On the Ethnographic Part of the Mix

A Multi-Genre Tale of the Field

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Michael Agar
Emeritus Professor of Linguistics and Anthropology, University of Maryland, College Park MD
Consultant, Ethknoworks, Eldorado NM
magar@anth.umd.edu
www.ethknoworks.com

Abstract

Ethnography plays a partial role in organizational development in ways that call out for a kind of "tale of the field" that represents what Geertz calls a "blurred genre." It is blurred by a mixture of ethnographic epistemology based on abductive logic and context/meaning questions, together with ideas from organizational intervention from work by such figures as Argyris, Schein and Stacey. This article defines and exemplifies the blend with two case studies from a state court and a cancer treatment center. The conclusion is that the tale, blurred genre though it is, is in fact clear and it works. The problem is that political and implementation issues typically block a trial run of the proposed solutions that it generates.

More than a decade ago, while still a professor, I did something strange and decidedly unacademic. It sounds like the beginning of a joke--An ethnographer, a Texan, and a Mexican go into a bar. We three were scuba divers on the island of Cozumel. To go to the grain, as one says in Spanish, out of that cerveza-fueled conversation came a plan to set up a joint venture in Mexico City. I lived there for four months as the *enlace*, the connection, the link, between the two sides. The business failed, but the experience addicted me to action research. And by the end, I had the eerie feeling that I'd learned more about Mexico than I would have had I done a traditional year-long academic study.

The tales to come in this article were originally presented as a talk at the Qualitative Research in Management conference in honor of John Van Maanen's *Tales of the Field* (1988). Thinking back on his book made me remember the Mexico story. In a moment I'll tell tales of two recent projects, one for the California courts, another for a cancer treatment center in New York. Sad but important topics, divorce and cancer, in two of my favorite parts of the world, The Bay Area and the Big Apple.

The moral of the story will be that there are *partly* ethnographic, *non*-academic tales of the field to be told, tales of a type foreshadowed in my Mexico City experience many years ago. Such tales lies in the excluded middle between qualitative and quantitative, between theory and practice, between the academy and the organization. In these tales, ethnography is a major character, a critical sub-plot, and part of the set design, but it is far from the whole performance.

To start, let me summarize some of the threads from the organizational literature that contribute to this strange mix.

I was trained in linguistics and anthropology, not in business and management. But over the years several approaches to organizational development have caught my attention because of their compatibility with ethnography. Let me briefly describe a few of them here.

The first is the work of Chris Argyris, especially his book *Knowledge for Action* (1993). Though he works in a shorter time frame than an academic ethnographer would, much of the process he describes sounds familiar. He works bottom-up with material he finds, uses that material to constrain and elicit subsequent data, and formulates models that he then presents to the organizational "natives" to continue the dialectic of data collection and analysis. The result is a negotiated representation ratified in part by the sense it makes to the people with whom he works. And his distinction between espoused theory and theory in use echoes the emphasis on gathering both interview and participant observation data, just because the differences between reported and actual events contain gaps are pointers to a deeper understanding of group life.

A second source is the work of Edgar Schein, especially the notion that his use of ethnography is "clinical" (1987). This makes sense to an ethnographer in two ways. First of all, a primary goal is to make explicit what for a group is "tacit knowledge" (Polanyi, 1966), to learn the rich out-of-awareness background understandings in terms of which group members conduct their lives, and then to formulate that tacit knowledge in a model so that what was originally strange or incomprehensible can be understood as coherent from an outsider's point of view. Traditionally an ethnographer does this to explain group behavior to an audience of outsiders, typically an audience of colleagues much like him or her. This brings us to the second value of Schein's concept, namely, that in his case the purpose of ethnography remains to make tacit knowledge explicit, but now *to the group themselves* on the occasion of a problem they have. In this sense, the use of ethnography is indeed clinical, with an organizational rather than an individual "patient."

The third source is broader and more recent, tied in as it is with complexity theory. I have written elsewhere about the general epistemological compatibility between ethnography and complexity theory (2004, 2005). Here I will mention just one of its features, though the literature contains many more concepts that will appear in the case studies to come in this article (See for example (Axelrod and Cohen, 2000, Olson and Eoyang, 2001, Stacey, 2001)). One emphasis is the value placed on the know-how and know-that of organizational members who actually engage in a particular process, front-line people if you will. They are viewed as a primary source of expertise in monitoring the organizational environment and in creating innovative responses. In complexity approaches, the argument is that they should be viewed as organizational experts. In this vision, leadership changes from command and control to coordination and enablement. Ethnography, too, has a long history of taking actual practices as the primary data for understanding how the world works, and it, too, presupposes that those closest to actual practices are the critical source of information. Not the only one, but a critical one that is often neglected.

Though I am not trained in business and management, these examples show a strong overlap with the tradition of ethnographic research. In the area of organizational development, applied ethnography at first glance offers appropriate methodological tools. Van Maanen pioneered this argument long ago, and a recent publication suggests that the concept has come of age (Neyland, 2007).

But hold on a minute here. First of all, the term "applied" is problematic in anthropology, since the ethnographic tradition is built on long-term academic research, usually in small communities, with the goal of participation in parochial academic discourse. Second, the context for its application is different, an organization with a problem rather than that typical small traditional community. And third, organizations have their own elaborate histories of problem identification and solutions, not to mention the usual skepticism towards ethnographic research that I know so well from decades of basic research and applied work in the substance use and abuse field.

So it isn't so simple, figuring out what kind of "tale" I want to tell here. It's part ethnography but also part a lot of other things. The tale I have to tell is, to use Geertz's phrase, a "blurred genre" (1983). It becomes confusing when I try and define it in the abstract in terms of traditional categories, but it makes perfect sense when I do it. I'll return to the problem in the conclusion.

First, though, I need to address the issue that my anthropological colleagues always raise, and fair enough. How can you call this thing you do ethnography, they ask me? It's much too abbreviated, way too constrained by the organizational agenda that initiates it, devoid of anthropological theory or traditional methodology. Compared with the academic tradition of ethnography in anthropology, my colleagues are right; it is *not* ethnography. I completely agree. I know the difference. But woven into the tale I want to tell are some strong ethnographic threads—emphasis on the "-ic" in "ethnographic," changing the noun to an adjective, not the thing but having qualities of it. So first I need to make those threads explicit, at least in an introductory way.

There was *something* ethnographic about what I did in Mexico City, if you recall the story with which this article started, just as there was in the courts and the cancer hospital, to mention the cases I will get to shortly. Anthropologists and sociologists complain that such work isn't *really* an ethnography at all. But they are some kind of a kissing cousin, or to put it more elegantly, they bear a Wittgensteinian family resemblance. There's some ethnographic part of the mix, to echo the title of this article, even if it doesn't look like Margaret Mead on Samoa. So, before I describe the two case studies I want to tell you about, let me first mention a couple of the characteristics that I think show the genealogical connection. They are things that I've talked about in other writings. (1996, 2006a). I'll just summarize them here.

The first connection is a kind of logic that I call *iterative recursive abductive logic, IRA* for short. IRA associates to individual retirement accounts for an American audience, to the Irish Republican Army for the Irish and English, and to the International Reading Association for educators, so I know that at least a few readers will remember it.

Abductive logic was created by Charles Peirce, a founder of pragmatic philosophy in the U.S. He invented this logic because he wanted something to explain where *new* concepts came from. In his words, it goes like this (1906):

- The surprising fact, F, is observed.
- If H were true, F would be a matter of course.
- Hence, there is reason to suspect that H is true.

Notice what this logic calls for. It calls for taking surprises seriously and creating new concepts to account for them. No more tossing the problem into error variance. No more testing the goodness of fit of new data against available concepts, as in inductive statistics. Peirce goes on to show how, once the new proposition is in place, more traditional logics are in fact in order to test it out, but for present purposes I'm just going to focus on the abductive part.

Abduction is the heart of ethnography, its great strength. It seeks out unexpected data and creates new concepts to explain them. Notice that it's impossible to write a traditional social science proposal before the study begins. Who knows what surprises and new ideas will come up until you get started? And notice how, in contrast to the usual form of social research, ethnographers are supposed to finish a study with a new concept. or their career is over. If a traditional researcher finishes with a new concept, *their* career will be over. This is only one of many signals of the major epistemological differences among the different logics.

In ethnography, abduction doesn't just apply once. It needs an engine to show how the logic drives and is driven by the research over time. The key question in this kind of work is not, what is the research plan? The key question is, how do I start? After that, the same question, repeated over and over until time and money run out, goes like this: What am I going to do *next*. Ethnography at time n + 1 is a function of what was just learned at time n. It is a path-dependent kind of research.

One of the engines that drives it is *iteration*, which simply means that the logic is applied over and over again. Surprises never stop; just the time and money do. The second engine is *recursion*. That means that as one is working through the logic, building an H to explain a surprising F, another surprise will often turn up. Now one applies abduction *within* the process of applying abduction.

Iteration and recursion, together with abduction, give us IRA, iterative recursive abductive logic. It is not a particularly catchy phrase. But it *is* a key to understanding how ethnography works, and it is a key to the cases I will describe here, even though those applications are not an ethnography by traditional academic rules.

It would be good to describe examples of IRA logic now, but space limits on an article prevent it, and another key characteristic needs to be put on the table. IRA logic is the heart of ethnography. But questions about *context* and *meaning* are its soul.

There are many classic statements here, like Geertz's famous line about people living in webs of significance that they themselves have spun (1973), or Thomas' foundational statement in sociology that if people define a situation as real it will be real in its consequences (Thomas and Thomas, 1928). Another version, one I've written about elsewhere (1995), holds that ethnography is fundamentally about *translation*, about the exploration, learning and documentation of one perspective together with a mapping between it and another.

In a nutshell, context/meaning questions, C/M for short, foreground the simple fact that there are multiple perspectives—multiple points of view—different mental models—in play in any human social research. If one just uses IRA logic, without attending to the meanings and situation as interpreted by members of the various relevant groups, nothing will be learned of those other points of view. And investigating those other, initially unknown, points of view is the goal of the ethnographic process.

IRA logic has to be linked to C/M questions to make it ethnography. Here is an example of IRA logic without C/M questions. In the 1990s I was asked to talk with some youth involved in an epidemic of LSD use. A group of old white guys met to talk about why this epidemic had occurred. We IRA'ed away and figured out that the kids must be children of old hippies who had somehow transmitted pro-psychedelic values to their offspring. Then I talked with some kids. When I suggested the old white guy theory, they looked at me like I'd just stepped out of a flying saucer. Their C/M had to do with a visual adventure in a boring suburban world, not social/political critique and exploration of alternative consciousness. *Our* IRA was way off. But with *their* C/M added in, the epidemic made sense.

Here's an example of C/M *without* IRA. Some speakers of a second language know the rudiments of grammar and have a reasonable vocabulary. In that limited sense, they have a notion of a different context and meaning system. But when they speak, they sound just like they do in their native language, same mentality, same topics, same interpersonal style. My favorite example is George W. Bush speaking Spanish. When I saw him on the Spanish language news in the U.S. during the campaign in 2004, he sounded just like Bush in English. Nothing much changed except the surface symbols. No evidence of IRA at all as a result of learning anything about the different point of view of native speakers of Mexican Spanish.

IRA logic and C/M questions are the heart and soul of ethnography. They guide an ethnographer into another point of view and then help craft a translation between the point of view of the researched group and that of the audience of an eventual report.

It is interesting in workshops and lectures that newcomers to ethnography usually understand and see the possibilities of IRA logic right away. It's more difficult to get them to see how critical the C/M questions are. A robust finding of social psychology's is that humans normally operate in terms of what they call *naive realism,* the belief that one's own mental models equate to objective reality and any others are therefore defective (Moskowitz, 2005). Perhaps naïve realism explains the difficulty. The problem is that use of IRA logic without C/M questions produces a new and interesting way of missing the explanation of what others are up to.

IRA logic and C/M questions are at the core of traditional academic ethnography. They also play a key role in the organizational work to be described shortly, though they are not the whole story by any means. Now let me shift to the first case study. Here is an example of how different--and useful--that logic and those questions look when faced with an organizational problem, but also an example of how their application under those conditions isn't a traditional academic ethnography at all.

I left the university in the 1990s to work independently. One thing I do now is help organizations figure out a problem. It is well known and often discussed that this kind of work means that quick answers and limited time and budget will be the crosses that such projects must bear. A traditional ethnography of a year or two written for an audience of academic peers is simply not relevant here.

Let me describe two examples in chronological order. The California courts first. I'll use that example to build the model. Then I'll describe the cancer hospital to show how it worked there as well.

The central administrative office of the California courts contacted me because they didn't know what to do with the concept of *performance*. They were supposed to start *measuring* it. All of a sudden state government had told them that court budgets had to be centrally administered. Needless to say, this initiated a war among counties and different legal departments, all of them claiming that their unique features meant they couldn't be measured in any *standardized* way. Take family law for instance, said the people in the central office who contemplated hiring me, look at what a mess *it* was. How in the world was a person supposed to *measure* family law performance? All the indicators showed slow and inefficient processes, cases that dragged on forever, lots of people coming in without lawyers, and of course everyone dealing with one of the most traumatic moments in most people's lives.

So I picked family law as the most difficult case and came up with a proposal to see what performance might mean there.

My colleague Steve Guerin and I worked in the family law department in one court in Oakland for two weeks. We did a fair amount of homework on the web before we arrived, that being a major source of background knowledge that helps take the *short* out of *short-term project*. Then we went home to New Mexico and worked on the material, me on the text for a written report, Steve on the court database to develop a visualization of case flow through the system. Finally, we went back to the Bay Area for a few days to tell people--in the court and at administrative headquarters--what we'd learned.

I wrote, at the beginning of the report, that family law was "my baby done left me" versus "order to show cause." The words made a hit because they were funny/sad and rang true. More formally, I called the key dilemma the "narrative/legal schema" conflict. A person in the middle of a divorce is not happy, to put it mildly. They have a *story* to tell, the stuff of soap opera and blues and country-western and gossip and the list goes on. The courts, on the other hand, give out stacks of incomprehensible forms with boxes that need checking and small spaces where proper words have to be used. The story doesn't matter much to the court; correctly completed forms matter a great deal.

Now hold that fact in mind and prepare for a second. Over the previous decade or so, the proportion of people coming into the court *without* an attorney had gone through the roof. We heard several percentages, lower in courts that serve affluent communities, but on the whole the number was large, claimed to be in the range of 70 to 90% in some places, including the court where we worked. Over a fairly brief period of time, voices at the counter and in the courtroom changed. They weren't attorneys as much as they were people suffering one of the most severe emotional traumas of ordinary life.

Now hold that second fact in mind and prepare for a third. Court staff had to enforce the distinction between *legal information* and *legal advice*. The former they could help out with. The latter, absolutely not. Where was the dividing line? Staff interpretations of what they could and couldn't say varied. Some staff had been cowed by judges who called and asked who gave legal advice to a defendant who had tied up an hour with a bunch of gobbeldy-gook. Some staff said the reason for the rule was to insure fairness. No client should get an unfair advantage through insider knowledge. But now that most everyone at the counter or in the courtroom was *not* a lawyer, ability to understand anything at all was at a premium. Insider knowledge wasn't fair. But outsider *lack* of knowledge was not, either.

It was pretty obvious by the end of two weeks that the barrier between personal narrative and legal schema, plus the fact that the number of people appearing without a lawyer/translator had gone through the roof—Performance measures weren't necessary. It was obvious what was going on.

Staff--at the counter, in the help center, in the courtroom--were as frustrated, stressed out and angry as the clients. Considering the impossibility of it all, I was impressed at how even-keeled most people remained, both staff and clients. It was painfully obvious that smoothing the translation between personal story and the official forms would make the family law department much more efficient and much less stressful for all. That story/schema barrier appeared, in practically every move anyone made, and it consumed time and energy and made life more miserable than it already was, in much the same way, over and over again.

This is what the organizational experts would call a *leverage point*. It's not a mystery, the concept, and it has a noble ancestry. In the third century BCE Archimedes said "Give me a place to stand and a lever long enough and I will move the world." An example I found on the web (http://www.thwink.org/sustain/glossary/LeveragePoint.htm): If you try to alter a freighter's course by pushing on its side, you get nowhere. If you apply the same force to the ship's rudder, you change its course. The rudder is a leverage point.

The concept has a formal history in operations research, along with its own think-tank type center at the Department of Defense. On the same web page just cited above, they note that Peter Senge devotes an entire chapter of his book *The Fifth Discipline* to "The Principle of Leverage" (1990):

> The bottom line of systems thinking is leverage—seeing where actions and changes in structures can lead to significant, enduring improvements. Often leverage follows the principle of economy of means: where the best results come not from large-scale efforts but from small well-focused actions.

That quote fits the case I'm describing here well, but Senge also says that leverage isn't all that apparent to most of the actors in an organization. In the California court case that is wrong. Court staff and clients knew what the problem was and both told and showed us repeatedly.

The leverage point in the court study turned out to be pretty obvious. It was even announced on the court web pages in several reports that I read before I left New Mexico. The so-called *pro pers*--legalese for the person representing him or herself--had become the *typical* case in family law, and the typical case didn't know what to do or how to do it, and the staff were told not to help them figure it out, because that would be legal advice.

If that barrier between personal narrative and legal schema could be removed, the results would amplify throughout the system and improve staff and client morale and efficiency. An obvious and likely candidate for a leverage point. A ship's rudder with legs, you should excuse the mixed transportation metaphor.

Now, how to fix it? Monique and Jerry Sternin practice something called Positive Deviance (http://www.positivedeviance.org/). It's a simple idea out of evolutionary biology. Here's how they define it at the start of their web page.

> In every community there are certain individuals (the "Positive Deviants") whose special practices/ strategies/ behaviors enable them to find better solutions to prevalent community problems than their neighbors who have access to the same resources. Positive deviance is a culturally appropriate development approach that is tailored to the specific community in which it is used.

The Darwinian source of the concept is the fundamental idea of inheritance with variation and natural selection. A biological species experiments with different genetic mixes through reproduction and mutation, and the more successful experiments have higher survival rates and more progeny.

When we transfer the concept to humans in organizations, things aren't quite so straightforward. In fact there is an academic movement called positive organizational studies within which the concept of positive deviance is debated and discussed (Spreitzer and Sonenshein, 2004). The idea of *deviance* has a long history with multiple meanings and controversies in social research, and *positive* implies an evaluative stance that is far from simple or obvious. It is easy to imagine problem deviants and conflict over an evaluation as to whether or not it is positive or negative.

For the moment, though, let's ignore the problems and stay with the family law story. Given the leverage point-- the barrier between personal narrative and legal schema--positive deviance leads to the next question: Are there places in the organization where ideas and practices have already been tried that might help re-shape that lever, places where the fulcrum might be found for maximum effect with minimum force?

We found several places. First of all, even as we were doing the project, the court announced a new program that would translate jury instructions into comprehensible ordinary English. That looked like a promising precedent, one that could be investigated for ideas on how to accomplish something similar for the translation problem in family law.

Then there were clients whom we called *street lawyers*, a parallel to the famous phrase *jailhouse lawyer*, referring to a prisoner who spends so much time researching the law and filing petitions that he or she becomes an expert. My favorite example in the California work was a man who had been in the system for years. He ran a group in a community center for other men involved in family law issues. When we first saw him he walked up to the counter, handed over a form, took another one back, and left, all without saying a word. This contrasted with the usual scene where a client tried to find out what he or she needed to do when a staff person announced that the form was not acceptable as filled out.

People like the street lawyer were positive deviants, a powerful resource to help figure out ways to change the forms and the processes to reduce the narrative/legal schema barrier. They had already figured out how to translate for themselves. Several of the staff we talked with were also enthusiastic about the leverage point. For example, the state funded a mediation center, but it was limited only to issues that directly involved children. This bureaucratic rule frustrated both staff and clients, since it chopped an already messy situation into artificial pieces that were of course related to each other. But some of the mediators had also worked out translation models to get from legalese to ordinary language and back again. They, too, could have been positive deviant resources.

In the end, we returned and presented our results, first in the court where we worked--to several different groups--second to the higher-ups in the central state office who had hired us in the first place. The court staff by and large liked the presentation. It inspired different conversations and suggestions. The main criticism was that we hadn't learned enough in some areas, certainly true after only a brief visit, but the main argument as outlined here was well received.

The higher-ups were a different story. My general conclusion was that asking the top level of a state bureaucracy to consider change based on what was learned from lower levels was not a winning strategy. This was a personal reaction born of frustration and disappointment around the implementation issue. The problem will be revisited, briefly, in the conclusion. The state-wide administrators were mostly interested in the visualization of the database rather than changing practices to solve what was a serious problem in family law.

Even though the results died with the report, the model held up, and it produced results that made sense to everyone in the court.

What I and colleagues—court staff and clients—did wasn't complicated on the face of it. In general, an organization has a problem. The definition of the problem is based on one or more system-level quantitative indicators. I can't remember a single job I've done where this hasn't been true. The initial problem is always defined in quantitative terms. Such indicators remain the standard form of credible organizational knowledge, in spite of their superficiality with reference to on-the-ground organizational dynamics. It didn't matter, though, in this case. A giant problem shows up in any kind of data one collects or observation one makes or conversation one has.

Usually the organization will have tried to solve the problem in several different ways. None of them will have worked. This is the motivation for seeking *unconventional* approaches, ethnography being a banner example to most people of a strange thing to do. Less so as time marches on, but still mostly true. I'm always contacted because of ethnography, not because of organizational consulting.

Once the problem is defined in terms of quantitative indicators, I make the transition to IRA logic and C/M questions. The transition works like this: What the indicator measures, its data points if you will, are real moments that involve real people doing real things. Let's call the situations to which those measures refer and from whence they come *tasks*.

The first question: Is there some manageable subset of all the tasks that an organization engages in where those indicators come to life? Can I find the data points in living color?

If so, then I do *fieldwork* in a sample of those tasks, fieldwork being just another name for diving in and using IRA logic and C/M questions with the people who actually perform the tasks in question.

The next step: Analyze the data for patterns in the usual ethnographic way, this "usual way" being a systematic process beyond the limits of an article to describe. And then the next question: Are there one or more patterns that replicate whose dynamics, either historically or cyclically or both, explain the movement of the indicator?

If so, then I have found candidate leverage points.

The next step: More fieldwork. Now, I look at task variation. I try to find tasks where leverage points have already been discovered and used, or are discussed or speculated about, on the part of those who perform it. If I can find them, then I have positive deviant cases. So I do more fieldwork with the positive deviant cases to come up with suggestions for how to use leverage points to solve the problem.

Though I am not a business/management insider, it seems to me that this is classic organizational problem-solving that relies on IRA logic and C/M questions. The logic and questions are the same as any academic ethnography would have used. But they have produced something that is clearly not an academic ethnography, though it is an example of organizational development in the tradition of those gurus of the field cited earlier, people like Argyris, Schein, and Stacey. In fact, I could title what I did like this:

An indicator-driven search for leverage points and positive deviance to find and recommend a solution to an organizational problem.

What I did translates easily into organizational jargon. That easy fit is a gift to ethnographers who want to explain what they do, are doing, and have done to an organizational audience.

Does this approach add anything to what organizational developers, or good managers for that matter, already do? I'm not at all sure. It would of course work for me because of my peculiar background as a linguistic anthropologist. Given the gurus I have already cited, and given the old models of things like TQM and BPR and Six Sigma, and given buzz words like "appreciative inquiry" and "participatory action research"—Perhaps I've just reinvented yet another in a long line of wheels. Readers more familiar with this rhetorical maze than I am can evaluate for him or herself.

I can say that some of the steps—fieldwork and "being there," knowing how to follow on a surprise into an abductive dynamic and let it lead the project someplace new, systematic analysis of uncontrolled data to frame an argument based on the approrpriate logic, iteratively and recursively testing hypotheses on the fly, and that ability to grasp and apply context and meaning questions and know how and when to formulate them—These are tried and true skills of traditional ethnographic training in anthropology, as is the fine-grained attention to ways of talking that goes with the linguistic part of "linguistic anthropology." The linguistic part goes beyond the limits of this article, but see the article by Shotter in this special issue where it is treated in detail.

The model itself is pretty straightforward. It requires a background in IRA logic and C/M questions, which is why I think ethnographers are a good choice to do it, even if they aren't doing academic ethnography. But the larger context of such work isn't so straightforward. Recall that the court project worked as far as its immediate goals went, but it failed as something that the higher-ups in the state bureaucracy wanted to implement.

Here is my naive theory of the organization that describes the problem.

First, organizations come to people like me when they're trying to figure out what I call a paradigm crash. Things that they deal with, externally and internally in some combination, change dramatically. Paradigm crashes now occur on a continual basis. Why? Many are the theories. One argument is global networks. The ever more densely connected world shifts and moves more frequently, and in more surprising ways, than it ever has before. The likelihood that turbulence in any location will reach a particular organization is now higher than it ever has been historically.

Now, most of the problems that organizations have are pretty easy to locate on the ground and come to understand. That's the part I do with the model I just described and exemplified.

But, most recommendations require what the physics types call a *phase transition*, a fundamental change in at least part of the organization. The structure has to change or the process won't change. I think of this as the political part, and in the world of social services it usually blocks the strategy suggested by the fieldwork part. I think the old saw created by Chandler is relevant here, that strategy precedes structure (1966).

Finally, most of those who control capital in an organization--capital in all the senses that Bourdieau invented, material, symbolic, social, cultural--have a vested interest in blocking a phase transition. It means, in their view, and probably accurately, potential loss of those kinds of capital on the part of those who have most of it. This is of course also political, but I think of it as the *implementation* part.

For me, figuring out the model I'm describing here, this blurred genre ethnographic tale, is theoretically interesting, and using it in practice is personally gratifying. But the politics and implementation almost always guarantee that the results go nowhere, even with an enthusiastic response to the results I deliver.

I know how to translate the problem and do the model. I don't know how to influence the political and implementation stages that come later. I know it can happen, but I don't know how to make it happen. The two levels of politics and implementation may well define the limits of the outsider's job. More on this in the conclusion.

Now let's look at a second example, a different kind of organization on a different coast. We shift from the Bay Area to New York City, from law to medicine, from divorce to cancer.

In the California court example, all sorts of system indicators showed a problem. Were there indicators showing a problem in the cancer treatment center? Yes, the problem went by the name of *patient waiting time*.

Waiting time occurred because a patient had to come in, get a blood test, possibly see their doctor, and then usually wait while the lab mixed their chemo, and then wait again until a room was available, *room* being the name for the curtained area where the chemo was administered. Waiting time was measured by the number of minutes from check-in, when the patient arrived, to room-in, when the patient started their actual chemo treatment.

The staff felt that waiting time was too long and that it needed to be reduced. They had done studies, tried experiments, looked at alternative ways to sequence activities--None of the solutions had changed the amount of waiting time by much. In fact, their in-house research suggested that there just wasn't that much slack in the system. You could cut a few minutes off, but not much, given all the things that had to be done and the way they had to be done, while making sure that they were all done carefully. Chemo treatment is dangerous.

None of the tinkering had solved the problem, so they decided to try an ethnographer. They sent me a lot of material and even found a dissertation written by an anthropologist in New York about waiting time in a different cancer facility. Once again, documents and the web played a major role in providing background material. I read them all, and then I dusted off the notebooks and headed to New York.

It was pretty easy to locate where a fair amount of waiting time occurred. The first hint was that the location was called a "waiting room." Waiting involves more than the waiting room, but for present purposes I'll keep the focus there.

The question shifted to what waiting *meant* rather than what waiting looked like. What it looked like was mostly people sitting and reading or staring at a TV or computer screen or talking quietly with a companion. What it looked like was waiting. Observational data as always did play a role. But for this problem I needed to sit with a patient and talk for awhile to get a sense of what waiting meant, because what it meant was less acted out and more thought and felt.

Did patterns appear that explained the movement of the indicator? I learned that the waiting time indicator moved around because cancer itself is a disease that moves around. I learned, from patient interviews, but also from many other sources too numerous to detail here, about the continual changes in the disease and in its treatment over time. An unexpected change—they occurred frequently—impacted on an individual patient, on the flow of patients through the treatment center, and on the way staff spent their time.

The analysis was more difficult than the California court case. In that project, the barrier between personal narrative and legal schema was obvious wherever you looked. In the cancer treatment center, the pattern was more complicated, more challenging. It had to do with the nature of cancer and the way an already complicated treatment process had to be monitored and tweaked on a continual basis, the interaction of those two causing schedules to shift, all the time.

Alp Omur of the hospital staff worked with me on the project, since he wanted to learn ethnography on an apprentice basis. He had already written a report about how surprising changes in treatment were normal, about how the only thing certain about cancer treatment was that it could change from visit to visit. He had also done a study of aggregate data to see what variables might explain waiting time. One of the main variables was an unexpected emergency during chemo treatment, something that usually occurred at least once a day. Throw a dangerous patient reaction into the work flow and everything stops while it is attended to.

The abstract outline of the pattern came clear. Unpredictable change in a patient's disease meant unexpected but necessary changes in their treatment. As a result, waiting time for a single patient could vary in surprising and dramatic ways.

And that same pattern explained variation in waiting time at the system level as well. An early morning with just a few individual surprises could create a scheduling nightmare for the rest of the day. It's like airport delays at LaGuardia caused by a thundestorm in the Midwest, versus departures on a calm spring day with no storms coast-to-coast.

Think of it on an individual level: A patient who had been on a regimen for some time, for whom their blood test showed no changes, for whom the visits were so regular that the pharmacy had their cocktail pre-mixed, for whom the planned use of rooms for the day was on track--Things would go pretty quickly for that patient.

Unless ... Unless something changed with their disease, and things change all the time with cancer. Usually the change appears in the blood test shortly after their arrival. The change in blood test then triggers new contingencies that can cascade into a very very long wait. The details of the many ways this could happen are well beyond the scope of this article. We heard enough stories, from patients and staff, to know that the space of possibilities is large and their occurrence is frequent.

Now consider the consequences at the system level. Changes for a few patients on the same day can cascade into a much longer average waiting time as well. Suddenly one patient requires a lot more time than scheduled for, so other schedules are delayed.

This kind of problem is not news among the organizational cognoscenti. Organizational experts have studied and applied queuing theory and supply chain models for years. But this isn't six sigma country. Frequent "errors" are guaranteed to occur, because they are not errors; they are the normal behavior of an unpredictable disease. The hospital, as best as I could tell, worked with the usual organizational vocabulary of averages and plans and standard procedures. By the nature of the disease and its treatment, this kind of language did not fit the problem.

The traditional way of thinking made even less sense juxtaposed with the hospital's commitment to quality of individual care. Virtually all patients I listened to, whatever complaints they might have had, praised the quality of care, the more so if they had had experience with other facilities in the area. But, when the disease suddenly changes, or the treatment creates an unexpected problem, that same institutional commitment meant schedule and plans and averages be damned, the individual crisis had to be resolved. The organization had created its own Batesonian double-bind (1972).

So where was the leverage point? As far as bringing an unpredictable disease and a complicated treatment process under standardized control, I couldn't find many. The two possibilities that came to mind were:

1. Add facilities to handle the worst case scenario, which would mean a lot of wasted and expensive people and space much of the time. Or,

2. Overhaul the entire organization to a complexity model that was more flexible in the face of frequent and unexpected change.

Neither of those recommendations were going to be implemented in my lifetime, though I did recommend the second one as a long-term strategy. It is, in fact, the right answer, but well beyond a brief visit and high on the scale of probable political and implementation issues.

But there was something else, a pattern linked to the meaning of waiting time that we learned from patients. Aggravation or anxiety with waiting time was linked, in their conversations and interviews, with *not knowing* why a delay had occurred or how to *interpret* it. Remember, this is cancer, an uncertainty and anxiety amplifier if ever there was one.

Readers can compare this with a comparatively trivial situation, a delayed flight, to return to the LaGuardia example, an ever more frequent event in our spit and scotch tape air transport system. Variation in how pilots handle delays is striking. Some captains got on the public address system right away and tell the passengers in detail what happened and why and what it means and then keep them updated. Others just say there is a problem. The difference in atmosphere is the difference between people, including the crew, who bond around a shared problem, versus innocent persons in court awaiting sentence for a crime they didn't commit.

The example is trivial compared to a person with cancer, where not knowing is about life and death, where control of one's life has already been taken by a disease and the experts who treat it.

Patients told story after story where an unexpected wait was a source of anxiety or anger. Even if they *did* know something about the wait, they often did not know exactly what was going on or how to interpret it. If it was their own case, they wondered if it was bad news or, more likely, they wondered how bad the news was. If it was a system wait, they wondered what had happened and why the institution wasn't better run. When the people who have taken control of your life run it badly, from your point of view, you get angry.

Front-line staff also told stories, stories about how patients asked them what was going on and they just didn't know. Different parts of treatment were distributed across different locations and involved different staff. A crisis in one location often wasn't known in another.

We also looked at case records to check out some of the stories. The records didn't contain enough information to know exactly what had happened, though there were sometimes hints. For example, one patient complained about a particularly long wait. No one could tell him why it had happened or what it meant. His words and tone in the interview were angry. His case record showed a blood test problem, followed by the absence of his regular doctor, followed by a need to alter his cocktail. Clearly his complaint was justified. Clearly a major disease change had cascaded into a long delay. As another example, a staff member at the desk in the chemo room told us that patients with a long wait might complain but she had no information on her computer except that it was, in fact, a long wait. Overall, fieldwork did support the "something happens, no one knows what it means" pattern that we learned.

But there were some promising threads in the interviews as well. They showed how much easier it was to handle unexpected waiting when the reasons for it and the meaning of it were known. Even if it was personally threatening, or even annoying in how it affected plans for the day, at least it made sense as part and parcel of cancer and cancer treatment, possibly to a fellow patient's benefit if not to one's own. Some patients had simply learned through experience, like the street lawyers in the California court case. Some had relatives or friends who were medically trained. But, especially at the beginning, uncertainty was usually extremely high, so any "wait," of whatever kind, was likely to be felt as a massive unknown threat.

This looked like a leverage point. Figuring out that uncertainty makes waiting aggravating, especially when coupled with a life-threatening disease and a life controlled by the expertise of others--It looks obvious in retrospect. And it is, once you learn it with C/M questions and change how you think with IRA logic.

That problem could be addressed. Handling that leverage point wouldn't eliminate waiting, but it would make the reasons for it and its causes and its consequences transparent, right away, easily, to any patient who wanted to know.

What would such an information system look like? What about positive deviance? There were many small things that looked promising that I neglect here. The strongest example of positive deviance came out of some two-year-old quantitative data. Earlier the hospital had conducted what they called a chemo redesign project. As part of that project, they put some experienced front-line staff into the waiting rooms to work as ombudspersons and answer questions. Informal stories suggested an enthusiastic response from patients. A quarterly survey of patient attitudes showed a remarkable positive jump in evaluations of waiting time during that brief experiment.

We can't be sure exactly what caused what, the usual problem in social research. But those stories and other data suggested that more accessible and transparent information about the system and one's individual situation would make the experience of waiting more tolerable. What we saw in our interviews was a link between knowing what was going on and the stress of waiting. Again, obvious in retrospect. The chemo redesign data, along with the connection between knowing and stress from the fieldwork, made me think the ombudsperson experiment had been on the right track.

Based on this positive deviance, I argued that more transparent information, together with optional links to the background knowledge to interpret it, would at least change the nature of waiting time, if not the absolute number of minutes.

So the final report proposed more detailed analyses of specific cases on specific days. We could build a model of the treatment process, its contingencies and how to track them, so that we could find out just what needed to be known and how to know it. The hospital could take those results and build an information/education system, accessible on demand by any patient or staff member. The system would let a patient check on his/her case as well as the current state of case flow in the facility, and it would hyperlink to the necessary background knowledge to interpret that information, with as much or as little as a patient needed. And, why not hand development and evaluation and modification of the system over to the patients themselves as much as possible. Why not provide the wherewithal to let those who were motivated help build, evaluate and modify it?

This story has a happier ending than the court example. The hospital responded with interest to the project and to the final report. The buzz was that work to make knowledge about the disease and the system transparent to reduce the anxiety and stress that waiting produces would go forward. The case analyses would also reveal inefficiencies in the system that had more to do with the organization than with cancer and its treatment. That would be a bonus to an already clearly defined goal. And the analyses would also produce ideas for indicators more grounded in the details of the process, in real time.

But that project was then put on the shelf because of another urgent issue, prevention of hospital borne infections like MRSA. As far as I know, though, the waiting time recommendations remain on the hospital to-do list for the future. It's hard enough living with a visible life horizon, as most of the patients were. There was no reason to add to the suffering with waiting time due to unknown causes when time has become an enemy rather than an ally. The hospital staff, the patients and I all agreed on this, and together we put together a way to improve the situation, or at least try to. I hope the hospital staff carry it forward. It was designed so they could do so with insiders.

Let me summarize the model once again:

Clarify indicator-based problem

Locate actual tasks that those indicators are meant to measure

Do fieldwork in those tasks using IRA logic and C/M questions

Find leverage points

Find positive deviance

Translate back to the problem with a new strategy

Loop back to the top of this list, probably with better indicators, and try the new strategy out

But encourage experimentation, variations and modifications on the part of those involved in the task

What kind of tale of the field is this? It isn't quantitative or qualitative; it isn't a choice between theory and practice. It certainly isn't an ethnography, though it has ethnographic-like threads of IRA logic and C/M questions running throughout.

Whatever it is, the model summarizes the two cases described here as well as the story about the Mexican-U.S. joint venture with which this article began. It of course wouldn't always be the suitable tale to use as a guideline. I can imagine many situations where it would be impossible to do, other kinds of organizational tales that mix ethnography with other perspectives.

In the two cases described in this article, though, the flow from one part of the model to the next went smoothly. All the steps worked. The mesh of indicator-based problem, organizational development process, and fieldwork yielded new ideas for how to solve the original problem. But, in neither case was the new strategy implemented and evaluated. That final step in the model, the *try it out* step, never happened. That final crucial step, where something new is actually put into practice on an experimental basis--It was missing. And that, of course, was the point of it all.

Why is that?

There are several possibilities here. One is that the work I do and the model I've built are just too weird for words in the eyes of people who work in organizations. I don't think so--I wouldn't, of course--because reactions of people I work with are usually favorable. I get lots of "ahas," a valued ethnographic result, which means I'm telling them things they knew but didn't know they knew it.

Another possibility is that the approach and the strategies are unusual, given the mainstream tradition of organizational research and development and business/management practices. Their application requires better timing, more of a broader trend to reassure the local organization, a tipping point. One example—The well-known Plexus Institute (www.plexusinstitute.org), a center for the use of complexity theory in health care, has now had success with applications around the MRSA issue, under the guidance of the Sternins' "positive deviance" group, mentioned earlier. As in the classic case of Rogers' diffusion of innovation theory (1995), a few early adopters who reported successful stories ignited the logistic growth curve that shows take-off in a larger community.

Or then there's a gloomy possibility, at least for social service programs. A few years ago, when I first started this work, a colleague who consults on complexity in the private sector and I were talking. I told him I wanted to steal the private sector applications and hand them over to social services. He laughed. "They can't adapt," he said. He may have been right, but not because staff and clients don't want to change. Social services do not have the "bottom-line" as a rapid-feedback metric to evaluate changes in strategy. Instead, they depend on funders who are distant from actual practices and who wrap the money in numerous regulatory constraints. The organizations are typically conservative and hierarchical, less fluid when compared to the private sector. And they tend to view their "clients" as the problem cases that they often are, people whose voice isn't to be trusted when it comes to ideas for changing organizational tasks. After decades in the drug field, I wrote a book whose conclusion argues for small no-strings grants to community groups to experiment with local approaches (2006b)

Another possibility are the politics and implementation barriers described earlier in my simple theory of the organization. Hierarchy, ego, control--All those words that come so easily to the lips to describe those at the top, words that in some cases reflect stereotypes rather than reality. Add on non-competitive environments in some cases. If you don't like the courts, what are you supposed to do, go to another court? Even for cancer treatment, where options do exist, if you're at one of the best cancer treatment centers in the country, what do you do next, consult with a tree surgeon?

One thing I know, the model does work, even if it is old Argyris/Schein wine in an ethnographic bottle. It is only one among many possible ways to mix IRA logic and C/M questions with an organizational learning process. Ten years after leaving the university, I can say that the model lets me see and say more clearly what I think is possible when an organization asks me for help.

But what kind of tale of the field is it? Is it qualitative or quantitative, or is it about theory or practice, or is it a real ethnography or not? And who are the authors—Me, the organization, the constraints of the world around it? Yes and no and maybe, all at the same time. And really in the end, it turns out that for the work I do, those arguments just get in the way. The problem with projects like the courts and the hospital isn't how to integrate them into the historically distinct lexicons of multiple academic and practical fields. The problem is how to translate organizational enthusiasm for what such projects can produce into actual and enduring organizational change. It may be that insiders have to do that, not outside consultants. And it may be that insiders in the social service world require high-level change in the highly regulated funding they receive and a social service equivalent to the rapid feedback of a "bottom-line," grounded in real organizational processes and goals, rather than in inappropriate private-sector models of profitability. That way they might be able to adapt and keep adapting as the world changes around them, improve both efficiency and efficacy, not to mention humanity, and solve problems that have been known to staff and clients for years.

Enough of the concluding optimism. President Obama made me do it.

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