the drug wouldn't let her. Rebecca explained: "They are giving you something that could possibly save your life, so, on one hand, why not take it and live with the side effects. On the other hand, it's like you never get out of that cancer world, because that's a reminder every day that you've got something…and maybe you don't necessarily want to do that." For her, the drug was intolerable not because of its bio-chemical formulation, but because of its symbolic resonance.

At first, Rebecca dealt with the situation by leaving her medication in its shipping box. Over time, she developed a more complex solution. On evenings when she knew she'd be at home – when she knew she could take the drug and then lie down if she felt sleepy or throw up if she felt nauseous or take a bath if she felt depressed – on those evenings, she'd take the pill. But, on evenings when she had plans to go out, she'd skip it. Every week, for example, she had dinner with her mother and her sister. In her Italian-American family, eating is everything. Rebecca was not about to risk experiencing symptoms with her family present. That wouldn't just ruin her dinner. It would highlight for her mom and sister that she was still not well, that she was still suffering and at risk for recurrence.

For Rebecca, as for many of the participants in our study, sociality is critically tied to compliance. There are countless examples. In Rebecca's case, both the revelation of her non-compliance and the explanation for it were tied up in her most intimate social relations. But this was not, *ipso facto*, the way our pharmaceutical clients would have seen things. Our clients' individualistic, cognitive model stood in stark contrast to an anthropological perspective that highlights sociality as a model for understanding seemingly "irrational" behavior.

This contrast is hardly unprecedented. Consider the Azande of Evans-Pritchard's time. Among the Azande, a growth of a reddish colour in the area of the intestine was not called gastro-intestinal cancer. It was called Mangu, and it represented the physical substance in the body that identifies an individual as a witch. In Evans-Pritchard's analysis, the presence of Mangu in and of itself was of relatively little interest to the Azande. It only became an issue when an act of witchcraft had been performed. The Azande, as Evans-Pritchard explains, "are interested solely in the dynamics of witchcraft in particular situations."[2] These situations are by and large instances of great misfortune. Take the example of the falling granary. The Azande know that granaries – elevated wooden structures that hold grain – are subject to the vicissitudes of wind and weather and termites. Sometimes they fall. So it goes. But, if people happen to be sitting under a granary when it falls, and they get hurt, then it is insufficient for the Azande to conclude that their luck was bad. The Azande demand a clearer causal explanation for the misfortune. The Azande look for a more rational, more scientific story. They look for malfeasance in local social relations. And they look – among the parties in question – for Mangu. Witchcraft.

Contemporary Western cultural logic is clearly quite different from that of the Azande. Our tolerance for coincidence is greater. In the granary example, we are satisfied with vague explanations about gravity and happenstance. People caught under the granary were in the wrong place at the wrong time. So be it. And – as our interest in genetics shows – we have a greater desire to understand the

physical substance of human beings prior to (rather than following) social phenomena. For example, the scientific principle behind Rebecca's targeted therapy has to do with the inhibition of genetically-triggered bio-chemical processes BEFORE they result in the phenomenon of gastro-intestinal cancer.

At the same time, the interpretive power of Evans-Pritchard's social anthropology helps unravel a pressing business question at the heart of the pharmaceutical industry. With the proliferation of oral therapies for various chronic conditions, the locus of treatment is increasingly shifting from the monitored context of the clinic to the private space of the home. In their homes, patients are free to do what they will, and report on it later. In this context, compliance has become an ever more pressing issue. The medical community has difficulty understanding non-compliance, because most approaches relegate it to individual irrationality or – in more enlightened treatments – dysfunction in the doctor-patient relationship.[3] Ethnography can open up the issue, to uncover the various elements of sociality that play into people's treatment experiences and decisions. In the context of an individual's treatment decisions, Rebecca's behavior appears irrational, even suicidal. But, in the context of her sociality, it's completely rational and imminently understandable.

Once we understand Rebecca's decisions, we find ourselves in a position to draft interventions that (with some luck) may increase her compliance --- improving both her health outcomes and the pharmaceutical company's bottom line. In this particular case, we made some concrete recommendations for this particular cancer therapy. Commitments to the client constrain the extent to which I can discuss those recommendations. But, broadly speaking, we suggested educating physicians to look for possible social roots of non-compliance, and provide patients with medically acceptable flexibility to address them. It is legitimate, for instance, for patients to alter the timing of their pill-taking to accommodate social events. We also highlighted the fact that side effects classed "mild to moderate" in clinical trials may feel more serious to some people, some of the time. The pharmaceutical and health care industries need to acknowledge those side effects, and offer real, workable ways for patients to address them. It's too early to know the extent to which the tactics we offered have worked. I am certainly optimistic. They are, in any case, a better start than our client had had previously.

CONCLUSION: PATIENT ETHNOGRAPHIES IN PHARMACEUTICAL RESEARCH

To be effective, patient ethnographies in pharmaceutical research must be much more than simply "in-context" studies of sick people and their families. Explaining the "irrationality" of Rebecca's non-compliance involves more than just sitting in her home with her husband. As her example clearly shows, the key to understanding patient non-compliance comes in shifting the lens from individual rationality to sociality. That is a theoretical move, not a methodological one. As Evans-Pritchard writes, "Anyone who is not a complete idiot can do fieldwork."[4] But, an active dialogue with the constructs of social anthropology a la Evans-Pritchard is critical to knowing what to look for in the field, and how to interpret it back at the office.

Theoretically-informed ethnography can supplement the individualist, rationalist models of Western biomedicine with a rich understanding of sociality. But, in the context of the pharmaceutical industry, this is far – very far – from a home run. As a whole, the pharmaceutical industry is invested in unpacking the mysteries of non-compliance. At the same time, it's invested in maintaining the individualist, rationalist epistemology of Western biomedicine. So, ethnographic work such as this is acceptable to the industry, but only when it's relegated to the vague, squishy, always-already suspect

world of marketing. Ultimately, the ethnographic intervention may improve health outcomes by helping to "sell" the best of modern pharmaceutical science. But, in this context, it's hard to imagine a scenario in which it might affect the direction of pharmaceutical science. For now, let's just say that mentioning Mangu to a pharmaceutical executive remains firmly out of the question.

NOTES

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The ideas, perspectives and ramblings herein are my own, and do not represent the official position of Hall & Partners Healthcare.

[1] In order to maintain the confidentiality of my client and the anonymity of all individuals discussed in this paper, I am unable to provide citations for certain proprietary corporate intelligence held by the company or its agents. I trust the reader will understand my predicament, and accept certain vagaries of citation.

[2] Evans-Pritchard (1976:4)

[3] The medical literature on compliance, persistence, and adherence is vast. I have included some representative works in the references section of this paper. Much of the literature provides psychological perspectives on patient medication-taking, suggesting that affect drives patient behavior. Some publications interrogate physician-patient interactions as the source of miscommunications (and non-communications) that result in poor adherence/compliance. Still others claim that the specifics of the therapeutic area (oncology, infectious disease, primary care, etc.) shape dynamics of patient compliance. To my knowledge, no systematic analysis of patient compliance takes an anthropological approach, grounding patient behaviors in broader issues of sociality and meaning.

[4] Evans-Pritchard (1976:243)

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ETHNOGRAPHY AND PROCESS CHANGE IN ORGANIZATIONS: METHODOLOGICAL CHALLENGES IN A CROSS-CULTURAL, BILINGUAL, GEOGRAPHICALLY DISTRIBUTED CORPORATE PROJECT

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We detail an ongoing, consultancy partnership, where ethnographic field methods are being used to elucidate the work practices of software engineers in a large organization. We focus on intellectual and logistical challenges that we face as a team – non-collocation; widely varying experience of ethnographic methods, local language and culture; and conflicting responsibilities and lines of accountability. We consider the social spheres in which our team members operate and the sociality of our team as a whole. As ethnographic teams are increasingly considered de rigueur within corporations for cultural translation in the face of globalization, the issues we face are likely to become more commonplace.

INTRODUCTION

Ethnographers are increasingly being called on by corporations to do cultural translation in the hope that competitive edge can be maintained in an increasingly global marketplace. In this paper we present our own experience in this regard by describing an ongoing study, where ethnographic methods have been engaged to highlight issues in software engineering practice in a large organization. In this instance the cultural translation is two-fold – to observe the culture of software engineering within the organization and illuminate the gaps between management ideals and actual practices, but also to consider innovations in software engineering practice world-wide and to consider what innovations may be suitably introduced given the existing organizational culture.

In this paper we focus not on the domain itself, but on our experiences as partner/consultants. We discuss issues in the maintenance of productive sociality in our multi-cultural, multidisciplinary, distributed ethnographic fieldwork team, addressing the question posed in the call: "How are we as researchers embedded in social collectives and how does that relate to our research questions, presentation of findings and the ways in which we conduct ourselves in our research?" We discuss some expected and unexpected challenges in establishing and maintaining productive working relationships within the team but also between the team and those under study.

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Product or Process?

We would first like to draw two distinctions we have found useful in situating our work within the broader arena of ethnographically inspired fieldwork within industries and corporations.

First, we distinguish between ethnographic methods directed at informing innovations in products and services for consumer markets (e.g., Cagan and Vogel, 2002; Squires and Byrne, 2002) and ethnographic studies focused on organizational change, or innovation in work processes. Of course this product versus process perspective does not necessarily mean a sharp split between time spent, observational or analysis method, or even number of settings. Both forms rely on the ethnographer's careful empathic eye, and arguably a deeper social connection between the "observer" and the "observed" than is required for many other forms of investigation (e.g., surveys). What most distinguishes the two in our view is the output, and in the way in which the "results" are considered to be "actionable". The former is focused on influencing the design of a tangible artifact or collection of artifacts. This product will likely lead to changes in the consumer's relationship with others. The latter tends to produce recommendations for changes in people's relationships to others and to processes; artifacts may be more or less designed as part of the process but are secondary. The distinction is ontologically tricky but important: One form leverages sociality that exists to create new product niches and considers changes in sociality as another opportunity for a market while the other sells the recommendations for a transformation of sociality. Our current study is an example of the second form of ethnographic engagement.

It follows that the work of the ethnographic process itself in these differing 'modes' may lead to differences in approaching the sociality of (and with) those under study, but also in the sociality of the fieldwork team itself. Studying processes and being committed to process change from the "bottom up", honoring existing practices while at the same time discussing the possibility of new practices requires an inductive and collaborative strategy wherein the social relationships between team members and those we are studying is central. Accounts of the work process are jointly constructed in a collaborative sense-making process, where meanings are negotiated and clarified with fieldwork team members and those "studied" participating. Such an engagement requires time, and strong trusting relationships to form between ethnographers and those under study and between the ethnographers themselves.

The second distinction we draw is between the work of process ethnography to lead to process change "and change management": "the process of developing a planned approach to change in an organization" where "the objective is to maximize the collective efforts of all people involved in the change and minimize the risk of failure of implementing the change" (wikipedia definition). Unlike many change management studies that use interview and survey analyses, we are more focused on the detailed, everyday work practices and the day-to-day sociality of employees. In this view, the existing organizational structures, procedures and processes are studied as they are actually accomplished by members of work communities, with the aim of collaborating with those communities to stimulate change. By contrast, in many change management studies these details are seen as epiphenomenal to structural understandings and strategic interests.

PRACTICE INNOVATIONS IN SOFTWARE ENGINEERING

As noted, our domain of study is software engineering practice. Software engineering is a set of diverse activities. It has been defined as "the computer science discipline concerned with developing large applications. Software engineering covers not only the technical aspects of building software systems, but also management issues, such as directing programming teams, scheduling, and budgeting" (webopedia definition). This definition underscores that software engineering is a broad term for many kinds of activity with many stakeholders and participants.

Our project addresses system and product development in a large organization. In general, within the software engineering industry there has been a shift away from proprietary software systems and 'closed' mainframe platforms to open architectures. Increasingly intense business competition has accompanied this change; centralized software development operations of the 1970s and 80s have given way to a more distributed work organization and more variation in the development process. However, many long-standing software development organizations in our subject country are tied to the tradition of centralization, making process innovation much more difficult to achieve. In addition, software development in our subject country relies on long-standing relationships with subsidiaries and especially subcontractors to carry out development work. Efforts to develop systems and products that are more closely tied to customer needs have met with limited success for a number of reasons: a contributing factor is the aforementioned dependence on subcontractor relationships and performance, but, more critically, little has been achieved in the way of innovation in the 'upstream' (requirements, design) phase. The move toward extreme programming and lean or agile methods which foreground social interaction and rich, tightly coupled collaboration between development team members and between the development team and the customer, has had little effect on development efforts - complex system integration projects continue to follow a structured linear, sequential design method.

Thus, in our work we are focused on the engineers who design and manage the development of products and services for their customers. Our focus is on the ways in which the engineers construct their customer's desires and needs (in engineering terms 'requirements') within the confines of their own organizational and professional culture. Our deliverable is a methodology for the effective creation of designed products and services for their customer; in particular, different ways for engineers to work and organize their projects in that creation process. In addition to our own fieldwork, we are also engaged in a long-term project to train an ethnographic eye into the practice of software engineering and project organization as practiced in the particular organization we are studying. This approach has methodological overlap with user centered design methodologies like Contextual Design (Beyer and Holtzblat, 1998; see also Wixon and Ramsey's edited collection on field methods for software design, 1996). Our task is therefore not one of addressing the system building processes directly, but rather in tackling the relationships with customer, issues in project coordination and leadership, and the sharing of practical knowledge across their entire software business about how to achieve success in these matters.

COLLABORATING AT DISTANCE

Figure 1 gives an outline of our team structure and reporting chains, and thus the social spheres in which people on the team operate; for the purposes of this paper we will call our partner company in this project Acme.

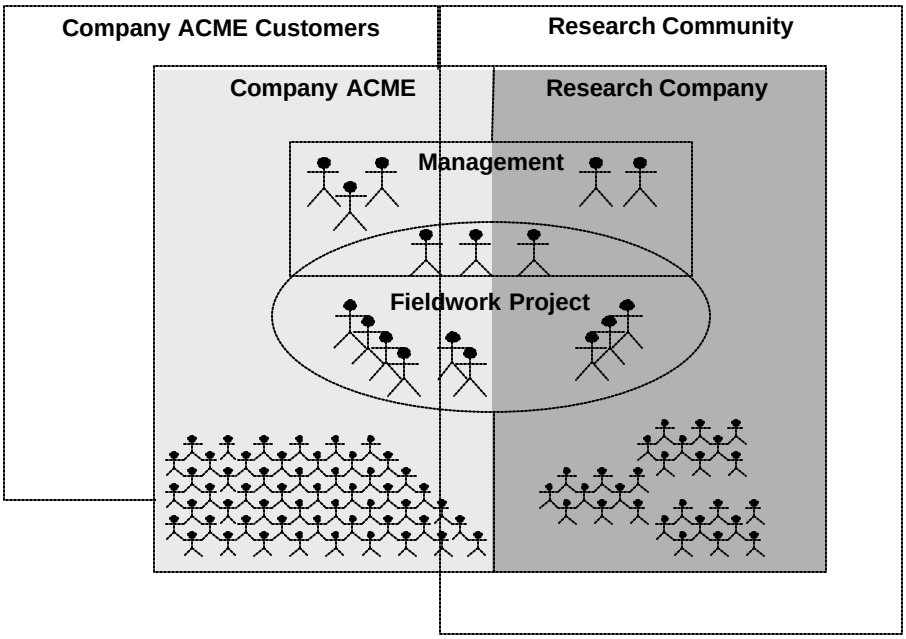

Figure 1 Stakeholders and group membership for the PARC/ACME fieldwork project

On the left are Acme and Acme's customer. Communications occur between Acme management and Acme workers through multiple means, including management meetings and emails. Acme sales and software engineering staff are the main point of contact between Acme management and Acme's customers, although alliances at management level between companies is also common. On the right, is our research group, and beyond that the research communities to which the researchers belong. The main point of interest for this paper is the Fieldwork Project, shown in the centre of Figure 1. The fieldwork team is made up of researchers from our organization with varied backgrounds (but all of whom have experience in field work studies), and seconded Acme employees who are trained software engineers. The intention is that these seconded Acme workers will be trained in ethnographic methods and will then disseminate the methods throughout the organization.

In terms of numbers, the fieldwork research team on the ground in our subject country is made up of three research staff from our organization, and seven software engineers from Acme. None of the software engineers have previously had training in ethnographic methods. Two of the research staff from PARC are natives of the subject country; the third is not, but can converse conversationally in the native language. Three research staff travel back and forth between the US and the subject country, none of these are natives of that country, and none speak or read their language. Finally, one translator

is based in the US, and three different translators are available on-site in the subject country. There is also one bilingual engineer who assists with translation on some occasions. Some of the other engineers speak and read English with different levels of competency, but none of these are able to function as translators in lengthy or detailed conversations. Three levels of management from the partner company oversee the project, with four managers present regularly in the team operations room, at meetings and in discussions.

Establishing the "Joint" for "Joint Construction"

Given this rather complex geographically and culturally distributed social group, which is unusually, perhaps uniquely, large for an ethnography-based project, it is interesting to draw some lessons and highlight some points that relate to the sociality and effectiveness of the team itself. Challenges we are experiencing are broadly broken into: *Access to places; Access to people; Access to data – conducting collaborative analysis; Data integrity; and Report integrity*.

All the issues we discuss affect members understanding of the fieldsite and therefore their communications with those under study, but also feelings of team membership. These in turn affect overall project effectiveness. While access to places and the integrity of data and reports are perennial problems for all fieldwork projects, and have been since the beginnings of ethnographic study, getting access to others and to data we thought would be less of a problem given the proliferation of communication technologies from media spaces to video conferencing to email and cell phones. Interestingly, however, in some senses our expectations led to a greater sense of disappointment and distancing than if we had entered into the project believing all communication would be limited to summary notes and infrequent face-to-face meetings. We will speak more to this in the sections below.

Access to Places – While we are working closely with certain members of the organization, it has not always been easy to gain access to other parts of the organization, or to customers and contractors. Given the organization relies so heavily on contractors, studying the project management process alone without gaining deeper insights into the contractor and customer settings is a problem. Further, as many of the software engineers in the organization work in customer sites, not understanding their work settings and the ways in which they relate to their parent organization while in these settings reduces our ability to fully comprehend the social dynamics of their work. In the end, as Auge puts it, "it is a matter of being able to assess what the people we see and speak to tell us about the people we do not see and speak to" (Auge 1995).

There are two other forms of access to place, however. First access to any site at all; those remotely located in California seldom have access to the sites of the work practice under study. And when access is possible, it is not sustained over a period of time; people cannot become accustomed to our presence, trust cannot build, and we have no insight into what are day-to-day, versus unusual, occurrences. The second form of (lack of) access is language and (national, rather than local) culture. Even when access to field sites and work artifacts is possible, activities cannot be understood without careful and in-depth language and cultural translation. Such problems have traditionally been overcome through long-term immersion – often on the order of decades (e.g., Moeran, 2005). The requirements of our project contract preclude such long term, detailed engagement. We do, however, have two native speakers from our company and several trainee ethnographers from the partner company on the fieldwork team. Which brings us to the question of how the remote project members gain access to *them* and what they know – bearing in mind this kind of (translation and) sharing is an additional task for them.

Access to People **–** With remote work and time differences, access to people is limited. Researching and designing to circumvent or alleviate problems caused by such time and distance issues has long been a topic of study within the field of CSCW (Computer Supported Cooperative Work). In our current project, the three Californian team members have been flying back and forth to the main team site, email exchanges are conducted and video and audio conferences are held regularly. Although some familiarity and rapport exists, for those of us who are traveling in this way, there is a constant game of "catch-up". With no access to asides and water cooler conversations, being "in the thick of it" intellectually and emotionally is not easy. Audio and video-conference technologies do not replace co-presence. Time differences mean we are out of circadian phase when talking; in California we are readying ourselves for the evening following a day's work when conferences occur (usually 5pm PST), while meetings are taking place at the beginning of the working day at the remote site. For those who travel back and forth, there is also a physical and emotional cost – there is a temptation to try to maintain working and personal relationships in both locations, and to keep on top of ongoing projects in both time zones, leading to exhaustion. These tendencies are obvious, but ones we have had to explicitly acknowledge, address and account.

Access to Data **–** Ethnographic analysis is discursive and artifact centered. Artifacts are a central part of getting to know the field site, and how those we are studying manage their relationship to their work, but also their relationship to each other. As Moeran (2005) states "'Things give people ideas of one sort or another. They lead to shared beliefs. Always and inevitably they bring some people together and exclude others. The social exchanges that take place through things are often strategic." Given the distributed nature of our team, and the differing language abilities and cultural background of the team as a whole, the sharing of artifacts for analysis with those not on-site has been a serious challenge. Network firewalls have meant file-sharing tools are not easily used and data size and bandwidth limitations at the field site have prevented sharing through email and through standard file sharing techniques.

Translation has been good with support from native language speakers on-site and professional translators, but again, an added burden is placed on colleagues located in the field site for translation, and even the most excellent professional translators may not think to translate the nuances and subtleties that are needed to fully appreciate people's relationships to each other and to their work. We have found many professional translators can elide what they consider to be 'unnecessary' or socially inappropriate words and phrases on the part of the speaker/actor (the force of an uncomfortable exchange may be filtered for example). Tone of delivery is not always easy to decode even when one is present given the cultural unfamiliarity. In translated transcripts, all paralinguistic cues are missing. But, as we know, impoliteness, "unseemly" behaviors and affect cues (e.g., gestures, facial expression, glances, beat gestures) are our clues to the 'real' social dynamics of a setting; politeness and public fronts may be considered "professionally appropriate" but they are not our allies in fieldwork.

Finally, when artifacts are available through shared folders, emails or even through video, deictic gestures so crucial for orienting and developing shared understanding through body and language orientation to an artifact are a challenge (Churchill and Erickson, 2003). Considerable overhead comes in simply orienting everyone to the same place in/on the artifact (comments like "No, page 3, not page 2" are commonly heard as someone waves a sheaf of papers at the video camera). In addition, the frame or conversational setting is missing. Without shared data, joint analysis suffers, but also opportunities for building good working relationships are not available. And, as noted above, language

translation cannot convey cultural aspects of the significance of, for example, documents that are collected even when they are shared.

In order to better exploit developments in digital data storage, tagging, translation and sharing further design needs to be carried out. While no technology will ever replace being there, such sharing technologies need to be *social* technologies not simply data storage and transport technologies. Certainly we need to allow access to artifacts such as transcripts, movies and images. But for *collaborative* data analysis to take place, technologies of *sharing* rather than simply storing are needed. That said, when concerns for data security mean shared networks are not possible, the quality of the application becomes irrelevant.

Data Integrity **–** If data breadth is one concern when problems exist in access to field sites, and concerns about data depth arise from lack of access to recorded field data and to deep sustained discussion, then data integrity is an issue that can arise from over reliance on inexperienced fieldworkers and from sanctions around information.

Our apprentice fieldworkers from the partner company who are highly familiar with the domain (indeed domain experts trained by and working for the partner company) are simply not always able to maintain an ethnographic stance of the other, the stranger, the stance from which much powerful analysis starts. On occasion, they take for granted and thus fail to highlight or even report items that for us, as experienced ethnographers with an inexperienced eye on the domain know to be crucial linchpins for developing understanding. Such expertise will develop over time. But even if such expertise develops, that does not solve the potential problem of reporting uncomfortable findings to their managers. This is the subject of our final observation in this paper.

Report Integrity **–** With so many stakeholders with different understandings and different emotional investments, many different forms of reporting are required, often when results are preliminary. The pressure to draw premature conclusions is often a problem in participatory design settings, but we are keenly aware of the push to elide uncomfortable findings and to legitimate existing narratives about process and to thus preserve the status quo. Typically, this pressure comes not from the senior management level that engaged us in the partnership relationship, but from those who consider they have most to lose - those who are closer to the work and those who are tasked with carrying out work practice change. Maintaining professional integrity in the face of this push to be simply a certification process for existing stories and practices (providing the "Ethnographically Approved!" stamp) is a serious challenge. And resistance to this pressure without risking trust (and therefore access to the work itself) is tricky and requires energy, time and negotiation skills. This situation is compounded because, as noted, some of the project team members (those from the partner site) are answerable to us as intellectual leads and trainers as well as to their company managers who are not themselves being trained. There is on occasion discomfort at reporting things that will make their managers (or their managers allies) "look bad" for fear of reprisal. Our apprentices live in a potentially uncomfortable social interstitial, between us their fieldwork mentors who are relying on them to deliver observations and insights and their corporate management for whom those insights may be deeply uncomfortable. To be effective as a team we must honor the constraints on all the team members for what they can deliver in the frames in which they are working, as well as the frames in which we are working.

SUMMARY COMMENTS

In this paper we have introduced our work in an ongoing field project investigating software engineering practices in a large organization. Given the distributed nature of the team and the multiple social worlds in which team members operate, we have been forced to reflect on our team sociality, mediated and face-to-face, to a greater extent than in previous projects. Practical, informational and cultural barriers to establishing a shared world-view from which to generate joint understandings exceed those we have previously encountered.

Issues have arisen due to geographical distance and language and cultural difference. Although networked technologies for reducing distance exist, these have helped minimally due to time zone differences (a problem for synchronous communication tools) and concerns on the partner side for data security (a problem for asynchronous tools such as shared folders). In addition the project was designed to be a tightly coupled *collaboration* with team members from both organizations. Participation in the social collective of the fieldwork team is challenging when the team is distributed, culturally diverse, have different skills, different levels of aptitude and commitment to the methods, and different levels and lines of accountability. Our own lines of accountability are complex as we are answerable to our own managers, but also to different levels of management at the partner company, each with their own sensitivities and concerns about what will and will not be revealed. Constant, gentle, polite resistance is needed to maintain the integrity of the fieldwork and reportage of findings. This kind of ongoing defense of findings and reestablishment of ground rules for ethnographic engagement in projects is, we believe, an essential part of the work of conducting effective fieldwork – work that is too often invisible when accounting practices are more focused on "tangible" deliverables results, and thus, work that is underestimated in planning and budgeting. As multi-sited, multidisciplinary ethnographic fieldwork teams are increasingly engaged as cultural translators within, between and for corporations, the kinds of issues we are experiencing will, we hope, come further to the fore.

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INVESTIGATING MOBILITY, TECHNOLOGY, AND SPACE IN HOMES, STARTING WITH "GREAT ROOMS"

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Certain American-style homes include large multifunctional spaces, often with vaulted or otherwise high ceilings, that incorporate living, dining, and kitchen areas. As an American cultural phenomenon, these "great rooms" symbolize and instantiate a particular vision of the good life or ideal home, including for example concepts such as openness and togetherness, or in less favorable interpretations, wastefulness and lack of privacy. As such, we see great rooms as complex and contradictory symptoms of unresolved tensions in the politics of everyday life. We describe our approach of starting with a provocative and problematic topic within a larger domain of interest and examining it from a number of perspectives. We argue that sites that are contentious are particularly interesting candidates for technological innovation, in which technology is not limited to assimilating to well-established and understood processes, but rather can participate in an ongoing process, responding to and challenging concerns.

Be not afraid of greatness: some are born great, some achieve greatness and some have greatness thrust upon them.
William Shakespeare, *Twelfth Night*, Act II, Scene V

INTRODUCTION

Over the last decade, homes have attracted considerable interest from the information and communications technology (ICT) industry as a domain for substantial growth and innovation, as information-intensive workplaces had previously. But in addition to opportunity, homes also present daunting complexity and barriers to ICT adoption (Hindus 1999). New technologies can help overcome some of these, as when low-cost wireless networking systems like WiFi allowed provision of broadband connectivity without the huge cost of wiring (or, worse, re-wiring). Indeed, one important way of looking at homes is in terms of infrastructures that both facilitate and limit technological

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deployments (Brand 1994). Nevertheless, no single approach to homes is sufficient. Their complexity calls out for multiple perspectives and methodologies. In addition to being co-evolving sets of technical infrastructures, homes are also architecturally designed complexes of connected spaces (Hanson 1999); stages or sets on which interpersonal relations play out and are organized (Hughes et al. 2000); political assertions in favor of, intolerant of, or ambivalent towards certain ways of living and certain social relations (Chapman and Hockey 1999); and fields of rules and norms emerging from their inhabitants as they work out how to live together (Wood and Beck 1994), to list but a few fruitful ways of approaching the domain.

With these concerns and interests in mind we set out to conduct an exploratory ethnographic study of the use of wirelessly networked notebook PCs (WiFi laptops) within homes. We visited six households in the Portland, Oregon and San Francisco Bay Area, California metropolitan regions; a small sample, but each household was studied in some depth, through diary keeping, observations, interviews, activity mapping, and time-lapse photography. Space here does not permit a full report on this research and its findings. Rather, for this short paper we will primarily reflect upon our approach – the reasoning behind it, some general themes it uncovered, and how it might generalize to other domains. In short, our approach was to start with a provocative and problematic topic within the larger domain of interest, to examine it from a number of perspectives, and to consider what different paths lead away from it into that larger domain. In our case, the domain was in-home use of mobile technologies and the topic was "great rooms," but we believe this kind of approach could be usefully employed for other complex domains as well.

GREAT ROOMS

Great room denotes a category of architectural space within certain American-style homes centered on a prototypical image: a large multifunctional "family room" often with a vaulted or otherwise high ceiling. *Home Magazine* elaborates, in a how-to article on designing and furnishing such spaces:

> A well-designed great room really lives up to the name, combining the kitchen, eating area, and family room into a smooth-running hub of the home. But the same qualities that make great rooms so popular—service, versatility, comfort, and generous dimensions—can also raise some tricky design issues. (Home Magazine N.d.)

From even this brief introduction to the term a number of contrasting features can be discerned. Great rooms are about composing previously isolated, and better defined, spaces into something new but "tricky" and not immediately understandable. (Conceptually, ICTs can be seen to do something similar – see Meyrowitz 1986.) They are touted as "popular," praised in both experiential and efficient terms, but are also presented as risky and needing to be "well-designed". They are a mixture of both solution and problem.

The term *great room* is not neutral, well-defined, or even in common usage (not even, we found, among people who have and use them). The spaces described by this term are found in only a small minority of dwellings, even under the loosest of definitions, and in their most developed form only in a certain genre of recent, upscale construction. It would seem then a curious starting point, but we

found the term and the kind of space it describes fascinating and relevant to our research topic for a number of reasons.

The Greatness of Great Rooms

Some of these reasons were purely pragmatic. For example, from the standpoint of mobile technology use, large spaces such as great rooms are interesting if for no other reason than that they offer room to move, to gather together, to separate, to subdivide. They pose spatial questions, both to their users, designers, and analysts, about what and who goes (and belongs) where. In thinking about wireless laptops, we wondered whether they would make their way into great rooms and freely move about, untethered, or become fixed to particular zones as if furniture. And we found that laptops were indeed used in all of these ways, but only in certain zones such as kitchen counter vs. dining table vs. couch, and often in highly constrained, nuanced, and territorial manner.

But the great in great room refers to more than just large amounts of space – it also refers to luxury, to grandeur, to hedonic experience. We became interested in what makes a great room great, and the roles ICT has or could have in this regard. Books promoting great room design and decorating often depict big-screen TVs and home theatre systems as part of their luxury appeal, and we could imagine great rooms being used as a kind of technology showcase. But other technologies, we thought, might be seen as a threat to luxury, or at least to the intended experiences of immersive enjoyment – particularly devices like laptops, which often have carried a workplace connotation.

The Trouble with Great Rooms

As we began to read more about great rooms, and to discuss the project with colleagues and other researchers, we quickly became aware of the problematic cultural or symbolic meaning of great rooms (in addition to their troublesome nature as challenges for design). Our proposal to study them was sometimes misinterpreted as an ill-conceived endorsement, or unreflective U.S.-centrism. This only intrigued us further, as it suggested there were important underlying tensions implicit in the idea. As an American cultural phenomena, great rooms symbolize and instantiate a particular vision of the good life or ideal home – or rather, a recent incarnation of an old, perhaps even medieval, vision (McCracken 2004). What this vision consists of is hard to pin down exactly, but at the very least it includes openness and togetherness – or, from a critical view, wastefulness and lack of privacy.

Wastefulness – Great rooms, at least in their fully realized, two-story, big-windowed versions, conspicuously consume space. They take considerable resources to heat, cool, clean, furnish, and maintain. They use for one room the space of what could be two stacked single-story rooms. This can be rationalized as economical in yielding a smaller square footage of floor space (and an associated reduced property tax burden, as one participant explained), but perceptually the experience of their large scale is hardly one of efficiency. The ideal of bigness and acceptance, indeed celebration, of wastefulness is often seen as a central component of mainstream American culture of super-sizing, Hummers, and "McMansions" – and one that through forces of globalization is threatening to spread (Collier 2005).

Invasiveness – In addition to expressing an ideology of bigness, great rooms also express an ideology of family togetherness (Leavitt 2002, Madigan and Munro 1999). They represent a tradeoff of privacy for connectedness and publicness – a tradeoff the implications of which may not be born equally by men and women, or by adults and children. In terms of internal connections, in addition to

being itself a capacious gathering place, the great room is often topologically a central hub, visually and acoustically well-connected to the other spaces in the house. In terms of external connections, unlike a traditional American den or family room relatively removed from the public gaze of visitors upon entrance to the home, great rooms often comprise this public zone of reception, entertainment, and display. Thus, great rooms are open to the same sorts of criticism that *The Feminine Mystique* leveled against the open-plan ranch or split-level houses of 1950's America:

> *There are no true walls or doors; the woman in the beautiful electronic kitchen is never separated from her children. She need never feel alone for a minute, need never be by herself. She can forget her own identity in those noisy open-plan houses. The open plan also helps expand the housework to fill the time available. In what is basically one free-flowing room, instead of many rooms separated by walls and stairs, continual messes continually need picking up. A man, of course, leaves the house for most of the day. But the feminine mystique forbids the woman this*. (Friedan 1983:246).

Although the homes we visited have true walls and doors, and cohabitants all of whom leave for much of the day, the gist of Friedan's argument remains intact, and questions of privacy, identity, and great rooms complex and unsettled.

IMPLICATIONS FOR TECHNOLOGY DESIGN

But why should these political and cultural issues around great rooms, however interesting from the standpoint of social science, make them interesting from the perspective of ICT research and development? We think there are at least two arguments to be made in this regard.

Going with the Flow, and/or Counter-Flow

First, issues such as bigness/wastefulness and togetherness/invasiveness – and the underlying cultural conflicts of which they are symptomatic – can directly affect the success or failure of particular technologies in this domain. Technologies could be designed to take advantage of cultural currents, such as cults of immersive bigness or family togetherness, or to serve counter-trends such as "not so big" homes (Susanka 1998) or "living together apart" arrangements (Levin 2004), but only if these currents and their eddies are understood as they play out in practice, not just in theory. Often both trend and counter-trend are active at once.

For example, while most of the households we visited all aspired to a goal of togetherness and did use their great rooms to achieve this in everyday life, it would be a mistake to assume (and to design technology that assumes or requires) togetherness in the great room takes the form of joint, focused engagement in a shared activity. Such activities did occasionally take place, but the keyword here is occasion – the special TV show, the spontaneous Monopoly® game. Much more frequent was hanging out together, each individual in their designated spot, each engaged in their own activity, but aware of and enjoying each other's presence. Such awareness, and the flexible boundary management it affords, would seem a promising direction for new technology.

Engaging Unsettledness

Second, we think unsettled domains, such as great rooms, may present opportunities for ICT adoption not despite their unsettledness, but because of it. Some, perhaps even most, sites of potential technological intervention may be fairly static and mature, well-understood by a large consensus of their various stakeholders – users, designers, regulators, vendors. In these cases, the primary problem of technology design would be one of assimilation: fitting in. Even in more dynamic or immature contexts, this is not a bad heuristic – certainly better than supposing people, institutions, and environments will adapt to whatever technology is made available or deployed.

But we would like to suggest that in many cases there is not a well-defined or objective domain for the technology to assimilate to, that the role of technology is not to provide an answer to a well-posed question, but to participate in an ongoing process, responding to and challenging concerns, not just "solving" them. (In discussing processes of continual refinement and artful design in the home, Taylor and Swan (2004) espouse a similar approach.) The goal in such cases would still be to fit in, but to fit into a messy, dynamic process, and not necessarily in an unambiguous or uncontentious way. For some technologies, there may be more opportunity to engage a domain when it is in dispute, rather than waiting until it becomes better, and perhaps more narrowly, defined.

An illustration of the potential virtue of unsettledness from our field visits is the way that laptops had managed to make their way into the great room, though not without some degree of discomfort and tentativeness. Perhaps at some future point, great rooms will have more well-established norms for what is and is not permissible in them, and laptops (or more specialized successors) will clearly either fit or not fit into this worked-out schema. But at present, we found the fit between great rooms and laptops to lack this kind of clarity, and potentially to the technology's benefit. In some of our households, laptops were not allowed to be *based* in the great room. Nevertheless, all our households allowed laptops to be *used* in the great room, to fit into one of its multiple (and often conflicting) purposes. And indeed laptops could be left there for long periods of time, since, we think, they could be "tidied up" and returned to their rightful home base, in the office or bedroom, as required. This "welcome but don't stay" policy on laptops may not be stable in the long term, but it did appear to be working in practice.

More Workable Unsettledness: Great Rooms as Workplaces

This discussion of laptops in the great room naturally leads to one of the main themes that emerged from our observations and conversations in all of the homes we visited: the great room as a work space. Great rooms, even those incorporating a kitchen (Bell and Kaye 2002), were not primarily about work and productivity; they were about pleasure and sensory experience. Regardless of the presence or absence of technology, great rooms were valued as pleasant spaces to enjoy, in terms of light, space, views, décor, unclutteredness. All of these attributes were sources of luxury and delight. For many of our households, their plush couches and comfortable openness made them lovely places to take a nap or otherwise just spend time.

But these hedonic aspects and uses did not preclude great rooms from being used as workplaces as well. They were often sites of individual, solitary work, be it folding laundry, working on a school assignment, or wandering about with cordless telephone and muted headset on a conference call meeting. They could be briefly taken over for a project – from homework to laundry folding, with space to spread out materials – though rarely could they function as a project room, in a persistent

state of clutter and "work in progress." In part this was because they were public, social, connected spaces: they could be used for work, but only when consistent with this more fundamental property.

They were thus, unsurprisingly, valued as sites for working while in the presence of others, in a kind of social-multitasking or "being alone together" kind of mode. This generalized somewhat to more tenuous sorts of social presences, ranging from being able to hear the presence of others in the households behind their closed doors, or being kept company by people on the TV, or even just feeling connected to the outside community by looking out into and hearing the surrounding environment. For those who worked at home, it was a welcome relief from being cooped up in their home offices, sitting at their desktop PCs.

Just how much a great room could be a work room was by no means a settled issue in the households we visited. Great rooms were part of a larger system of workplaces, with other nodes in home offices or bedrooms. Significantly, these other locations offered affordances that were perceived as unavailable in the great room, notably privacy and project space to spread out materials and leave them for extended periods.

Work technologies could be brought into the great room and used there, but their being based there was far more problematic; it was as if this threatened to officially make the space a workplace, on the record so to speak, not just off it. We would predict that great room furniture and design will evolve along similar lines, allowing for work but only in reversible or unofficial ways – as was the case of a coffee table, demonstrated to us with pride, out of which a cantilevered desk surface could emerge, but be retracted and hidden when finished. The whole issue of ICTs allowing the workplace to move into the home, and creating obligations by workers (and their families) to repurpose and multi-purpose their dwellings, is only just beginning to be felt, and great rooms are a space where we would expect to see this culture's conflicts in this area play out.

FUTURE DIRECTIONS

Many research directions lead from the topic of great rooms. They are surely just one example of a larger class of various new multipurpose public/private spaces, creating unsettledness at different scales and in different cultures. They raise questions about the trajectory of the housing industry and the spaces it produces and markets, and of the social norms formed in response to new types of home spaces, activities, and technologies. They call attention to the experiential delight of openness and connectedness, and its contrast with (as well as potential enhancement by) most ICT-based experiences. But they also call attention to the ecological damage and potential behavioral coercion that can be caused by infrastructural choices, and the potential for ICTs to mitigate as well as exacerbate these effects. Finally, we hope this discussion of great rooms as a starting point for research serves as a useful illustration of a research style that looks for, rather than avoids, trouble and ambiguity.

Acknowledgments – We thank our colleague Ken Anderson for suggesting great rooms as a fruitful area of exploration, and for many stimulating and thought-provoking discussions. Thanks also to Nina Wakeford and the participants at the 2005 INCITE Privacy Workshop at the University of Surrey for spirited discussion and reflection on an early version of this material. And we thank Ryan Aipperspach at the University of California, Berkeley for assisting with the fieldwork.

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FERTILE GROUND: HOMEGROWN LOYALTY MAKES FOR GLOBALLY COMPETITIVE INDUSTRY

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This paper proposes a theory to explain how rural sociality has influenced workforce behavior and productivity at a Global Manufacturing Systems' automotive assembly plant in mid-Michigan. The paper argues that for over 100 years, rural and farming families in the region have been appropriating GM factories in order to sustain their rural life ways and remain part of their own 'moral' community. Loyalty to the company is conceptualized from the families' perspective as a requirement of sustainable communities, motivated by an intergenerational desire to keep GM in Michigan. Employee loyalty also benefits the company by ensuring high performance and quality. The link between sociality and performance is illustrated through statistical modeling of attendance data and maps produced through ArcGIS.

BACKGROUND

In this paper we provide a theory to explain how rural sociality has influenced workforce behavior and productivity at General Motors' (GM) Lansing Grand River (LGR) assembly plant. LGR was the first facility in North America designed around GM's Global Manufacturing System (or GMS). The heart of GMS is lean production, the elimination of waste[xix], and the standardization of practices as a basis for continuous improvement. GMS provides a set of both high and low technology tools that permit employees to participate in taking waste out of the manufacturing system. LGR produces the Cadillac CTS, CTSV, SRX, STS, and STSV models, which are all based on a common architecture. Since the plant began production in 2001, its products have had notable success in the marketplace, and the plant has emerged as a leader in quality luxury car production in the U.S. market. In the 2004 J.D. Power and Associates Initial Quality Study, vehicles produced at LGR ranked the highest in North and South America, and third in the world, for initial quality[xx]. In 2005, the Harbour Report named LGR the 'benchmark' (best in class) plant for luxury vehicle production. There is great interest by a

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number of stakeholders as to the source of this success, and the competitive advantage it conveys to the Cadillac brand. Our research revealed a link between sociality and worker performance.

Our study began as an industry-university partnership. The university team included scholars from Michigan State University's (MSU) School of Labor and Industrial Relations (SLIR), Eli Broad Business School, and Department of Anthropology. LGR Human Resource (HR) management was interested in the project because it promised to tell the story of LGR's success, and the plant's unique technological and "people system" that had never before been documented. There were various hypotheses regarding the plant's success. Our SLIR colleagues thought the success derived from the unique collective bargaining agreement between the UAW Local 652 and LGR. The anthropologists focused on the cultural, historical, and community-based roots of the cooperative union-management relations and special "people system" at the plant. We began to explore our hypotheses in 2004 with a volunteer sample of 54 employees that we engaged in personal interviews and three focus groups, as well as direct observation in the plant, and participant observation during GMS training. Our interviews revealed a 'native hypothesis' for the plant's success. Both hourly and salaried workers told us that the plant's success should be attributed to the values that had been instilled in the workforce during their socialization in the mid-Michigan area surrounding the plant, particularly experiences that workers had within rural and farm-based families. Our interviewees described several interrelated characteristics of the workforce that they believe were instilled during socialization in a rural community. These included: discipline, punctuality, dedication to long hours, commitment, loyalty, pride in quality, and the desire to "do the right thing." Their 'native hypothesis' was that the plant benefited from their rural sociality which taught them that in order to succeed one must a) show up to work, b) work hard while there, and c) make a quality product.

A PLACE-BASED THEORY OF COMPETITIVENESS

The ethnographic data led us to theorize a place-based model of competitiveness. We postulate a link between three place-based propositions, the combination of which has direct implications for productivity. These include: 1) socialization in a Midwestern, rurally-based family/community instills particular norms and values which persist to form a key part of one's identity (these norms and values were itemized above); 2) working at a plant with others who also have been socialized in the same manner (either relatives or management, or both) re-enforces and enforces expected normative behavior (e.g., family policing); and 3) members of the workforce choose to continue living within these rural communities, creating a loyalty to other community members, and generating an on-going incentive to sustain their place of employment, which in turn sustains the viability of a rural life-style. The following three quotes illustrate these propositions.

> *Us from the smaller towns, basically we do our jobs and we're more patient, more disciplined on how to get there...we're more disciplined in our culture. It's because the way we were raised. I had a lot of morals instilled in me growing up. I learned really quick in life that there's rules and you have to follow rules, and there are always repercussions if you don't. I come from a disciplined household.*

> *If your father went to work every day, you more or less did too and if you didn't, he jumped on you maybe more than your boss ever did. I think it made us a little more aware of [being] fortunate to have a good job, fortunate to have to go to work.*

The town I'm from [has] a very high percentage [of people who] work in the automotive industry…and I think that's an issue too because I myself feel a lot of pressure living in that environment, knowing – I guess you have a loyalty – you know all your neighbors and family members rely on the business that is sustained over the years. Versus if I move to California and work at a plant for two years knowing that I'm leaving. The commitment, the loyalty, I think, is not there. It [moving for work] is more personal interest versus I know I'm living there forever and I'm surrounded by people that work in that industry and I'm going to do my best to make this thing work.

Based on our interviews, we postulated that rural workers want to remain a part of their geographically based community because they believe rural sociality is responsible for their "good" and "moral" qualities, but they need external economic resources to sustain their rural way of life. The workers appeared to have sacralized the places in which they were socialized. We hypothesized that their loyalty to their home communities motivated them to 'hold the plant down' with an exceptional work effort that has direct implications for productivity and quality in the plant. We conceptualize the relationship between these rural families and the industrial plant as one of *loyalty*, but not traditional corporate loyalty that has been presented in the literature as a top-down management strategy.

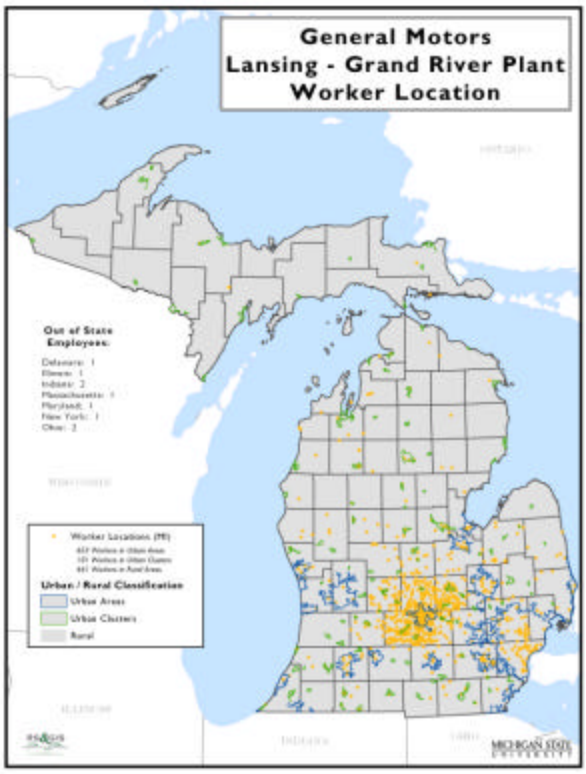
Continuing social bonds, both within intergenerational families and in small communities, place pressure on their members to engage in practices that will ensure that neighbors and families can sustain themselves. Loyalty thus has a community or family origin and a localized, situated or 'rooted' nature, since it depends upon actual and presumed continuity of social relationships. We postulate that bonds of loyalty within the community are transferred to the plant through a strong work effort, one that enables the plant to be sustained so that the community can be sustained. LGR and General Motors benefit from this transference of family and community loyalty, which translates into high productivity and high quality products. The transference of loyalty is a mechanism through which the community exchanges its resources for those of the plant in a form of local-global symbiosis, where both parties gain something that neither could gain alone, resulting in economic competitiveness for the region.

METHODS

When we told HR personnel (many of whom were from rural Michigan themselves) about this theory, they became interested in supporting us so that we could test it. It was at this time that we expanded our research team to include expertise to facilitate statistical analyses and GIS to visually represent our findings. HR granted us increased access to demographic and performance data for the entire plant population. Moreover, they facilitated a mail survey with the entire hourly population (n= 1685) to garner information about location of employee socialization. We received a 27% response rate.

We have two different geographic databases: 1) the current residences of 100% of the hourly workforce, and 2) the location of socialization for 27% of the hourly workforce (i.e., 454 of the 1685 workers responded to the mail survey). Comparison of worker current residence with the survey data collected on place of socialization showed us that the places people were talking about in our interviews are places where workers still reside (50% of the workforce currently live in "rural" Michigan).[xxi] Survey and interview data also suggest that many other workers who were socialized in rural Michigan currently reside in urban areas. The following map visually displays the current

geographic distribution of LGR's hourly workforce. The map was produced through a process called "geocoding," which creates map features from addresses. The orange points reflect the current mailing addresses of all hourly employees.



In order to test the hypothesis that rural workers have higher performance levels than urban workers, we are conducting statistical analyses on the relationships between five performance areas and the geographic place ("rural" or "urban") of worker residence as well as the geographic place of socialization. Worker performance is tracked in five areas by LGR, known as the PSQRC metrics. These performances areas are: 1) People, which is an attendance log; 2) Safety, a record of lost workdays and on-the-job injuries; 3) Cost, a log of wasted materials; 4) Quality, the degree to which expectations are met or exceeded; and 5) Responsiveness, operating under the short lead time principle (i.e., addressing problems and limiting assembly line downtime). Analyses are still in underway, but we do have some key conclusions for one of the performance metrics: attendance.

We selected attendance as the first section to analyze because it is arguably one of the most significant performance areas in a GMS lean manufacturing plant. The hourly workforce operates in teams comprised of 4-6 team members who rotate jobs throughout the day. The team is led by an hourly (i.e., union represented employee) team leader whose responsibilities include decision-making, job assignment, assistance with repairs, and taking over during bathroom breaks. Unlike traditional plants, facilities that run "lean" do not build in floaters (i.e., extra buffers in the hourly workforce to fill in for absent employees). Therefore, whenever a team member needs a bathroom break, requires help with a problem on the line, or is absent, the team leader must work on the line, prohibiting the team leader from fulfilling his/her normal duties. If the team leader is online, there are no other hourly workers ("floaters") to step in for their co-workers for breaks or repairs. Help must come from the group leader, who is a salaried employee in charge of all teams within a work group (groups usually consist of 3-6 teams). If two members of a team are absent, hourly team members and team leaders and group leaders from other areas of the plant are temporarily reorganized to replace those absent.

One can see from this description how as few as two unplanned absences could have a significant impact upon worker productivity and the overall quality of the vehicles.

Attendance is tracked at the individual, team, and group level: 1) HR personnel log individual attendance in two categories: 1) contractual absenteeism, or absences permitted within the GM-UAW collective agreement (e.g., bereavement, sick leave, vacation) and, 2) controllable absenteeism, or unplanned absences (e.g., the employee does not show up). We used year-to-date (January-July 2005) controllable absenteeism and perfect attendance records in our statistical analyses. Attendance is also monitored on a daily basis by the team leader and group level for both day and night shift.[xxii] The data is recorded on oversized paper and hung on large poster boards throughout the plant. The fact that the plant has hourly workers track their own attendance and display it publicly for all to view even though attendance is already monitored by HR, forces mindfulness of work behavior and illustrates the significance of attendance in a lean manufacturing plant. We hypothesize that attendance is a key driver of productivity and should therefore affect other performance indicators.

FINDINGS

To test the hypothesis that workers from rural areas have better rates of attendance than workers from urban areas we ran a one-way analysis of variance (abbreviated ANOVA) with a 100% sample of hourly workers. The results of this test (Table 1) indicate that there is a significant difference in the net year to date absenteeism hours between rural workers and urban workers. The F statistic is equal to 3.846 degrees of freedom numerator = 1, degrees of freedom denominator = 1682) and is significant at .05 confidence level. On average, the 926 rural workers were absent for 17.52 hours, while the 758 urban area (and urban cluster) workers were absent 21.66 hours. As such, rural workers have significantly lower levels of absenteeism when compared to urban workers, leading us to reject our null hypothesis.[xxiii]

Table 1: ANOVA Results for Rural-Urban Attendance

	Sum of Squares	Df	Mean Square	F	Sig.
Between Groups	7138.818	1	7138.818	3.846	.050
Within Groups	3121820.518	1682	1856.017		
Total	3128959.337	1683			

Further, to determine the link between attendance and other factors, such as safety, a correlation analysis was performed using the PSQRC team performance. The results indicate a significant positive correlation (.192, $p < .01$) between perfect attendance and perfect safety. This suggests that attendance is a key driver of other performance metrics.

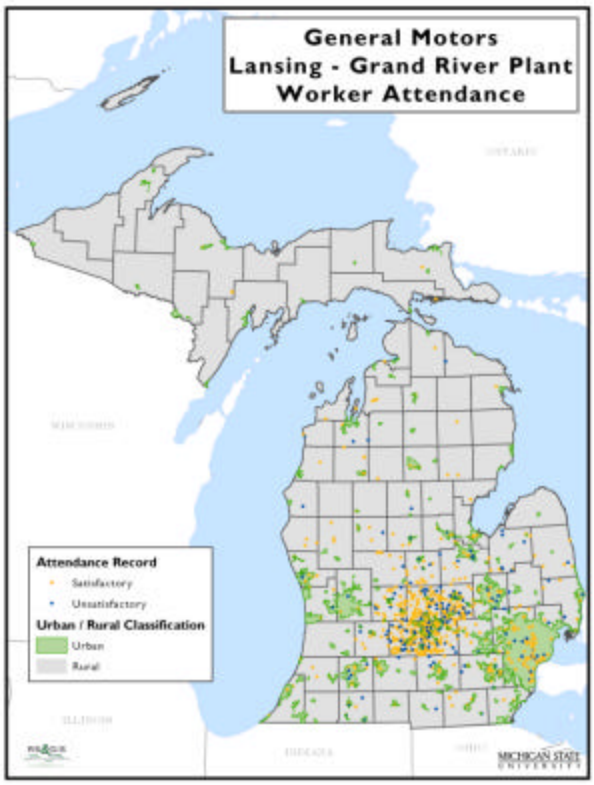
Analyses of the survey data (Table 2) support the results from the 100% sample, in that they revealed that rural workers have significantly lower levels of absenteeism than urban workers (significant at the .1 confidence level). The survey data enabled us to look at the relationship between attendance patterns and other variables, including differences for employees hired through referrals (as opposed to open hires), for workers who transferred from out of state (as opposed to individuals who spent their lives in Michigan), and for individuals who had a long daily commute to work.

Table 2: Logit estimates for the effects on perfect attendance

Covariates	Coefficient	Std. Error	z	P > |z|
RURAL	1.071	.643	1.67	.096
REFERRED	1.768	.708	2.50	.013
OUT OF MI	-2.534	.940	-2.69	.007
DISTANCE	-.004	.018	-.21	.837
RURAL	1.071	.643	1.67	.096

Note: control variable effects omitted from table

Our findings show that workers referred to GM have significantly better attendance than those who were hired through open-hires (significant at the .05 confidence level). Moreover, we found that individuals who transferred from out-of-state have poorer attendance habits than workers transferred from other places in Michigan (significant at the .01 confidence level). Further, the length of commute did not have a significant impact on worker attendance. All of these recent findings support our place-based theory of competitiveness. While we do not have concrete data, we speculate that most workers refer family or other members of their community for work in the plant. That referred employees have better attendance suggests a transference of loyalty from the community to the factory. Worker home communities could be nearly two hours away, but their loyalty drives them to make the regular commute without having a negative impact on their attendance. Our theory is further reinforced by the finding that transfers to the state have worse attendance patterns – their loyalty is not to mid-Michigan, but lies elsewhere.



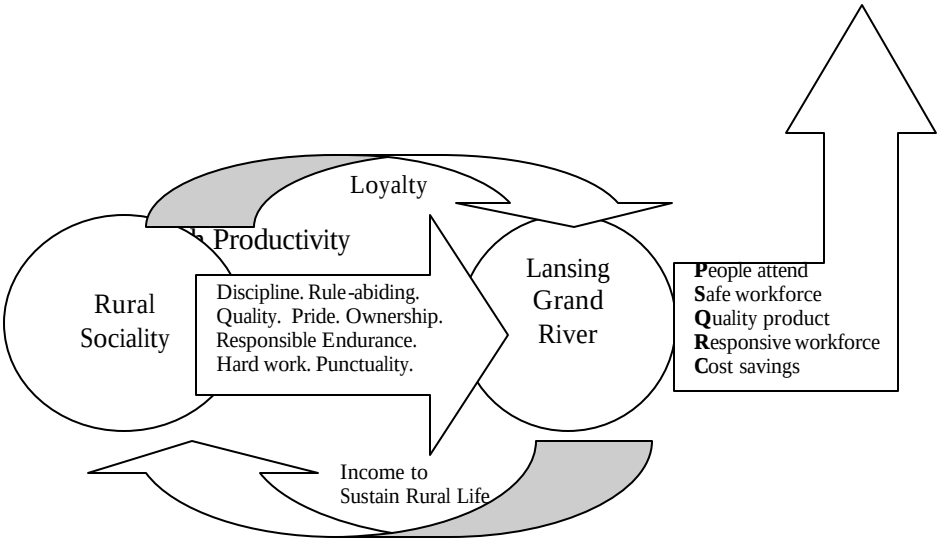
COMMUNICATING FINDINGS

To communicate our findings to management about rural versus urban sociality and attendance patterns we employed ArcGIS Desktop software. The figure to the right is an example of the maps we produced. It displays attendance data for the hourly workforce by placing dots on their current residence. We used year-to-date net percent absenteeism and applied the GM corporate standards for acceptable levels of absenteeism. Orange dots signify workers with an acceptable level of absenteeism (this category includes individuals with perfect attendance). Blue dots signify workers with an unacceptable level of absenteeism. The areas shaded in green are urban areas and urban clusters. While it is difficult to see in this map, when satisfactory attendance and unsatisfactory attendance are displayed on separate maps, rural and urban worker attendance levels are distinctive.

THEORETICAL IMPLICATIONS

Our conceptualization stands corporate loyalty on its head: loyalty also may emerge from below - from the generations of rural and farming families who comprise the LGR workforce. Loyalty springs from a geographically situated, 'moral' community of these employees, and from there it is transferred to LGR via a system of team-based work practices that share certain features with farm work (e.g., flexibility, discipline, dedication to hard physical labor and long hours). The workforce, in turn, obtains 'loyalty' from General Motors in the best way they know – with outstanding, globally competitive work that yields return on investment. In this scenario, the employees receive more than wages; they retain a way of life.

The following image models our theory.



MANAGERIAL IMPLICATIONS AND LIMITATIONS

The findings of this initial study offer some potentially interesting insights for managers. One implication is that employees are motivated to comply with social norms. Therefore, it may be beneficial for the management at LGR to institute programs and activities that will help to instill a sense of community within the plant, particularly as new workers from other locations are transferred to the facility. Additionally, it appears that employees' allegiance is to the community, rather than to the corporation or the plant. The findings suggest that the self-interest of the individual employees, who wish to maintain the nature of their community, drives their performance. The implication for managers is that integrating the plant with the community (through a variety of involvement initiatives), as well as clearly elucidating the joint destiny of management and workers, will help to ensure the maintenance of a high level of productivity and quality measures. Plant management should continue and their community-building initiatives, and it may be advisable for GM's corporate group to pursue greater efforts along these lines to increase loyalty to the organization as a whole.

In order to add greater depth and breadth to the contribution of this study, a more in depth survey based on the development of latent constructs and survey responses of the entire plant population should be conducted. This ongoing study has been undertaken in the very limited context of one industry, and in one geographic location; therefore, the findings are limited in generalizability. Future studies can extend this research to other contexts, for the purposes of both comparison and extension of the conceptual development.

NOTES

Acknowledgments - The authors would like to thank Mike Reinerth,Vickie Gaudard, Dan Wedley, Pat Lefevre and the employees of GM's Lansing Grand River Assembly (LGR) plant for their participation and support of this study, and General Motors, who generously provided financial support through a GM Foundation Grant.

[1] Reduction of waste is the responsibility of everyone in the plant: corrections-errors, overproduction, material movement, motion, waiting, inventory, processing (do what is necessary, but no more).

[2] The J.D. Power and Associates study measures the number of problems consumers experience during the first 90 days of vehicle ownership.

[3] We are analyzing our data using the 2000 Census classification for Urban and Rural territory. Urban Areas consist of an urban nucleus of 50,000 or more people with a population density of 1,000 people per square mile and associated area with a population density of at least 500 persons per square mile. Urban Clusters meet the same criteria as Urban Areas, but have populations of 2,500 to 49,999. All other territory is classified as rural. For the purpose of our analyses, we collapsed people from Urban Areas and Urban Clusters into one category of "urban workers" to compare with a second category of "rural workers." We combined the two because the population of workers who reside in Urban Clusters (n=101) is too small to provide statistical significance when comparing with the populations of Urban Areas (n=653) and Rural Areas (n=641).

[4] We have proprietary data that we cannot include in this paper that explains in more detail how attendance is tracked as well as GM corporate metrics regarding acceptable levels of absenteeism for the plant to run smoothly.

[5] We were concerned about the racial implications of our rural sociality hypothesis, given the demographics of the plant population. Ethnic minorities comprise 20% of the hourly workforce and

LGR is in full compliance with EEO and even has its own self-imposed affirmative action procedures for hiring women and minorities. Their goals are set based on the Standard Metropolitan Statistical Area and the plant continues to meet their affirmative action goals each year with regard to hiring women and minorities. Our statistical results indicate that there is no significant effect of minority status on attendance. One-way ANOVA results indicate an F statistic of .387 (degrees of freedom numerator = 1, degrees of freedom denominator = 424) for minority status and this is not statistically significant at a .10 confidence level. Minority workers have an average absenteeism of 13.43 while the non-minority workers have an average absenteeism of 17.71, but the difference is not statistically significant at the .10 confidence level. Although the comparison of the groups does not yield statistically significant results, we do see that ethnic minorities, on average, have relatively good attendance habits (i.e., a mean score of 13.43 hours missed year-to-date). Yet we did find that gender has a significant affect on attendance (women comprise 20% of the hourly workforce). The ANOVA results for the survey data indicate a significant effect of gender on attendance. The F statistic is equal to 8.117 (degrees of freedom numerator = 1, degrees of freedom denominator = 424) and is significant at .01 confidence level. On average, the 84 female respondents were absent for 29.40 hours, while the 342 male respondents were absent 21.61 hours. As such, among the survey respondents, male workers have significantly lower levels of absenteeism when compared to female workers. The relationship between gender and performance levels is a topic for further research and something we hope to explore in our next phase of the study.

TO THE END OF THEORY-PRACTICE 'APARTHEID': ENCOUNTERING THE WORLD

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A historical and comparative examination of ethnographic practice in sixteen nations around the globe reveals that theory-practice relations in anthropology and ethnography (A/E) have been shaped and re-shaped over time and space by complex contextual influences. This paper explores the evolution of theory-practice relationships in A/E over various regions of the world, tracing the beginning of a theory-practice 'split' from its origin under British colonialism, to its reappearance and institutionalization in post-World War II America, and postulating its absence in the 'Second and Third Worlds'. Global practice in ethnography now appears to be converging toward a re-integration of theory and application across multiple disciplines and professions (a 'hybrid' approach), as ethnographers work to address urgent and poorly understood problems that are not well theorized.

INTRODUCTION

Rick Robinson's opening remarks challenged us with the thought that there is a potential theoretical space represented within the frames of practice that we have contemplated here, but that the space often has not been explicitly engaged intellectually. Others have noticed the theoretical lacuna in applied ethnography and practice, whether in industry or cognate fields. For example, in a major review for the journal *Current Anthropology*, Bennett's (1996) chief criticism of applied anthropology was the failure to produce its own body of theory, and the tendency to engage in a horizontal build-out of case studies with no effort to synthesize vertically. There are many reasons for this pattern, all readily understandable, but it is not my purpose to root-cause analyze this pattern or recommend ways to address it.

Rather, in agreement with Robinson, I would like to take up the idea that theory is action -- the potential to change what we might become in the future. Toward that goal, I will share a perspective gained from a study of anthropological and ethnographic (A/E) practice in sixteen nations around the globe that has been on-going since the early 1990s, and what has been learned in that project about the so-called 'split' between theory and practice[24] (Hill and Baba forthcoming). Through an historical and comparative examination of theory-practice relations in A/E on a global scale, we can see that this interface has been shaped and re-shaped, historically and culturally, over time and space. If such relations have been different in the past, then why could they not change in the future? This is the territory I will explore in these closing remarks[25].

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BRITISH COLONIAL LEGACY

By now it is commonplace to acknowledge that the theoretical project of British social anthropology was financed and legitimated under the auspices of empire (Mills 2002). What may not be recognized, however, is that our current view of this fraught relationship between theory and what we now see as a tainted form of practice is refracted through the lens of our times – an era in which academic institutions are culturally dominant and hold a near monopoly on the criteria for determining professional status. In the past, relationships between pragmatic and scholarly interests were fuzzier and more entangled than the received version would have us believe. Indeed, academic anthropology only emerged as an autonomous academic discipline after the 1930s (Mills 2002); before that time the ethnographic tradition was shared with a constellation of professionals and amateurs that included colonial administrators, missionaries, and travelers. Scholarship and pragmatics were more or less co-mingled in the ethnographic practices of the early days, and only became separated from one another after anthropology was captured by academia and the British colonies fell. The boundary between theory and practice thus is sensitive to contextual influences, and may shift over time.

My first glimpse of a theory-practice interface different from that which we are accustomed to today came when I noticed that there are only two countries, and only two points in history, in which the term 'applied' anthropology appear: 1) in Great Britain during the colonial era, after which point the term 'applied' was essentially dropped, and 2) in the United States, from before the time of World War II up to the present[26]. In all of the other fourteen countries represented in the sample of cases, there was ample evidence of applied *activity*, but without formal institutions named as such. Many anthropologists and ethnographers in these other nations (regardless of whether or not they are academic or non-academic) engage in what might be called 'applied' work – that is, they use anthropological and ethnographic knowledge and methods to address problems beyond those defined by an academic discipline. Yet, the name 'applied anthropologist' is not used. As an anthropologist who recognizes that names reflect cognitive categories and cultural meanings, I wondered about this separation of activity from nomenclature, and its potential significance for theory-practice relationships.

I set out to explore the origin of the name 'applied anthropology' and learned that the term dates at least to 1881 (see Gardner and Lewis 1996), when British anthropologists used it to advocate the potential utility of their emerging profession, which did not yet have a firm constituency (Kuper 1983). In the years before World War II, the British colonial government provided virtually no funding for social science research in Africa, the main theatre of anthropological and ethnographic operations (Mills 2002). There was funding for training of colonial administrators in anthropological and ethnographic skills, and that is how the first anthropology department was founded at Oxford (Van Willigen 2002). Some colonial governments created positions for a 'government anthropologist', and even sponsored applied studies that anthropologists could take on, but otherwise, anthropologists and ethnographers had to be creative in their search for funds. According to Adam Kuper (1983), leading anthropologists such as Malinowski and Radcliffe-Brown became known in the 1920s and 30s for touting the practical virtues of anthropology and ethnography as means to address colonial problems, but this was primarily a 'sales pitch' aimed at securing ad hoc research funding. Kuper (1983) argues that once the money was in hand, anthropologists were likely to do what amounted to a 'bait and switch', conducting a basic research investigation, assuming that the colonial sponsor could extract the necessary information from it, without the anthropologists' help. Further, according to Kuper (1983), many British anthropologists at the time were functionalists and/or liberals, and therefore were both theoretically and/or ideologically disinclined to aid and abet the administrators' interests in

understanding 'social change' (which many anthropologists viewed as 'dangerous'). Needless to say, these proclivities did not endear the anthropologists to the administrators (the latter often stereotyping the former as 'romantic reactionaries'). Another anthropological practice of the time was to assign one's protégé or a junior scholar to do an applied study, as such work was thought to be better suited to less well prepared individuals:

> *When, more or less reluctantly, the anthropologist 'did some applied work', he tended to pick one of a limited range of topics. (I say he, but applied work was often regarded by the more mandarin as less demanding intellectually, and therefore as best suited to women. Malinowski's first student to be dispatched to do a study of 'culture change' in Africa was chosen because it was thought she was still too new to anthropology to do a conventional tribal study)…(A/Es participated) only grudgingly (as a rule) in the little studies dreamt up by the administrators, and accepting the view that they should not speak out on matters of policy, not being 'practical men'.* Kuper (1983:110-12)

Kuper goes on to argue that as a result of such practices, A/E was little 'applied' in the British colonies, either by the colonial governments, or by the A/Es themselves. However, other scholars have noted that there was complicity and symbiosis between the two, as the A/Es used the *promise of applied solutions* as a means to extract funds to do basic research, and this is how the initial theoretical foundation of social anthropology was formulated (Mills 2002; see also Pink forthcoming).

My interpretation of the foregoing is that early colonial practices established an implicit two-tier model of knowledge production in anthropology and ethnography, and that this model provided the grounds upon which theory and practice later were separated. The first tier was reserved for free-wheeling 'pure' theory, with the 'Other' or second tier intended for more short-term, derivative 'applied' studies. In accordance with the paternalistic tendencies of colonialism, the Big Man was given 'right of first refusal' to the second tier, but generally speaking, it was considered peripheral work. By implication, those who were assigned to work on the second tier, however, could not choose to work on the first tier. This implicit two-tier structure was not so cut and dried as I am portraying it, however, because there were very few academic posts available, and A/Es had to be flexible regarding postings.

Things began to change in the 1930s and 40s with shifts in British colonial policy and the beginning of World War II. As a response to critics who charged that the colonies were isolated and not 'developing' economically, it was decided to engage in more affirmative administrative planning that could provide a stimulus to the economic growth of the colonies. Funding began to flow toward social science research in Africa during the 1930s through a number of mechanisms, including grants from the Carnegie Corporation and Rockefeller Foundation. Some of these funds supported anthropological research in Africa (Kuper 1983). Then, in the 1940s, the British enacted the Colonial Development and Welfare Act (CDWA), a legislative reform agenda for the colonies that finally provided substantial government funding for social science research in the colonies, including funds for anthropology and ethnography. The principal contextual shift prompting this official change in policy was the start of World War II in 1939, and Britain's need to respond to those who criticized its empire (especially the Americans). Mills (2002) argues that Lord Hailey, a pro-colonial architect of the

African research program, viewed anthropologists as integral to this work, and fended off criticisms and slights of the discipline from other quarters.

Importantly in the present context, Mills (2002) demonstrates that the CDWA intended that both 'pure' and 'applied' research be included within its purview, such that scientific knowledge could advance. This suggests that in 1940, official British policy encouraged the integration of theoretical and pragmatic interests; indeed, the CDWA formalized this linkage. A Colonial Social Science Research Council (CSSRC) was established to set and implement policy for the allocation of research funds that would fulfill the CDWA mandate. Initially, it was anthropologists at the London School of Economics (LSE) who became most closely affiliated with the CSSRC (Raymond Firth and Audrey Richards). This is significant, since both were protégés of Malinowski, one of applied anthropology's great protagonists, and they embraced the reformist goals of the CDWA and were proponents of integrating scientific and pragmatic research objectives. Mills (2002:171) notes:

> *There is a little doubt that the members of the CSSRC saw themselves as intellectual pioneers, leading the way both in mapping out uncharted territories of African social research problems, and in trail-blazing the new possibilities for a problem-oriented multi-disciplinary social science.*

The CSSRC supported four regional research institutes in Africa and the Caribbean that were intended to strengthen colonial research capabilities as part of the reformist agenda. The new research council also was centrally engaged in the selection, training, supervision and support of younger scholars to pursue research through these institutes. Since it was difficult to identify suitably prepared candidates for training, the council often dismissed the traditional disciplinary requirement for language skills or previous time in country, and instead developed its own training regimen that was successful in preparing a substantial number of young scholars for fieldwork. Some of these trainees later made important contributions to the discipline (Mills 2002).

A sharp distinction between 'pure' and 'applied' research, such as some would describe these activities today, was not evident in work supported by the CSSRC. The government did not attempt to micromanage the research agenda, as it recognized the importance of advancing the knowledge base overall, and viewed both fundamental and pragmatic research as pursuant to this goal, since little was known about human society in Africa. Also, it should be noted that while colonial social problems provided an overall context within which research was framed, anthropology was able to transform such research into a satisfying theoretical product that could enable the discipline to gain legitimacy within the academy (Mills 2002). Without this transformation, the entire project would have collapsed; anthropology had to emerge as a 'science', or it would not receive funding. That this process was successful is attested to by the fact that the number of academic departments and positions in anthropology grew steadily over this period. By 1953, there were 38 teaching positions in A/E, compared with only a handful prior to 1940. Indeed, an entirely new professional association was spun-off in 1946 to represent strictly *academic* anthropologists (i.e., the Association for Social Anthropology or ASA[27]).

Significantly, however, tensions mounted between anthropologists based at the LSE and those based at Oxford, especially between Audrey Richards and Max Gluckman, the latter being an anti-colonialist who did not agree with CSSRC funding policy. A serious rift developed regarding the funding of the regional research institutes and the program of study for doctoral students[28]. Those at Oxford believed that such matters should be under the control of academic departments, not the

colonial office or the regional research institutes (as was the practice under CSSRC funding)[29]. The opposing sets of interests were both *institutional* (Oxford versus LSE) and *political* (anti-colonial versus reformist). There also was the problem of a perceived pro-LSE bias on the CSSRC (i.e., the other academics had this perception). It must be emphasized, however, that although the Oxford scholars did not hold practical anthropology in high regard, they still wanted to receive the CSSCR funding – they just wanted to control it themselves. Tensions eventually mounted to the point that the ASA demanded a meeting with the British Secretary of State, which was granted, and during the meeting Radcliffe-Brown insisted that academics have more professional control over funding decisions and student training. As a result, anthropologists from different universities (e.g., Oxford, Cambridge) were placed on a CSSRC sub-committee panel to give input to decision-making (Mills 2002).

While the reformists and anti-colonialists battled for control over resources and against each other's institutional agendas, the British empire continued to sink, and would abruptly close with the outbreak of violence in Africa during the late 1940s and early 1950s. After the CSSRC finally wound up its affairs in 1961, it became clear how dependent the British anthropologists had been upon it for student training funds, as these went dry and there were no ready substitutes until later in the decade.

The relationship between theory and practice within the colonial context of Great Britain thus represents a paradox (Mills 2002). The colonial regime provided financial support and a framework within which anthropology could develop as an independent academic discipline. The context for anthropology was decidedly pragmatic, at the very least because the government needed the cover provided by intellectuals who appeared to be doing careful studies that supposedly were going to lead to colonial 'development and welfare'. At the same time, academic anthropology was struggling to emerge as an autonomous profession from the midst of many others that proffered ethnographic skill (colonial administrators, missionaries, and travelers; see Pels and Salemink 1999). Arguably, the struggle between LSE and Oxford was just as much about "the criteria of what constitutes knowledge, what is to be excluded, and who is qualified to know" (i.e., power; Foucault 1971; c.f. Garner and Lewis 1996:71) as it was about anything else. The financial support and the autonomy provided by the CSSRC enabled the discipline to expand and legitimize itself, and, at the end of the day, British academia gained the upper hand in the production of anthropological and ethnographic knowledge. Ironically, however, a part of the process by which anthropological knowledge was produced went 'out of business' when the CSSRC closed down, a shift that was linked to the end of empire. As the British shut down their colonies, 'applied anthropology' was politically disgraced, and academic anthropology ended its use of the 'applied' name. Theory thus became separated from practice, and as empire ended and anthropology lost its pragmatic value, the discipline became increasingly marginalized.

APPLIED ANTHROPOLOGY AND ETHNOGRAPHY IN THE UNITED STATES

Intriguingly, although the United States had no external empire in a formal sense, a similar tale can be told of American anthropology. Prior to World War II, academic and theoretical anthropology in the United States were not well developed, and many anthropologists and ethnographers found positions related to the administration of the internal colonies (i.e., Native Americans; see Gardner and Lewis 1996). The US still was relatively isolationist as a nation, and was focused on its own troubles (e.g., recovery from the Great Depression). It was within this context that an interdisciplinary group of anthropologists and ethnographers joined together at Harvard in May, 1941 to form the Society for Applied Anthropology. The Society was founded by a distinguished group that included Conrad

Arensburg, Gregory Bateson, Ruth Benedict, Elliot Chapple, Margaret Mead, George Murdoch, William Foote Whyte, among several others (Partridge and Eddy 1978). Like the anthropologists at LSE, their goal was to *integrate* 'scientific' and pragmatic objectives, as is clear in the mission statement of their journal, *Applied Anthropology* (later renamed *Human Organization*):

> *to promote scientific investigation of the principles controlling the relations of human beings to one another and to encourage the wide application of these principles to practical problems* (cited in Arensberg 1947:1).

The founders of the SfAA viewed theoreticians as primarily interested in the search for abstract laws or principles, while they were more intrigued by concrete applications of knowledge to specific cases in the modern world, and what could be learned from investigating such instances. They recognized the intellectual synergy between generalized knowledge and practice, and they wanted their own journal, devoted to "attempts to appraise and use the agreed-upon core of knowledge or tested method", which might otherwise be crowded out by 'abstract' science (Arensberg 1947:1).

There are significant parallels between the American anthropologists of the World War II era and their British counterparts at the LSE. Both envisioned their brand of anthropology as a new kind of interdisciplinary practice that could *join science with application on equal terms* to solve important contemporary problems. Many disciplines and professions were invited to join – this was not an exercise in 'pure' anthropology. Attention was warranted by the most distinguished practitioners of the time, names that we continue to recognize today. In both Atlantic cases, the majorities in the anthropological core did not embrace the notion of 'application', but still were more interested in abstract, theoretical pursuits, and ultimately, these mainstream colleagues became more prominent and dominant, even as the discipline was marginalized.

Already discussed were the developments in Great Britain after 1940 that led to an academic monopoly on knowledge production and the demise of applied anthropology. In the United States, the post World War II rise of America as a hegemonic superpower set the conditions for an expansion of academic and theoretical anthropology during the 1960s and 70s, and with this growth came an array of epistemological, political and ethical issues that influenced the status of application. For example, there arose the notion that 'real' anthropologists could only do research outside their home culture and language, and that one must spend at least one year at a field site abroad, if not more (see Messerschmidt 1981). Methodological training was not necessary, as fieldwork was essentially a rite of passage, with the criteria of competency being the production of an 'ethnography', whose quality was judged by others who had produced one. Such epistemological assumptions and standards guarded the gates of professional membership and guaranteed an academic monopoly in anthropology for decades. They also served to de-legitimize and de-value anthropology that focused on contemporary problems within the United States. Another may be found in the ethical and political issues associated with employment outside the academy. As a result of a scandal related to the proposed use of anthropologists in covert military research, the American Anthropological Association's Principles of Professional Responsibility (1971) were written to include a provision that prohibited any research that could not be published openly. This code was in place up through the 1990s, meaning that – if taken literally -- no anthropologist could be employed by an organization that conducted proprietary research during that two decade period. Such subtle and not so subtle stigma tended to lower the epistemological, ethical and political status of application within academia, gradually maneuvering it into an ever more 'Other' category. These moves replicated the two-tier knowledge structure first seen

under colonialism. The top tier was devoted to the production of 'pure' knowledge, and an 'Other' tier was devoted to the production of applied knowledge for 'other' venues. Baba and Hill (forthcoming) hold that only the hegemonic power has resources sufficient to support a 'pure' knowledge production tier with a monopoly on theory, and a second tier whose products are available to be deployed into the expanding realms of the superpower. In the case of the US, some of the first extensions of the second tier were into the arena of international 'development', where practitioners collided head-on with post-colonial academic critics, producing a rift between 'applied development anthropology' and the 'anthropology of development' (Escobar 1995, Gardner and Lewis 1996).

Just as in Britain, the expansion of academic anthropology, with its monopoly on knowledge credentialing, had the effect of producing a split between theory and application. This split had a fundamental difference in the United States because of the popular 'applied anthropology' degree programs that credential anthropological practitioners. My interpretation, however, is that these programs do not alter the existence of what is still basically a two-tier structure; indeed, they embrace and institutionalize it, and endow it with a high degree of internal complexity and differentiation (e.g., see van Willigen 2002).

Ironically, one might conclude from all of this that the integration of theory and practice in A/E is most likely to occur when academic anthropology is not yet fully developed or when it is relatively weak, and that in turn might suggest that such integration could only happen at very specific points in time. Indeed, within the recent past, the post-modern critique of anthropology may have so weakened the discipline's theoretical structures that theory and practice have again begun to merge, but this time via the escape of ethnography from anthropology (as evidenced in EPIC).

THE SECOND AND THIRD WORLD CASES

In the global sample of nations, the institutional use of the name 'applied anthropology', and the two-tier knowledge production model, were not found for the 'Second or Third World' cases. It is postulated that in the former Second World (prior to 1990), the two-tier structure of knowledge production was not necessary and/or not permitted, due to the entanglement of anthropology with 'scientific socialism', and the need for anthropology to serve socialist purposes (Baba and Hill forthcoming). We suggest that there was no 'pure' anthropology per se, for such could be interpreted as a bourgeois conception. Under this hypothetical scenario, only the 'correct' form of A/E would be permitted by the socialist state, and all comrades should adhere to this form, which changed as the party line changed. In Russia, after the 1990s, resource scarcity appears to have militated against the further specialization of roles into pure and applied forms. Many A/Es in universities and research institutes have been forced to take on project work to supplement their incomes (Yamskov forthcoming), and this has led to a further blurring of lines between theoretical and applied roles. No one calls herself 'applied', but many A/Es do at least some 'applied' work.

In the so-called Third World, a two-tiered knowledge structure also is not in evidence, based on data contained in the sample of nations. Visitors from the West generally play the theoretical role, or theory is imported from the West, and 'native anthropologists' are prepared to engage in 'applied' roles often related to nation-building, even if they hold academic posts (Freidenberg 2001). Just about all Third World A/Es do some 'applied anthropology', but few calls themselves 'applied' per se, neither are there formal institutional structures named in this way (at least in the sample). Some Latin

American colleagues have called such structures 'apartheid', and they are glad not to have it (Hill, personal communication, 2005).

When I first heard the term 'apartheid' used in this context I was stunned, for it seemed a shocking and alien way of conceptualizing the American structure of 'applied anthropology'. Yet, if apartheid is a policy of segregation and discrimination based upon 'inherent' characteristics, then perhaps the American approach of creating separate tracks for 'applied' anthropology is a form of 'apartheid'. If that is so, we should question ourselves more seriously about the reasons why we need to keep theoretical anthropology segregated from the production and reproduction of practice.

GLOBAL CONVERGENCE 2000 AND BEYOND

Globalization theory predicts that the world is in the process of de-centering, with the global center of power now shifting Eastward[30], toward China, India and the other nations of the Pacific Rim and Asia (e.g., see Friedman 2003). If such theories are valid, the United States may be slowly losing its hegemonic status, as a new economic platform rises in the East to compete with those in North America and Europe. This shift could represent the latest stage in a centuries (or millennia) long cycle through which global de-centering enables new areas of the world to become centers of capital accumulation. With this shift, resources may be reallocated within and between centers, and new opportunities may emerge for people in places that previously were impoverished. The possible rise of a new economic and technological platform in the East has been theorized as a moment of *convergence* in structures, meaning that social institutions across regions of the globe may become more similar (e.g., educational institutions' standards of competency). We could be experiencing such a worldwide convergence in anthropology and ethnography.

Fischer (2003) has argued that new methodologies and conceptual tools are necessitated by the fact that cultures of every kind are becoming more complex and differentiated at the same time that globalization is bringing them into exponentially increased interaction. He states that anthropology now operates in a series of 'third spaces' beyond the 19th and 20th century dualisms of us/them, primitive/civilized, East/West, North/South, or applied/academic. The opening of a 'third space' gives rise to a peculiar sense of 'oneness' or a blurring of boundaries that anthropology has not evidenced since the dawn of the discipline. The current survey of A/E practice across sixteen nations reveals four dimensions of convergence in A/E practice, including: 1) interdisciplinary research teams; 2) participatory and collaborative methodologies; 3) stronger profiles in policy-making and political influence; and 4) multiple occupational roles (Baba and Hill forthcoming). These common practices are emerging across academic and non-academic settings, and globally across Europe, Asia and the Americas. Each of these convergent features are linked to globalization processes.

In the highly turbulent, uncertain and complex world of the 21st century, intellectual attention shifts to urgent, contemporary challenges and opportunities that are transnational in scope. These issues cannot be interrogated with traditional 'pure' disciplinary concepts and methods. While traditional A/E practices (solo, single discipline, abstract, 'ex otic esoterica') are marginalized, the epistemological heart of A/E has been transformed and is embedded in post-critical approaches to methodology and theory-building (Schweizer 1998). The potential synergy of problem-focused, yet fundamental research is recognized across the natural and social sciences (Stokes 1997), and is producing gains in cutting-edge areas such as environmental science, economic and 'institutional' anthropology, and complexity (e.g., Moran and Ojima 2005, Jian and Young 2002, Agar 2004). Lines

between theory and practice are blurred in such contexts because emergent problems are poorly understood and not well theorized. Those with access to the field may gain information that is crucial to new understanding. Staying cloistered risks failure to comprehend the evolving reality, while old theory is obsolete. Gaining access to the field, however, often requires an exchange of value (i.e., a problem-orientation, a deliverable). A/Es must be able to transform knowledge gained in more pragmatic contexts into theoretical intelligence and not become disillusioned that such knowledge is automatically 'impure' or represents a 'sell out'. Partnerships between academics and practitioners, and across disciplines and professions (i.e., hybrids), can empower such theory-practice transformations. All modern disciplines (e.g., economics, psychology, political science) and professions (e.g., medicine, law) make this transformation and gain knowledge and power from it. Relatively strong links between theory and practice in these fields empower professionals with clout in policy-making and practice, compared with the weaker policy influence of anthropology and ethnography, fields that have negligible links between theory and practice. Academic anthropology has been hung-up on the theory-practice interface, continuing to express a dysfunction, as if our discipline had experienced a childhood trauma that wounded us deeply. It is time for us to join our British colleagues in the process of confronting the past, coming to terms with it, and moving beyond its limitations.

Just as the ethnographic tradition once belonged to many professions and was no captive of the academy, so today that same tradition has burst out of the academy and now is in the hands of many 'Others'. Its economic value has been recognized, and with that acknowledgement comes its transformation from the world of 'pure' to the 'real world'. I am not certain that the academy recognizes what has happened, but that doesn't matter. The transformation points to a contextual shift – the emergence of a change in the theory-practice boundary. Time has moved on, and the boundary is blurring once again, driven by the rising economic value of knowledge and innovation. The frontier has opened, and it beckons to us to push the horizon of knowledge beyond the narrow confines of the questions we ask today and toward the larger frames of meaning that can help us see more clearly tomorrow.

History has been harsh with our forebears, and with their intellectual descendants. We have lived through the post-modern, post-colonial critique that may have been in atonement for what our British counterparts did and did not do long ago, and for what critics in developing nations say that the United States is and is not doing now. As we have recognized at EPIC 2005, a profession cannot be competitive in the 21st century with theory-practice 'apartheid'. We need the power that derives from their integration, joined with that of our imagination. And, as a community, we also must remain aware of our historical and political situation, recognize our responsibility within it, be prepared to speak and act in accordance with our moral sense, while remaining engaged in the context, no matter how difficult or painful that might be. Anything less may seem like a 'fair day's work', but it will not yield what we truly seek -- an intelligent, responsible, and lasting contribution to the people of our world and to ourselves – a contribution that tomorrow's anthropologists and ethnographers will be justly proud to claim as our legacy to them.

NOTES

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volumes, Carole E. Hill, and to Keri Brondo who provided valuable commentary on earlier drafts and technical assistance on the manuscript.

[1] Theory, in Aristotle's terms, is defined as epistemic or scientific knowledge, that which is invariable over time and space, and which is achieved through analytic rationality. Practice (techne, craft or art) is defined as an activity that is concrete, variable and context-dependent. Practice is concerned with the application of technical knowledge and skill toward the achievement of a goal (see Flyvbjerg 2001). It is recognized that these poles of knowledge are not opposed to one another, but complementary, and that the distinction between them is itself context dependent and shifting over time and space.

[2] I must acknowledge two volumes of collected papers by 'native' ethnographers and anthropologists in sixteen countries around the globe, one published in 1997 (Baba and Hill, *The Global Practice of Anthropology*, William and Mary Press), and one forthcoming (Hill and Baba, *The Globalization of Anthropology*, American Anthropological Association). The volumes include papers from Australia, Canada, China, Costa Rica and Central America, Ecuador, Egypt, France, Great Britain, India, Israel, Japan, Mexico, Nigeria, Portugal, Russia, and the United States. The purpose of these collections was to enhance sharing of knowledge among practicing A/Es across the so-called First, Second and Third Worlds, and to facilitate development of a global community of practice.

[3] There are no other organizational forms like the Society for Applied Anthropology or the National Association for the Practice of Anthropology, no other degree programs with 'applied' or 'practicing' titles, no local practitioner organizations, and so on. In the 1980s and 90s, three other countries launched experimental efforts to create new institutions named 'applied' or 'practicing', but these failed to thrive. The three experiments include: 1) Canada – Society for Applied Anthropology of Canada and its journal *Proactive*; 2) British Association for Anthropology in Policy and Practice; and 3) an applied anthropology track in the Behavioral Science Department of Ben-Gurion University. None of these structures or names exist today (see Baba and Hill forthcoming). It should be noted, however, that 'Anthropology in Action' still exists in Great Britain as an e-mail discussion group, and that since 2003 the ASA has initiated 'Apply', which is an applied anthropology network that is active in promoting applied events and has its own website.

[4] This is in distinction to the Royal Anthropological Institute, which was more inclusive, and existed previously.

[5] The reformists wanted to develop the regional institutes as part of the colonial development strategy, and training doctoral students within the context of these institutes was integral to their growth and vitality as institutions. The anti-colonialists, however, wanted the doctoral students to return to England during their training for a prolonged 'write-up' period, as they believed the intellectual stimulation would be greater on an English campus than at a colonial institute (Mills 2002).

[6] The colonial reform agenda called for strengthening the regional research institutes, and creating a base for student training there, closely linked to the students' fieldwork. The A/Es at the LSE supported this latter position (Mills 2002).

[7] Or Westward, if one is on the West Coast of the United States.

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WORKSHOPS

Defining the impact of physical spaces on social interactions

Workshop Facilitators: Rich Radka & Lillian Shieh, NEST - The Home Lab

Activities: The characteristics of physical space can determine how social interactions develop in any human experience. Clear, rigorous, and reusable tools for analyzing physical spaces can allow us to better understand this dynamic and to better communicate the abstractions and nuances of ethnography to cross-functional audiences. In this workshop, participants will be presented with an initial framework that could be used to study the influence of spaces upon interactions. The group's reaction to this framework will act as a catalyst as we break into smaller teams to develop other potential approaches to the problem and then test these methods against use scenarios in nearby physical spaces. Teams will then report back to the group and we will attempt to identify the most useful techniques and frameworks. A record of the workshop dialogues, activities, and conclusions will be sent out to participants after the end of the conference.

Business Ethnography for the Bottom of the Pyramid

Nirmal Sethia, California State Polytechnic University (Organizer), Alexandra Mack, Pitney Bowes, Darrel Rhea, Cheskin, Erica Seidel, Pitney Bowes, Jeff SmithLunar Design, Philip Swift, Pitney Bowes, LiAnne Yu, Cheskin

On corporate horizons a new and totally unfamiliar opportunity is emerging: the &lquot;Bottom of the Pyramid&rquot; (BoP) markets-representing almost four billion people, or nearly two-thirds of humanity, who live at the bottom of the economic pyramid. This workshop is designed to engage the presenters and participants in a collective and collaborative exercise with the three-fold purpose of:

- *Defining the potential role of anthropological approaches in connecting the Corporate World to the BoP world in mindful, responsible and reciprocal ways.*
- *Assessing how the ethnographic research philosophies may have to be revisited and methods re-tuned to serve the design imperatives for the BoP customers. And,*
- *Identifying the effective means to gain recognition for ethnographic research as a foundation for enlightened and viable design and business strategies in the BoP markets.*

The expected outcomes of the workshop include the following:

- *Articulation of special professional responsibilities and desirable goals for BoP business ethnography.*
- *Realistic assessment of challenges in doing high quality ethnographic research in BoP markets.*

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- *Clear and convincing arguments that make business executives fully appreciate the importance of user research for BoP business.*
- *Practical guidelines for effective integration of ethnographic research, design research and market research for BoP business.*
- *Identification of the value of, and avenues for learning from the BoP customer, and using this learning to benefit customers in the established top-tier markets.*

About half of the workshop time will be devoted to presentations about significant issues pertaining to business strategies, design and innovation practices, ethnographic fieldwork, and case studies--all with a clear BoP focus. The remaining time will be used for discussion of collective recommendations for the effective integration of ethnographic research with design and business strategies for BoP markets.

Working the Process: Anthropological Approaches to Designing and Evaluating Organizational Work Processes

Workshop Facilitators: Julia Gluesing, Pamela Crespin, Christine Miller, Tara Eaton, Amy Goldmacher Wayne State University

The overall goal of this workshop is to create awareness of existing and newly forming anthropological approaches to the challenges of investigating work processes that are socially embedded in increasingly complex work settings that span organizational, geographic and societal boundaries. We seek to create a dialogue and flow of ideas among business anthropologists to evolve the practice of anthropology in modern organizations.

The question we will explore in the workshop is what do we know and what can we learn about the sociality in which processes are embedded, and the implications of this sociality for process design/re-design and implementation in increasingly complex settings? Workshop participants will have the opportunity to share approaches to design and fieldwork issues and problems and to initiate relationships with one another for further exploration of the issues and for collaboration in future research. They will also learn about practical approaches and tools they can use in their own work.

Participants will be asked to complete three activities prior to the workshop: 1) read two or three articles, 2) complete a short web-based survey and 3) come prepared to discuss examples and pressing questions or issues from their own work.

The workshop will include poster presentations showcasing different perspectives and examples from recent research projects at Wayne State University to spark a full group discussion of sociality and work processes as well as focused small-group roundtable work targeting specific topics of interest. Maximum number of participants: 20. Pre-registration required.

Holy Hanging Out: Exploring Spirituality and Religion in the Corporate Environment

Workshop Facilitators: Joseph 'Jofish' Kaye & Genevieve Bell, Intel Corporation

In "Holy Hanging Out: Exploring Spirituality and Religion in the Corporate Environment", we intend to explore matters related to the corporate ethnographic study of religion or spirituality -- with or without technology - as well as notes from the field, applications of a religious or spiritual nature, problems inherent in religious research in a corporate environment, and studies of individual or multiple sites of spiritual, religious or technospiritual practice.

Participants are to prepare a five-to-ten minute presentation about a pilgrimage or epiphany they have had as an introduction to themselves and their work. The workshop will focus on discussion of these presentations and take appropriate steps for building a community of researchers.

Framing Ethnographic Praxis for Innovation

Workshop Facilitator: Patricia Sachs, Social Solutions, Inc.

This two-and-a-half hour, interactive workshop presents a framework for shaping ethnographic praxis toward innovation and invites participants to engage in discussing, co-shaping, and articulating the framework itself, as well as developing implications for projects that would further the framework. The workshop will be conducted in two parts.

Part I: Framework: Innovation
Companies today are concerned about maintaining an innovative edge. Shifts in global population, aging, and health, alongside broader participation in the global work force, the global silicon network, and stark global divisions of labor have created a context in which sustainability and strategy are imperative for survival. Innovation, in this context, takes on new meaning.

What is the role of professional anthropology and ethnographic praxis for understanding and participating in the dynamics of innovation? How is it meaningful to think about sociality in relation to innovation given this context? What are the implications for integrating anthropological insight and analytic practice into decisions and strategy?

We ask participants in this workshop to consider their own work in light of this perspective toward innovation.

How would you:
Add to the framework?
Articulate elements of it?

Part II: Projects
As researchers, what do we imagine would enable the use, validity, and expansion of the framework? We ask participants to generate the kinds of projects that would further the

framework. We will assess whether there are "next steps" that will enhance the practice of anthropology and the power of its impact through action.

Collaborating across social, organizational and disciplinary distances

Workshop Facilitators: Melissa Cefkin, IBM, Jens Pedersen and Joachim Halse, IT University of Copenhagen and Elizabeth Churchill and Jack Whalen, PARC

Almost by definition, corporate ethnographers have been faced with the opportunities and challenges of engaging the boundary-crossing promise of ethnographic work. What particular challenges do corporate ethnographers face? What new insights and developments have come from these experiences? Bringing a rich mix of collaborations in different settings, both local and distributed, national and international, and within and among people in various kinds of roles-among ethnographers, with researchers of other disciplines, with project team members from other divisions or units, and with stakeholders, customers and research participants-we will facilitate an exploration of two key dimensions of collaborative work engaged by corporate ethnographers: (1) framing and negotiating expectations of the purpose and expected results of the work and thus the design of the work to be done, and (2) interpreting and negotiating representations of the work as it unfolds. We will focus our exploration and discussion on such questions as: How do we aim to position our work and what challenges do we face initially in designing the work to be conducted? How is the goal of 'change' understood and invested in by parties in differing positions? And how are understandings / knowledge / data / perspectives shared and interpreted within the doing of the work?

Participants will be introduced to a "design game" derived from visual data collection as a part of the workshop. We ask that participants come prepared having reflected on the challenges and insights they've experienced in working collaboratively particularly in the areas of establishing the aims of the work and in facilitating interpretation of research material.

Studying Distributed Sociality - Online and/or Offline?

Workshop Facilitator: Brigitte Jordan (PARC) with panelists Kris Cohen (University of Chicago + INCITE, Surrey) and Daniel Neyland (Oxford University)

Sociality has exploded into virtual space. In this interactive workshop we intend to generate a productive discussion about the transformation of "conventional" ethnographic methods as we increasingly do research in the virtual world. By "virtual" we most broadly mean to refer to those situations where the researcher is not, or only occasionally, physically co-present with the people whose working and domestic lives we are studying.

In the course of our conversations, we will consider the circumstances within which early ethnographers did their work and think about the historical, material/spatial conditions in which ethnography was originally formulated as a method. We then intend to consider the implications of the commercial/global-industrial conditions under which ethnography is currently redefining itself. These trajectories raise major methodological and epistemological questions. And so we wonder:

What about the Online and Offline domains that constitute the constituent backdrops of our society? In other words, to what extent does ethnographic work in such hybrid environments need to look at what happens on the screen as well as what the person-at-the-keyboard is doing? How are the lives of these people and their families transformed? Or: What becomes of "participant observation", the mainstay of conventional ethnography when we never lay eyes on the people whose lives we are investigating or experience the settings in which they operate? What does it mean to "participate" or to "observe" in this world? How do we now reconceptualize the notion of "fieldsite", when traditionally our claims to authoritative knowledge were based on intimate co-experience of localized sociality? What happens to "informed consent" when the thing we are studying is an avatar? How do we understand the sociality of avatars? How do we make sense of those seductive machine-based data (like logs, video feeds, blog archives) that seem to do a major part of our data collection for us? What analytic categories do they privilege and what new questions do they allow us to ask?

As a final goal, we expect to move towards providing (more or less in the words of Kris Cohen) "the theoretical foundation and prototype for a new form of ethnographic research, which would be complementary to, but also inherently critical of conventional ethnographic methods."

Here is what we expect from participants: We'd like you to think ahead of time about the ways in which your own work (done, planned or fantasized) is or could be illustrative of the issues we intend to raise here. Beyond that, please give some thought to what you would be willing to say at this point about general trends in the cybermethods arena. And finally, we'd like you to consider the question of what kinds of research could be exemplary in this new field.

Object sociality. Researching living things

Workshop Facilitators: Simon Roberts, Ideas Bazaar and Heinrich Schwarz, University of Colorado at Boulder

In this workshop we want to push our thinking about objects beyond the traditional understanding of objects as tools or infrastructure or even social facilitators. Instead we will explore what it means for us as ethnographic researchers to take things seriously as central participants in personal relationships, communities or social networks - as 'companion species' not just supporting actors.

We will approach such a concept of object sociality on a theoretical and practical, and a personal and professional level. We will discuss frameworks from sociology (object centred sociality), anthropology (the social life of things), and science and technology studies (actor-network theory), examining how these disciplines approach or construct objects. We will also reflect on our own rich experience of living with, and studying, objects to find thick descriptions of things that can restore objects to the centre of ethnographic accounts. Finally, we will examine what such a perspective on objects could and should mean for our ethnographic research. What would such an ethnography of objects look like? How can we make 'voiceless' things speak?

We ask participants to come prepared with two stories: one about an object important in their own personal lives; the other about an object that turned out to play a central role in one of their research projects.

The Sociality of Fieldwork (or Personal Experiences with Interpersonal Connections)

Workshop Facilitator: Steve Portigal, Portigal Consulting

In the activity of fieldwork, the ethnographer creates and facilitates a series of powerful interpersonal connections including (but not limited to):

- *ethnographer and respondent*
- *ethnographer and fellow ethnographer/researcher/designer*
- *ethnographer and "client"*
- *"client" and respondent*

We leverage these connections to accomplish our most basic purposes: creating empathy and gathering data through establishing rapport. But perhaps there is more going on here. The goal of this workshop is to explore and consider these closely-felt connections and collectively begin to build a deeper understanding of the roots, power, impact and further potential for these interpersonal connections.

Through the workshop we will share our respective experiences, looking both for common themes that emerge, but also unique perspectives that the workshop participants may have, drawing those out and ideally building specific techniques that we can employ in the future, looking towards fieldwork that is more stimulating, impactful, and satisfying for each of the different stakeholders.

What to bring: We ask participants to come prepared with a 5-minute (or less) story (or set of stories) describing their own notable experiences (success, failure, or other).

CONTRIBUTORS

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