

How to Ask for a Study in Qualitatisch

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This article summarizes a keynote lecture presented to the Advances in Qualitative Methods conference. The core problem is the following: A variety of research approaches are now summarized under the term qualitative, yet it is not clear what such approaches have in common. Four characteristics are offered that describe a prototype for what are here called "translation" studies: rich points, abductive logic, a puzzle-solving mode of analysis, and a complexity theory representation. The prototype includes, but is not limited by, the usual terms used to describe such research approaches, including the term qualitative itself. Three examples are offered to show how the prototype applies to public health intervention as well as research.

Right after New Years Day, 1999, I sat in my office thinking about the presentation that forms the basis for this article, thinking that "99" was SPSS code for "missing data." I knew I had a problem I wanted to talk about for the Advances in Qualitative Methods conference, but I could not find the words to articulate it. Then I picked up the Sage Publications catalogue that had arrived in the mail. I leafed through it and noticed what a growth industry qualitative research has become. Roughly 30 titles with publication dates of 1998 or 1997 had something to do with qualitative research. *Thirty* titles, from all over the disciplinary map!

To someone with as many years in the business as I have, this explosion of interest and boom in publications is overwhelming. It does not seem so long ago that people would think anthropology had to do with bugs, ethnography with maps, and qualitative with chemistry. Now, fields as diverse as public health, business and management, education, even architecture and artificial intelligence have discovered "qualitative research" as a "new" approach to problems in their respective fields.

But what, exactly, have they discovered? That is what I wondered as I read through the diverse list of publications in the Sage catalogue. What would you call it? How would you define it?

As befits a group for whom "post-positivism" is an introductory greeting, my guess is that you will agree that a definition based on the old model of necessary and sufficient conditions will not answer the question. The approaches, in the Sage catalogue and in many other books, conferences, and conversations, are too diverse to fall neatly under any simple checklist.

Wittgenstein's (1953) notion of "family resemblance" serves as a better guideline. Family resemblance means what it sounds like—go to a family reunion and you see how relatives resemble each other, "more or less," although you cannot

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always say who is definitely in and who is definitely out. In one example, Wittgenstein writes about the various kinds of "games" that people play. Think of any two games you know about. What features do those two games share? It will not be difficult to think of a third game, one that does not share all those features, although it will have a feature or two that the first two do not have. Necessary and sufficient conditions will not do the job, even though all three games are in the same "family."

Now skip ahead a few decades to Eleanor Rosch's work in cognitive science (Rosch & Lloyd, 1978). She argues that to identify the family you need to specify a "prototype," the "best example" or the "real example" of the family, or the game, or whatever else you are interested in. Once a prototype is identified, other members of the family can be evaluated as more or less members until the examples finally fade so far away from the prototype that one can say it is not a member of the family at all. Families are "fuzzy" categories, with clear prototype centers and ranges of more or less membership.

So, what is the prototype for qualitative research? What are the best examples that a person can use to evaluate the 30 books in the Sage catalogue as good examples, more or less examples, or no example at all?

As it turns out, the label *qualitative* is no help. We are stuck with it, like it or not, because it is an established part of research discourse now, at least in English, German, and Spanish. But it is a dangerously ambiguous term. Qualitative is a kind of data, a propositional rather than numeric construction based on some experience in the world. Yet, the term may or may not be used to signal a kind of method, a kind of theory, or a kind of epistemology. In fact, the most hide-bound positivist might well use "qualitative" data to measure prior variables, and I have never done an ethnography where I have not used numbers. Qualitative confuses the quest for a prototype. We may have to deal with the term, but we do not have to pretend it has a clear meaning.

Another strategy to find a prototype lies in recent tendencies to divide qualitative research into different branches. As some philosopher once said, "When faced with a contradiction, make a distinction." By this strategy, one picks a particular subtype—ethnography, phenomenology, and grounded theory are usually mentioned—and then announces that that is what they are doing. The problem here is this: The subtypes confuse the search for a prototype with its historical sources. Back in graduate school, I was training as an ethnographer, although no one would tell us what that was. We read phenomenology for clarity in the philosophical foundations of what we did, and we were delighted in 1968 when Glaser and Strauss's *The Discovery of Grounded Theory* came out because they made explicit a critical part of ethnography that we all recognized. Because of this personal background, I cannot understand why in the world qualitative should be chopped up into types, all of which are historical contributions to its development. My inner ear tells me it is a step backward rather than forward.

It would be pleasant to reach into my own disciplinary hat and pull out a prototype, but I cannot do that either. Anthropology has the longest history with qualitative research by the very nature of its foundational problem—the exploration of previously unknown and undocumented lifeways. But at the moment, the definitions of *ethnography* and *culture* are contentious issues with no end in sight as the former specialists in "traditional" peoples adapt to the postmodern world. Besides, most anthropologists would argue that "participant observation" is the sine qua non of a

proper prototype. Much as I value participant observation, I do not think that is necessarily true. Participant observation provides the only access to expressions of procedural memory, but semantic and episodic material results from interviews.

So, qualitative will not serve as a prototype, and substituting "historical types" for prototype does not do the job, and the home discipline of ethnography is no longer sure of its own prototype, never mind one that covers the range of things now being done under the charter of qualitative.

As I tried thinking of a name for this family of research, I could not find anything in English that I liked. So, I slid into German, rusty as I am at the moment, and found *Uebersetzung*. I liked the associations, *Uebersetzung*, "over-setting," putting something from one place over into another place. Among other things, *Uebersetzung* means the gizmo you use to transform current from 110 to 220 volts. More to the point, the term is also "set over" into English as *translation*. "Translation," that is what this family of research is all about.

What we usually call qualitative research forges connections between different *perspectives*. "Perspective" itself calls out for an analysis that cannot be done here because connecting perspectives might represent something as grand as earthling versus alien or as local as neighbors discussing the vacant lot between them. Nevertheless, "translation between perspectives" is what I'll concentrate on for now because it helps clarify a prototype for qualitative research while also linking that research with investigative journalism, second language learning, and intercultural communication, among other fields. I think those links are the right ones to have.

For the rest of this article, I will write about translation because that is what the excitement over qualitative is all about. The excitement makes historical sense because *copresence of different perspectives around a shared task* is now the normal rather than the exceptional situation. It really does not matter what location in the world or what particular activity you focus on—the normal case now brings people together who have different perspectives on a shared activity. They might have a common vocabulary and grammar—then again, they might not—but the perspectives that drive those surface symbols can no longer be assumed to be shared.

What is the prototype for a "good" or "real" or "best" example of translation? I am going to go after an answer by thinking of the process of learning to translate because this is what research is all about. I will suggest four aspects of the learning process. After that, three examples will be described, some of which would not usually be thought of as "research" at all. The prototype is an outline but an outline that points toward future work that I hope will yield a theory of translation to characterize this family of research.

THE PROTOTYPE: PART 1

One thing about translation, the problems one deals with must be *discovered* in practice, not assumed beforehand. The first part of the prototype, then, has to show how one finds out what needs to be translated in the first place. Let us borrow some terms from translators and talk about *source* and *target* groups. The target is the perspective that the researcher brings to the project. The source is the perspective of the

group in which he or she is interested. Over the past several years, I have used the term *rich point* to describe the problems in translation that surface when source and target come into contact (Agar, 1994). A rich point, then, is some expression of the source group, verbal or nonverbal or both, that the researcher cannot make sense of. A rich point signals a disjunction between source and target perspectives. Rich points define the problems for translation work.

We assume that a rich point does, in fact, make sense to those with the perspective of the source group. What we, the researchers, do not know is how to explain *how* it makes sense, given our own perspective. Our problem, then, is to go after the source perspective, to model that perspective so that we can see how the rich point does make sense. We build connections between source and target perspectives so that a person from either side can now tack back and forth between the two. From the target point of view, what initially was a problem in understanding now makes sense. Translation between source and target, in either direction, is now possible.

The term rich point has worked well over the years to underline how translation differs from normal social research: Namely, you do not know what the rich points are going to be until after you start the work. Starting a project without knowing exactly what you are going to focus on is not normative scientific practice, to put it mildly. The problem is that rich point does not go far enough. Rich point aims us at a sophisticated *theory of noticing*. Given source and target perspectives in contact, what appears as a rich point, and why?

Let me suggest a few different ways that rich points appear. One kind of rich point, the classic moment of traditional anthropology, is simple *incomprehension*. Something occurs in the village that just does not make any sense. A second kind is *contradiction*. A researcher thought he or she understood the source perspective, but then something happens that is the opposite. A third is *departures from expectations*. One thinks that a concept is now understood, but then another example comes up that fits but not quite. A fourth kind of rich point occurs through *repetition*. Source people do or say something over and over again, and the researcher cannot figure out why. Another type involves packaging information. We know from linguistics that people package information into *old* and *new* when they communicate. Source folks might package things we know but in ways that jolt us because they package as new something that is old to us or something old that is new. A sixth type of rich point involves *arousal* on the part of the researcher—something that happens arouses anger or anxiety in a researcher that is not present among the source group.

Unfortunately, I have just recited a list, not a theory, and a theory is what we need. If rich points truly are the fundamental problems for translation work, then a more sophisticated understanding of how and why they occur is long overdue. Such a theory will make more systematic the relative nature of translation as well. Rich points that emerge are a function of the source and target perspectives, as well as the perspective of the audience for a final report. We never say that we are translators of Hindi into anything. We say that we are Hindi/German translators. The development of a theory of noticing is a major agenda item for the future.

Despite the missing theory, rich points belong in the prototype. Research that does not include problems that arise in the course of the research itself does not belong in the translation family.

THE PROTOTYPE: PART 2

What kind of logic guides translation work? We often speak of *inductive* versus *deductive* logic, inductive meaning any approach in which one gathers data and figures out a theory from there. However, if we go back in time, to the previous turn of the century, we find a different notion, one that helps clarify translation logic.

Charles Peirce, along with William James and others, was one of the founders of American pragmatic philosophy. He defined deductive logic as the process of deriving truths from a set of premises and valid rules of inference. Inductive he defined, in a more limited way when compared to modern use, as showing the truth of the premises from the conclusion. For Peirce, a scientific experiment was an example of inductive, not deductive, logic. From the results of an experiment, one asserts the truth or falsity of a theoretical premise. His definitions of deductive and inductive share one thing in common: Both are closed with reference to the premises. Premises do not change.

This is not so with a third type of logic, abductive logic. Abduction is the logical engine that requires new premises, new ideas about what happened and why. In Peirce's (1903/1997) classic formulation, abductive logic looks like the following:

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. (p. 189)

Notice the fit with the earlier discussion of rich points, the "surprising facts" F that arise in the course of translation research. Furthermore, notice that H is *created* as a plausible explanation, not lifted wholesale from the target perspective that the researcher brought when the study began.

An abductive inference is valid if two conditions are met. First, F must follow from H as a necessary conclusion. One cannot just hallucinate *any* antecedent condition; there has to be some plausible connection between H and F that makes sense, and making sense is, of course, what translation is all about. Second, the inference must be capable of test through "experimentation," what Peirce calls induction. In other words, whatever one comes up with, it has to be put to the test against subsequent experience.

Abduction is now a hot item of research in artificial intelligence. For example, in Hoffman's (1997) work, he speaks about abduction as the business of *recontextualization*, to use a term familiar to linguists, one that echoes the earlier word play with *Uebersetzung* or "setting over." The rich point that does not make sense in the target context and that does make sense in the source context has to be recontextualized in the target perspective. Hoffman also writes about the limits on possible Hs that might be created, depending on what is *historically available*: again, the theme that translation work is a function of what is available from source *and* target. He also shows how abduction develops over time: The results of one abductive inference lead in turn to others further down the road of translation.

Rich points are rich because they are surprising facts F without an available H to explain them. So, a second part of the translation prototype is to create, to invent an H that shows how the rich point does make sense and to do so in a way that leads to further "inductive" work to test it. Abduction, like the theory of noticing described in the preceding section, calls out for more clarification among translation

researchers as well as among those in artificial intelligence. But the fact remains. Abduction belongs in the prototype.

If a piece of research does not show abduction—the creation of new Hs that did not exist prior to the research—it does not belong in the family.

THE PROTOTYPE: PART 3

Rich points define a problem; abductive logic guides toward a solution. But what does the *process* of solving it look like? We already know from Peirce (1903/1997) that one process will, in fact, lead translation researchers down the path of traditional science. Recall that a valid abductive inference must lend itself to experiment, or induction in Peirce's terms. For translation work, "experiment" comes in many guises besides the usual structured type. In the midst of conversations with source group members, one might repeatedly introduce a particular topic as an emergent experiment. Another example—the "natural experiment"—allows one simply to look for the variation that the world naturally provides and then organize it according to experimental logic. As Margaret Mead supposedly said, when asked if she could design an experiment, "The Good Lord usually provides them."

But experiments are not the whole story. Like rich points and abduction, translation research involves figuring out what to do based on the context and what is known at the moment. In fact, the research process follows Piaget's definition of *intelligence* as what you use when you do not know what to do next (Calvin, 1996, p. 1). Given a rich point and an abductive inference, what does a translation researcher do next?

Jump ahead to the end of the process for a moment. At the end, one wants enough kinds of data—of whatever type, from whatever source—so that the H of an abductive inference is supported beyond a reasonable doubt. The legal phrase "reasonable doubt" is not an accident. Philosophers as diverse as Toulmin (1974) and Ricoeur (1981) have suggested legal reasoning as a basis for this kind of research. I have suggested the acronym MOP for this "case closed" end state, in which MOP stands for "multiple overdetermination of pattern" (Agar, 1996). We might call it "multiple overdetermination of H." To make a powerful case for the role of H in understanding a rich point, one must show how H is supported by data from so many different kinds of people, situations, and activities that its *translation efficacy* cannot be doubted. If one can show that H works to translate multiple instances of the original rich point, then its *validity* has been established.

Once again, the artificial intelligentsia have worked on this kind of model under the awkward name of *constraint propagation technique*. I prefer to call it *puzzle-solving*. When you solve a puzzle, first you look for corner and border pieces. Those pieces set some constraints on the possible ways the puzzle can look. Then, you start working with clumps of pieces that fit together, more constraints. Eventually, the pieces start falling into place and you complete the puzzle.

Or consider another example. My old 1978 Toyota finally fell apart, so I decided to buy a new car. How was I going to pick among hundreds of cars on the market? First, I wanted something economical—that constraint ruled out most of them. Then, I decided that after years of driving in a box, I wanted something with flowing lines. Another constraint. I wanted a sunroof, a passenger side air bag—this was a

few years back when they were not always included—and a good sound system, not to mention something that could go 0 to 60 in less than half an hour. By the time all these constraints were in place, I had limited the choice to only a couple of new cars.

This MOP process is key in translation research. To construct an H that plausibly explains the rich point and then validate it, we range across all kinds of situations to get ideas of what it should look like, tinker with it, try it out, tinker with it again, until finally we get to an H that works. MOP work, like the rich point that inspires it and the abductive inference that guides it, is *emergent*, something that develops and unfolds in the course of the research. And MOP work is even more interesting than puzzles, because new pieces appear even as you work on a solution.

Implicit in the notion of MOP are some characteristics of method that will not be fully developed here, although they are discussed in more detail elsewhere (Agar, 1996). One critical notion is the *dialectic* rather than *linear* nature of the research. One is forever tacking back and forth between “data collection” and “analysis” in an unfolding temporal sequence, with the nature of both changing with each step of the process. Another critical notion is the idea of *maximizing differences*. Once an H is pretty solid for some segment of the target group, we seek out other segments that are different as possible from that initial segment to complicate and contradict, *falsify* if you will, the model we have built.

The MOP process needs more development, more details about how it works. But taken together with rich point and abduction, MOP continues the development of the prototype from problem to proposed solution to process for developing and validating it. Once again, I want to argue that a finished piece of translation work will show some MOP processes as part of the final product. If it does not, it does not belong in the family.

THE PROTOTYPE: PART 4

The last suggestion for the prototype is the one I am least sure of because I have just begun exploring it myself. *Complexity theory*, made popular by Waldrop's history (1992), arose at the Santa Fe Institute in the 1980s, where it was developed by an interdisciplinary group—in fields ranging from biology to economics—all of whom had come to the same conclusion: The static linear causal models characteristic of research could not adequately explain the systems that interested them. These systems they described as *nonlinear* and *dynamic*: dynamic meaning the systems had to be observed and explained as they unfolded over time and nonlinear meaning they consisted of multiple, interlinked positive and negative feedback loops that gave them an emergent character capable of surprising the bejesus out of their participants.

Complexity theory is more than I can introduce in an article, although such introductions are available (Holland, 1998). For the present, let me just say that complexity is a kind of “qualitative holistic mathematics,” a phrase designed to awaken the interest of any translation researcher. The process of translation research is, in itself, an example of a complex system, with its self-organization, learning, and emergence, characteristics that traditional linear models of social research do not

handle. The translation research process lends itself to modeling as a complex system.

But there is more than that. For purposes of argument, make an *ontological assumption* that the worlds that the target groups live in are complex as well. They, like the translation researcher, participate in a system organized at different levels made up of interlinked systems of feedback, a system that produces "perpetual novelty" while exhibiting "coherence under change" (Holland, 1998). If we take that ontological step, then we can derive some consequences for what the results of translation work should look like.

Let me try to boil this all down, however preliminary it might be. Complexity looks like a useful general model for translation, with its interlinked feedback loops organized at different levels. Such systems behave and respond in unexpected ways, so they are impossible to predict in any traditional scientific sense of that term. However, they can be *explained*. An explanation consists of laying out the mechanisms that make the system work. In brief, complexity leads us to model how things work rather than how they are.

Complexity, then, serves three purposes for the translation prototype. First, it models the translation research process itself, suggesting ways to show a skeptical reader how a particular model was built and validated. Second, it models the world of the source group, a changing world that nonetheless moves in terms of certain explanatory mechanisms. And third, it models the interactions between source and target group in terms of mechanisms that show the constraints and possibilities for translation when that interaction takes place.

A tall order, poorly justified because of my novice status in complexity theory, but one that fits my intuitions about how translation research works and what it tries to accomplish. Although I cannot conclude as strongly as I did with rich points, abduction, and MOPs, I think it will not be long before complexity will land in the prototype as well. Without a representation of the research process and the research product as complex systems, a piece of research will not belong in the family.

INTERLUDE

Let me recap what has gone on so far. First, there is clearly a dramatic growth of interest in a style of research that is called qualitative. Second, the usual terms used to characterize this research create more problems than they solve, including the term qualitative itself. Third, we need a way to label and define this new research stream, a task aided by Wittgenstein's (1953) notion of "family resemblance" rather than with a traditional closed definition. Fourth, at least for purposes of this article, I have used the term translation as a cover term, translation in the sense of connecting source and target perspectives.

Following Wittgenstein (1953), then, I suggested characteristics of a prototype for the translation research family. One characteristic calls for rich points, problems in translation, that appear after the research process begins. A second characteristic is abductive inferences, in which antecedents are created to explain the rich points. Inferences are tested using traditional research logic. However, they also enter into a process of multiple overdetermination, MOP, using diverse data sources from the

source group's world. Finally, the report of research process and research results exhibits characteristics of nonlinear dynamic systems, of complexity theory.

Using the prototype, one can evaluate particular cases of translation research as "more" or "less" members of the family. Note that the prototype does not distinguish between numbers or propositions or among ethnography, phenomenology, grounded theory, and the like.

Now, I would like to sketch a couple of examples. Over the past several years, I have been working on a variety of projects in the so-called real world, a place I have never successfully located. I have picked two to present here: One project involves setting up crack cocaine prevention programs along the coast of Honduras, and the other examines a TB community-screening program in Baltimore. I picked them because neither of them is easy to classify. They involve numbers and propositions, theory and action, and both are limited in time and resources. In fact, I would not call either of them research in any traditional sense of the term. I include one more example, one that does represent research, and show how that fits the family as well. Due to space limitations, I will only use a part of each example to show how they fit the prototype, the family of translation research.

EXAMPLE 1: CRACK IN HONDURAS

The coast of Central America is different from the mainland, with mixes of English and Spanish speakers and indigenous groups. For some years, the coast has been a "trampoline" for cocaine shipments from Colombia to the United States. Recently, some of those shipments started to leak into local populations; young people along the coast started to use crack. Sponsored by the Organization of American States, I was asked to take a look at one location and make recommendations for starting a program.

Almost no data were available. My colleague from Tegucigalpa, Dr. Kenneth Vittetoe, and I decided that the community was interested in starting a program, not doing research. We first did what we called an *ethnographic exploration*, a fancy term for talking to everyone we ran into and reading the local newspapers. What we quickly and easily learned—it replicated across sources and kinds of people—were the main outlines of problem drug and alcohol use on the island. In a small rural community, it is not difficult. Our next step was to identify a coordinator; again, this was not too difficult because several people immediately pointed out an individual with a history of working with kids, a bilingual minister from a local Pentecostal church. Next, we organized focus groups of people from the affected neighborhoods, although the focus groups were more community organization tools than data-gathering sessions. Finally, we worked with the more motivated members of the focus groups and the coordinator to plan the beginning of a program, emphasizing the need to change and adapt it as they saw fit with experience.

This was most emphatically *not* a research project, although we hoped that the community would generate its own research questions. Nor did the project have much depth and breadth, although at least Dr. Vittetoe was Honduran, and I am a fairly good Spanish speaker with some knowledge of the area. Furthermore, I have to say that my self-indulgent insistence on an early morning scuba dive before the long day and evening of work linked me to the oceanic interests of most of the

residents, not to mention giving me a lot of data from the tourism industry I would not have otherwise obtained.

I want to argue that this community intervention fits into the family of translation studies. Rich points emerged after we started the work. Abduction guided our thinking as we tried to figure out how island life made them comprehensible. We used the MOP process continuously to figure out program ideas collaboratively with the people we worked with. Complexity also figured in the final representation because we linked our model with dynamic ongoing processes in the development of tourism, island population shifts, and patterns in global drug trafficking, among other things.

Ken and I joked that we worked out of an *estamos haciendo algo* model—we're doing *something*. To summarize a bit better, consider the usual intervention, which emphasizes hierarchy with the experts on top; teachings from those experts; research before action; models developed at a distance; numerical data; focus on person; and imposition of structure. Running down the same list, we emphasized participation with experts on an equal footing, learning, organizing a program, local models built on what people were already doing, attention to stories that were told, a focus on neighborhood and community, and improvisation. Those simple differences made for a profoundly different approach, one that the prototype for translation research not only accommodates but requires.

EXAMPLE 2: TUBERCULOSIS (TB) IN BALTIMORE

Johns Hopkins Hospital received a grant to conduct a TB-screening program in an impoverished African American community in nearby East Baltimore. About the end of the second year, it was clear that the program had not worked. Quantitative evaluations showed low success rates, and the community alliance that enabled the project in the first place had disintegrated. At this point, naturally, they called for an ethnographer. I have been there before—when programs are going under, they ask an ethnographer to retrieve something of value from the thousands of dollars spent. My associate Owen Murdoch and I decided to take on the project.

I had done workshops with business students in Austria and learned something about organizational ethnography, so I decided to try a strategy that would fit the limited time for the research. Among all the rich points that came up, I would focus on the ones that signaled organizational problems.

Among the many results, I will just mention two. Everyone I spoke with—from the foundation, the hospital and the midlevel staff—described the program as a failure. Then, I talked with the outreach workers. They were all African American, most of them from Baltimore. When I asked them, and only them, how the program was doing, they all said, "Fine," it had worked well. As I listened and read the transcripts, I saw the word *caring* repeated over and over again. I learned that caring and health care were not the same thing, not for the outreach workers. It was the caring that had gone well.

For many residents of the neighborhood, life was—is—a struggle every day: health care, housing, jobs, all those interlinked problems in which poverty turns up the volume. As a colleague, a psychiatric epidemiologist, said when I asked him what his field had learned after all these years, "Bad things correlate with bad

things." Outreach workers had turned themselves into local referral experts and tried to help with whatever problems a person had. They might not always have gotten a TB screen, but they were able to pass on some helpful advice. For them, the frequency of new TB positives was only a minor part of their work. For them, they had successfully helped out—cared—during the entire project, so as far as they were concerned, the project was succeeding.

A second quick example—the program was built on an alliance between a professor in charge of a department of community health and a minister who ran a local community development organization. Their partnership had a history of success: They had written, been written about, and conducted workshops on research-community alliances. Toward the end of the second year of the TB program, though, the partnership disintegrated. Neither of them would talk to me until the very end because, I suspect, the principal investigator called them and told them about the rough draft of my report. Both, when I spoke with them, said they were writing books about how the partnership had gone sour.

The conversations with both were richer than I can cover here. Much of it boiled down to who controlled what in the many project details, details in which both God and the devil are said to reside. The minister, for instance, asked me at one point if I knew what "indirect costs" were. I did. "Well," he said, "I just learned about them last week." His point was that Hopkins received overhead, and his organization did not. The professor, to give another example, talked about how community decision making was a "fluid process" as compared to the "institutional process" of Hopkins, and the two were often difficult to bring into line.

The TB work was fascinating and frustrating because with the time limits, I could only skim the surface of a parable for race and class in America in the 1990s. In the end, I was able to come up with some recommendations for organizational process—work out a better translation between health care and caring, for example, and negotiate responsibilities and authority across the institutional/community boundary more explicitly. The TB report was more organizational development than research, more private sector diagnosis than traditional evaluation, something I hope to develop more thoroughly for social services one day.

For now, I just want to say that organizational development is another member of the family, mixed bag of research and action that it is. The TB project began with rich points and abductive inferences and featured MOPs as the principal guiding process. Complexity helped frame conclusions that showed the dynamic nonlinear interactions, not only within the program but also between the program and the changing history of hospital and community and the diverse social identities that make up the city. The TB work was also part of the translation family.

EXAMPLE 3: HEROIN TRENDS

I am in the middle of a study of heroin trends in Baltimore right now. In fact, this study is what finally drove me to explore complexity theory in a systematic way. In the Honduran and Baltimore work, I used it without knowing it. Now, I am trying to know it before I use it.

Drug trends are usually the numbers reflected in "indicators" gathered in treatment centers, emergency rooms, arrest records, and the like. What we are learning is

that a trend, drug or any other, depends on a number of systems—at least neurophysiology, biography, the historical conditions around the “risk groups,” the policy environment, and patterns of international and local trafficking. Any of these systems are complex in and of themselves, and they all interact with each other in a larger “heroin-using system” that moves the indicator numbers around.

The study is challenging, to put it mildly, in part because the usual way of thinking about a study—be it survey or experiment or any of the qualitative approaches—does not fit what we are doing. We need lots of different kinds of information from various sources. No single source group serves as the primary focus, and rather than test a hypothesis or investigate a particular group perspective, we need to build a model that links several different systems and shows how they interact.

As one quick example, my associate Heather Reisinger and I are looking at the recent increase in heroin use among White suburban youth in the Baltimore metropolitan region (Agar, in press). So far, we have built a model that recognizes three factors. First, the world of youth at the time heroin experimentation started has to be taken into account. In that world, “illegal drug use”—which for adolescents includes alcohol—has been “normalized,” to borrow the conclusion of a recent study in the United Kingdom (Parker, Aldridge, & Measham, 1998). Not that all youth use illicit drugs—far from it—but even the abstainers accept the fact that illicit drugs are a normal feature of the world, not something some unseen “deviant” group does. Furthermore, a new source, Colombia, flooded the Eastern United States with high-quality inexpensive heroin in the early 1990s, and with the growing awareness that the HIV virus could be passed through hypodermic needles, the idea of sniffing or “snorting” heroin gained currency. In a world like this, the stage was set for a possible increase in heroin experimentation among youth.

Second, the model holds that stories of use are the engines that circulate through social networks and drive the trend. Heroin, for many people, produces powerful and pleasurable effects in the early stages of experimentation. Such stories, in turn, fuel increasing experimentation and increasing availability. At the same time, experimentation and later physical dependency generate negative stories that also circulate, as do the youths’ personal observations of their peers. These positive and negative feedback loops increase experimentation initially but then reduce it with time.

Third, the model shows that the same “trend system” works differently in different historical contexts. We draw on a theory of “open marginality,” too elaborate to discuss in detail here, that labels groups who are in negatively valued social identities in which it is known that things could be better but probably will not be any time soon. The model holds that youths from open marginal groups will show higher rates of problem use growing out of initial experimentation. When we compare the working-class suburbs, recently deserted by manufacturing industries, with the more affluent middle- and upper-middle-class suburbs, that is exactly what we find in the treatment and arrest data.

Sketchy as this summary is, it shows that this model-building exercise is part of the translation family because all the characteristics of the prototype play a role. We are awash in a sea of rich points, some of which we are now busily abducting on the way to taking in information following MOP guidelines. Furthermore, although complexity was an implicit part of the earlier examples, because I had not studied it yet, that theory is now an explicit framework that guides this study and its

conclusions. In fact, I see the trend study, in part, as an evaluation of the theory as we try and fit it to the details and ambiguities of an actual project.

Unlike the work on crack cocaine programs in Central America and with TB in Baltimore, the heroin trend study is a pure research project. But unlike most research projects I have done, it aims toward a model of interacting systems rather than a study of a particular group. I know that what I am doing has something to do with my training, but I also know I was not trained to do it. Nevertheless, the trend study fits the prototype of the translation family.

CONCLUSION

What I have presented here is a summary of a beginning, a project to figure out the prototype of a style of research and action—of thinking—that is on the rise as the century ends. The style transcends the boundaries of our shared interests as social researchers, as earlier references to artificial intelligence and organizational development and complexity theory show. Indeed, it is difficult to find any classification in the library or the bookstore in which someone is not arguing that nonlinear dynamic approaches should replace static linear ways. We are simply one group that fits this new historical flow, and we are enjoying a growth in interest as a result. Location may be everything, but timing comes in a close second.

Just when the timing is right, a lot of us start to squabble. With this prototype for translation studies, I have tried to step back and look at the common interests rather than the differences. I will, like the rest of you, continue to talk about qualitative research because we are stuck with the term as a high level code for what we all do in general. But I hope I have left you with the notion, in case you did not have it already, that defining what we do in terms of type of data or intellectual-historical source is not the right way to go. Translation may not be the right term either. I picked it in part because of my own background in linguistics, more as a way to step outside the usual debates than anything else.

The prototype I have proposed is also a sketch, a suggestion, but one that captures some of the critical characteristics of translation-type work. It is important to remember that it is a prototype, not an either/or definition. Different examples of qualitative work will, of course, vary in depth and breadth, usually because of time and resource limits under which some of us work. At the same time, a prototype also should help us talk with colleagues and show what is reasonable given those same limits, help us judge when to hold 'em, when to fold 'em, when to walk away, and when to run. Furthermore, a prototype helps us deal with "sort of" translation work, in which some but not all of the prototype is part of a project-rich points and abduction followed by a laboratory experiment, for example, or a prior inference followed by MOPs, as another. There is nothing wrong in principle with "sort of" translation; given a particular project context, in fact, it might be the right thing to do. But one should be able to evaluate the work accordingly.

Finally, I meant in this article to avoid disciplinary imperialism, to avoid dressing up my own training as a global framework for all research. I can at least promise you that, over the years, comments by some colleagues in anthropology have suggested that my socialization was well south of perfect, and I should say that I trained as a graduate student at an interdisciplinary place called the Language Behavior

Research Lab and that my first job was at a place called the NIMH Clinical Research Center. Although much of this article does grow out of past thinking inspired by problems in ethnographic research, I hope that the translation prototype includes but then generalizes beyond them. We all need to talk about, propose, conduct, and evaluate the work we do, diverse as it is, in ways that transcend specific disciplines, institutions, and programs. The translation family prototype is a step, with any luck in the right direction. At least it is another example of the model my colleague and I used in Honduras, "estamos haciendo algo," "we're doing something." We need to do a better job of translating ourselves, turning the target, now and again, into the source.

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