



## “It’s Just a Matter of Common Sense”: Ethnography as Invisible Work

DIANA E. FORSYTHE

*Medical Anthropology Program, University of California, 1350 Seventh Avenue, Room 101 San Francisco, CA 94143-0850, USA, E-mail: forsythe@sccm.stanford.edu*

**Abstract.** Anthropologists have been using ethnographic methods since the 1970s to support the design and evaluation of software. While early use of such skills in the design world was viewed as experimental, at least by computer scientists and engineers, ethnography has now become established as a useful skill in technology design. Not only are corporations and research laboratories employing anthropologists to take part in the development process, but growing numbers of non-anthropologists are attempting to borrow ethnographic techniques. The results of this appropriation have brought out into the open a kind of paradox: while ethnography looks and sounds straightforward, this is not really the case. The work of untrained ethnographers tends to overlook things that anthropologists see as important parts of the research process. The consistency of this pattern suggests that some aspects of ethnographic fieldwork are invisible to the untrained eye. In short, ethnography would appear to constitute an example of invisible work. Drawing on my own decade of experience as an anthropologist working in design, I attempt to clarify the nature of ethnographic expertise, describe six misconceptions about ethnography that I have encountered among scientists, and present real-life examples to illustrate why quasi-ethnographic work based on these misconceptions is likely to be superficial and unreliable.

**Key words:** ethnography, anthropology, medical informatics, computers and medicine, qualitative methods, user studies, evaluation

### 1. Introduction

Many people look to computerized technologies to help solve problems of information access and management in work settings. If computer systems are to achieve this goal, their developers need detailed knowledge about both information-related problems and the nature of the settings in which these problems occur. Questions about work-related problems and their organizational contexts extend into the realm of the social. As the design world increasingly recognizes, social scientists can contribute to the development of more usable technical tools by providing useful answers to these questions. One research approach that has demonstrated utility in this context is ethnography, used for over a century by anthropologists and qualitative sociologists to illuminate real-world work processes and work settings.

Ethnography is useful at all stages of system development and evaluation. Since Lundsgaarde’s pioneering work in the 1970s (Lundsgaarde, Fischer et al., 1981),

the application of ethnographic research skills to aspects of software design and evaluation developed gradually through the 1980s. There is now a substantial literature on the subject (Lundsgaarde, 1987; Suchman, 1987; Fafchamps, 1991; Forsythe and Buchanan, 1991; Blomberg, Giacomi et al., 1993; Nyce and Timpka, 1993; Forsythe, 1995; Suchman, 1995; Nardi, 1996).

Early applications of ethnographic skills in software design and evaluation were generally viewed as experimental, at least on the part of computer scientists and engineers. Over time, however, many members of the development community have come to see the utility of ethnographic input to design. The CSCW community has been particularly open to the addition of anthropological perspectives and methods to the development tool kit (Galegher and Kraut, 1990). By the mid-1990s, ethnography is becoming established as a useful skill in technology design.

As this message has become accepted, corporations and research laboratories have employed anthropologists to take part in the development process. In addition, growing numbers of non-anthropologists have begun attempting to borrow ethnographic techniques. The results of this borrowing have brought out into the open a kind of paradox: ethnography looks and sounds straightforward – easily borrowed, in fact. However, this is not really the case. The work of untrained ethnographers tends to overlook things that anthropologists see as important parts of the research process (Nyce and Lowgren, 1995). The consistency of this pattern suggests that some aspects of ethnographic fieldwork are invisible to the untrained eye. In short, ethnography would appear to present us with an example of invisible work.

In the sections below, I explore this phenomenon, drawing on my own decade of experience as an anthropologist using ethnographic methods to investigate and support design in artificial intelligence and medical informatics. Comparing the way ethnographic research is understood by anthropologists with examples of the application of quasi-ethnographic techniques by people from other disciplines, I attempt to unravel which aspects of ethnographic expertise are invisible and to explore why they seem to be overlooked.

## **2. What is ethnography?**

Ethnography as a research process entails the use of three elements in combination. First, fieldworkers make use of a set of ethnographic data-gathering methods that include participant observation, formal and informal interviewing, and sometimes also analysis of documentary sources (Powdermaker, 1966; Wax, 1971; Werner and Schoepfle, 1987). Flexible and unobtrusive, these field methods were designed for use in uncontrolled (and uncontrollable) real-life settings. In use, they enable the fieldworker to detect consistent patterns of thought and practice and to investigate the relationship between them – an important comparison, since what people do is not always the same as what they say they do.

Second, these methods are grounded in theory, as are the methods anthropologists use to analyze their field data. The large body of literature in anthropology and

qualitative sociology provides a theoretical framework for distinguishing between different sorts of knowledge and for investigating the relationship between beliefs and action in social situations. The cross-cultural ethnographic record also provides a kind of testbed against which to compare particular findings and to evaluate general theories about human traits.

And third, anthropologists apply these methods in the context of a distinctive philosophical stance. Based in social science theory and intended to help researchers to take as little as possible for granted, this ethnographic stance promotes the conceptual distance necessary for systematic comparison of multiple perspectives on events and processes.

Ethnography produces in-depth understanding of real-world social processes. Properly done, it provides detailed insight into the concepts and premises the underlie what people do – but that they are often unaware of. The power of ethnography as a research approach derives from use of the data-gathering methods together with the philosophical stance and the conceptual structure in which they are grounded. In general, ethnographic fieldworkers do not use preformulated research instruments. Instead, the fieldworker herself is the research instrument, one which is “calibrated” first through training in theory and methodology and then through experience.

Learning to *do* ethnography involves learning to *see* social situations in a way that problematizes certain phenomena. It also involves learning to maintain careful epistemological discipline. Such attention to disciplining the researcher may sound easy compared to research methods that focus instead on controlling the research subjects and/or the context in which they appear. However, field research is by no means straightforward: it takes talent, training, and practice to become a competent field researcher, and careful data-collection and analysis to produce reliable results. As with any kind of skill, what makes ethnography look easy is expertise.

### **3. Clarifying the nature of ethnographic expertise**

The philosophical tradition from which ethnography derives is somewhat different from the philosophical tradition that underlies the natural sciences, computer science, cognitive psychology, and engineering. The former is known to social scientists as relativism, the latter as realism or positivism. In part because of this difference in underlying philosophy, practitioners from science and medicine often misconstrue what anthropological fieldworkers are doing. Such differences in perspective can be difficult for positivists to see and discuss, however, because many scientists do not realize that they have been trained in a distinct philosophical tradition. Below, I illustrate the implications of these differences in perspective by addressing six common misconceptions that I have encountered in working on software design projects.

### 3.1. SIX MISCONCEPTIONS ABOUT THE USE OF ETHNOGRAPHY IN DESIGN

- 1) Anyone can do ethnography – it's just a matter of common sense.
- 2) Being insiders qualifies people to do ethnography in their own work setting.
- 3) Since ethnography does not involve preformulated study designs, it involves no systematic method at all – “anything goes.”
- 4) Doing fieldwork is just chatting with people and reporting what they say.
- 5) To find out what people do, just ask them!
- 6) Behavioral and organizational patterns exist “out there” in the world; observational research is just a matter of looking and listening to detect these patterns.

### 3.2. CORRECTION

- 1) Anyone can do ethnography: Many technical people see ethnography as something that either requires no particular expertise or for which their present expertise already equips them. To them, it's “just a matter of common sense.” Actually, ethnography runs counter to common sense, since it requires one to identify and problematize things that insiders take for granted (and thus tend to overlook). It takes a good deal of training and experience to learn to do this. It may also take courage on occasion, since insiders tend to experience their own assumptions as obvious truths. The lone anthropologist in a technical or other field site may be the only one to question these truths.
- 2) Being insiders qualifies people to do ethnography in their own work setting: The assumption that senior insiders make the best observers of a social situation informs the “expert” model used in knowledge acquisition for software development (Buchanan, Barstow et al., 1983; Forsythe and Buchanan, 1989; Forsythe, 1993). However, competence as an insider does not make one an accurate observer. In fact, ethnography usually works best when conducted by an *outsider with considerable inside experience*. The reason is that the ethnographer's job is not to replicate the insiders' perspective but rather to elicit and *analyze* it through systematic comparison between inside and outside views of particular events and processes. This task includes detecting tacit knowledge, something that by definition is generally invisible to insiders. The ethnographic stance requires mental distance. Insiders do indeed know what is going on in their practice settings, but such inside knowledge is not the same thing as a systematic and analytical overview of the situation (see example below).
- 3) Since ethnography does not involve preformulated study designs, it involves no systematic method at all: People trained in the natural sciences tend to equate scientific research with randomized controlled trials, double-blinding, and quantitative analysis. They take the anthropological reliance on qualitative analysis and subjective experience, as well as the tendency to avoid “controlled experiments” and rigid, pre-set “research instruments,” to imply the absence of any research method. In other words, they place ethnography

in the realm of the “anecdotal,” the term with which they normally dismiss evidence viewed as unscientific. The interpretation of the more improvisational ethnographic approach as a complete absence of method may also reflect the intentional unobtrusiveness of ethnographic inquiry, which may be invisible to an untrained eye. (With respect to the latter point, anthropologists’ success in fitting into work settings without drawing attention to themselves with disruptive data-gathering methods may work against the recognition of them in technical circles as scientific colleagues.) In any case, since non-anthropologists often perceive ethnography as devoid of scientific method, their own approach to trying it sometimes amounts to “anything goes.”

In contrast, anthropologists see ethnographic work as technical in nature and take seriously issues of methodological appropriateness, procedure, and validity (Werner and Schoepfle, 1987). Proper ethnography involves systematic method and epistemological discipline, neither of which is seen by anthropologists as necessarily requiring rigid adherence to preformulated research protocols. Qualitative researchers are wary of preformulated questionnaires because they often turn out to ask the wrong questions, just as so-called “controlled experiments” don’t always tell us much about complex social behavior. In doing ethnography, initial research questions are carefully refined and pursued as fieldwork develops. When field anthropologists discover that they have been asking the wrong questions, they adjust their research formulation in the course of a study. Experienced anthropologists learn to expect such mid-course corrections (Rosaldo, 1993, p. 7), and value the increased accuracy they produce. While it certainly differs from so-called “controlled” research, ethnography is nevertheless a matter of careful, conscious method.

- 4) Doing fieldwork is just chatting with people: Viewing qualitative research as anecdotal in nature, people trained in the sciences often understand ethnography as “just talking to people and reporting what they say” – perhaps equivalent to transcription. Presumably this is what they experience when fieldworkers study them or do ethnography on their behalf; the selectivity of question-asking and observation, and the process of inferential data analysis are invisible to them as informants and research colleagues. Doing fieldwork certainly involves talking to people, but this is no more the entire task than system-building is “just typing” or medical diagnosis is “just talking to patients.” The important point is what one is *doing* when typing or talking. Competent fieldworkers do not take what people say at face value; they treat people’s views as *data*, not results, just as what patients say about their condition is not the same thing as medical diagnosis. The job of the social scientist is to *understand and analyze* what people say. Perhaps because the uninitiated see only the fieldworker’s interaction with her respondents and do not see the analytical expertise being deployed at the same time, they may assume that they already have the skills to carry out ethnographic fieldwork.

- 5) To know what people do, just ask them: Many people in the cognitive sciences treat verbal representations as congruent with and predictive of what takes place “on the ground” (Forsythe, 1993; Forsythe, 1993). They also tend to assume that human patterns of action in the world are consistent over time. This accounts for the widespread reliance on “think aloud” and “cognitive walk through” narratives (which are taken as accurate descriptions of human problem-solving) and for the tendency to move from a very small number of cases to general statements about how the human mind works. When non-anthropologists undertake ethnography, they act on the basis of these assumptions; this leads them to take for granted that what people say is what they will do, and that if people do something once or twice they will always do it. The resultant approach treats focus groups and short-term (e.g., two hour) synchronic observation as revealing general patterns of human action.

For anthropologists, in contrast, the predictive value of verbal representations and the generality of short-term observation are questionable. Ethnography does of course entail eliciting people’s understandings of their own and others’ behavior, but only the most naive of fieldworkers would treat such understandings as reliable data about systematic behavioral patterns. Anthropologists see the relation between representation and visible action as complex (Geertz, 1973; Geertz, 1983), and know from our observational tradition that people’s verbal representations of their own behavior are often partial and sometimes incorrect. In other words, it is imperative to watch people engaged in activity as well as to ask them about it. Such observations in classical ethnography tend to be quite extended – a matter of months or years. While observational periods may be much shorter in the design context, they are still extremely useful when conducted by a competent observer. In system evaluation, for example, it is advisable to observe people using the system as well as to elicit their opinions about it. Focus groups and surveys are no substitute for the combination of participant observation and interview data.

- 6) Behavioral and organizational patterns exist “out there” in the world: Observational research is sometimes perceived by others as just a matter of looking to see what is “out there.” Many technical people seem to assume that social and organizational patterns are visible and audible; one need only look and listen to detect them. This leads them to imagine that an audio- or video-recording itself constitutes qualitative analysis. This is a misunderstanding. Patterns of human thought and action are no more visible than the diagnosis of an individual’s illness. To imagine that behavioral patterns become visible and self-explanatory in a videotape is analogous to believing that a photograph reveals the diagnosis of a patient’s illness. It may be that a skilled physician can diagnose certain maladies from a photograph, just as a skilled social scientist picks up patterns from analyzing audio and video recordings. But in both cases, the expertise is in the mind and technique of the analyst, not in the recording itself. What this common misconception fails to grasp is the selectivity and interpretation

that go into the process of gathering careful ethnographic data, writing useful fieldnotes (Sanjek, 1990; Emerson, Fretz et al., 1995), and analyzing the data in an appropriate and systematic way.

#### **4. Applying these misconceptions**

I have described some misconceptions about ethnographic methods that I have encountered among software designers (and others). As these points suggest, people untrained in anthropology or qualitative sociology may overlook important aspects of ethnographic work. These include the understanding that doing ethnography requires expertise; that analyzing a social situation entails much more than just having “inside” familiarity with that situation; that ethnographic research involves the application of conscious method that – while unobtrusive – is systematic and theory-based; that people’s self-reports about their own and each other’s actions are not taken at face value by anthropologists, but rather are systematically tested against other self-reports and against observable behavior; that seasoned fieldworkers carry out a good deal of observation and amass considerable data before producing generalizations about social patterns; and that good social analysis is the product of careful selection and thoughtful interpretation. When people from other disciplines attempt to borrow ethnographic research techniques, the result often fails to manifest these principles.

##### **4.1. EXAMPLES**

To illustrate the types of difficulties caused by relying on the misconceptions described above, I will offer two examples in the sections below. First, perhaps the most widespread strategy adopted by would-be ethnographers is Misconception #5: “If you want to know what people do, just ask them.” This common sense approach to social research is liable to produce unreliable data when used on its own. The problem is that human beings – no matter how expert – rarely possess a broad overview of the social practices in which they engage. Since they tend not to be aware of this, however, they may believe that they are providing accurate data when they are not. To illustrate this, I will contrast an expert’s reconstruction of a familiar social process with observational data on the same process.

Second, to illustrate the characteristic superficiality of “do-it-yourself” ethnography, I will describe some recent quasi-ethnographic work in medical informatics. This work overlooks the epistemological and methodological challenges that ethnographic methods are intended to address. These include:

- 1) The problem of perspective – understanding what events mean to the actors themselves, as opposed to what they might mean if the fieldworker had done them. The epistemological discipline that constitutes an essential part of the ethnographic method requires maintaining a scrupulous and systematic distinction between the knowledge and assumptions) of particular informants (or cate-

gories of informants) and the knowledge and assumptions of the observer(s). To fail to pay attention to this issue is to take for granted that the fieldworker's world view is universal – a naive assumption indeed.

- 2) The problem of order – discerning patterns characteristic of particular actors and events *over time*, as opposed to the order apparent at one moment in time or the order that actors believe to characterize their social practices. Social processes are complex, as are the human beings who engage in them. The consistencies of thought and action that order human practice are not necessarily apparent on the surface, nor are they likely to manifest themselves during a single brief period of observation.

#### 4.1.1. *Insiders' conscious models do not necessarily constitute reliable data (Example 1)*

A design team setting out to build a patient education system for migraine sufferers asked a neurologist to provide a realistic sample of the way doctors and patients talk to each other about migraine. Veteran of many years' interaction with migraine patients, the neurologist provided the following dialogue:

- Patient I'm feeling tired a lot now.  
 Doctor Do you also feel sad or depressed?  
 P. I'm not sure.  
 D. Do you cry often, maybe with no obvious reason?  
 P. No.  
 D. What is your pulse rate?  
 P. 56.  
 D. Maybe you'd better take one or two less Inderal per day and see if you feel better.

The design team also contained two anthropologists. Following provision of the sample dialogue above, they carried out extensive observations of interaction between neurologists and migraine patients, as well as private interviews with doctors and patients. Their ethnographic fieldnotes contained reports of many conversations between neurologists and migraine patients. Here is a piece of one such conversation:

- Doctor What are your headaches like? Can you describe one for me?  
 Patient I was about ready to jump [out the window] – the pain. It wasn't a constant pain ... I would get like, right back here [patient touches the area below her left ear] it would beat like a heartbeat. I would get like I'm gonna faint. ... If I've got a cold and am coughing quite often, then I get it.  
 D. Have you ever had headaches before?



- P. Yeah, I've had headaches . . . maybe once a month or so.
- D. What was that like?
- P. [Describes headaches. Doctor decides that these were tension headaches.]
- D. These headaches now, they're not like that. [Tone of voice indicates that this is a question.]
- P. . . . I would get, like, after the pain leaves I'd get hot, break out in a sweat, you know? . . . I get roarin' in my ear, and heat . . . [passes hand in front of face, indicating waves of heat over the head.]
- D. What medicines are you taking?
- P. [Patient takes a bottle of pills out of her purse and shows it to the doctor.]
- D. What else do you take?
- P. He [another doctor] had me on the sprayer [decongestant.]

There is a considerable difference between the "realistic" dialogue made up by the physician and this actual dialogue recorded by an anthropologist. Despite the neurologist's expertise, the sample he provided does not constitute good data on the way doctors and patients actually talk to each other. In the expert's model, the interlocutors speak in short, clear messages; in the actual dialogue, the patient's speech is repetitive and rambling. Utterances in the expert's model are unambiguous, whereas some of those in the actual dialogue are more difficult to interpret. For example, it requires some contextual knowledge to understand "He had me on the sprayer." Similarly, the physician asks a question in the form of a declarative statement: "These headaches now, they're not like that." The information-seeking nature of this message is conveyed by the speaker's tone of voice. Messages in the model dialogue are entirely verbal, whereas the real conversation consists of a mixture of verbal and non-verbal messages. In the made-up dialogue, the messages are syntactically much simpler than the real-life utterances. Finally, the questions and answers in the model dialogue are flat – just the facts. In contrast, the actual conversation is quite vivid. The patient uses dramatic imagery to convey the pain of her headaches: "I was about ready to jump [out the window] . . ."

In short, the expert's reconstruction of a doctor-patient dialogue does not provide a useful model of the real-life process he was trying to characterize. Despite his years of experience in talking with headache patients and his insider status in the world of neurology, his model dialogue is much too simple to be realistic. Reliance on experts' self-reports about their work has long been a standard method of data-gathering for system-building purposes (Forsythe and Buchanan, 1989). For reasons made clear by this example, this has led to systems based on simplistic assumptions that have been unable to accommodate the needs of real users in unpredictable situations (Forsythe, 1993).

The addition of good ethnography to the design process can help to avoid this problem. Collecting systematic observational data, interviewing a range of practitioners, and addressing disparities between observed and reported phenomena can all help to provide a much more complex and accurate picture of social processes than reliance on experts' conscious models.

#### 4.1.2. *Do-it-yourself ethnography in medical informatics (Example 2)*

Recognition of the contributions of trained fieldworkers to the design process has led to the phenomenon of do-it-yourself ethnography, a trend that anthropologists tend to view with strong reservations. Non-anthropologists don't always understand this reaction. As one reviewer of this paper commented, surely some knowledge of a situation is better than none. The problem is that in ethnography as in some other pursuits, a little knowledge can be a dangerous thing: superficial social research may confer the illusion of increased understanding when in fact no such understanding has been achieved. This problem is illustrated by the nature of recent do-it-yourself ethnography in medical informatics, in which brief exercises in shadowing, observation, and interviewing have been undertaken from a common sense stance without engaging the questions that define ethnography as anthropologists understand it. Such an exercise can result in a cognitive hall of mirrors. Without addressing basic issues such as the problem of perspective, researchers have no way of knowing whether they have really understood anything of their informants' world view or have simply projected and then "discovered" their own assumptions in the data.

I will introduce this example with some background information. Medical informatics is a rapidly-growing field devoted to the development of information systems for medical practice settings (Shortliffe, Perreault et al., 1990). Professionals in medical informatics include physicians, nurses, medical librarians, computer scientists, and information scientists. Of these, the most influential in shaping the field are physicians, some of whom also have doctorates in computer science. Least influential are nurses and medical librarians (Forsythe, 1994), a fact of relevance here because these are also likely to be the individuals with the most exposure to qualitative research.

Many informaticians have had some acquaintance with ethnography from reading publications that draw on ethnographic research, hearing talks at professional meetings, working with social scientists on research teams, and/or being subjects of ethnographic inquiry themselves. They naturally bring to the understanding of this research method theories of knowledge, assumptions, and practices derived from their own professional training. Elsewhere, I have attempted to characterize something of the intellectual world view of informaticians (Forsythe and Buchanan, 1991; Forsythe, 1992; Forsythe, 1993; Forsythe, 1993). Here I will mention only a few things. They tend to be positivists, committed to what they think of as "hard science." They tend to share the medical model of research as requiring randomized, controlled clinical trials that involve double-blinding and thus often

exclude consideration of subjective experience. Informaticians equate “research” with “experiment” and “analysis” with “quantification.” For many, the notion of “uncontrolled” research is an oxymoron; accounts of subjective experience, including publications by professional anthropologists, are dismissed as “anecdotal.” Finally, many people in this field, particularly the physicians, have had no research training at all. Informaticians with doctorates in computing sciences have been trained in research but in a tradition in which “research” means building a software prototype. To sum up, these are people with a strong bias in favor of a positivist image of science, but very little real-world research experience that might serve to temper that image.

What do informaticians do when they set out to do ethnography? The 1995 and 1996 proceedings of the American of Medical Informatics Association (AMIA) meetings contain roughly half a dozen papers based on “ethnographic” and “observational” work by researchers who are not social scientists but who have read work by anthropologists and sociologists working in medical informatics. Here I will consider the following papers: (Coble, Maffitt et al., 1995; Rosenal, Forsythe et al., 1995; Tang, Jaworski et al., 1995; Coiera, 1996; Tang, Jaworski et al., 1996). These studies demonstrate their authors’ conception of what qualitative research in general and ethnography in specific entails. The results differ considerably from ethnography as anthropologists understand it. In the discussion below, I will conceal the names of individual authors to protect their privacy.

First, none of these authors appears to have had any serious ethnographic or anthropological training. One author is a physician who was inspired to try ethnography himself by having read some of my own papers. His ethnographic training was limited to several discussions with me. Another author is a physician who in a past job directed a design team that included an ethnographer. In his present position, he directs a group of nurses in carrying out a new, more “scientific” form of “ethnography” he has developed that owes something to industrial engineering. The third author has a Ph.D. in computer science. Finally, the fourth senior author has had (as far as I know) no advanced training in either social science or medicine, and learned a method of superficial social scientific inquiry from a weekend workshop conducted by a psychologist. Despite this lack of training and familiarity with anthropological literature, none of the publications expresses reservations about the authors’ qualifications or ability to carry out ethnographic research. In short, these authors appear to share an assumption that qualitative research is something anyone can do.

Second, it is instructive to note what the authors of these papers report having done. One author carried out 44 hours of observations in a critical care unit of a university hospital over a period of one month. In addition, this author interviewed five people. In a 1995 study, a second author reports having shadowed 30 clinicians for two to four hours each, for a total of 78 hours. This author’s team also interviewed 30 clinicians. In a 1996 study, the same author’s team observed a total of 38 clinicians for two hours each. They also interviewed 33 people. A third

author reports having undertaken 23 “preliminary semi-structured interviews” and shadowing 12 people for four hours each. Finally, another group reports having carried out approximately 300 hours of interviewing and videotaping for a study of “physicians’ true needs.”

Except in the case of the latter study (in which it is unclear how much of that 300 hours was devoted to interviewing as opposed to videotaping), these are rather small numbers. Shadowing people for a total of two to four hours each is very short-term observation, especially for a fieldworker with little or no previous observational experience. Such short periods of observation imply an assumption that what people can be seen to do in a two or four hour session is typical of what they do on other occasions, an assumption anthropologists would find hard to justify and that such short-term studies make impossible to test. Use of single observational sessions for each individual also precludes use of some types of triangulation that anthropological fieldworkers find very useful. These include comparing observations of the same individual over time and in different settings; comparing interview and observational data from the same individual, investigating apparent disparities between them; and comparing what people say about each other with what they can be seen to do, again using apparent disparities to guide further investigation. In short, the type of one-shot observation used in these studies implies that seeing and understanding what people are doing is unproblematic, an assumption no anthropologist would share.

The “ethnographic” research published by AMIA has considerable credibility within the medical informatics community. It was presented at the main professional meeting of this community and appears in the peer-reviewed proceedings. The work of one of these teams is supported by NIH. On the basis of what qualifications is this home-grown social science taken so seriously? From the above examples, it appears that for a physician or computer scientist, sufficient qualification is conferred by having talked with, worked with, or simply read the writing of a trained ethnographer. The rest is apparently a matter of “common sense.”

While several of these neo-ethnographers refer knowledgeably to the richness of their “ethnographic” findings, from an anthropological standpoint they actually do very little with the relatively small amount of observational and interview data they collected. A major strength of ethnography is its ability to uncover tacit assumptions, thus making them available for questioning and testing. As Nyce and Lowgren have noted, however, information scientists tend to borrow ethnographic data-gathering techniques without also borrowing (or understanding) the conceptual structure from which they derive (Nyce and Lowgren, 1995). Decontextualizing ethnographic techniques in this way reduces their analytical power. The resultant “insider ethnography” takes local meanings at face value, overlooking tacit assumptions rather than questioning them. None of these “ethnographic” studies by informaticians raises problems of epistemology or meaning, although anthropologists working in informatics invariably encounter and report such issues when doing ethnography in similar settings. Instead, the informati-

cian ethnographers seem simply to quantify everything they can, supporting their findings with copious statistics and pie charts.

These analyses are superficial. They lack the critical perspective that many anthropologists derive from their ethnographic work. While the absence of a critical edge in this “insider” ethnography may make it more palatable from the standpoint of those who fund (and perhaps carry out) software design, it also means that this work adds little in terms of new understanding. In particular, such superficial quasi-ethnography misses the opportunity to make visible and call into question tacit assumptions held by design teams and/or end-users – a characteristic of good ethnographic research that can save both time and money in the process of software development (Forsythe, 1995).

## **5. Taking ethnographic expertise seriously**

Anyone with a little knowledge can carry out do-it-yourself social science, just as anyone can write a small program and get it running on a computer. But just as it takes more than a little knowledge to write substantial programs in elegant code, valid social research requires genuine expertise. Yet for some reason, it has been difficult for some technical people to understand that reliable qualitative research requires training and practice.

People in computer science and medical informatics often ask me to suggest “just one article” to enable them to do ethnography themselves. This is absurd. It takes as long to train a competent, Ph.D.-level anthropological fieldworker as it does to train an expert neurosurgeon. Yet who would request the name of a single article on medicine so that he or she could do brain surgery? If nothing else, someone who did so would be subject to stringent legal sanctions, whereas no formal sanctions await those who carry out do-it-yourself ethnographic research. On the contrary, in medical informatics at least, such research is welcomed and is taken at least as seriously as work by experienced social scientists. This too is absurd. If it doesn’t make sense to trust medical diagnosis by an amateur in a white coat, why would anyone trust amateur “ethnographic” research by people with no training in social science?

This pattern is particularly puzzling in medical informatics, a field dominated by physicians. Doctors take their own expertise quite seriously; why do they treat ethnography as something that anyone can do? I suggest two reasons. First, educated in a realist tradition and generally given no training in social science, physicians genuinely do not see that ethnographic research requires expertise. As this paper suggests, much of the work required by good ethnography is apparently invisible to them. Second, physicians in the United States occupy a powerful position in society at large as well as in the particular institutions in which they work. Unaccustomed to having their disciplinary assumptions challenged, doctors take for granted that their common sense beliefs about social phenomena (as expressed, for example, in the “social history” that forms part of the standard History and

Physical Exam) are true and complete. This is presumably one reason for the low rate of user acceptance of some computer systems built for medical settings (Anderson and Aydin, 1994). Such systems tend to be based on assumptions about users' needs that are rarely subjected to systematic testing (Forsythe, 1992).

## 6. Discussion

The misconceptions about ethnographic work that I have described illustrate the fact that people trained in different disciplinary traditions may view the same phenomena in very different ways. The particular conflict in perspectives addressed in this paper has some very real material implications for anthropologists who work in technology development, as well as for their supervisors and coworkers from other disciplinary backgrounds. Below I outline some practical consequences that may result from two issues mentioned above: the apparent invisibility to non-anthropologists of the selectivity and interpretation of the ethnographic data-gathering process; and the fact that the anthropological propensity to identify and question assumptions is not necessarily welcomed in other disciplinary settings.

### 6.1. IN WHICH I AM CALLED A "WALKING TAPE RECORDER"

Anthropologists who work in inter-disciplinary settings sometimes feel that their skills are undervalued by sponsors and colleagues who don't understand what they do. I remember my own chagrin shortly after joining a medical informatics project at hearing the senior physician characterize my role in observing hospital work rounds as being "a walking tape recorder." As it happened, I had tried carrying a tape recorder for several days for this project and had taped the work rounds. The resultant audio tapes contained an indecipherable babble from which this physician's highly competent secretary was unable to create a meaningful transcript. In contrast, on the basis of my written fieldnotes, I consistently produced readable transcripts with additional analytical comments. The physician's interpretation of this situation was to see me as a better sort of tape recorder, ostensibly because I had legs. (Actually, the original tape recorder had in effect had legs as well, since I had carried it around.) My role in selecting and interpreting the information that I recorded, and in compiling the data into narrative accounts, was completely invisible to the physician, perhaps because he experienced morning work rounds as unproblematically ordered. He did not perceive the creativity of the work I was doing or the fact that another anthropologist would have produced a different narrative.

A corollary of this disparate understanding of fieldwork is a disparate view of the written products of such work. Anthropologists treat field data as intellectual property. One would not normally use another's data without permission, nor would one normally publish from such data without attribution and a possible offer of coauthorship. In contrast, non-anthropologists tend not to see ethnographic field

data as intellectual property. On the contrary, consistent with the view of ethnography as something that anyone can do and of fieldworkers themselves as “walking tape recorders,” people from science and medicine tend to focus on (quantitative) data analysis while viewing ethnographic data as simply grist for the statistical mill.

This difference in perspective can be a source of conflict. In some projects on which I have worked, physician and computer scientists have wanted to distribute field data that I produced, inviting graduate students and colleagues to conduct and publish their own analyses of the data. One project leader proposed to put hundreds of pages of interview transcripts on the World Wide Web as a sort of public service. (In these cases, the data had been rendered anonymous so that the privacy rights of the individuals studied were not at issue.) Many anthropologists who work in technical settings have encountered some version of this situation, which is typically quite upsetting for the anthropologist. It is distressing to have one’s intellectual work overlooked; since ethnographic fieldwork is typically time-consuming, the amount of intellectual work at issue tends to be substantial. In addition, people without social science training may choose to use ethnographic data in ways that strike the fieldworker as invalid or even unethical. The anthropologist involved in this type of conflict may be seen by others as ungenerous or intransigent; in turn, she may see them as exploitative of her and as unconcerned with valid interpretation of the data. What really underlies such conflicts are disparate understandings of the nature and value of ethnographic work and its products.

## 6.2. CRITIQUING COLLEAGUES’ ASSUMPTIONS: BITING THE HAND THAT FEEDS US?

A second example of such disparate understandings concerns the ability of the skilled ethnographer to “see” underlying assumptions. Anthropologists are trained to be reflexive; that is, to attempt to identify and evaluate their own research assumptions as well as those of their respondents. For experienced fieldworkers, it becomes second nature in any situation to listen for what is being taken for granted.

When an anthropologist joins an interdisciplinary design team, it seems natural to apply this ability to the analysis of assumptions held by the team itself as well as those of the formal subjects of ethnographic analysis (such as end-users). In my own experience, this kind of reflexive analysis of design assumptions can be very useful. For example, in the early 1990s, I served as senior anthropologist on a project to build an intelligent patient education system for migraine sufferers. In support of this process, I carried out ethnographic research on neurologists and people with migraine, aided by Myra Brostoff (then a graduate student in anthropology). Attending project meetings, I could not help noticing that the design team was making some assumptions about end-users that did not match what we were seeing during field research. Bringing this disparity to the attention of the designers enabled them to re-think the development plan early in the project, before a great

deal of effort had been devoted to developing a prototype that would not have met the user needs we had identified (Forsythe, 1995).

As this case demonstrates, the questioning analytical style in which anthropologists are trained can have practical utility in technology design. Not everyone reacts positively to such questioning, however. People from other disciplinary backgrounds and people in positions of authority on design teams may be offended by having their assumptions pointed out, especially when the questioner is someone they view as “non technical.” In some cases, they may not even recognize that they hold particular assumptions quite noticeable to an observer, a manifestation of the not uncommon problem of “seeing” one’s own cultural position.

In addition, in medical informatics, in which physicians are seen as senior to social scientists, the anthropological propensity to name and query tacit assumptions tends to collide with the hierarchical nature of American medicine. This reflects two different views of what it means to question others’ assumptions: what anthropologists tend to see as a piece of their ethnographic work may look like insubordination or even betrayal to people trained in other disciplines. As should be obvious, this difference in perspective can create political difficulties for the anthropologist. It may also be awkward for a supervisor who has taken a risk in including an anthropologist on a development team. Even designers who welcome ethnographic insights into users’ assumptions and expectations may be less happy when the ethnographic gaze is turned on them. In contrast to the previous example, in which the problem from the anthropologist’s point of view is that much of her work is invisible from the standpoint of others, the problem here is the expectation of others that ethnographic work *should* be invisible in a context in which it is not.

### 6.3. DELETING ETHNOGRAPHIC WORK

In my previous ethnographic work on system-building in artificial intelligence and medical informatics (Forsythe, 1993; Forsythe, 1993), I found that designers consistently discounted those aspects of their own work that involved social interaction or maintenance activities, such as teaching, planning, discussion at meetings, reading and sending email, or backing up their computers. While the people I studied regularly carried out such tasks and often spent a good deal of time on them, they resented having to do so. They dismissed these tasks as “pseudowork.” Such activities were not included when I asked people to describe their work to me. In their accounts, their “real work” was the technical job of system-building, which they saw as restricted to sitting in front of a monitor and writing computer code.

This is an instance of what Leigh Star has called “deletion,” a process (often unconscious) in which certain kinds of social phenomena are systematically rendered invisible to those who have reason to know about them. A commonly-deleted type of activity is what Star calls “articulation work” (Star, 1989, p. 110). In thinking about their own work processes, technical people tend to delete social



(which they think of as “non-technical”) work; as I have argued elsewhere, this deletion is carried over into system design as well (Forsythe, 1993; Forsythe, 1993).

The tendency for social and communicative work to be rendered invisible in technical settings helps to account for the phenomena described in this paper. As I have tried to show, technically-trained people may engage in several sorts of deletion in relation to ethnography, which may appear to the naive observer to consist entirely of talking to people. First, when computer scientists and physicians treat ethnography as something that anyone can do, they delete the training, skill, and experience that go into producing good ethnographic work, as well as the analytical process it entails. Several specific examples of deleted ethnographic work were described in the list of misconceptions offered above. Second, as I illustrated with examples from medical informatics, when non-anthropologists borrow ethnography, they tend to delete the accompanying philosophical stance and to treat it as a decontextualized bundle of data-gathering techniques. This defeats the purpose and much of the power of the ethnographic approach. And third, when people take offense at having their own design assumptions identified, they overlook the fact that all of us take things for granted that may affect our work. Uncovering tacit orthodoxies is precisely what ethnography is supposed to do. In other words, not only is ethnography not “just a matter of common sense,” part of its purpose is to identify our common sense – and to help us to assess it.

### Acknowledgement

I am grateful to Bonnie Nardi and to the anonymous reviewers for helpful comments on an earlier draft of this paper. I also thank my informants and colleagues in the world of software design.

### References

- Anderson, J.G. and C.E. Aydin (1994): Overview: Theoretical Perspectives and Methodologies for the Evaluation of Health Care Information Systems. In J.G. Anderson, C.E. Aydin, and S.J. Jay (eds.): *Evaluating Health Care Information Systems*. Thousand Oaks, CA: Sage Publications, pp. 5–29.
- Blomberg, J., J. Giacomini, A. Mosher and P. Swenton-Wall (1993): Ethnographic Field Methods and Their Relation to Design. In D. Schuler and A. Namioka (eds.): *Participatory Design: Principles and Practices*. Hillsdale, NJ: Erlbaum, pp. 123–156.
- Buchanan, B.G., D. Barstow, R. Bechtel, J. Bennett, W. Clancey, C. Kulikowski, T. Mitchell and D.A. Waterman (1983): Constructing an Expert System. In F. Hayes-Roth, D.A. Waterman, and D.B. Lenat (eds.): *Building Expert Systems*. Reading, MA: Addison-Wesley, pp. 127–167.
- Coble, J.M., J.S. Maffitt, M.J. Orland and M.G. Kahn (1995): Contextual Inquiry: Discovering Physicians’ True Needs. In R. M. Gardner (ed.): *Proceedings of AMIA Annual Fall Symposium*. Philadelphia: Hanley & Belfus, Inc., pp. 469–473.
- Coiera, E. (1996): Clinical Communication: A New Informatics Paradigm. In J.J. Cimino (ed.): *Proceedings of AMIA Annual Fall Symposium*. Philadelphia: Hanley & Belfus, Inc., pp. 17–21.
- Emerson, R.M., R.I. Fretz and L.L. Shaw (1995): *Writing Ethnographic Fieldnotes*. Chicago: University of Chicago Press.

- Fafchamps, D. (1991): Ethnographic Workflow Analysis: Specifications for Design. In J.H. Bullinger (ed.): *Proceedings of 4th International Conference on Human-Computer Interaction*. Amsterdam: Elsevier Science Publishers, pp. 709–715.
- Forsythe, D.E. (1992): Blaming the User in Medical Informatics: The Cultural Nature of Scientific Practice. *Knowledge and Society*, vol. 9, pp. 95–111.
- Forsythe, D.E. (1993): The Construction of Work in Artificial Intelligence. *Science, Technology and Human Values*, vol. 18, no. 4, pp. 460–479.
- Forsythe, D.E. (1993): Engineering Knowledge: The Construction of Knowledge in Artificial Intelligence. *Social Studies of Science*, vol. 23, no. 3, pp. 445–477.
- Forsythe, D.E. (1994): *Disappearing Women in the Social World of Computing*. Presented to the 93rd Annual Meeting of the American Anthropological Association, Atlanta.
- Forsythe, D.E. (1995): Using Ethnography in the Design of an Explanation System. *Expert Systems With Applications*, vol. 8, no. 4, pp. 403–417.
- Forsythe, D.E. and B.G. Buchanan (1989): Knowledge Acquisition for Expert Systems: Some Pitfalls and Suggestions. *IEEE Transactions on Systems, Man and Cybernetics*, vol. 19, no. 3, pp. 435–442.
- Forsythe, D.E. and B.G. Buchanan (1991): Broadening Our Approach to Evaluating Medical Information Systems. In P.D. Clayton (ed.): *Proceedings of 15th Symposium on Computer Applications in Medical Care*. Washington, D.C.: New York: McGraw-Hill, pp. 8–12.
- Galegher, J. and R.E. Kraut (1990): Technology for Intellectual Teamwork: Perspectives on Research and Design. In J. Galegher, R.E. Kraut, and C. Egidio (eds.): *Intellectual Teamwork. Social and Technological Foundations of Cooperative Work*. Hillsdale, NJ: Lawrence Erlbaum Associates, pp. 1–20.
- Geertz, C. (1973): *The Interpretation of Cultures*. New York: Basic Books.
- Geertz, C. (1983): *Local Knowledge: Further Essays in Interpretive Anthropology*. New York: Basic Books.
- Lundsgaarde, H.P. (1987): Evaluating Medical Expert Systems. *Social Science and Medicine*, vol. 24, no. 10, pp. 805–819.
- Lundsgaarde, H.P., P.J. Fischer and D.J. Steele (1981): *Human Problems in Computerized Medicine*. Lawrence, KS: University of Kansas Publications in Anthropology No. 13.
- Nardi, B. (1996): The Use of Ethnographic Methods in Design and Evaluation. In M.G. Helander, T. Landauer, and P. Prabhu (eds.): *Handbook of Human-Computer Interaction II*. Amsterdam.
- Nyce, J.M. and J. Lowgren (1995): Towards Foundational Analysis in Human Computer Interaction. In P.J. Thomas (ed.): *Social and Interactional Dimensions of Human-Computer Interfaces*. Cambridge: Cambridge University Press, pp. 37–47.
- Nyce, J.M. and T. Timpka (1993): Work, Knowledge and Argument in Specialist Consultations: Incorporating Tacit Knowledge into System Design and Development. *Medical and Biological Engineering and Computing*, vol. 31, no. 1 (January), pp. HTA16–HTA19.
- Powdermaker, H. (1966): *Stranger and Friend: The Way of an Anthropologist*. New York: W. W. Norton & Company Inc.
- Rosaldo, R. (1993): *Culture and Truth: The Remaking of Social Analysis*. Boston: Beacon Press.
- Rosenal, T.W., D.E. Forsythe, M.A. Musen and A. Seiver (1995): Support for Information Management in Critical Care: A new approach to identify needs. In R.M. Gardner (ed.): *Proceedings of AMIA Annual Fall Symposium*. Philadelphia: Hanley & Belfus, Inc., pp. 2–6.
- Sanjek, R. (ed.) (1990): *Fieldnotes. The Makings of Anthropology*. Ithaca, NY: Cornell University Press.
- Shortliffe, E.H., L.E. Perreault, G. Wiederhold and L.M. Fagan (eds.) (1990): *Medical Informatics: Computer Applications in Health Care*. Reading, MA: Addison-Wesley Pub. Co.
- Star, S.L. (1989): *Regions of the Mind: Brain Research and the Quest for Scientific Certainty*. Stanford, CA: Stanford University Press.

- Suchman, L. (1995): Making Work Visible. *Communications of the ACM*, vol. 38, no. 9 (Sept.), pp. 56–64.
- Suchman, L.A. (1987): *Plans and Situated Actions*. Cambridge, England: Cambridge University Press.
- Tang, P.C., M.A. Jaworski, C.A. Fellencer, N. Kreider, M. P. LaRosa and W.C. Marquardt (1996): Clinician Information Activities in Diverse Ambulatory Care Practices. In J.J. Cimino (ed.): *Proceedings of AMIA Annual Fall Symposium*. Philadelphia: Hanley & Belfus, Inc., pp. 12–16.
- Tang, P.C., M.A. Jaworski, C.A. Fellencer, M.P. LaRosa, J.M. Lassa, P. Lipsey and W.C. Marquardt (1995): Methods for Assessing Information Needs of Clinicians in Ambulatory Care. In R.M. Gardner (ed.): *Proceedings of AMIA Annual Fall Symposium*. Philadelphia: Hanley & Belfus, Inc., pp. 630–634.
- Wax, R.H. (1971): *Doing Fieldwork: Warnings and Advice*. Chicago: University of Chicago Press.
- Werner, O. and G. Schoepfle (1987): *Systematic Fieldwork*. Newbury Park, CA: Sage.