From:

Thomas S. Weisner (ed.), 2005, *Discovering Successful Pathways in Children's Development: Mixed Methods in the Study of Childhood and Family Life.* Chicago: University of Chicago Press, pp. 185-221.

Tom Fricke is professor of anthropology and director of the Center for the Ethnography of Everyday Life at the University of Michigan. His research, teaching, and publications from fieldwork in Nepal and the rural United States are in the areas of family and kinship, the intersection of work and family relations, and cultural moralities.

6

Taking Culture Seriously: Making the Social Survey Ethnographic

Tom Fricke

> Building on earlier knowledge of the village, living smack in the research site (like any good anthropologist), and keeping the population to a manageable size is paying off with excitement and the riches of detail not possible in Sangila. And look! The return to a village pays off in quantum in the kind of information that can be gotten—I feel like a real live anthropologist again, strolling the village in search of stories, pulling out forms completed by interviewers to tell them that this or that fact needs checking because I *know* the person involved. . . . I am exploding with enthusiasm at being back here. I had hoped that some of the disappointment of the Sangila site (re: coming up with the real mix of anthropology and the survey) would be cleansed here. And so it is. I am a practicing anthropologist here and I'm running a survey at the same time. And I'm having a ball. . . . [Y]ou will be interested to know that the interviewers have been coming back with the censuses and genealogies . . . and have been saying, "Hey, these questions really work here. I see now why you made them that way!" . . . I feel vindicated in constructing some of those questions the way we did.
>
> Nepal field letter, November 16, 1987

> Not so long ago, driving an empty grain truck down a scoria graveled road, hauling my trail of red dust west into the blue sky, following the other grain truck driven by [my friend], listening to the recently returned meadowlarks, and taking in the sweep of all this purity of space I woke up to myself and started laughing. I am enjoying this work too much to call it that! It feels so good to try getting these gears to shift up or down in the ancient truck, grinding away and finally slipping in with that sweet click of accomplishment. Feels good, too, to smell dust and oil and the near ferment of old grain in the bins. And to have grease and dirt worked into the cracks on your knuckles so that you can't quite get it all out by washing your hands. Grass-stained, oil-stained, grain-stained jeans. Cow shit on the boots. With these occasional epiphanies, I break from thinking about all this as data, just long enough to savor the clear stream of my joy at what I do.
>
> North Dakota field letter, April 29, 2000

That famous Bostonian Willie Sutton, when asked why he robbed banks, replied, "Because that's where the money is." In a similar spirit, I would like to suggest that the short answer to why a social scientist would want to conduct field research is because that's where the people are. Although it is not often stated quite so straightforwardly, this seems to me to be the main attraction for the cross-disciplinary appeal of combining social surveys with other forms of data collection that bring researchers closer to the people they study.

Of course, one common way of thinking about mixed methods is to place the use of social surveys against the foil of something called ethnography. The advantages of ethnography and mixed-methods approaches to social research have been repeated in so many publications that it begins to become an issue as to whether anything new can be said. Well-done ethnographies, we are told, give attention to meaning; they are able to do so by requiring observation and immersion in the lived realities of the everyday world (Van Maanen 1988; Shweder 1996; Weisner 1996). In combination with the social survey, ethnography contributes all manner of advantage to the survey alone. Mixed-methods survey-ethnographies, we are told, reduce errors of nonobservation and measurement; they allow relevant new variables to be added to surveys during data collection itself; and they provide insight into the meaning of variables (Axinn, Fricke, and Thornton 1991; Caldwell, Reddy, and Caldwell 1988; Caldwell, Hill, and Hull 1988).

Putting it like this seems to close the book on the topic very much like the way responses to a closed-ended survey question are hoped to exhaust the range of relevant possibilities. But I believe there is more to be said. The problem with putting this in terms of "advantages" is that it leaves intact a

number of assumptions about the relationship between ethnography and the social survey. To speak of advantage is to imply a standard of evaluation. Opposing the social survey to ethnography seems to mask an underlying argument that can never be satisfactorily answered outside the disposition of a particular researcher.

Thus, survey and ethnography in the received view stand in some kind of fundamental opposition that roughly tracks such other oppositions as quantitative and qualitative or science and humanities. In addition to establishing a misleading parity of level that suggests that something called the survey is properly compared to something called ethnography, oppositions like these miss more fundamental issues and confuse discussions. On the one hand, "ethnography" seems to be a synonym for "participant observation." On the other, it appears to include any qualitative method where people are allowed relatively unconstrained talk. The former is discouraging for researchers who lack the time or inclination to spend substantial chunks of a research calendar in the field. The latter leads us to weirdly conflate a variety of methods, such as focus groups and long interviews, as doing essentially the same thing (Fricke 1997a, 1997c). Finally, those researchers who use surveys as a key component of their ethnographic work get categorized as odd hybrids doing neither one thing nor the other. To avoid this, it makes sense to clarify that "ethnography" is not a method in the same way that the social survey is clearly a method for collecting a specific kind of information.

Willie Sutton's answer to his interlocutor entices us by all that it says with so few words. It assumes a whole structure of relationships and a social universe that makes this thing called money workable and desirable to obtain (Searle 1998, 126–128): the cultural agreements, the political economy of banks, and all the rest. Although I cannot compete with Sutton's elegance in my "going where the people are," I imply a good deal with that phrase and would like to use this essay to place it in context and illustrate the value of this activity for specific research questions. This requires a discussion of ethnography, of useful theories of culture that can guide mixed-methods research, and of examples from my own field research. It requires that we rethink our consideration of ethnography as a method and revisit the values of particular data collection activities to our analytical goals.

I want to argue that going to where the people are, that is, having primary researchers actually spend time with the subjects being studied in their "natural" settings, confers special data collection and analytic possibilities of great use to certain kinds of questions for which survey instruments are also a data collection tool. The issue of the researcher's location

with respect to data collection seems to me to be behind many discussions of ethnography's advantage. As Basso puts it, place is the thing that is taken for granted until "as sometimes happens, we are deprived of these attachments and find ourselves adrift, literally *dislocated*" (1996, xiii). The dislocation that researchers feel with respect to reliance on social surveys is relative to their analytic desires. And the place the researcher needs to be is similarly relative.

All this is to say that, without quite realizing it, the contemporary interest in bringing ethnography into conversation with surveys may be a way of smuggling the notion of place back into social research. Thus, in discussing the role of ethnography in human development research, Weisner (1996) was explicit about the centrality of something he called a "cultural place" to any human's well-being and, because of that, to any research pretending to be concerned with human beings. "Ethnography," he writes, "gets us out there in the midst of some cultural place and in the midst of cultural practices and it gets at the meanings and experiences and moral significances of those cultural activities to the participants themselves" (1996, 309). Becker spends a substantial part of his book *Tricks of the Trade* discussing the importance of place: "Everything has to be someplace" (1998, 50–57). Being someplace, however, depends on where you draw the boundaries. One can be different degrees of distance from the people being studied and still share a cultural space. Weisner's attention to behavior or practices points to a location in everyday life. The bounds of shared meaning systems can also extend beyond this local community of face-to-face encounters.

For many, it must seem a contrived kind of housekeeping to make the distinction between ethnography and the researcher's location. After all, most ethnographers are precisely among the people they are studying during their data collection. But the issue arises because several disciplinary traditions highly reliant on survey modes of investigation have recently turned to a concern with meaning and culture (Kertzer and Fricke 1997; King, Keohane, and Verba 1994; Jessor, Colby, and Shweder 1996). Nearly any cultural anthropologist would argue that culture is always important, at the very least by constituting a background horizon against which any individually measured variable achieves its meaning. Still, a good deal of sound research gets done by keeping that meaning in the background. The turn toward an interest in meaning is a novel enterprise for many who have been wedded to the social survey as a primary method. They have appropriately turned to anthropology, the discipline most associated with a concern for culture, for help in pursuing this new interest. The paradox in

that turn is that anthropologists, already doing everything within an ethnographic context and with a notorious, if not universal, distrust of method (Shweder 1996, 15–16), were often ill-prepared to offer what these other practitioners needed. A situation like this cries out for a return to fundamentals on all sides (Fricke 1997a, 248–250; MacIntyre 1988, 355).

Ethnography as Taking Culture Seriously

One way to begin considering what ethnographically informed social research might look like, as opposed to social research that could not care less about ethnography, is to look at the relevance of culture to the specific questions being asked. Shweder suggests that the product of anthropological investigations, also called ethnography, "is about something called culture" (1996, 19). This seems the essential criterion for defining ethnography as an approach in a way that avoids the logical mistakes that come from thinking of ethnography as a discrete method.

We know that outstanding ethnographic accounts have been written in the absence of participant observation. Martin's *The Woman in the Body: A Cultural Analysis of Reproduction* is an example of how a solid ethnography can be written using a sample of women, a team of research assistant interviewers, and a healthy reliance on the long qualitative interview as a primary method (1987, 3–14).[1] We can also think of accounts that are based on some form of participant observation but that are not centrally "about something called culture," or perhaps no more centrally than much survey-based social science. George Orwell's *Road to Wigan Pier* and James Agee and Walker Evans's *Let Us Now Praise Famous Men* are both powerfully detailed documentaries but are generally felt to lie outside the canon for ethnography.[2]

Some of my own work shows how data collected in the social survey can be used to answer ethnographic questions. One such question about meaning and behavior, for example, is whether people grouped into an undifferentiated kinship category such as cross-cousin are treated differently by virtue of whether they are first cousins or a more distant degree of cousin. Some anthropologists might argue that they are not if there is no term to make them different. My own information from a social survey in Nepal suggests that they are treated quite differently in spite of sharing a common kin designation (Fricke 1995, 211). Are the data that allow me to look at this question any less ethnographic for being from a survey?

I define doing ethnography for my students as using a suite of methods

to gather information within an overall orientation and set of research questions that are directed toward cultural understanding. Ethnography is research that takes culture seriously. The methods are various and the specific ones chosen are directed toward gathering data that will open up aspects of culture. The suite includes participant observation, the long unstructured interview, genealogical reconstruction, content and other textual analyses, social surveys designed for later quantitative analyses, and more. The anthropologist Roy Rappaport, when speaking to the classes that we taught together at the University of Michigan, defined anthropology as no more than "the study of the human condition." To the extent that culture is a key element of the human condition for living people, then any method that unlocks a portion of that culture can be thought of as ethnographic.

The Cultural Framework

This raises the issue of what, exactly, culture is. If interdisciplinary borrowing of methods has occasioned its share of mystery, the uses of culture outside anthropology have been at least as confusing and the source of even greater frustration. Hammel comments on one element of that frustration:

> Without putting too fine a point on it, the use of "culture" in demography seems mired in structural-functional concepts that are about 40 years old, hardening rapidly, and showing every sign of fossilization. . . . Over the last 40 years, anthropological theory has moved away from the institutional, structural-functionalist approach it has long presented to its sister social sciences, toward the elucidation of local, culture-specific rationalities, in the building of which actors are important perceiving, interpreting, and constructing agents. (1990, 456)

The example of demography's engagement with the concept is a particular instance of a more general condition in the nonanthropological social sciences. But it is also true that within anthropology itself the urge to be current gives some anthropologists pause when I argue that Geertz's now aging definition of culture, with some modification, may be adequate for contemporary uses (Fricke 1997a, 1997b). The frustration for nonanthropologists comes from this moving-target quality.[3] There seems to be little in the way of a stable definition for this concept across the history of the very discipline to which it is most central.

The concept's tenacity, however, is testimony to its usefulness. Moreover, most theories of culture since the shift from the structural-

functionalist models of the past bear a strong enough similarity to inspire confidence. The title of Hammel's essay, "A Theory of Culture for Demography," implies that beyond the agreed-upon similarities among definitions, the main criterion for favoring one over the other has something to do with its usefulness to the problem at hand.

Geertz's now classic definition of culture as "an historically transmitted pattern of meanings embodied in symbols, a system of inherited conceptions expressed in symbolic forms by means of which men [and women] communicate, perpetuate, and develop their knowledge about and attitudes toward life" (1973, 89), and his further notion that culture constitutes both models *of* and models *for* reality are still serviceable in spite of their age. More recent theoretical statements recycle some version of them (Alexander 1988, 1990), and the crucial mechanisms for bringing Geertz's emphasis on shared meanings and the relative autonomy of culture into individual variation have been usefully developed (Strauss and Quinn 1997; D'Andrade and Strauss 1992).[4]

The important element of Geertz's distinction between culture as a model of and a model for reality is that it distinguishes between culture as worldview (the perceived worlds of human actors that should define the significance of behaviors and institutions for the analyst) and culture as motivation (or the moral worlds of human actors). These two elements find their parallels in another, more recent definition of culture that also highlights the elements of community and construction so important to contemporary thinking:

> I have been telling my own students that a culture is "a reality lit up by a morally enforceable conceptual scheme composed of values (desirable goals) and causal beliefs (including ideas about means-ends connections) that is exemplified or instantiated in practice." Members of a culture are members of a moral community who work to construct a shared reality and who act as though they were parties to an agreement to behave rationally within the terms of the realities they share. (Shweder 1996, 20)

These models of and models for quality of culture suggest something about location. If culture can be taken as a metaphorical place, as Weisner has it, then it is a place that is under construction within a community of existing commitments. Categorical models for reality may exist at different levels of generality beyond the local group. Given the "ethnographizing" of virtually all human cultural groups, researchers have access to many of these without necessarily experiencing them on the ground. But models for

reality, the moral universe in practice, are instantiated in behavior by and among people in concrete communities. Research that means to explore this aspect of culture in action is obviously enjoined to go to where the people are.

Making Culture Usable

These concepts are useless if they imply no procedures for getting at them in the field. Fortunately, Shweder's point that these models or schemes are "exemplified or instantiated in practice" offers an entry into the world of meaning beyond the self-reflection of respondents who might be asked, "What does this mean?" Philosophical literature and anthropological lore both make the case that the relationship between behavior and implied moral goods need not be conscious to the cultural actor (Flanagan 1991, 21; MacIntyre 1992, 16–17). For the analyst, people share a frustrating lack of concern about their culture-based motivations or the details of their underpinning. My own field questions as a doctoral student in Nepal usually elicited the "because that's how it's done" response. But "we do it because that's how we've always done it" is a quite reasonable answer given the process by which cultural notions of the good are internalized (MacIntyre 1992; Blum 1994). This process is largely an unconscious building of habitual orientations and practices. The unconscious quality means that all access to cultural models requires analysis. Both Weisner's and Shweder's mention of behavior ties one possible avenue for entry into cultural analysis to tangible, observable information available to any researcher. This suggestion that such cultural indices exist outside a person's head echoes those other cultural theorists who emphasize that cultural analysis should not deter the empirically minded (Wuthnow 1987; Ortner 1973).

To speak of behavior is to talk about culturally meaningful action. Making analytic sense of that meaning involves interpretation, itself an imaginative act. This is the crux of nervousness for researchers concerned with replicability; it ups the ante for those who are used to accepting their data at face value. It helps, of course, that interpretations are subject to evaluation; they are judged more or less good, or plausible, against the standard of coherence: "Ultimately, a good explanation is one which makes sense of the behavior; but then to appreciate a good explanation, one has to agree on what makes good sense; what makes good sense is a function of one's readings; and these in turn are based on the kind of sense one understands" (Taylor 1985, 24).

In agreement with nervous empiricists, Shweder acknowledges that we cannot get inside other people's heads. But he insists that it is possible to make interpretations of meaning through a similar process of "mind reading" or looking for pattern and coherence among disparate actions or other indices of cultural meaning:

> Whichever interpretation we settle upon, a true ethnography is a mind read in which we rely on our mental state concepts to interpret the discourse and praxis of members of some moral community. Whatever interpretation we settle upon, we do not treat what people tell us in an interview as an incorrigible representation of their inner life but rather as one more piece of information to be made use of, as we construct a model of the mental state concepts exhibited in their behavior. (1996, 28–29)

Both Taylor and Shweder are here concerned with the subjective meaning of action for an individual. But the materials, the unstated frame or background horizon, conferring much of that meaning come from the cultural context in which behaviors occur. That being so, any analysis seems to require a prior interpretive act involving discovery and characterization of key themes, symbols, or scenarios for a given culture. Most anthropologists would argue for a limited number of such key, or recurrent, themes (Ortner 1973, 1989; Shweder 1996). Ortner suggests that among the indications that the analyst has stumbled onto a key symbol are the following: being told it is so by an informant, noticing that people react positively or negatively to it, noticing that an element or theme appears across different domains, noticing an unusual elaboration around a theme or symbol, and noticing a higher level of sanctions and rules around a particular theme (1973, 1339).

Such themes could be of the level of generality as the popular notion of American individualism (Bellah et al. 1996) or the characterization of the leitmotif of reciprocity and exchange in other societies (Fricke, Axinn, and Thornton 1993). They may be highly localized, too, bringing the operation of these more general themes into specific settings where details of local history and face-to-face interactions are relevant (Fricke 1990, 1995; Ortner 1989).

Shweder illustrates the process of imaginatively reconstructing experience in another culture in an ethnographically oriented research program. "Mind reading," he writes, "begins with conceptual analysis" (1996, 27). He suggests we start with the local concept of person and that we work out

from there to local conceptions of the world and the possibilities for action in that world (see also Fricke 1997b, 196–200).

Going to Where the People Are

The nature of the social survey is that it emphasizes information about individuals. The individual is the case (Ragin and Becker 1992). Although it is certainly possible to gather some information on context[5] in the social survey, the overwhelming advantage of this method is its ability to gather standardized information on individual characteristics, behaviors, and attitudes from the point of view of the interviewee. The most suitable analyses for this kind of information are examinations of variation across individuals on specific measures and the patterns of variation between multiple measures across individuals. Because of costs in time and money, the survey by itself imposes severe restrictions on the kinds of information that can be gathered for both individuals and contexts, although that range has been dramatically expanded in some recent experiments (Axinn, Barber, and Ghimire 1997; Axinn, Pearce, and Ghimire 1999).

Although the social survey emphasizes information about individuals, the respondents are meant to reflect the experience of people within some large or small unit. National- or regional-level surveys involving random samples most clearly dislocate information for individual people from the concreteness of their local context but are nevertheless intended to refer to a kind of imagined context for the sampled population. Surveys within communities or neighborhoods most obviously lend themselves to analyses that make use of information from the localized moral community in which the surveyed people live.

While it is always true that culture is implicated in any human activity, the direct relevance of cultural understanding to any particular research varies with the questions being asked. It is possible to imagine a continuum in which, at the one end, a narrow set of individual-level causal questions allows the researcher to more or less ignore questions of constructed meaning and, at the other, a deep concern with meaning makes culture a central theme. Thinking of culture as either a place or a moral community suggests that it is a context in which individual behavior becomes meaningful. The more important concrete and local context is to the research problem at hand, the more valuable it is for the researcher to know about it firsthand. The more important the concern with moral enactment, or models for reality, the more important the local context.

In my own work, I have analyzed survey data sets representing the full

range from national, through regional, to highly localized community surveys. All of the studies I have been involved with have concerned issues relating to family life, individual life course transitions, and intergenerational relationships. The survey data for these studies include nationally representative samples from Taiwan (Thornton and Lin 1994), a regional sample from the Pakistani Punjab (Fricke, Syed, and Smith 1986), and ethnographically oriented community surveys in Nepal (Fricke et al. 1991). I am more recently involved in data collection for a rural community on the Northern Plains in the United States. Although this work varies in the extent to which survey and other methods are combined in the actual data collection process, all of it has been concerned with cultural meanings and themes.

My location as a researcher across these projects has been at varying removes from the people being studied. The Taiwan research involved an island-wide representative sample in an entirely unfamiliar culture region with which I became familiar only through reading existing ethnographies. The Pakistan work also used a representative survey data set but was located within a general South Asian context which I knew from my own research and training. My Nepal work most closely represented the mix of bringing the researcher to the people in combination with social survey since it took place in two communities in which I lived during various data collection phases. And the North Dakota work, still ongoing, brings me closest of all to the people I study, both because of the participant observation that is primary and because of the cultural assumptions I share with people in my study community.

In the remainder of this essay, I want to use the experience of three different projects to illustrate the uses of ethnography and mixed methods in social research with reference to different levels of integration between the survey and going to where the people are.

Culture at a Distance: Making Existing Survey Data Ethnographic

While it is true that all concern with meaning involves an imaginative act, the specificity of the analyst's required mind reading varies. Even without going to the field, ethnographic knowledge can be used with the social survey. This is good news for researchers lacking the time to actually engage in fieldwork; it also means that existing survey data sets can use ethnographic materials, although it places limitations on the range of methodological integration. Bringing ethnographic materials to bear on existing survey data is a useful way to more precisely define the meaning of variables.

Even the addition of meaning as models of reality can leave results open to dramatically altered interpretations. The simple example below comes from an analysis of a survey conducted in a single region in rural Pakistan and involves an analysis of age at marriage (Fricke, Syed, and Smith 1986). The survey benefited from an international collaboration in which variables for local marriage practices were incorporated into the data set. In this example, however, I want to look at a measure that is less obviously thought of as a "cultural" matter.[6]

One of the classic "modernization" variables in the demographic analysis of age at marriage is education. Across the world there are few relationships more consistent than the positive one between these two measures: any schooling at all seems to be related to later ages at marriage; and the higher the schooling levels, the greater the age at marriage. Nevertheless, our understanding of this relationship is deeply interpretive. Older demographic transition theory took education to be an indicator of modernization and argued that it was correlated with secularization, increased rationality, and heightened individual autonomy. Even the successors to this theory seldom question the common meaning of education across settings. Their interpretations of its impact on other variables may differ, as for example when they suggest that parental desires to educate their children keep them out of the marriage market longer. The explanations for a positive relationship between age at marriage and educational attainment have two basic forms in these different approaches. One focuses on the autonomy education confers on individual children (modernization theory), and the other allows for parental controls over the marriage timing of children but suggests a fairly mechanical relationship between the incompatibility of schooling and marriage.

But neither of these explanations is concerned with the specific meaning of education in context, and this might vary considerably across settings. In our analysis of survey data gathered in rural Punjabi villages, my colleagues and I noted that women's education had the usual positive relationship with age at marriage even when controls for numerous other individual characteristics were accounted for in multivariate models. Table 6.1 displays the relationships.

Knowing nothing about the setting, the analyst might be tempted to interpret these results in terms of a modernization framework: education leads to greater autonomy, which leads to more control over one's destiny and results in higher ages at marriage. But the results also show that the substantial fraction of women who attended school only briefly, without

Table 6.1. Education and Age at Marriage among Rural Punjabi Women in Pakistan

Woman's education	A	B	C	(*n*)
No schooling	16.1	16.3	16.4	(72)
Attended, 0 attainment	17.7	17.3	17.5	(74)
1–12 years completed	19.3	19.1	18.5	(19)

Source: Fricke, Syed, and Smith 1986, 501.
Note: A = zero-order mean. B = controlling for birth cohort, father's occupation, and woman's work before marriage. C = controlling for variables in B plus the following: relationship between families before marriage, age at menarche, engagement status, type of dowry, payments to husband's family, marriage type, and wealth flows at marriage.

completing a year, marry at later ages than those who have never attended school at all.

Neither the demographic transition theory explanation nor the incompatibility explanation works for these relationships. My coauthors and I argued that the relationship could not be understood without reference to highly specific features of the local context. We argued that education had become part of a larger world of symbolic indicators of status and that any schooling at all conferred a standing on the natal family of a woman and allowed them to wait longer to marry off their daughter since this status counterbalanced the loss of a woman's value in marriage as a result of increasing age.

We noted that the relationship between education and employment was hardly relevant here. Women were not likely in this setting to be serious supporters of their families in monetary work, certainly not in monetary work requiring education. We also noted that all marriages in this setting were arranged marriages. The failure of standard explanations that ignored context caused us to turn to the ethnographic literature on Punjabi marriages. We focused on literature that discussed marriage within the context of wider relationships organized by family and kinship, including material on the symbolic significance of women, the relation between person and group, the social organization of marriage, prestige systems, and cultural theories of personhood and gender. Our reading of education as a marker of quality, influencing the desirability of marriage connections, placed the experience of schooling within a wider array of prestige markers that operate in marriage negotiation. The actual educational content, and the implications for autonomy in a setting where no woman chooses her own spouse, were secondary to our thinking.

Our examination of schooling converted the measure from a story about education to a culturally meaningful symbol that made a statement

about a woman's family. In so doing, we drastically altered the possible interpretation of an empirical relationship. In the same way any researcher is concerned with plausibility (Becker 1996), our interpretation trumped received models devoid of cultural content because it more coherently accounted for what was anomalous in these other models. It did so, moreover, using a framework that established a consistent culture-based story for all other variables in the analysis.[7]

While attention to existing ethnographic materials can, in a sense, reconstitute the social survey toward ethnography by modifying our understanding of variables, the use of materials from data collections separated by time and space make our interpretation plausible but less conclusive than if we were able to provide testimony from the surveyed sites themselves. Our analytic process in this reconstitution parallels Shweder's injunction that we begin, like the survey, with the individual and locate her within ever widening contextual circles or fields of meaning. Because of the separation between ethnography and the survey, however, these fields are imagined and confined to general cultural models. They do not include the concrete locations of the actual study participants, pay little attention to the details of physical place, and are more inferential in drawing the connection between shared models and individual behavior than if survey and fieldwork were simultaneous. Our confidence was based on the coherence of our interpretation in its ability to bring empirical relationships revealed by survey data into conformity with ethnographies removed from the actual sites in time and space.

Culture Up Close: Social Survey and Participant Observation

> The paper is disappointing. The research was conducted in small villages of Nepal and as such belongs to the new, emerging field of microdemography. The analysis, however, could have been done with a much larger data set. It neither has the advantage of anthropological small-scale, in-depth studies, nor of a large-scale sample survey. (Reviewer's comments on the manuscript that became Fricke and Teachman 1993)

> Fricke's chapter is one of those *Mad Magazine* anthropological articles where you have to memorize the names of districts and clans in order to follow the argument. (Reviewer's comments on the manuscript that became Fricke 1995)

If existing representative sample surveys can make use of ethnographic materials to inform analysis, the possibilities for integrating cultural models and quantitative analysis are immeasurably enhanced by more intensive mixed-methods strategies. Part of this expansion of possibility in the combination of survey with the simultaneous ethnography of everyday life has to do with the sheer variety of data types that can be gathered from the same people. Connections only imagined in the reinterpretation of existing surveys can be made concrete. The concerns about the plausibility of interpretation in the Pakistan example can be addressed with correspondingly closer reference to the actual study participants; hypotheses can be generated with a much deeper specificity, and more information can be brought to bear in their testing; actual mechanisms by which a shared culture becomes personal are observed; the element of discovery is enhanced. Many of these characteristics have been accounted for in the already cited literature on ethnography.

The model of such research for most people is a study that simultaneously combines social survey and residence by the primary investigator in the setting of interest. Even here, there is a good deal of variation in how well the researcher knows her community or individual people in it. My own research of this type has been most developed in ethnographic survey and participant observation research in Nepal with the Tamang Family Research Project (TFRP), an effort to gather data on and understand transformations in family relationships and demographic outcomes in two rapidly changing communities called Timling and Sangila. Although sharing a general cultural orientation and ethnic identity, the two communities were chosen because they differed in their proximity to the large urban area of Kathmandu. The more remote of the two communities, Timling, had been the site of my earlier participant observation research in 1981. Sangila was added as a research site during the combined survey and participant observation phase of research in 1987. In that second research phase, survey instruments were administered to all 1,520 residents of the two communities aged 12 and older (Axinn, Fricke, and Thornton 1991; Fricke et al. 1991).[8]

Data collection by both trained interviewers and primary researchers included lengthy residence in both communities. My earlier fieldwork in Timling allowed an intensification of local knowledge that was not possible in Sangila, where the simultaneous supervision of the survey and involvement in more participant observation types of data collection created a tension between the two activities. As the first field letter excerpt at the head of this chapter shows, local knowledge gained previously at the

remote site allowed the design of a questionnaire well suited to that setting and the use of community knowledge that enhanced my ability to check the accuracy of questionnaires, and perhaps most important, it allowed a mutual familiarity and trust between researcher and study community that created a much more welcoming environment for data collection. Nevertheless, the multilevel analytic possibilities that grow out of this intensive mixed-methods ethnography are well illustrated by the overall project.

The theories of culture that are most useful to researchers who wish to use social surveys are those that allow hypotheses to be developed in terms of local frames of meaning and motivation. In the Pakistani example, we can see that the specific meaning of a common variable can be quite different across contexts. But the contexts include cultural frameworks for motivation that may have profound implications for developing the analytic stories we want to test with our quantitative data. The above discussion of culture as model of and for reality suggests that these frameworks may be made available to the researcher through the experience of everyday life in the community of interest. Shweder's suggestion that we begin with concepts of the person is in line with an interest in those models of reality. Other morally charged themes get at those models for reality that are a part of the motivational contexts for behavior.

Several statistical analyses from the TFRP data make direct use of such models. When primary researchers go where the people are, they have the luxury of both discovering these and identifying their operation in daily life while they are still in the data collection phases of their work. An important characteristic of these models is that they may seem far removed from the research problem motivating the data collection itself. At the same time, the cultural argument is that they are more or less shared frameworks and their variation from person to person will be restricted. Thus, while their exploration may not warrant the allocation of precious space and time on questionnaires, living in the study community allows for both discovery and deeper investigation.

For the Tamang, as for any people, the cultural construction of personhood is a central concept for understanding why people do what they do or how they see themselves in relationship to others. It makes a difference if, as for the Tamang, a person's physical substance is thought to be inherited in highly specific ways from each parent.[9] In Tamang reproductive models, mothers contribute the ephemeral flesh and blood of the body while fathers contribute the enduring bony parts. To an analyst concerned with, for example, explaining changes in age at marriage, this may seem

sufficiently removed from the research problem at hand as to be an item of cultural trivia. But this cultural idea powerfully undergirds the structure of relationships between kin related through the two parents. Coupled with key cultural themes that emphasize reciprocity and exchange, an understanding of these cultural elements informs any analysis that involves relationships between people. As an ethnographer looking for resonances across domains, I might note that the quality of flesh and blood is that it decays when not renewed through reproduction. I might further note that the alliances that are orchestrated through a woman's marriage create relationships that will also decay if they are not renewed through further marriages in subsequent generations. In a contrast that parallels that between flesh and bone, membership in and relationships organized among people sharing patrilineal clan names endure across time and generation. I have used these themes to develop statistical analyses of such demographically important topics as the timing of first birth (Fricke and Teachman 1993), age at first marriage (Dahal, Fricke, and Thornton 1993), and the local politics of marriage (Fricke 1990, 1995).

Discovering basic categories requires an iteration of direct questioning and observation. Specific notions of personal substance can obviously be had by asking. More general cultural themes such as reciprocity require observation in everyday life to gauge their resonance. Ortner's argument that a clue to a theme's cultural centrality lies in its appearance across many different domains (1973) suggests how one might discover them. In the case of reciprocity, its salience emerged as a consequence of its appearance in myth, in the layout of fields, in the everyday behaviors associated with hospitality, and in the request for explicit elaborations of what informants meant when they talked about a "good" wife or a "good" husband (Fricke 1990, 1997b).

For any society, behavior is loaded with meaning in light of such repetitive themes. Nearly any action can take on a symbolic load that conveys a message to those who share a set of cultural assumptions. Often, that message is conveyed in the failure to perform an action. For the Tamang, for example, the theme of reciprocity is played out in the necessity of sharing. Sharing implies an increase in value, not diminution, through the conversion of material items such as food into socially binding relationships. A widespread myth recounts the failure of a young man to report his capture of a tiny bird to his father-in-law when they were hunting together. When the father-in-law discovers the subterfuge, he rebukes the young man, who then gives him the bird. It immediately grows to such a size that

the two of them have to carry it on a pole suspended between them as they return to the village. The story is a charter for relationships between in-laws as much as for the general notion of giving as a moral virtue.

Local Knowledge in Questionnaire Design

If behavior is construed meaningfully as symbolic of wider cultural themes, knowledge of key symbols allows the survey to include measures for meaning in addition to those on attitudes and preferences that already form a part of the survey armament. The Pakistani example above showed how a standard measure might change its meaning in a given context. In the Nepal data collection, my prior fieldwork as principal investigator allowed the research team to include discrete measures of behaviors linked to exchange and reciprocity themes. The TFRP questionnaires included an elaboration of questions on events surrounding the marriage process because of the known importance of marriage to Tamang social organization and kinship relations. Many of these focused on exchanges of labor and goods. Others focused on precise accounts of the existing kinship links between husbands and wives before their marriages. The case of labor exchanges in which new husbands provide help to their in-laws is a good example of a variable that could be interpreted as purely economic or in terms of its meaningfulness within a cultural framework where such behavior is found in mythic themes.

Including such variables on surveys, easily measured because they are behavioral, can open up the possibilities for a dynamic ethnography. Confinement to cross-sectional slices of time is one of the common characteristics of ethnographic investigations, apart from those that involve longitudinal data collections (and even these rarely extend beyond the life of the principal investigator). The inclusion of measures with culturally loaded content allows investigators to talk about change with more empirical precision in spite of their actual presence in the field for single periods. Thus, those questions about the life course transition of marriage and exchanges linked to the ethos of reciprocity can be arrayed by cohort as in table 6.2. The case is discussed in greater detail elsewhere (Fricke 1997b) but serves here to make the point that the behavioral and symbolic buttresses to the ethic of reciprocity are changing across time. The practices of pong (flasks of alcohol) exchange, first-cousin marriage, indirect dowry (goods given by a husband's family and ultimately going to the wife at marriage), and bride service (labor service from a new husband to his wife's family) are all in decline. Such declines in individual practice across time are suggestive of the

Table 6.2. Percentage of First Marriages including Selected Practices by Marriage Cohort in Nepal

	Marriage cohort			
	<1960 (n = 61) (%)	1961–74 (n = 44) (%)	1975–87 (n = 80) (%)	Total (n = 185) (%)
Spouse choice				
Senior	49	41	31	40
Jointly	26	18	33	30
Respondent	14	41	36	30
Pong exchange	66	50	46	54
First-cousin marriage	31	25	24	26
Indirect dowry	61	39	41	47
Bride service	80	73	59	69

Source: Fricke 1997b, 201.

world into which succeeding cohorts of children are socialized and lead to ethnographic questions about cultural change in this community. By themselves, they are only suggestive, but coupled with the change away from arranged marriages and knowledge of community practices, such as the declining participation in ritual events that ratify clan solidarity, the investigator is better able to avoid oversimple and static portrayals of the moral community.

Redirecting Analysis in the Discovery of Meaning

Placing researchers in local contexts has implications that go beyond more sensitively nuanced questionnaires. Measures included in survey instruments for one kind of analysis may be discovered through the experience of everyday life among the subjects of study to have entirely new analytic uses that take advantage of newly discovered meaning. An example from the TFRP involves the inclusion of a question on whether or not a woman returned to her natal home for a period of a week or longer after relocating to her spouse's residence after marriage.

The question had originally been included as a control variable for an analysis of first-birth timing. In the course of combined data collection, I noticed the repeated clusters of recently married women working in the yards of their natal homes with their mothers and sisters. The pattern was

inescapable enough that I began to ask about it in casual conversations, the answers to which motivated me to tape some of the responses. One 34-year-old woman's response to the question of why a young wife would want to return to her natal family home was typical:

> Who knows? It's just something she likes. In her married house, it's a little like she doesn't know the people there. "What sort of work should she do there?" This is in her mind. She has to be small maybe, and where will the food come from? And what work will she do maybe? She's a little unfamiliar [with the new surroundings]. And her own parents' place where she has lived up to then is a little . . . Uhh . . . whenever they see you, it's "Oh daughter, here you must eat this and here's the old familiar work you must do. Here's how much work is left and how much work is finished." And this is what is in her own mind and her heart says, "Go, go [to her natal home]." Even after you've married, when you return home they treat you so special! [But in your marital home] they say, "Do this work! You have to work here!" They don't understand what comes from the heart. Oh yes, she [a daughter-in-law] has to do much work. (Quoted in Fricke, Axinn, and Thornton 1993, 399)

Comments like this sensitized me to the subjective feelings that motivated the action of returning to the natal home: the predictability of its pace, the contrast with the imperious demands of in-laws, and the comforts of residing with people who were well known. But other comments, especially from men, alerted me to other emotions implicated in these visits:

> Oh yes, there's definitely worry—if she stays for longer than a week, one's heart is touched by worry. "Has she gone with another?" or "Why isn't she coming?" This is the kind of thinking one does. . . . But you might hear that she has gone off with another and if this thing happens, you think, "Aho, so that's it." She's done a very bad thing. . . . Yes. If things become so very bad then the husband himself may decide to split, too. Some people, even though they are married, live in separate houses and eventually end the marriage. (Quoted in Fricke, Axinn, and Thornton 1993, 400)

Recognizing the larger context in which these subjective statements played out (the structure and meaning of Tamang marriage as an alliance experienced across generations between families and patrilines), I interviewed other relevant people—in-laws and parents—for their views on

these natal visits. The existence of a survey measure for making these visits, even though it had been included for an entirely different purpose, allowed my colleagues and I to address the issue of how these visits were related to a woman's social security after marriage and how the likelihood of making such visits increased with experiences of autonomy in work and travel before marriage (Fricke et al. 1993).

These findings were extended in a subsequent analysis that showed how such visits affected the timing of a woman's first birth (Fricke and Teachman 1993). We found that socially secure women were most likely to make natal home visits in the first year of marriage and also began their marital childbearing more rapidly than women who did not make these visits. Neither of these analyses would have been undertaken without the unplanned encounters and initial casual conversations in the process of simultaneous survey and participant observation.

How Local Do You Want to Get?

An intrinsic feature of going to where the people are is that the researcher is exposed to a bewildering mass of information, a huge part of it having potential as data. By itself, the simple act of being there is not enough to decide what is relevant. In many respects, the survey mode of investigation has it easy because of its constraints. If information is not recorded in the finite space allotted, then it can be fairly ignored. When the survey is coupled with information available through participant observation and deep community knowledge, analytic possibilities are magnified, and their presentation requires judgment and forbearance for those reviewers unused to such detail.

All interpretation is, at some level, inferential. And all results, even the most quantitative, require interpretation. Ultimately, our acceptance of a particular interpretation rather than another relies on its plausibility. When the investigator goes to a field site, speaks to respondents outside the context of a survey instrument, and gathers reinforcing kinds of information at both individual and other levels, her plausibility arguments go beyond the internal statistical relationships among variables gathered on the survey. There is an "I was there" quality to any argument from experience that cannot be entirely discounted, however maddening it can be for the empirically minded.

But that act of being there also opens up new kinds of investigation, especially those having to do with the discovery of moral communities. Shweder (1996, 34) writes that a moral community is that group of people

who "take an interest in sanctioning and regulating each other's behavior" and who are "usually conscious of themselves, and of their honor, prestige, and well-being as a moral community." One way to think of them is as a kind of "natural grouping" in the sense that their identity and membership are largely determined internally. They can be as small as members of a household, larger extended kinship groups, neighborhoods, and communities. Such groups are precisely those that random samples of individuals will likely miss since their bounds are unlikely to be known in the detail that will allow for sampling. Everybody is, of course, a member of many such communities, and even a sample of individuals will allow access to some of their features as they relate to individuals. But their dynamics as communities require a comprehensive investigation. These dynamics are inherently interesting for a number of cultural questions revolving around such issues as the force of family traditions, historical relations between moral communities organized at the same level, and relative power among groups organized at the same level. Their specificity confers exactly the *Mad Magazine* quality that puts off some reviewers of anthropological manuscripts.

The combination of survey, participant observation, and genealogy offer an ideal opportunity for the researcher to investigate dynamic relationships within locally constituted moral communities. My mixed-methods ethnographic work in Nepal, for example, uses historical relationships among clans and patrilines to show that extended family groupings in Timling have different morally charged traditions. These traditions, moreover, have implications for a whole range of behavior (Fricke 1995), from age at marriage to age at first birth, even when other variables are controlled for in multivariate models. When a variable such as "family membership" (a quick and dirty way to talk about patriline) retains its effects in a multivariate model, the temptation is to say that there is some unmeasured other variable that is causing the relationship. But why shouldn't family identity, or membership in a moral community, be a bona fide "thing" since it clearly can be for the people themselves?

Table 6.3 gives just a taste of how things can vary by this level of moral community. The complexity of the original analysis has been stripped down to just three patriline clusters to illustrate how the distribution of culturally interesting measures plays out against membership in a group. In the table, we can see that Gangle women are more likely than the other two groups to have their marriages arranged by seniors, more likely to have cloth exchanged at their marriages, and much more likely to marry first cousins of a particularly highly charged relationship.[10] Indeed, these Gangle report

Table 6.3. Clan Groupings and Selected Characteristics of Married Women in Timling, Nepal

	Patriline clusters			
	Tamang (%)	Chetgle (%)	Gangle (%)	Total (%)
Spouse choice				
Senior	39.3	33.3	59.0	42.1
Jointly	31.0	33.3	25.0	30.3
Respondent	29.8	33.3	16.0	27.6
Cloth exchange	45.2	41.7	62.5	48.0
Marriage and kinship link				
No relation	32.1	30.6	21.9	29.6
FZD/categorical	38.1	13.9	18.8	28.3
FZD/first cousin	16.7	16.7	18.8	17.1
MBD/categorical	9.5	30.6	18.8	16.5
MBD/first cousin	3.6	8.3	21.9	8.6

Source: Fricke 1995, 211.
Note: FZD = father's sister's daughter. MBD = mother's brother's daughter.

that, although things have gone downhill in terms of the power and wealth they once held in Timling, they must still honor their traditions because of who they are.

Insider Culture: The Anthropologist as Native

> Earlier, I had asked [my friend] why he stays in farming. He said because he doesn't have a boss, he can pretty much do what he wants. He said because he gets to work with his family. He said, "because I get to be out in this," gesturing to the land in front of us. To the north the land rose up beyond the alkaline and canted toward the sky, white clouds just over us and darker moisture bearing stuff on the far horizon; to the south the long hollow and then the abrupt rise to West River. [My friend] said he couldn't think of any other job that would let him wake up and stand in this. He can't imagine having to get into a car and drive to a place with four walls. (North Dakota field letter, April 13, 2000)

> In a dry country so much depends on rain. It's all the difference in the world for a farmer. He rises or he falls on the pinpoint splash of water on furrowed ground. Diamond hard truths of this order encourage most of us

> to more tightly link our futures to the present. We know what the payoff is for an hour's employment; we know how the money comes. For the farmer though, all the weeks of planning and all the work of putting seed into the ground are more obviously acts of faith, gestures toward an incalculable future. I can't think of another kind of life where so much is unknowable. And this kind of uncertainty makes for a general reluctance to speak in definites. You don't want to jinx things. Nor do you want the gaudy reputation earned by reckless hubris. No one here would taunt the skies by demanding their due; better to assume the postures of reverence. A good season invites gratitude for unearned grace. (North Dakota field letter, June 21, 2000)

This section proceeds in a dicier fashion than the previous discussions. It rests on work that is still ongoing for one thing. So it lacks the finished quality, the ability to refer the reader to the full published discussion, of the earlier examples. It is also work that up to now has not included a survey of the closed-ended, formal-instrument type that most people have in mind when they hear the word.[11] Most distinctively, I am a near native of the place I am studying—a near native because I was raised just 80 miles east of the field site but native enough in that the topic of my study has to do with the transformation of family relationships that results when children leave their home place as I and my brothers have done.

Since the summer of 1999 I have been doing ethnography in the world of farms, ranches, and small towns centered on a place I call West River. I went there interested in how the culture of work and family gets shaped by a place and its history. And I was interested, too, in how changes in work and career choices might affect relations between those who stay and those who leave. Life course theories tell us that events and contexts will have much to do with how people see the world. My work concentrates on how enculturation in a rural world, with all its implications for how family and work are defined, will structure the responses of people to contemporary American work and family changes.

Studying West River is one project of many conducted through the Center for the Ethnography of Everyday Life. The town is my case study, a single instance of all those places in rural America, especially in the Great Plains, founded on scuttled dreams of robust growth. West River is one of many that advertised its "excellent farming and grazing lands all around, healthy climate, congenial people, fresh air, and sunshine" in an effort to attract immigrants in the early 1900s. People came, mostly German

Catholics, and settled the land in 160- and 320-acre chunks. The town grew, but never much.

West River's history is mostly a history of leaving. By 1970, it began its steady loss of young people and population decline after edging up to its census peak of 799. I looked at the graduating classes of 1973–1975, people in their mid-forties, and found that of the 100 (out of 116) people for whom I could find addresses only a quarter still lived in West River or on its neighboring farms. When the high school principal assigned an essay to the graduating seniors of the class of 2000, only 3 out of 25 thought they would be living in the area five years from now.

For this phase of research, the numbers were scaffolding for the real focus of my data collection. Working as a cultural anthropologist, I was concerned more with the key cultural categories and symbols that local people used to structure their world. Out here, those symbols turned on the relations of work and character, family and place. My efforts to understand West River had me collecting data of many kinds. I spent hours in the community's Benedictine Abbey archives ferreting through a hundred years of historical documents. I collected genealogies from selected families to discover the movements of people from home communities in Europe to the Northern Plains and the later spread of families out of the area. I pored over microfilmed newspapers from the early years of the town, looking for stories from the current residents' ancestors. I lived and worked with a farm family to open up the intimacies of their world in the most direct way possible.

Sharing lives may be the most classic of ethnographic methods. In my case, it meant sleeping in a farm family's spare room and rising at 5:00 A.M. to start the day with them. It meant driving tractor and combine, breaking machinery, and helping with repairs. It meant walking fence line and being bitten by deerflies in a high hot wind. It meant pulling calves when cows needed help with a birth: learning how to tie chains around those delicate hooves, attach them to a pulley, and avoid the pour of afterbirth when the newborn calf yanks free, and learning who calls whom in an emergency. And it meant staying in the fields until the red sun crossed the western buttes at 10:00 and we could all go home to eat dinner.

This project brings me up against one of the paradoxes of doing ethnography: when working with cultures with which we are least familiar, we try to find a means to enter that cultural place or to read the minds of others, but when we work closest to home, we look for distance. Worried about objectivity, we get skittish at the edge of subjectivity in our research.[12] In

this chapter, I have structured these examples along a scaffold of increasing engagement with a local community. The closer we can be, I have argued, the greater our opportunities to make use of materials that link individuals directly to their cultural models. By seeing as much as possible, we enhance the plausibility of our analytic stories.

I have argued, following Shweder's point that we cannot get inside people's heads, that ethnography involves imaginative reconstructions of the categorical and moral worlds of the people we study. The clues we look for are public—in behaviors and stories and the way people talk about the lay of the land. My return to North Dakota and the experience of familiarity after a 20-year absence were also recognitions of the striking difference between that place and where I now live. My sense of homecoming at the return to a cultural place is, to some extent, true for any of us doing ethnography in the United States. The question is whether we can uncover cultural clues that are more subtle than the public symbols and behaviors that we rely on in more unfamiliar settings.

The joke in international demography is that to get a native perspective on international survey research one often relies on a local collaborator who went to the United States to have the culture trained out of him or her in the course of getting a Ph.D. Natives learn to distrust what they already know, in part because that knowledge is part of an unconscious background horizon against which action achieves its meaning (Taylor 1985). Coming back to my cultural place after a long absence brought some of that dilemma to the foreground for me. The question was, could I trust my knowledge? Was it merely subjective? Was I giving up mind reading for self-analysis?

When the farmer whose family I lived with, and with whom I worked when I was not running around conducting interviews or digging through the Benedictine Abbey archives in West River, said, "because I get to be out in this" in the excerpt from the field letter heading this section, I thought I knew exactly what he meant. It was not long after that letter that I wrote another one with a story about learning to drive the tractor while trailing a grain tank, a seeder, and a fertilizer tank:

> [My friend's] tractor is a Versatile with eight wheels steered by a pivot joint in its center. A finger touch on the wheel is enough to move the rear wheels to either side and turn. It pulls the air tank that holds the seed, the air seeder with its shanks, and knives, and coils of pipe that place the seed into the ground, and the tank of anhydrous ammonia that serves as fertilizer for the grains—altogether 97 feet of linked machinery, a third of a football field coiling along the earth. The effect is of mass and motion. [My friend's] fully loaded assemblage is 37 tons of machine, grain, and liquid making its way across the ground at the speed where a fast walk elides into a jog, between 4.6 and 5 miles per hour. A 160 acre field amounts to something like a 40 mile drive on a tractor. (Field letter, April 29, 2000)

The story became one of those classic accounts of anthropological ineptitude and ended with me breaking the machinery. I thought it was funny and innocuous and showed it to my farmer friend and informant.[13] He liked it, and, because I number the letters when I write them, pointed out that this one was number eight and asked to see the other seven. And so began an experiment in which I tested my subjective states and my trial interpretations of the world around me against a native's view of things.

After five months of living with this family, the letters included stories about work, about children leaving home for school and coming back to get married, about the uncertainties of farming, shared work with neighbors, angry disputes over inheritance, and trial shots at the key symbols of Northern Plains living. My informant's response to all this was, "You put this stuff into words that I feel, but I can't say it this way."

Discovering the Salience of Key Symbols

As with my Tamang informants in Nepal, many of the key symbols that organize West River culture play below the consciousness of cultural actors in everyday life. Even when a suspicion exists that a theme or symbol is relevant in a setting, its salience may emerge only from the serendipitous encounters that are a feature of participant observation and follow-up. My discovery of one such symbol, the inside/outside distinction, and its structuring of intergenerational tensions illustrates how being where the people are might generate greater analytic possibilities for understanding how culture and individuals are connected. As Ortner writes, clues to the existence of such symbols are found in their appearance across different domains of cultural expression and in the heightened reaction of people to them. For West River, clues to the existence of the inside/outside distinction as a cultural category can be found in a reasonably sensitive reading of existing materials, but its relevance to interpersonal relations might be more elusive if this were the only source of information. Although the following

sketch is not a complete analysis, it draws attention to the value of increasingly close engagement with the people we study.[14]

The idea of local distinctiveness relative to the outside world is a widespread feature of Great Plains identity. James Shortridge documents the distinctive self-image that Great Plains residents have of themselves. Within the core region of the Northern Plains, 93% of his respondents mentioned characteristics related to the Jeffersonian ideal of yeoman farmers when asked to list features of Midwestern identity. These traits included any mention of words such as "friendly," "easygoing," "naïve," "thoughtful," "honest," "moral," and "modest" (1989, 79), and their mention in this region is at a much higher level than for respondents from elsewhere in the Midwest or beyond. The self-image he documents is consistent with the casual reports I heard in my own conversations with West River residents (see also Shortridge 1997).

Although Shortridge was not concerned with comparative reflections on the character of outsiders in other regions, contemporary sentiment rests on a history of tense relationships that have emphasized differences in character and power. North Dakotans are painfully reminded of their current status as residents of "fly-over country." In an especially well-known incident, they were even excluded from one edition of the *Rand McNally Road Atlas* because the major highways cutting through the state were already evident from the national map. Such casual disrespect from the outside takes its toll and even finds itself ratified in official representations of their history:

> Striving for equality of status permeated North Dakota life. There was, of course, nothing unusual about either the feelings of inferiority or the compensations for them. . . . But universality made such feelings no less real and significant in the history of North Dakota. They were solidly based upon North Dakota's status as a rural, sparsely settled, semiarid plains and prairie state, *a colonial hinterland exploited by and dependent upon outside centers of trade, manufacturing, and culture.* (Robinson 1966, 551–552, my emphasis)

Robinson's account points to the ambivalence that North Dakotans feel about their status, the hint of inferiority and the resentment that might translate into defensiveness. These are the complex reactions that emerged in long, taped interviews with West River residents. On the one hand, the outside is viewed as a world where local virtues are unappreciated or difficult to live out, as the following two excerpts illustrate:

> [B]ut, you hear stories and you read things and it just seems like, people in this area it's just like we all grew up in the same type of background with the same type of values. You know, we saw it here three years ago with the flooding situation, people helping out. Our own situation in June when we had flooding problems we had four neighbors in our basement all night with their wives helping us keep the water out of our basement, all we had to do was give them a call. People are just so willing to help and it's just the type of values that it seems like everybody has, to help out. And it doesn't appear that it's like that in other larger metropolitan areas. (Interview with JM, a 45-year-old man, July 27, 2000)

> I think family does mean more [here]. Because it does seem like you talk to other people in bigger cities and . . . their kids are scattered all over creation. And they don't get together with them and they don't seem like they have the closeness. . . . But yeah, I don't know, yeah, I think they just do [treat family differently]. (Interview with SZ, a 44-year-old woman, August 4, 2000)

At the same time, the outside world is viewed as a source of potential threat to the distinctiveness and soundness of local character:

> I think there's a lot of factors contributing to [a distinctive local character]. . . . Maybe being from a small town had something to do with it. Maybe being from a large family where there were responsibilities of taking care of the younger ones had something to do with it. . . . Do you know when I was ten years old we lived fifty-five miles from Glendive following the road and seventy-five miles from Dickinson following the road? That's not very far away and yet if I was in Dickinson and Glendive combined four times a year that would have probably been a lot. So we were isolated. Now, kids will jump in a car in Beach and go to a movie in Dickinson. So, the world is coming in and the morality, [because of things] presented to children through television or through the magazines or in the newspapers or on billboards, has eroded. Everyone is becoming homogenized. (Interview with KK, a 64-year-old man, August 18, 1999)

Methodologically, this incremental movement from existing sources to lengthy interviews corresponds to an increasingly close association with the subjects of study themselves. My research is, however, concerned with the implications for family processes, including intergenerational relationships, of movement away from West River by young people in pursuit of

their careers. It took a closer step into everyday life, in this case working a fence line with a farmer a few days before his daughter's wedding, to bring this cultural symbol of inside/outside to life in the emotional world of a cultural actor.

On a hot June morning, that farmer and I drove and walked along the fence line on his southeast pasture looking for slack wire and talking about whatever came to mind, his father's steady move to real retirement, neighbors' farming practices, his son wanting to farm for a living. The truck jarred along the line in rising heat and high wind and we hopped in and out of the pickup to staple line to 100-year-old cedar posts. My friend's mind was on his daughter's impending wedding and we eased into that topic. He talked about how tough it was to have his daughter away at college in Minneapolis and the consequent need to wedge all the unsaid things into the short space of her visit. The rhythms were wrong, not like it would be if she lived in West River, where you could build to a conversation, where the minor irritations of family got diffused every day and solved themselves.

"It's her attitude since she's been back," he said between the twists and clipping of barbed wire. "I need to talk with her." But he could never quite find the time. With the wedding was just two days away, his daughter slept late when he needed to move early. She'd be off when he wanted her around. It hurt when she and her fiancé crunched those pickup tires out of the yard without stopping in the shop to say where they were headed. His daughter's leaving was bad enough, but this fast and this abrupt change just made it harder. Part of it was the quick change from shaper to bystander, but there seemed to be more to it than that. The clues to what else began to emerge in that pasture itself and in subsequent conversations that tied it to the tensions of her leaving home for the outside.

As we moved along the wire, I noticed that to the west were three lines strung between a quarter section's worth of posts. Up ahead to the south, at the boundary with another farmer's land, there were four lines. Asked why some rows have three lines and some four, my friend looked and said, "Well, maybe it's when the next field is our own and when it's somebody else's. . . . I don't know, we just follow what was there before." Later, he decided that the inside/outside boundary explained it. It's a lot more trouble if the cows get into a neighbor's field. My friend saw that his daughter was crossing a kind of line by marrying and he was trying to figure out if this was a three-wire or a four-wire boundary. I followed up with questions in longer taped interviews. The first comment suggests a sense of rejection:

TF: [Your daughter] never got interested in [the farming life]?

JJ: No. Never. And it's getting worse.

TF: This upsets you?

JJ: Yeah. Her attitudes about North Dakota and what we do here, it, and if it's not for her that's fine, but I wish she would quit shoving it at us. That's, if she doesn't like what we do, fine. . . . I mean, as she grows and matures she's more vocal about how she feels about it. When she was a kid and you'd take her out she was willing to do things but now it's completely off the other end and she's vocal about it. (Interview, September 12, 2000)

In elaborating, he draws in some of the contrasts brought about by context:

JJ: And I think that's a part of being in the city and among their friends. You know, they need to have the new cars, they need to have a nice place to live, they need to put on a show. And when they come home [my son-in-law] talks a lot about how good this job is and how much money he's making and, we don't need to hear that, just tell us you're doing okay. But he has a, and I think they both have, a need to put an outward show on that they're successful. I have no pride. Look at the pickups I drive and the vehicles around here. (Interview, September 12, 2000)

The emotional relevance of this cultural category, inside/outside, begins to emerge with the increasing localization of available data. My explorations in general interviews were motivated by understandings available to any researcher who takes time to read the existing literature. It took the concrete case of a farmer's interactions with a daughter planning to marry and already certain that she would live elsewhere to demonstrate how the cultural category is dynamically integrated into an emotional world that turns on intergenerational relations, movement, and sense of self. My encountering the case was serendipitous, but its concreteness allowed me to anchor my opening research questions to a case study.

My argument about how this cultural category operates in everyday life is independent of survey data. In the complete analysis I suggest that the sense of historically constituted local identity finds its way into people's sense of self. In this context, coupled with the ambivalence of images of West River and its relationship to the outside, the common American occurrence of young family members seeking careers can take on heightened emotional meanings that color intergenerational relations.

This insight has the potential to inspire new kinds of questions in survey instruments. As in any culture, rural North Dakotans have their share of key symbols and motivations that are local elaborations of the larger culture of individualism that they share with other Americans (Bellah et al. 1996; Shweder 1996). Some of those key symbols have to do with the grid of township and range that shapes their physical environment and the recurrent theme of insider and outsider that marks the kinds of fences that get put up along section lines, the attitudes marking town and country relations (Williams 1973), and the way local people view the world of cities and jobs that siphon their children away from the Northern Plains and threaten to intrude on their world.

Family as moral community, inside and outside, the importance of place in socialization—these are among the things that need to find their way onto measures in an eventual survey. And the lessons of this ongoing work for ethnography and mixed methods are that we find ways to make the most local kind of knowledge respectable in social research.

Some Final Remarks

I opened this chapter with two suggestions: first, that the recent attraction of mixed-methods approaches is motivated by a sense that we need to get closer to the people we study; second, that we are better off defining ethnography in terms of an orientation to cultural questions rather than as a coherent method unto itself. The first of these suggestions frames my strategy of providing examples that move progressively closer to bringing the researcher together with his or her subjects. The second is intentionally provocative. It asks us to more directly consider as researchers the links between what we do and why we do it. If there are cultural questions that can be answered with survey data, then what are the questions that urge us into everyday life?

Approaching this question requires some usable notion of culture for the survey researcher. Because the definition of culture is a rolling stone that shifts with the disciplinary questions and orientations of the moment, there is no point in searching for that stable angle of repose where all anthropologists can rest in final agreement. Given that, I argue that the general agreement on the contours of the Geertzian framework, with some suitable modifications to address the question of how public models of and for reality can get into the heads of people, suggests a viable working model with application to survey research. Most social scientists who use

survey modes of investigation direct their inquiry to questions other than the mechanisms of cultural dynamics that concern cultural theorists. For these outside interests, the choice of a cultural framework is largely a practical matter.

Culture as worldview is a useful framework because it directs the researcher's attention to the logic of human action within a setting. Going further to look for the symbolic content of behaviors allows researchers to develop behavioral hypotheses that take account of the perceptual world of human actors. As I have illustrated with examples from three field settings, the cultural categories that people employ may be available in many cases from existing literature. At the same time, new categories and the symbolic content of behaviors may emerge from closer contact with people. Long interviews, more or less standardized, with a range of people can reveal clues to the symbolic content of behaviors. More intensive relationships with informants carry the connection between these categories and individual circumstances even further by allowing an exploration of the links between public meanings and private actions.

Finally, throughout this chapter, I have excerpted letters written in the process of my own fieldwork in various settings. My purpose was partly to illustrate the extraordinary concreteness of the fieldwork enterprise. The details from such close, experiential encounters with the people of study are the sources of plausibility for all subsequent arguments. I also wanted to convey the well-kept secret that going where the people are is often fun and always transforming.

Acknowledgments

My thanks to Dilli R. Dahal, Sallie Han, Rebecca Upton, and Tom Weisner for helpful comments. Support for writing this chapter was provided by the University of Michigan's Alfred P. Sloan Foundation–funded Center for the Ethnography of Everyday Life.

Notes

1. I recognize that Martin could be said to be very much "in the field" by virtue of living as a woman in the broadly defined culture system characteristic of the United States. Nevertheless, her discussion of methods makes it clear that her approach involved important methodological differences from her earlier work in Taiwan in its sampling and decision to forgo a more standard community-based fieldwork.

2. This is not to say that they should be excluded from bibliographies of ethnographic interest. See Coles 1997 for other examples of this method of participant observation used for written works that are, at most, ambiguously ethnographic.

3. I remember the exasperation of a demographer colleague when I pointed out in a seminar that the definition of culture she was using was exactly of the fossilized character immortalized by Gene Hammel. "Anthropologists are *always* changing the definition!" she said. And so they are.

4. See Fricke 1997a, 252–256, and 1997b, 189–190, for additional discussion of why these models are useful for mixed-methods research involving ethnographically oriented survey research.

5. We are all familiar with the practice of requiring the interviewer to indicate features of the setting on questionnaires.

6. The original survey and the article from which this example comes include attention to kinship relations and other locally relevant variables that were incorporated into the analysis. Notably, our interpretation of these variables often involved a reconstruction that differed from the purpose behind their original inclusion in the survey.

7. These other variables included a range of more obviously "cultural" measures such as kinship links between families and locally relevant marriage exchanges.

8. There were actually 1,521 eligible respondents in that age range. We missed 1 person.

9. It makes a difference, too, if physical inheritance is conceived of as being equally possible from either parent as in the general American culture of reproduction. This is irrespective of whether the American model is biologically closer to the facts. Still, even in America, biology and cultural models do not completely overlap, as with the curious theories that are widespread concerning inheritance of male pattern baldness and some notions prevalent on the Northern Plains that mental instability is a sex-linked characteristic inherited through females.

10. For those who just can't get enough of this sort of thing, the marriages they are more likely to contract are with their matrilateral cross-cousins, a marriage that connotes a particularly high status in this community and that is tied to a long-standing tradition in this family group that they are the "kings" of Timling (Fricke 1995).

11. It does, however, include an open-ended, long qualitative interview component in its data collection. These interviews are being conducted with a random sample of high school graduates over a three-year period.

12. The anthropologist Rosaldo examines the issue of objectivity in a collection of essays (1993). Ortner, another anthropologist working in the United States as a native, touches very briefly on some of these concerns (1993). Chodorow addresses these concerns most directly in a work that also poses important modifications to the Geertzian framework that I use as a starting place (1999).

13. This is the same letter as the excerpt at the start of this essay.

14. The more complete analysis is taken up in a work in progress (Fricke, forthcoming).

References

Agee, J., and W. Evans. 1989. *Let Us Now Praise Famous Men.* New ed. New York: Houghton Mifflin.

Alexander, J. C. 1988. *Action and Its Environments: Toward a New Synthesis.* New York: Columbia University Press.

———. 1990. Analytic Debates: "Understanding the Relative Autonomy of Culture." In *Culture and Society: Contemporary Debates,* ed. J. C. Alexander and S. Seidman, 1–27. New York: Cambridge University Press.

Axinn, W. G., J. S. Barber, and D. J. Ghimire. 1997. "The Neighborhood History Calendar: A Data Collection Method Designed for Dynamic Multilevel Modeling." *Sociological Methodology* 27:355–392.

Axinn, W. G., T. Fricke, and A. Thornton. 1991. "The Microdemographic Community Study Approach: Improving Survey Data by Integrating the Ethnographic Method." *Sociological Methods and Research* 20 (1): 187–217.

Axinn, W. G., L. D. Pearce, and D. J. Ghimire. 1999. "Innovations in Life History Calendar Applications." *Social Science Research* 28:243–264.

Basso, K. 1996. *Wisdom Sits in Places: Landscape and Language among the Western Apache.* Albuquerque: University of New Mexico Press.

Becker, H. 1996. "The Epistemology of Qualitative Research." In *Ethnography and Human Development: Context and Meaning in Social Inquiry,* ed. R. Jessor, A. Colby, and R. A. Shweder, 53–71. Chicago: University of Chicago Press.

———. 1998. *Tricks of the Trade.* Chicago: University of Chicago Press.

Bellah, R. N., R. Madsen, W. M. Sullivan, A. Swidler, and S. M. Tipton. 1996. *Habits of the Heart: Individualism and Commitment in American Life.* Updated ed. Berkeley and Los Angeles: University of California Press.

Blum, L. A. 1994. *Moral Perception and Particularity.* New York: Cambridge University Press.

Caldwell, J. C., A. G. Hill, and V. J. Hull, eds. 1988. *Micro-approaches to Demographic Research.* London: Kegan Paul International.

Caldwell, J. C., P. H. Reddy, and P. Caldwell. 1988. *The Causes of Demographic Change: Experimental Research in South India.* Madison: University of Wisconsin Press.

Chodorow, N. J. 1999. *The Power of Feelings: Personal Meaning in Psychoanalysis, Gender, and Culture.* New Haven: Yale University Press.

Coles, R. 1997. *Doing Documentary Work.* New York: Oxford University Press.

Dahal, D. R., T. Fricke, and A. Thornton. 1993. "The Family Contexts of Marriage Timing in Nepal." *Ethnology* 32 (4): 305–323.

D'Andrade, R. G., and C. Strauss, eds. 1992. *Human Motives and Cultural Models.* Cambridge: Cambridge University Press.

Flanagan, O. J. 1991. *Varieties of Moral Personality: Ethics and Psychological Realism.* Cambridge: Harvard University Press.

Fricke, T. 1990. "Elementary Structures in the Nepal Himalaya: Reciprocity and the Politics of Hierarchy in Ghale-Tamang Marriage." *Ethnology* 29 (2): 135–158.

———. 1995. "History, Marriage, Politics, and Demographic Events in the Central Himalaya." In *Situating Fertility: Anthropology and Demographic Inquiry,* ed. S. Greenhalgh, 202–224. Cambridge: Cambridge University Press.

———. 1997a. "Culture Theory and Population Process: Toward a Thicker Demography." In *Anthropological Demography: Toward a New Synthesis,* ed. D. I. Kertzer and T. Fricke, 248–277. Chicago: University of Chicago Press.

———. 1997b. "Marriage Change as Moral Change: Culture, Virtue, and Demographic Transition." In *The Continuing Demographic Transition,* ed. G. W. Jones, R. M. Douglas, J. C. Caldwell, and R. M. D'Souza, 183–212. Oxford: Oxford University Press.

———. 1997c. "The Uses of Culture in Demographic Research: A Continuing Place for Community Studies." *Population and Development Review* 23 (4): 825–832.

———. Forthcoming. "The Geography of Moral Sentiment: Placing Work and Family in Western North Dakota."

Fricke, T., W. G. Axinn, and A. Thornton. 1993. "Marriage, Social Inequality, and Women's Contact with Their Natal Families in Alliance Societies: Two Tamang Examples." *American Anthropologist* 95 (2): 395–419.

Fricke, T., D. R. Dahal, A. Thornton, W. G. Axinn, and K. P. Rimal..1991. *Tamang Family Research Project: Summary Report on Ethnographic and Survey Research Conducted March 1987–January 1988.* Kathmandu: Center for Nepal and Asian Studies, Tribhuvan University.

Fricke, T., S. Syed, and P. Smith. 1986. "Rural Punjabi Social Organization and Marriage Timing Strategies in Pakistan." *Demography* 23 (4): 489–508.

Fricke, T., and J. D. Teachman. 1993. "Writing the Names: Marriage Style, Living Arrangements, and Family Building in a Nepali Society." *Demography* 30 (2): 175–188.

Geertz, C. 1973. *The Interpretation of Cultures.* New York: Basic Books.

Hammel, E. 1990. "A Theory of Culture for Demography." *Population and Development Review* 16 (3): 455–485.

Jessor, R., A. Colby, and R. A. Shweder, eds. 1996. *Ethnography and Human Development: Context and Meaning in Social Inquiry.* Chicago: University of Chicago Press.

Kertzer, D. I., and T. Fricke. 1997. "Toward an Anthropological Demography." In *Anthropological Demography: Toward a New Synthesis,* ed. D. I. Kertzer and T. Fricke, 1–35. Chicago: University of Chicago Press.

King, G., R. O. Keohane, and S. Verba. 1994. *Designing Social Inquiry: Scientific Inference in Qualitative Research.* Princeton: Princeton University Press.

MacIntyre, A. 1988. *Whose Justice? Which Rationality?* Notre Dame: University of Notre Dame Press.

———. 1992. "Plain Persons and Moral Philosophy: Rules, Virtues, and Goods." *American Catholic Philosophical Quarterly* 56 (1): 3–19.

Martin, E. 1987. *The Woman in the Body: A Cultural Analysis of Reproduction.* Boston: Beacon Press.

Ortner, S. B. 1973. "On Key Symbols." *American Anthropologist* 75 (4): 1338–1346.

———. 1989. *High Religion: A Cultural and Political History of Sherpa Buddhism.* Princeton: Princeton University Press.

———. 1993. "Ethnography among the Newark: The Class of '58 of Weequahic High School." *Michigan Quarterly Review* 32 (3): 410–429.

Orwell, George. 1937. *The Road to Wigan Pier.* London: Victor Gollancz.

Ragin, C. C., and H. S. Becker. 1992. *What Is a Case? Exploring the Foundations of Social Inquiry.* New York: Cambridge University Press.

Robinson, E. 1966. *History of North Dakota.* Lincoln: University of Nebraska Press.

Rosaldo, R. 1993. *Culture and Truth: The Remaking of Social Analysis.* Boston: Beacon Press.

Searle, J. R. 1998. *Mind, Language, and Society: Philosophy in the Real World.* New York: Basic Books.

Shortridge, J. 1989. *The Middle West: Its Meaning in American Culture.* Lawrence: University Press of Kansas.

———. 1997. "The Expectations of Others: Struggles toward a Sense of Place in the Northern Plains." In *Many Wests: Place, Culture, and Regional Identity,* ed. D. Wroebel and M. Steiner, 114–135. Lawrence: University Press of Kansas.

Shweder, R. A. 1996. "True Ethnography: The Lore, the Law, and the Lure." In *Ethnography and Human Development: Context and Meaning in Social Inquiry,* ed. R. Jessor, A. Colby, and R. A. Shweder, 15–52. Chicago: University of Chicago Press.

Strauss, C., and N. Quinn. 1997. *A Cognitive Theory of Cultural Meaning.* New York: Cambridge University Press.

Taylor, C. 1985. "Interpretation and the Sciences of Man." In *Philosophy and the Human Sciences,* 15–57. Cambridge: Cambridge University Press.

Thornton, A., and H. Lin. 1994. *Social Change and the Family in Taiwan.* Chicago: University of Chicago Press.

Van Maanen, J. 1988. *Tales of the Field: On Ethnographic Writing.* Chicago: University of Chicago Press.

Weisner, T. S. 1996. "Why Ethnography Should Be the Most Important Method in the Study of Human Development." In *Ethnography and Human Development: Context and Meaning in Social Inquiry,* ed. R. Jessor, A. Colby, and R. A. Shweder, 306–324. Chicago: University of Chicago Press.

Williams, R. 1973. *The Country and the City.* New York: Oxford University Press.

Wuthnow, R. 1987. *Meaning and Moral Order: Explorations in Cultural Analysis.* Berkeley and Los Angeles: University of California Press.