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DIALOG

The Practice and Uses of Field Research in the 21st Century Organization

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INTRODUCTION

The transcript that follows is an edited version of a transcript of a symposium that I organized for the 1995 Academy of Management Meetings in Vancouver, BC, Canada. The symposium was entitled, "The Practices and Uses of Field Research in the 21st Century Organization." My motivation for organizing the symposium rested on some difficulties I had been facing when working on my own dissertation. I had come back to graduate school with 7 years of experience as a practicing research and development engineer at the Boeing Corporation. To no surprise, I entered school with some clear research interests based on my previous work experience, but I possessed a certain naivete about what academic research was.

In developing my dissertation research, I had come to feel strongly that understanding organizations in the 21st century would involve the sort of detailed knowledge of activities like research and development that I was bringing into the Ph.D. program. Initially, I found it hard to deploy that knowledge in my own academic research. As much by chance as by design, I took a field research class from Howard Becker in the Sociology Department at the University of Washington. In taking his class, I found out three things. First, field research was a very good vehicle for drawing on and challenging the lessons that I had learned in my previous career. Second, actually doing field research was quite different from what I had been led to believe in some of the methodology classes that I had taken in the business school. I found that

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field research forced me to employ many of the same inductive problem-solving strategies that I had employed as an engineer, and I felt very comfortable doing this sort of research. Finally, I found that doing field research well was very demanding in terms of time and, conceptually, it was very challenging. Field research was time consuming precisely because field researchers had to be both open and systematic. They had to be open to having their pre-conceived notions being falsified by what they were observing. At the same time, field researchers had to be systematic in collecting data, analyzing data—qualitative or quantitative—and extending and refining their research in ways suggested by both their own observations as well as previous empirical research.

Because I felt this way, I assumed that other people might feel the same way and might appreciate hearing four experienced and recognized field researchers discuss what it would mean to do field research in 21st century organizations. Fortunately, I found four highly experienced and highly talented field researchers to participate in the symposium. To make the symposium more useful, I came up with three questions for them that would have broad interest. The first question was intended to draw out their motivations for doing extended field research and the importance of field research to the study of 21st century organizations. The second question was intended to allow them to discuss the ways in which they had generated and validated research results during their extended fieldwork. The third question was an attempt to get the participants to discuss some aspect of what is normally called research design. Collectively, these questions seemed to get at many of the issues that I was concerned with personally, and I assumed they would be of interest to a wider audience. The symposium involved the researchers answering these three questions in rotating order.

William N. Kaghan

EDITED TRANSCRIPT

Moderator: The first question is "What role do you envision for field research in studying the transformation of work and organizations in the 21st century or, alternately, what would scholarly research on the transformation of work and organizations lack if no one did fieldwork?"

Anselm Strauss: We are always being told that with the advent of the 21st century that the social landscape is being completely transformed. But I can tell you that the same thing was said in 1830. I can point you to the places where they say it. 1830 was when manufacturing first came into the United States, and the social landscape really changed. And if you wanted to study what happened to organizations in the 1830s and fourties and fifties, there is not a lot that you could say if you depended on economists, or on the statistics collected by the Census Bureau or even on journalists who were good at investigating details. We wouldn't really know what had happened inside the organizations. If we want to find out what happened, we go study something like the railroads which in those days were really in the forefront of change—the equivalent of the computerized organization in the 21st century. The answers are found in documents and autobiographies, journals and other sorts of materials. If we had field studies of what was going on then, we would have a comparable situation to what is going on today.

Now, I don't want to argue that field studies are the only way to study changing organizations. But I will argue that there are many changes that we just cannot get at except by getting in there and watching what is going on. Field studies as any experienced field researcher will tell you, if you don't already know, are not something that just involve observations or even joining the firm and being part of it, as sometimes happens. They also involve talking to people in formal interviews, informal interviews, using their documents and so on. There is nothing like fieldwork that I know of for really understanding what makes organizations, communities, or any grouping of people more understandable.

For example, we might be interested in finding out about some particular aspect of an organization such as how an organization uses physical space—how do they manage it? what things do they set up? where do they set them up? In our studies of hospitals, we tried to find out something about how technology was used in hospitals. A fundamental question that we wanted to know about was where machines were from day to day. They may have been in one place one day and have been moved the next day. It took time and energy to move the machines around. And the people that wanted to move them had to convince other people that it was alright to move them. So we started looking at space and then we had to take account of time. Then we had to think about the relationship between space and time.

If you are interested in work in organizations, and you should be because that is essential to understanding organizations, you cannot study work over time let alone the changing division of labor without field observation. You can study the division of labor with questionnaires and things like that. But if you want to know how people really work and how they find the ins and outs of all the rules and how they use the rules and how they exert their power and that is the story that you want to tell, you have got to be there. You have to be watching the actors and the actions.

Now, I can quickly summarize by saying that if you want to know how things change over time—if what you are really interested in is the emergence of relationships and you want to see how things are shifting—you might do it quasi-processually by surveying with questionnaires one year and another year and another year. But if you want to see the actual mechanics of the things that work, if you want to watch the people negotiating things out or fighting them out or doing them in silent ways or looking at the way they array themselves, you have got to be there watching them.

Even people that recount details in their interviews will leave out some things or give you a very slanted point of view. In doing fieldwork, on the other hand, you have the advantage of someone going in and finding some character, some officer, some workman, and following that person for 5 hours and watching every single contact in every single context, every interaction, every bit of talk, every maneuver. You pick up the strategies, you pick up the work relationships, you pick up the personality relationships if that is what you are interested in. You find out what they say in public and find out what they say in private and all the rest. You can't get conversations in an interview. You only get them from watching.

So I'll come back again to the point that I began with. Organizations are simply not what they were 20 years ago, and they are changing in very radical ways. And even the journalists will tell you, though it may be somewhat exaggerated, that with the computer and so on, things are changing in ways that we haven't dreamed of. I would say even further that all the technology that is emerging, every bit of that technology is something that has an aspect to it that you cannot predict in advance. If I tell you about my encounters with e-mail, it would make you laugh. I mean most people cannot imagine what that new technology will do when you first get it. And again, its only by being on the spot and watching things that we get the full impact of how that's going to affect the organization and how changes are going to occur. That's my observation anyway.

Robert J. Thomas: I promised myself that I was going to be nice today but Steve Barley said that I didn't have to. So I will start by saying that management science or any social science for that matter would be a virtual impossibility without field research because field research is where new ideas, by and large, come from. So, in a sense, field research is a very demand driven phenomenon. You won't be able to have those articles published unless someone is observing phenomena and generating new ideas.

But perhaps more importantly and more subtly, I think that the definition of what constitutes a field site is going to change, and this change will have important repercussions for management research. It seems to me that the field site that we now think of when we talk about field research is very much a physical field. It is a field site that each one of us can physically enter and can directly interact with other people in order to increase our ability to observe and understand phenomena. And yet I am struck by the fact that in some ways the field sites that are opening up are field sites where we will not be physically copresent. The opportunity to do field research in some virtual site in cyberspace is going to be an extraordinary opportunity in the 21st century, and one for which many of us have very little preparation no matter how much qualitative research experience that we may have.

For example, I am struck by the case made by Nicholas Negroponte in a recent book called *Being Digital* that in the future the quality of communication is really going to be, at least in part, a function of the cost of video screens. Negroponte argued that right now those screens are relatively small, poor resolution, fairly expensive and not terribly portable. But, in the future, he envisioned the possibility of a video screen that could vary in size enormously and could be rolled up like a sheet of paper and could be tacked up any place that you would like. With this sort of technology, it wouldn't be difficult to imagine having face to face contact with people all the way across the world in a wide range of different settings. You could simulate the actual meeting as opposed to having to physically be there.

If being a consultant qualifies as being a practicing manager or something close, for the last 7 months I have been a practitioner. During the time that I have been consulting, one of the things that has struck me is the fact that people continuously say that they have to be face to face in order to be able to do business. Yet I have also been struck by the fact that North Americans in general are not a particularly warm and touching people so that they don't seem to need to be able to touch one another in order to do business, they just need to be able to see one another. But if they could see one another much more cheaply than they currently do, they wouldn't have to physically be there. So I wonder about the effect that the screens that Negroponte discusses would have on business and on field research on business.

To sum up, just as in the past, field research is important because someone needs to go into the field to collect detailed data—qualitative, quantitative or whatever—to observe new phenomena and generate new ideas. But my second and probably more important point is that what we mean by "the field" in talking about field research is going to expand enormously and we are only beginning to get a sense now of what that meaning is.

Mary Yoko Brannen: I sort of liked the second version of the question which was "what would scholarly research look like without field studies," and I had originally thought of starting out with that phrase and substituting various endings to it. It became sort of a mantra-like word game for me. Among the endings I tried out were, "it'd be like generalizations without descriptions; reliability without validity; significance without reference; frosting without cake; the tip of the iceberg without its infrastructure." But, rather than start off like that, I think I'll just focus on what I think the most obvious shortcoming that would come about from having scholarly research without fieldwork—namely, having abstractions without substance.

At a basic level, what any of us are trying to find out through our research on work organizations is how things get done. Without field research, we would have abstract concepts of how things are, rather than observations of how things actually are getting done. We'd have outcomes rather than process. So, it would be hard to know how we got here from there. Though it has to be said that even with field research, those of us who do it know that when it comes to writing it up all we can do is deliver our best approximations of what we think we saw about how things get done.

A large part of the value of field research is that it tightens the link between what is happening and what we think is getting done. So in a rather circuitous way, I think that it's precisely in providing this link in seeing things get done that field research does best. And, it accomplishes this by participant observation. The participant part of participant observation (a big part of ethnographic field research) means that the researcher actually engages in the work that s/he's researching. Doing things helps people understand how things get done. In addition, fieldwork done well helps to take the blinders off of our *a priori* theories and hypotheses—some parts get confirmed, some parts don't. So, it provides a check and balance to armchair theorizing. Both Anselm Strauss and Bob Thomas have talked about face and physicality. I'd only add that by doing participant observation you actually experience doing things—you have tactile, first-hand knowledge of what it's like to do what you're observing. And, though still not exact, fieldwork done well brings us closer to the experience of how things get done in organizations.

I remember at a previous Academy of Management meeting one of Stephen Barley's students was presenting a paper on the work reality of laboratory technicians at a hospital and began talking about drawing blood. I was so impressed by the detailed description he gave of the task, that while I wasn't sure he had captured all aspects of a laboratory technician's reality, I was sure that in a pinch, he could indeed perform a blood test on me. Through participant observation, he gained a great deal of knowledge about how lab technicians do their job. That is the one thing that doing field research has indisputably above not doing field research in terms of making sense of reality.

In regards to the first iteration of the question which asks about the role field research has in studying the transformation of work and organizations in the 21st century, my thoughts are not far from those I just discussed. I'm not sure that we would all have the same take on how work organizations are changing, but perhaps we'd agree on some aspects of the transformations we're seeing. One of the most obvious ways in which work organizations are changing that I can think of is in the internationalization of the workforce and the growing global context for doing business. Without fieldwork, I don't think that we could begin to understand the complex embeddedness of work culture in internationalized contexts. The only way to begin to understand the interaction between the various layers of culture—from national culture and organizational culture, all the way down to multicultural, bicultural, and idiosyncratic family of origin issues that are salient in today's complex work environments—is to actually see and experience this complex interaction.

Moreover, field research helps to uncover linkages between abstract theoretical concepts such as organizational learning or organizational culture and the everyday work reality of the individuals enacting them. Such concepts are rather reified. In fact, organizational behavior itself is a reified concept. Organizations themselves don't behave! Organizations are enacted by individuals. From this perspective, it is important to understand the links between the learning which individuals do and how that individual learning is aggregated and translated into organizational learning. To get this understanding, one has to study individuals in action and observe how the organization somehow integrates and institutionalizes the learning experience of these individuals within the context of the organization. Similarly, people bring distinctive cultural orientations into the workplace. Understanding how the organization integrates and institutionalizes these various cultural orientations within the workplace is central to understanding organizational culture.

Other ways in which the organization is being transformed are downsizing, delayering, and the increasing importance of the Pacific Rim in world business. Delayering, or the removal of middle management, is a common strategy employed in downsizing. However, while organizational structure is becoming less complex, work itself is not. So organizations, in order to deal with the downsizing, are becoming horizontally more complex. And, with the flattening of organizations, structural sources of power become less pronounced, and interactions between people such as networking become more important. I don't see how we can understand such interactions without doing field research.

Finally, in regards to the increased importance of the Pacific Rim, forecasters have estimated that the world output generated from Pacific Rim countries will increase from 20% to 40 or 50% in the 21st century. Most of these projections are quantitatively generated. Very little is known qualitatively about what this shift means. For example, there has been very little field research conducted on companies in the one country on the Pacific Rim that we think we know most about—namely, Japan. If you really know the area, you can name only two or three actual ethnographies or in-depth studies of organizations in Japan. And, that's not enough really to understand what is going on in these countries, especially in organizations in these countries. I give the example of Japan because we think we know a lot about Japanese management. But if we know that little about Japanese management and how Japanese organizations work, then we know very little about the part of the world that is going to be producing the most output or a large share of the output in the 21st century. Without fieldwork we can't begin to know what the issues are, how interactions evolve, or how the nature of work itself is changing in these emergent organizational settings

Stephen Barley: The nice thing about going last is that you can reinforce what everyone else has said.

If a transformation in the nature of work is occurring and if this transformation is analogous to the first and second industrial revolutions, then we can expect the socioeconomic structure we now take for granted to start falling apart. Our concepts and theories are likely to go bad, as well. We may wake one day to find ourselves writing about a world that doesn't exist anymore. This day may be closer than we think. In fact, there is reason to believe that mainstream organizational theory is already becoming anachronistic. The managerial hype that fills the shelves of Walden Bookstores is evidence that people are grasping for clues that will help them understand what they fear they already know: the system has changed. The problem, of course, is that precious little of the hype is based on careful observation. Unfortunately, the organization studies community doesn't have many clues at the moment either.

Let me suggest three places where our theories are becoming anachronistic. As you know, there is a large, respected, and still growing literature in the sociology of organizations on internal labor markets. To have internal labor markets, firms have to have employees and these employees have to stay with the firm for a reasonably long period of time. If employees don't stay around, they can't be promoted. Without promotions, internal labor markets have no meaning. Current trends—downsizing, reengineering, outsourcing, the move to contingent labor—suggest that internal labor markets could become the sociological equivalent of a dinosaur or, at least, the spotted owl. It's worth remembering that the putting out system was driven to extinction largely by the second industrial revolution.

We can make a similar argument for most of what passes as career theory. I would guess that at least 95% of the research on careers assumes vertical mobility. To have vertical careers people not only have to be employed by an organization, they have to move up the organization's hierarchy. With the flattening of hierarchies, opportunities for vertical careers decline. As opportunities for vertical careers decline, most of career theory as we know it becomes superfluous. You don't need to pack much of a parachute if the most you can ever expect is a temporary stop on a corporate stepstool.

Finally, consider the M-form organization. The strategy of building multi-divisional, diversified organizations has spawned a reasonably large research literature. Chandler and others associate this strategy with the growth of corporations in the mid-20th century. Yet Jerry Davis has shown that the merger mania of the 1980s can be interpreted, among other things, as a death knell for the M-form.

My point is that at least some of the key concepts on which our theories rest are being emptied of descriptive relevance. Like it or not, this is because social theories are tied to historical contexts. If historical contexts change, then sociological theories, in general, and organizational theories, in particular, also have to change. As the other speakers have repeatedly noted, this is why fieldwork is so important as we head into the 21st century.

New concepts don't come attached to the data we buy from the federal government (or any other organization). They aren't manufactured by powerful statistical techniques either. Please don't misinterpret me. I have nothing against survey data or statistical inference. I use both when I think they're appropriate. What I am trying to say is that data and data analysis per se are only as meaningful as the conceptual lens you use to make sense of them. The scariest question that any discipline can face, especially one that has pretensions to science, is this: What do you do when you realize that your concepts are so out of touch that your data don't mean much anymore?

One answer is that you better start doing fieldwork. Without fieldwork we have little hope of building the kind of concepts we need to grasp the changes that are happening around us. This is because descriptive adequacy is the *sine qua non* of a good theory. As Anselm Strauss has tried to tell us for years, building descriptively adequate concepts is fieldwork's forte because it forces you to take comparison and observation seriously. Every natural science, no matter how deductive it may be today, was built on a foundation of naturalistic observation. Some say the social sciences differ from the natural sciences primarily because the phenomena we study change more quickly. If so, then the conclusion is inescapable: Social sciences must always be in the business of shoring up, if not rebuilding, their naturalistic base to remain, at minimum, descriptively adequate.

I want to close by suggesting four areas where I think organizational theory could use a little more of the descriptive knowledge that fieldwork could bring. First, is the contingent labor force. We know from macroeconomic data that the part-time and contract labor force is growing, that it is growing rapidly, and that the most rapid growth is among professional and managerial job holders. But what is contingent work? What leads people to do it? What is the world like for contingent workers? What implications does contingent labor have for the security of the individual, the structure of families, or even the welfare of a firm? I don't think we have answers to these questions and I don't think we will ever have answers, unless we start doing fieldwork and life histories with contingent workers.

Second, consider interorganizational relations, especially strategic alliances. Since the Reagan administration deconstructed the antitrust laws, numerous opportunities have opened for forms of collaboration between organizations that would have previously been thought suspect, if not illegal. At the moment, doing research and theory on interorganizational collaboration is something of a growth industry. Yet most of this work uses distant data and high level abstractions to explain what strategic alliances are about. I include here my own work on strategic alliances in biotechnology. Yet despite burgeoning interest, there is almost no systematic research on what people actually think about or do when they're forging the alliances that we later code, count, and analyze. What really matters when firms form relationships? How do firms choose partners? How do they break up? These questions and similar questions are crucial to understanding alliances and they are questions that are best answered by fieldwork because they require access to insider's knowledge that no network algorithm can produce.

A third area is management as an occupation. Our theories pretty much pretend that management is a single occupation. This is certainly not true today, if it ever was. Considerable specialization has occurred among managerial occupations over the last 30 years. Among the managerial labor force, proto-occupations seem to be proliferating. Some even look like professions. But how do these managerial occupations differ from one another? Do people in some of these occupations have different kinds of careers than others? Do we even know what these different kinds of manager's manage? I think not. I don't think we'll ever know without careful fieldwork and, I might add, fieldwork that does more than count the number of interruptions per minute in a manager's day.

A final area where we could profit from considerable fieldwork is our understanding of organizing in the military. Ironically, the field of organization studies drew heavily on studies of the military in its infancy. But sometime between World War II and the end of the Vietnam War organizational theorists stopped studying the military. Some of this neglect reflects practical concerns. Access to military units is not easy to gain. But I think a greater portion of the neglect is explained by ideology. Not only has the military changed considerably since Vietnam, but it may be one of the best places to study certain issues. Since Vietnam, the navy and the air force have become what may well be the most technologically complex organizations in the world. As a result, they have had to confront issues that firms are only just beginning to acknowledge. For instance, how do you collapse a hierarchy in order to take advantage of technical expertise? My sense is that careful fieldwork in military settings could teach us a great deal about how you dismantle hierarchies to get work done. Such studies might also help us understand that there are important social differences between hierarchies built on rank and expertise and hierarchies built on prerogatives and social status.

Moderator: Now all the participants on the panel will have an opportunity to make short second remarks on this question.

Anselm Strauss: I have two brief comments. One is that some people in the audience may be accustomed to statistical, quantitative types of research and nobody here is going to downgrade that, I don't think. Different kinds of research have different kinds of functions and values, but the one question that always comes up if you are statistically trained and confronted with these kinds of materials is how in the world from fieldwork can you generalize about any other organization? Again, if I study one military unit or study one hospital, how can I generalize not just to organizations or management generally, but how can I even generalize even to hospitals from one hospital?

That's a complex question and, if it's broken down, it demands a complex answer which we don't have time for. But in brief, if you keep in mind that though fieldworkers may focus on studying one organization, they often visit and observe many others in a less concentrated way. They may conduct interviews to collect information on other related organizations. They also have an awareness of other related empirical research and have some idea of what is happening elsewhere through examining a variety of archival materials. In this way, fieldworkers can get some sense of verification and validation of what is happening in other organizations. But just as importantly, fieldwork—even in a single organization—can challenge the generalizibility and perhaps falsify theories that are not based on close observation in natural settings. In particular, fieldworkers are typically concerned with developing theories of how things happen, how things work, and how things are changing in organizations. They are talking about properties of what they have observed, the mechanics of the things that they've seen, the range of strategies that people employ, and how these strategies operate in uncontrolled conditions. They don't try to generalize, as statistically oriented research does, in terms of the frequency that something happens or the distributions of outcomes of a process. They generalize in terms of what the mechanics of the organization look like and the processes through which organizations operate in natural settings.

The other point that I want to make is one that is very interesting and one obviously not solved simply with survey work is how a fieldworker handles people being scattered around and workers in the same organization not being spatially located close to one another. How can a field researcher handle networks in organizations that are related to each other but are physically far apart? In order to understand an organization, you may want to understand one part of an organization like a department or division and how it is related to other departments, you may want to understand it's competitors or its allies somewhere else as well. This is a problem that can be addressed because fieldwork involves—as I said—not only intensive field observation but also interviews and reading documents like e-mail or fax or whatever—even surveys. These are problems that field researchers have to face. But there are ways to address these issues.

Moderator: The second question is "Please describe how you generated, refined, revised and tested a concept or set of concepts in the course of one of your favorite field research projects." Bob Thomas will begin.

Robert J. Thomas: What I am going to discuss involves a concept that I called the "aesthetics of manufacturing" in my book *What Machines Can't Do*. What is a bit unusual is that this concept did not spring directly out of my field research. Rather, it emerged as I compared what I had seen in the field—or more accurately what I had not seen in the field—with what I was seeing in a different context which I came to feel had many unappreciated similarities to my collection of field sites.

To be less mysterious, as you might guess *What Machines Can't Do* is about how innovations in manufacturing processes are selected and implemented. I looked at these manufacturing innovations in four different industries: commercial aircraft, computers, aluminum, and automobiles. The bulk of the book describes and analyzes what I saw in these industries and how what I observed forced me to try to reframe more traditional theories about technology choice and implementation. I suspect that someone could take my theory and test it further. But I didn't choose to do that.

Rather, in the final chapter of the book, I discuss a concept that I call "the aesthetics of manufacturing." The book was intended as much for a practitioner audience as it was for a purely academic audience. So in the final chapter, I wanted to suggest some things that I had generally not seen in my field sites that I felt would help firms make manufacturing a true source of innovation and not just a location for implementing innovations that were designed elsewhere. I called what I thought was lacking in the firms that I had observed an "aesthetic of manufacturing."

Now, I happen to think that "aesthetic of manufacturing" is a catchy and meaningful way to express my ideas. But it was also a very grounded way. And it was grounded not in observations of manufacturing operations but in interviews with and observations of Twyla Tharp, the dancer, and her repertory company. These interviews and observations happened as I was struggling with my field data and helped me to see my field data in a new and useful way. The basic insight sprang from my comparing the activities of a dance company to the activities involved in manufacturing and noting similarities as well as differences.

Normally, most people think of dancing and manufacturing as very different. But, as I argue in the book, this is because most people think of dancing and manufacturing in terms of products or objects rather than in terms of the activities involved in producing these objects. When I saw the activities involved in putting a dance production on and talked to Twyla Tharp about what was involved in keeping a dance company going and how she interacted with her company, it made me reflect on what I had seen or not seen in the manufacturing operations that I'd observed.

Of course, there were a lot of things that I observed watching Twyla Tharp, but what seemed most important to me was the way in which she and her company conceived of the process of being a dance company. Specifically, they thought in terms of a repertoire of "products" that they were responsible for, in terms of choreography as a way of innovating in terms of their repertoire, and in terms of rehearsals in terms of maintaining skills and learning about the possibilities of the process itself. It seemed to me that the dance company had a language and a process that allowed them to achieve a mastery over their current performance while encouraging continuous adaptation of their performances to meet different circumstances. Because they understood the process of putting on a performance so well—understood the ins and outs of all the activities—they could simultaneously perform ably in the present and be prepared for change in the future.

As I detail in the book, this was precisely what I felt was missing in manufacturing operations. I felt that the concept "aesthetic of manufacturing" captured what was missing—a language for talking about and evaluating the activities involved in manufacturing and a full appreciation for the complexities of manufacturing operations. I won't go into any more detail since it is discussed in the book. I'll end by remarking again that the concept "aesthetic of manufacturing" was intended as much to alert practicing manufacturing managers to the similarities between manufacturing and performing arts like dancing as it was a construct for theory testing. Nevertheless, the concept does suggest that when viewed from a process perspective that dancing and manufacturing have more in common than is often assumed. This insight might well have implications for academic research that I have not tried to get into.

Mary Yoko Brannen: The longest time I've spent in the field at one site was during my dissertation research. Then, I spent 4½ years at a U.S. paper mill directly after it was taken over by Japanese management studying the evolution of organizational culture. During the two most rigorous data collection years, I spent an average of 15 hours a week at the site. Since entering the profession, people who know things about balancing one's teaching, administrative duties, and, of course, research, not to mention one's life outside of work, have admonished me that it's unlikely I'll experience the luxury of spending such intensive time in the field again—especially pre-tenure. This may be true. If you look at the careers of noted anthropologists like Malinowski, Evans-Pritchard, Dwyer, or Mead, this was so. In the traditional academic career path, anthropologists typically wrote a traditional "realist" ethnography based on some 2 to 3 years field work in a remote country. After they got tenure, the anthropologist would then "revisit" that site (perhaps only intellectually) by writing a more "candid" (and, I should say, much more fun to read) memoir of what they really did in the field a decade earlier.

Yet, I can't imagine that it's possible to go through a full process of inductive theory building unless one is in the field for a substantial period of time. Even when one does spend an initial lengthy period in the field, one is lucky to come out with some ideas or kernels of a model. You certainly shouldn't expect a full blown theory. This takes an iterative approach to concept generation involving data gathering, analysis, coding, reanalysis, refining, more data gathering, and then maybe testing. This just takes a long time.

The 4½ years of dissertation research provided me a strong base for theory-building around a concept I call "negotiated culture" that describes how I see organizational culture unfolding in complex cultural systems. Postdissertation I've been able to go back into the field several times both to the original field site as well as to new sites to extend and test my thinking in regards to this concept. Now, after a total of about 8 years of field work, thinking, writing, more field work, thinking, and writing, I'm finally at a point where I feel capable in pulling my thoughts on negotiated culture together formally in the form of a book. So, what I'll talk about in regards to this question of inductive theory building is the process I've engaged in towards building a theory around the core concept of negotiated culture.

It is quite common for researchers to declare themselves engaged in grounded theory and to cite Glaser and Strauss's *The Discovery of Grounded Theory* as a reference. Somehow, citing Glaser and Strauss seems to make them feel exempt from discussing a priori assumptions or biases brought to the field. But, there was simply no way I could use the Glaser and Strauss trump card and feel intellectually honest! I felt I had a sufficient amount of intellectual baggage in tow I had to come clean with up front before entering the field. I was, after all, entering a bicultural field site to study how Japanese and Americans come together and work out shared ways of doing things with a great deal of prior experience with these sorts of phenomena. I had been born and raised as an American in Japan. I'd had 15 years of cross-cultural consulting experience with Japanese and American firms. I'd spent several years formally studying cultural theory.

In fact, when I started my fieldwork I had well-developed assumptions about the general cultural traits of Japanese and Americans, and I even had hypotheses about where I would and wouldn't find conflict emerge in the course of their interactions at work. As a consultant, I prided myself in being able to deliver what I now have cynically come to call a "two-billiard-ball" analysis of cross-cultural interaction. I would put up a list of the 8 to 10 core Japanese cultural traits and then an accompanying list for the Americans, and point to areas of significant differences and advise clients on how to deal with them. A good example of this is the difference in formality of business style. Japan being an hierarchical culture where many social transactions are mediated through vertical relations, on average, Japanese tend to be more formal than Americans, especially when conducting business. As a consultant, I would spend much time preparing clients to deal with this difference effectively by practicing being more formal through role-plays of typical cross-cultural business situations. For example, we'd spend an hour or so bowing and practicing the proper way to exchange business cards with a Japanese business person—handing the card with two hands, right-side up, and referring to him by his last name with a title (like "san" or "Mr."). The problem was, even though this advice may have been based on robust comparative-cultural data, it didn't pan out in the real world where complex cultural interactions are becoming commonplace. More often than not, the people I trained would come back, thank me for the training, and then proceed to describe how the Japanese they met almost always shook their hands, dismissed the formalities, and said something like "Hey, just call me Mitch, or Kaz, or Tak," or whatever transnational version of their name they had adopted for such cross-cultural encounters.

This same phenomenon where individuals from both national cultures were adjusting their cultural norms to ease interaction, was rampant at my research site. So, early on I had to abandon my "two billiard ball" notion of cross-cultural interaction and begin to watch more carefully and document the cultural change processes at the plant. What I began to realize was that the interactions weren't at all like billiard balls. After all, billiard balls are constrained to do one of two things: they can collide and bounce off of each other without changing each other's shape, or they can miss grazing each other and roll side by side, coexisting but not influencing each other. Rather than billiard balls, the cross-cultural interactions I documented seemed more like nerf balls coming together—where the collisions would leave impact marks on both balls. And, as I observed more of these interactions, wrote them up as field notes, studied them, talked about them, my thoughts continued to evolve.

I began thinking of the interactions as situation-specific, interchanges between individuals that often led to one or the other or both making adjustments in the way they went about work. Also, the more I observed, the more I became aware of power relations between individuals, and gradually the nerf ball analogy seemed insufficient to account for the political dimensions that governed the course of the cultural interactions. I began to see the interaction more like the card game "gin." The cultural norms of the organizational actors were like a hand of cards they were dealt in their socialization up to the point of the cross-cultural exchange. The interaction was like the card game itself—each actor with her own set of cards, neither actor knowing exactly what the other one's hand was like, bringing their cards to the table and trying to play the game. As each actor plays their hand, the other actors react to what has been played based on what has previously been dealt them. Sometimes the card discarded by one, changed the way others looked at their own set of cards. And so, as I became aware of the power dimensions of cultural sense-making, I gradually began to think of the process of organizational culture evolution as a series of negotiations about how things should or would get done at the plant.

Focusing on culture as a negotiation led me to examine the cognitions and actions of organizational members particularly in situations of conflict, because it was in such situations that basic assumptions regarding work that were generally tacit and therefore unarticulated became explicit. Negotiation gradually came to mean to me the construction and reconstruction of divergent meanings and actions by individual organizational actors. Organizational culture therefore was the sum total of all of the negotiated outcomes at any point in time that one chooses to stop and reflect upon the general gestalt of the organization.

Ironically, it wasn't until I took my first academic job at the University of Michigan Business School that one of my colleagues asked me how I saw my concept of negotiated culture as distinct from or adding to Anselm Strauss' notion of negotiated order. I had been so focused on grounding my work in anthropology and organizational culture that I hadn't even heard of Anselm Strauss' theoretical work! I then checked his book on negotiated order out of the library and began to read all he has written on the topic. An initial reaction originating as panic, something akin to, "Oh my God, I thought I was doing something new here, but someone's already figured it all out long before me," slowly subsided as I began to learn from his work and think more deeply about my own. After having read Strauss' work and that of his student Gary Alan Fine on the relationship between organizational culture and negotiated order, I was able to see where my work begins and theirs left off.

My contribution lies in extending the negotiated culture perspective to the international arena where complex cultural systems involving daily interfaces between national and organizational culture have become the norm. Through this method of iterative theory building grounded in field research and constant comparison with existing theory, I have been able to weave together a theory of negotiated culture for understanding organizational culture formation in complex cultural settings

Stephen Barley: The story I want to tell is about technology, language, power, and, of course, barium enemas. I will simply assume that you know what all four are.

My dissertation was a study of how computerized, medical imaging technologies—like the CT scanner and ultrasound—have affected the social organization of work in radiology departments. I didn't have to spend too many days in the field before I began to sense that radiologists had a different kind of relationship with the technologists who ran the computerized technologies than they had with the technologists who worked with traditional x-ray equipment. Part of the difference was that radiologist and technologists seemed to respect each other and collaborate more around the newer technologies. Thus, two questions became important to me. Precisely what was different about that low-tech and the high-tech settings that led me to sense that relations were more egalitarian in the latter? This was a descriptive question, it was about identifying indicators of difference. The second question was: why did the differences occur? This question was about explanation, what led to the differences.

Over time, I came to realize that there were many differences in the social organization of work surrounding old and new technologies. I also came to recognize that there were multiple causes. But the discovery I most remember, because it was so exciting for me, was the day I realized that radiologists actually talked differently to technicians depending on the technology they were using.

At the time, I was standing in a fluoroscopy room watching a barium enema which is a procedure used to diagnose polyps in the small intestines, among other things. Barium is radio-opaque, which means, in effect, that it appears white in an x-ray. The idea is to take a rapid series of films as the barium fills a patient's small intestines. In the hospitals where I was working, these films were taken by radiologists seated in a lead-shielded control booth. Using a remote control, the radiologist would move a fluoroscope over the patient's body while monitoring the flow on the barium on a video monitor. From time to time, the radiologist would record what he was seeing on a film.

Meanwhile, the technologist who was assisting with the procedure would run out from behind the lead booth to remove film plates and insert new ones. The technologist would also sometimes run out from the booth to move the patient around on the table; sometimes to say comforting things to the patient (whose predicament, I can assure you, was not one of particular comfort), and sometimes to control the flow of the barium. So the flow of action was made up of the radiologist sitting calmly at a control panel taking pictures while a technologist repeatedly ran back and forth between the control booth and the examination table on which the patient lay.

What made the technologist run back and forth? It was commands from the radiologist. As the radiologist worked he was constantly telling the technologist what to do: "Change the film," "Roll her up on the side," "Slow the flow," "Check the plug," and so on. The commands were many, they were spoken quickly, and there was never a "please." The technologists rarely said anything to the radiologist during this part of the exam. They simply did what they were told. One day, I had two epiphanies about what must by now be obvious to you. The first was that the flow of talk was always from the radiologist to the technologist. The second, and more important, was that at least 90% of the utterances I had been hearing the radiologist speak were imperatives. The syntax was: verb—direct object.

I remember thinking, "My God, the difference in authority relations across the technologies is actually encoded in the syntactical structures of the radiologist's utterances to the technologists." How elegant! How sociolinguistic!

I immediately launched a field experiment to prove the point. I bought a small hand held tape recorder and began taping barium enemas, upper GI series, and intravenous pyleograms (which are exams done with older technologies) as well as ultrasounds, special procedures, and CT Scans. After I had recorded approximately 20 of each kind of exam, I had the tape recordings transcribed. I then trained coders to classify each radiologist's utterance as an interrogative, declarative, or imperative sentence. I subjected this data to an analysis of variance to show that interrogatives and declaratives were far more characteristic of radiologists' talk when they were working with technologists in the computerized technologies. Conversely, radiologists used more imperatives with technologists who assisted with x-ray and fluoroscopy.

I was also able to show that imperatives were most common in those exams that made use of a control booth. The control booth apparently exacerbated the authority relationship between radiologist and technologist by creating a physical environment in which it was more common for radiologists to use technologists as their gofers. Interestingly enough, technologists claimed they least liked working in fluoroscopy because, there, the "radiologists treat us like slaves." But they couldn't say exactly why they felt this way.

In short, through observation I had found not only a linguistic indicator for why I sensed relations were more authoritarian in the older technologies, I was able to use that indicator to show that the differences were real and that they were tied to physical aspects of the technology. Ironically, I have yet to publish these results.

Anselm Strauss: My answer to this question will talk to the origins and some of the impact of the concept of negotiated order. This influential concept originated and was developed through extensive field observations.

Back in 1958, a group of three sociologists did extensive field observation for about 3 years in two mental hospitals, one a very large state hospital and the other a small private hospital. We began with the small one.

The focus of study was on the ideologies of mental illness evinced by the psychiatrists who were treating their private patients at this hospital (Michael Reese Hospital in Chicago). Some of the psychiatrists believed in psychotherapy and practiced one or another version of talk therapy; others believed that mental illness was somatic—had a biological etiology—and in their treatments mainly used drug therapy; and there was one psychiatrist who adhered to what would later be called community psychiatry.

However, the hospital units were actually run by nurses. The nurses had their own beliefs about causation, and took variable stands on the uses of medication. But these nurses were also much concerned that the wards be run in an orderly fashion, and, of course, had experiences with, and so had attitudes toward, particular patients—particularly recalcitrant patients—and their psychiatrists. Important in the ward administration, too, were a group of residents, of varying lengths of training (first and second year), and who were by and large socialized into the different varieties of psychotherapeutic belief and practice.

So we quickly discovered that the small private hospital was full of people who had very diverse ideas about a great many issues and practices. For instance, the nurses might be pressing a reluctant resident administrator to talk to, and persuade, a psychiatrist whose patient was greatly upsetting the ward that he ought to medicate his patient to calm him down. Under their angry urging, the young resident did finally talk to the psychiatrist, who finally—and angrily—acceded to some medication, provided that it be a small amount and that it be ceased fairly quickly after the behavioral crisis was over.

This represents only one instance of hundreds of incidents that fairly quickly had our researchers talking about people in this place: pacting, making arrangements, bargaining, wheeling and dealing, and the like. As we pursued the implications of what we later summed up under the concept of negotiation, we realized that the work of this hospital—administration, treatment, just plain keeping order and keeping things going—could not exist without an extensive making and remaking of a host of arrangements. True, there were rules, but many of them weren't even known by some of the staff, and others were used and ignored selectively.

We developed another concept which was that of *arena*. We conceived of the hospital as a place where people worked who had come from many different social worlds, and whose psychiatrists had been trained in different settings that had given different kinds of training. In this hospital, conceived of as an arena, people necessarily had to negotiate and create (and recreate continuously) a negotiated order. This conception of an organization as a continuously changing but nevertheless discernible continuity, went directly counter to the then predominant functionalist conception of organizations. Now, since we were primarily interested both in those psychiatric ideologies and in the social order of the hospitals operating in terms of those ideologies, we ended up organizing much of our thinking around the concept of "negotiated order."

When, towards the end of our project, we were asked to write an article for inclusion in an edited volume called *Hospitals*, we wrote about our hospital in those terms. Under the urging of the editor, Eliot Friedson, we suggested that the idea was more generally applicable to organizations. The paper was published in 1963 and got picked up within a very few years because it offered an antidote to functionalist views of organization, and because some sociologists reaching toward more organizational kinds of studies that would still strongly include the perspectives of organizational actors. Ironically, the long monograph we published in 1964, which had a long and detailed chapter on negotiation and negotiated order, never had the kind of extensive impact that the previously published paper did. But from the standpoint of influence, it hasn't much mattered.

The point I would additionally emphasize, for purposes of this panel, is that this concept evolved from intensive field observation at the small hospital by which we began to generate theory, which was followed by interviews and observations in the larger state hospital which helped to extend the theory. I doubt if interviewing alone would have allowed for the full development of the concept.

Moderator: The third question deals with the relation between fieldwork and theory, how theory helped to shape the design, analysis, and reporting of field research results and how theory was revised in the course of field research.

Mary Yoko Brannen: My work on the Walt Disney Company's internationalization experience comes to mind in regards to this last question. When I was a graduate student and reading voraciously in anthropology and cultural critique, I shared the impression of most anthropologists regarding transcultural materialism as a type of cultural imperialism leading to the cultural leveling of the unempowered recipient Other. Accordingly, when I heard talk of a Tokyo Disneyland being built in Japan, my theoretical inclinations were to see it as yet another act of Western imperialism. And, in fact, this theoretical agenda was so strong in me that it led me into the field for the first time. My aim was to study Tokyo Disneyland as a transferred cultural artifact and to use the Disney example to explicate the hegemonic process of transcultural imperialism.

Ironically, after actually doing the fieldwork, I found that in contrast to the prevailing anthropological view, the commodified cultural artifacts of Disneyland were recontextualized in Japanese terms at Tokyo Disneyland. And, in this case, it was Japan as cultural Other that appropriated a cultural artifact from the West (Disneyland) and used it to advance a sense of its own unique identity in relation to its Western and Asian Others. So, fieldwork turned theory on its head, as it were.

Since this initial foray into ethnographic work and thinking about Disney, I've continued to study Disney's internationalization through a similar dialectical process of weighing what theory I had going into the field with what I was actually seeing on site. After completing a Ph.D. program in management, I was armed with management theories in addition to those from anthropology on cultural transmission. So, when I was compelled to do a comparative study of Disney's internationalization attempts with the inception of Euro Disney, my theoretical base had evolved to combine anthropological theory with internationalization theory. I was intrigued to look at Tokyo Disneyland and Euro Disney as cases of global integration versus local responsiveness and make a case for Disney's success in the former case versus dismal lack of success in the latter based on this aspect of internationalization theory. But, I found that, even though the original joint-owners of Tokyo Disneyland, the Walt Disney Company and the Oriental Land Company, espoused a global integration strategy, they actually changed a lot of the Disney formula to meet local demands. And, I also found that the lack of success of Euro Disney (now called Disneyland Paris) could not be attributed in whole to a failed localization strategy. However, by juxtaposing internationalization theory and theories of cultural transmission from anthropology with my fieldwork, I have been able to build theory around the construct of recontextualization that sheds light on the two disparate case histories in internationalization. In brief, a theory of recontextualization explains how the meaning of a product, a core competence, or any firm offering evolves in new cultural contexts. The evolution can present opportunities, as in the case of Tokyo Disneyland, or threats, as in the case of Disneyland Paris, to a firms internationalization attempts. So, again in this second attempt at Disney research, fieldwork turned theory on its head, but it became an occasion for theory induction around the concept of recontextualization.

I think this example of theory building shows that fieldwork and theory can't be separated at all. Fieldwork is, as Anselm Strauss has said, a constant comparative method, where you go in with theory and see whether or not it helps to explain what you are seeing. If it doesn't, you either add to theory or substitute another theory for the one that's not helping you explain what you are seeing.

Stephen Barley: I'm not sure I can speak very intelligently about the relation between theory and fieldwork and still give an honest description of what I do. I know that I try to start a study with a general question that I find personally interesting and that is substantively relevant, whatever that might mean. For example, the question that motivated my dissertation was, How does the social organization of work differ when people use and don't use computerized technologies? "What do microcomputer support technicians do?" is a less well-developed research question that motivated my more recent research. When possible, I try to begin with a comparison and if I don't start with a comparison, I usually try to find one that is suggested by the context itself as soon as possible. This is because I don't think you can build a theoretical understanding of phenomena without comparisons. I also know that I build theory after I have collected data by looking for comparisons, contrasts, and similarities in my field notes or whatever other data I have collected.

But aside from doing comparisons and drawing on my general knowledge of the social science literature as I work, I am not sure I can tell you how I weave theory and fieldwork together. This is because theorizing is for me a situated, cognitive act. Theorizing is a way of understanding and of lending order to what I observe. I sort of know how to do it, but I don't know how to talk about it. I don't have access to my thoughts or even some of my behaviors as I work. Thus, this question actually seems to cry out for an ethnomethodologist interested in the situated practice of ethnographers. To my knowledge, no ethnomethodologist has ever focused his or her analytic tools on the fieldworker at work. Perhaps to learn what we do we need people to watch the watchers because watchers can't watch themselves.

Anselm Strauss: It's a hard question for me to address because I only have a few minutes. The part that I am going to address is this. Many, many people believe that fieldwork is ethnography. But ethnography is a very vague term. It runs all the way from people who simply do descriptions of what it is they think that they see and hear, sometimes very good ones, very accurate. It runs all the way from just pure description all the way to highly conceptualized interpretations of the data.

From my standpoint, most people who do work in ethnography don't squeeze out enough abstraction. They are often extremely nonreflective. By this I mean that their reports are very carefully made, but they haven't thought much about what they didn't say. Part of what you want to be aware of as you think about things to write about is that you can write good theory with qualitative data. And I don't care whether it is field observations or interviews where you are just talking to people and not observing. In fact, many of my students ended up doing not fieldwork, because of pressure of time or jobs or whatever, but did interviews. Yet they did very good theoretically oriented theses with sometimes very good writing.

So my argument has not been that interviews yield unsystematic data but arguing for a more systematic or analytic use of the data than you often find being published. The only proviso on this reflection is that writing of whatever sort—descriptive or analytic—should really reflect what was observed in the field. I think especially that the advice to be flexible and provisional and all the rest of it as you work with data is very beneficial. I don't downplay ethnography. But we should do more of the rigorous kind of data analysis and I think that there is plenty of motivation for writing of this kind.

Robert J. Thomas: I will be mercifully brief. I just set up for myself one problem when I go into the field to do research. There is no way that I can go into the field *tabula rasa*. There is no way that I can go in without presumptions. So what I try to do is set up two sets of books. One set of books has to do with everything that I think is going on, which I would like to bet is going on, which I would like to write about going on. And the other set of books is what I see. And I am constantly aware of the fact that what I see is influenced by what I want to see so I also create circumstances wherever possible where I might be surprised. So one of the things that I tried to do in teaching doctoral students to do field research was that the critical thing was to learn how to be surprised, how to put yourself in a situation where your most deeply held, your most valued hypothesis is put at risk. Unfortunately, there is no single formula for doing that. However, everyone knows or, at least, should know what their weak point is. So my one basic suggestion in addition to all the other ones that have been proffered is that you should, whenever possible, go out into the field with the notion that you will find some way to expose your own particular weakness or bias.