
ETHNOGRAPHY AND HUMAN DEVELOPMENT

Context and Meaning in Social Inquiry

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14 Why Ethnography Should Be the Most Important Method in the Study of Human Development

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Ethnography Brings Cultural Places Around the World into the Study of Human Development

I have asked various groups of students and colleagues, "If you could do one thing to influence a child's development, and could pick something that would be the single most important influence, what would it be?" Another version of this thought experiment is, "Imagine a newborn baby, neurologically sound and healthy. What is the single most important influence that you would point to influencing the life of that baby?" The responses are quite predictable: touch and hold the child close for attachment and bonding; provide it with nutrition, physical security, and good medical care; talk with it responsively and often; provide it with a quality education, both formal and informal; find playmates for it; give it a sense of self-esteem; make sure its parents are wealthy (Weisner 1993). What comes to mind are qualities of the dyadic interactional system in which a child develops, or the physical needs of the developing organism, or the need for stimulation to encourage cognitive and social competencies, or the importance of a kind of self, deemed important in contemporary North American economy and society.

Although every one of these responses undeniably is important in the development of a child, in my view none of these is the most important—the most important is to give the child a specific culture in which to mature and develop. Almost never does anyone spontaneously mention the cultural place where the child is going to grow up. By a "cultural place" I mean the cultural beliefs, practices, meanings, and ecological setting characteristic of members of that community. The most important thing you

1996

could do would be to place the child within some cultural community on earth. Ethnography is the most important method in the study of human development because it ensures that the cultural place will be incorporated into understanding development.

Place the child in a community of Tamil Hindus, Muslims, and Sinhala Buddhists in Sri Lanka (McGilvray 1988); or among Brahmans in the temple city of Bhubaneswar, Orissa, India (Shweder, Mahapatra, and Miller 1990); or among the Balinese of Indonesia (H. Geertz 1961; Mead 1955; Mead and Macgregor 1951; Wikan 1990); or among families living in a kibbutz in Israel in the 1950s (Spiro and Spiro 1975); or among the FulBe or RiimaayBe communities of the Fulani of Burkina Faso (Riesman 1992); or among the horticultural and migratory wage earning Abaluyia in Western Kenya (Weisner 1979); or among Gusii mothers and wives in Western Kenya (LeVine and LeVine 1979; LeVine et al. 1994); or among the !Kung foragers and hunters of Botswana and Namibia (Draper 1976; Shostak 1981); or in a squatter settlement in contemporary Cape Town, South Africa (Reynolds 1989); or among the horticulturalist and small-animal hunting Sambia of the New Guinea Highlands (Herdt 1987); or among the Mehinaku, a small Brazilian fishing and mixed-subsistence Indian village community in the Amazon basin (Gregor 1977); or in a Sioux Indian community in South Dakota in 1969 (Dorris 1989); or among Navajo of the Southwest in the 1970s (Chisholm, 1983); or among Inuit (Eskimo) communities of the Canadian Arctic (Briggs 1970; Condon 1988); or in a Samoan village (Ochs 1988); or in Tahiti (Levy 1973); or in a native Hawaiian community in Honolulu (Gallimore, Boggs, and Jordan 1974); or in a poor neighborhood in Mexico City in the 1950s (Lewis 1961); or in an impoverished neighborhood in a town in contemporary northeast Brazil (Scheper-Hughes 1992).

In all of these studies, the sense of childhood being lived in another cultural place, with their different cultural careers (Goldschmidt 1990), stands out. If you went to these or a hundred other cultural places, you would discover the startling, disturbing, wonderful variety of what it means to be a child or parent and the variety of forms development can take. Children there would surprise you with the remarkable cultural abilities they have acquired and which they clearly are developmentally prepared for cultures to discover. The theories, findings, and developmental concerns of the authors of these cross-cultural studies vary enormously, but that is another matter.

The cultural place, it is fair to say, is not a routinely thought-about part of the accepted cognitive schema for thinking about human development in most of the developmental sciences, which is why ethnography should be among the most important methods in those fields. Yet why doesn't

the cultural place, which is everywhere recognized as important once it is mentioned, leap to mind immediately and forcefully as crucially important? It should, because culture offers us the tools for the mind which make development possible.

Of course, recognition of the cultural place as a powerfully important influence in development immediately suggests that there is no "one" important thing, and that development is multiply determined in cultural context. All of the influences which usually come to mind are important in every cultural place. Developmentally sensitive and appropriate interactions are indeed crucial, for example, but the existence of those dyadic interactions is due to the everyday cultural routine of life and to shared understandings which surround and scaffold them. Self-understanding and esteem are important as well, but culturally provided settings and their meanings make these possible. Attachment and trust are important, but how do infants and children experience strangers and learn whom to trust?

Ethnography brings the importance of the cultural place to the center of attention, transforming it from ground to figure. An important goal of ethnographic research is to describe and understand the cultural place and its influence on the everyday lives of its members. Whatever one's opinions are about epistemological issues and methodological concerns regarding ethnographically derived knowledge (and there surely are such concerns, as for all methods), the remarkable *findings* from ethnographic work regarding the varying cultural tools children use to develop in cultural places throughout the world alone provide sufficient reason for ethnography's deep incorporation into developmental work.

The chapters in this section offer interesting findings and their own models for how to integrate ethnography into developmental research. My comments on the chapters take advantage of their work to develop some general points about fieldwork and ethnography. First and foremost, ethnography and fieldwork get the researcher out into the cultural place of children and families. Once there, many ways of doing ethnography are possible and are illustrated in these chapters. Second, "methodocentrism"—the exclusive use of one method and fear of others—should be resisted, as these chapters illustrate. It is not plausible that any important question in developmental studies can be answered with a single method. Ethnography can and should be complementary with other methods. I suggest a way to talk about research methods different than the iconic qualitative/quantitative contrast, which seems to encourage polarizing discourse and is in any case not very useful or accurate. Third, ethnography is not limited only to early exploratory stages of research and to description of local meanings. It can and should be question driven; it

provides valid evidence to test against our models of the world; and it produces findings, as these chapters demonstrate. Next, I suggest that ethnography is to the developmental sciences as siblings or cousins are to one another—a part of the same broad lineage in the naturalistic traditions of the social sciences. John Modell imagines ethnography and development as two fascinated and mutually dangerous lovers. Both metaphors are probably appropriate at times. Finally, I suggest that a number of salutary things would happen if fieldwork in another cultural place, like learning statistics, was a normal, expected part of every developmentalist's training.

Varieties of Ethnographic Methods

When I went to East Africa to study family change and child rearing, I had a plan. I chose the Abaluyia, a patrilineal, patrilocal, horticultural group of communities in Western Kenya to work with. I was going to study Aba'uyia families living in rural and urban settings, adapting to cultural, political, and economic change. I planned on using ethnography, systematic observations of children and caregivers, informal interviews, questionnaires, and child and parental assessments of varying kinds (Weisner 1976, 1979, 1987, 1989; Weisner and Abbott 1977). I went as a participant in a systematic cross-cultural research program in human development (Whiting and Whiting 1975; Whiting and Edwards 1988). It took months of confusion and stubbornness on my part, and constant involvement with and interviewing of families (not to mention struggles with languages, my role in the community, my health, and so on) before it gradually dawned on me that the cultural place did not always fit my design or prior expectation.

For one thing, children were being taken care of by other children much of the time, not by their mothers or fathers, and even when mothers were present, they were not directly involved in care, but rather managed, coordinated, and struggled with the tasks of domestic life in a way I did not grasp. The cultural system of caretaking—that is, the rules of discipline, talking with children, evaluations of child competence, beliefs about gender—or the principles used in maintaining domestic life, were complicated; I didn't understand these. Mothers and fathers talked with me about parenting not primarily in terms of dyadic interaction and stimulation with their children but in terms of inheritance, generations, pride, obligation and obedience, and family-adaptive tasks. Mothers were more concerned with the complex adult relationships in their large households than they were with childrearing as a specialized activity. Modernization, mother-child stimulation, and stress may have been themes in North American developmental work at the time (and still are), but there was a lot more going on in the families I was observing.

Not only that, my "urban" and "rural" samples did not stay put. After I had worked with my urban sample for three months, 40 percent of them were gone. They kept commuting back and forth between city and country locations or to other parts of the country altogether. My sample members left their assigned groups even while I was trying to identify or relocate them. I had to scrap that design and try to find a way to study family change using a sample and cultural categories which were socially and culturally meaningful to Abaluyia. I had to completely change my sample to a rural-urban family-matched network because that represented the shifting social world children lived in. I had to come up with a matched, family-pair design, since that was the only way to embed my study into their cultural place, as children experienced it.

I had to figure out the system of sibling caretaking, or socially distributed nurturance. Changes in the sibling group due to complex patterns of migration turned out to have a lot to do with changing child social behaviors, more so than changes in mothers' caretaking. The older and younger children were separated in the city, leading to many ramifying changes in children's social behavior and mother's adaptations in the city. I had to change some of my ways of measuring child competence, since parents' own criteria for recognizing competence (social and task competence being highly elaborated, not individual cognitive or verbal expression) made a huge difference in children's abilities and the ways necessary to elicit them. Everything I anticipated doing did not have to be scrapped. I still could use many of the methods I originally had planned, and the comparative enterprise in which I participated has made lasting contributions to the systematic study of children and families. But the topics I studied got modified, the samples and designs were changed, the questions were sometimes quite different. I was learning from my fieldwork.

Ethnography 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. Ethnography, among other uses, assists in what Geertz (1988:147) proposed as a "next necessary thing to do" in anthropology and, I would suggest, in human development as well:

It is to enlarge the possibility of intelligible discourse between people quite different from one another in interest, outlook, wealth, and power, and yet contained in a world where, tumbled as they are into endless connection, it is increasingly difficult to get out of each other's way.

The possibilities are enhanced for better understanding of development in these cultural places after reading most of these chapters. Through Saxe's work, I could at least start trying to count things with the Oksapmin in

Papua New Guinea, or sell things with Brazilian street sellers. I could have lunch with a couple who had "fallen from grace" (Newman 1988), or meet their children in my classes and have a sense of empathy I would not have had before. I could see a Chinese-American family scold their young child, participate in their family activities, and begin to grasp through Miller's work the reasons why. Sullivan's work expands the debate about those "truly disadvantaged," through grasping the meaning of the notion of wanting to "get paid," as a local cultural model. I could appreciate through Heath and her junior ethnographers the fierce loyalties inner-city youths attach to their local sports clubs and church organizations as they seek to achieve a sense of place within their communities and display often intense achievement striving.

Ethnographic methods in human development typically include direct observation and interviewing of families in naturalistic settings in homes and communities over long periods of time. Fieldnotes and qualitative information are central. But ethnography is not limited to informal participant observations, informal interviews, and notes, as even a casual perusal of standard texts and references in the field with varying points of view about fieldwork will confirm (Agar 1980; Bernard 1988; Edgerton and Langness 1974; Levine et al. 1980; Lofland and Lofland 1984; Narroll and Cohen 1970; Pelto and Pelto 1978; Spradley 1979, 1980; Werner and Schoepfle 1987a,b). Most of the studies in this book used varied ethnographic methods and used methods in addition to ethnographic ones. Nothing in ethnographic methods, theory, or epistemology precludes this, and much encourages multiple methods.

As with any serious method, there are conventional (and hotly debated) ways available to assess the validity, reliability, and bias in qualitative ethnographic data (Miles and Huberman 1984, 1994). Although overtly out of fashion nowadays (but nonetheless still very often sought after for field use), there are standard topical lists and domains around which to orient general ethnographic inquiry in fieldwork (Johnson and Johnson 1990; Murdock et al. 1961). There also are lists focused more specifically on childhood and human development (Hilger 1966, 1992; Gallimore et al. 1993; Weisner 1984; Whiting and Edwards 1988; Whiting and Whiting 1975).

Every particular cultural place will require that alterations be made to the standard topics presented in these kinds of comparative summaries, sometimes profound alterations. But the overwhelming evidence is that we will not need to completely start over each time we go out in order to understand human development in another cultural place, because there are enduring adaptive problems and developmental concerns that will appear always or very often, and because there are ecocultural features that

appear to influence child development everywhere. Ethnography, in complement with other methods, provides much of the basic data for the understanding of cultural and human universals (Brown 1991), as well as differences. It includes systematic comparative developmental research using ethnographic techniques and findings in complement with others (Cole et al. 1971; Edgerton 1971; Hewlett 1992; Munroe and Munroe 1994a; Whiting and Whiting 1975; Whiting and Edwards 1988; Wozniak 1993).

Ethnographic work often involves a sustained, long-term commitment to research, with intense personal involvement by the researcher. Ethnographers typically are engaged with *participants* in their research, rather than assessing subjects, clients, or patients. This often places the ethnographer as a participant in events and openly an active constructor of the data being collected. The implicit choices made in other methods are seen more explicitly in ethnographic work. The buzzing confusion and complexity of everyday events comes to the fore; neat analytical categories are tested by this intense exposure to real activities, multiply shaped. The meaning and interpretation of events is an immediate and continuing problem to the researcher. Ethnography is not a unilateral process whereby a naive and "open-minded" fieldworker is shaped by the circumstances of the field situation. It is, rather, the process of matching the prior, evolving schema in the mind of the ethnographer against the changing and evolving materials gained from the field experience.

Is this why ethnographic and qualitative methods frequently seem to evoke ruminations by fieldworkers, including several of the authors in this book, about their personal biographies, why they chose these topics and cultural places (Saxe), or whether they were accepted by preschoolers and had good rapport and language skills (Corsaro)? Some of Miller's research findings *are* the reflexive processes her ethnographers experienced in the discourse practices they went to the field to study. Experiments, social surveys, or questionnaires do not seem to bring forth such personal disclosures, perhaps because of their standardization and because they offer an illusion of distance and objectivity.

The studies in this group made highly *selective* use of the various components of the ethnographic method. No one did a full ethnography or even a partial holistic study. Each study utilized a piece of the broader ethnographic method. These methods included field behavioral observations, interviews, and structured arithmetic tasks for Saxe; sociolinguistic transcriptions and observations for Miller; and clinical interviews for Horowitz and colleagues. Heath entered into collaborative ventures with a group of twenty "junior ethnographers" who were themselves from the inner-city organizations. Heath and her colleagues wanted to study. These

“research-apprentice-participants” tape-recorded and transcribed language data and context. Sullivan and his colleagues used local expert informants to understand delinquent peer groups. Taken together, these various methods constitute many of the tools of a full ethnography.

Nor did any of the studies involve full participant observation, or a comprehensive attempt to provide a full cultural account of a community. Each instead carved out a particular activity setting (teen sports and community organizations for Heath, street selling of candy for Saxe, family sociolinguistic events for Miller) to concentrate on. These pieces of ethnographic method can stand alone, even without a full ethnography of the families and communities—although a full account is the ideal.

Ethnography has too many uses right across the research process and takes too many forms to be compartmentalized as a method only used for exploration but not useful for verification; or useful mainly for the study of subjective meanings but not for analyzing behavior patterns or child outcomes. The authors in this volume followed the practice of using their “ethnographic imagination” even before going to the field to think about the cultural context and conceptualize their study. They used ethnography once in the field, or while interviewing, to explore—talk, participate, observe, listen closely, or work with co-ethnographers. They then discovered relationships between events, new terms and categories, possible new explanations for patterns of family life and childhood. Some then used ethnographic methods to assist in the work of verifying what they thought they had discovered—along with complementary methods, such as more structured interviews or surveys, analyses of videotapes, school records, tests, and so forth.

In spite of these varied and productive uses of ethnography, there are heightened anxieties, ironies, and concerns over ethnography today. Ethnographic research and writing is vigorously debated and worriedly critiqued (Geertz 1988; Clifford and Marcus 1986; Sanjek 1990). Critical and feminist theories alike question the historical bases of gender, power, or control from which ethnographies and ethnographers come (di Leonardo 1991; Marcus and Fischer 1986). All the epistemological and philosophical concerns raised by the ancient philosophers and present in the social sciences since their emergence, such as problems of reflexivity and knowing another, not surprisingly continue to remain at issue. So has developmental research “found” ethnography at the very moment when the ethnographic enterprise is lost in a cloud of criticism, doubt, and confusion?

Quite to the contrary, an enduring ethnographic project remains. To write and speak about others in as clear and plain a style as possible. To assemble a scholarly, high-quality, enduring ethnographic record of family

life and human development around the world for systematic comparative research (Whiting and Whiting 1970; Ember and Levinson 1991; Munroe and Munroe 1994b). To get the news out, as Margery Wolf puts it, citing Watson (1987:36), in order to try to understand why people around the world do what they do (Wolf 1992:1). To retain the core ethnographic commitments to empathy, scrupulousness, concreteness, fair-mindedness, and hopefully “revelatory” discovery and understanding (Fernandez 1993:183, citing Feeley-Harnik 1991).

Ethnography Has a Complementary Relationship to Other Methods; “Methodocentrism” Should Be Resisted

I and my colleagues watched a mother watch her child being assessed in the Strange Situation Procedure (SSP) many years ago. When the mother left the room and the child was left with a stranger (part of the experimental procedure), the child did not fuss or howl, but played alone. When the stranger also left and then the mother returned, the child fussed but did not require its mother for comforting. The SSP scoring for this pattern of behavior was that the child was not “securely” attached to the mother. The child was “avoidant” in this scoring scheme compared with a child who would cry upon the mother’s departure and then be comforted not by the stranger but by the mother’s return to the experimental room.

The mothers could watch portions of this experimental drama unfold through a one-way window while they were out of the room. This particular mother watched her child acting in a way attributed to “avoidant attachment”—and proudly commented to the researchers standing there, “This is what I have been working for by having him be with other kids and families while I am working. Look how independent he is! See how he can play by himself?” This mother was a single parent by choice. She had told us about her cultural goals for independence for herself and her child, her commitment to feminism, her struggles to sustain work and parenting, and many other values. Her construction of her child’s behavior came from this framework of beliefs and practices. In this ethnographic moment in which the mother and I were observing the administration of the SSP, the tension between the intimacy and interdependence of the ethnographic researcher with what he or she researches, on the one hand, and the distance and “independence” presumably present in the formal SSP itself, on the other hand, was revealed. Our longitudinal research suggests thus far that the cultural goals mattered for long-term child outcomes, while SSP scores do not (Weisner et al. 1991; Weisner and Garnier 1992).

Understanding trust and attachment in its cultural context requires

fieldwork and ethnography. Harwood, Miller, and Irizarry (1995), LeVine and Miller (1990), Reed and Leiderman (1981), Takahashi (1990), and many others have commented on the wide variations around the world in how children are exposed to strangers, who takes care of them and in what manner, and the consequences of these variations in trust, sociality, and emotional expression for understanding attachment. The SSP method depends on processes of intimacy and basic trust which may be culturally conditioned; it depends on the cultural practices around infant care and parent-child experience to give it meaning. There may well be lasting differences in the dispositional tendencies in children presumably measured in the SSP, and it will matter in children's development to understand those tendencies. Although I am dubious that the SSP is the gold standard against which to assess that dispositional tendency, it is one among several methods worth trying. But first I would make "attachment" itself a subject of ethnographic study.

This moment also stands as a positive experience regarding my greatest fear about methods: that the methods chosen will not bring me close enough to what it is I am trying to understand. Yet that is not the concern, fear, or anxiety about methods one hears very often. The concerns are almost always regarding possible biases, sample size, validity, reliability, veridicality, verifiability, replicability, efficiency of a method, its cost, and so forth. All worthy and important concerns—and all concerns in fact that ethnography, used in complement with other methods, can help us deal with.

Several methods are required to get us closer to understanding attachment or other developmental topics. Resistance to or denigration of other serious methods, which might be called methodocentrism in the study of human development, should be resisted. Methodocentrism, like ethnocentrism, may be inevitable and be understandable in certain circumstances, but generally both should be avoided. Methodocentrism can lead to invidious ranking of methods, and to imperialistic tendencies where one discipline or method tries to overshadow another. It can lead to a confusion of a measure of something with what is being measured.

The chapters in this volume share the view that other methods should play a complementary role with ethnography. Peggy Miller uses the personal experience of field observers, but also engages in intensive analysis of tapes and transcripts using standard analytical methods in sociolinguistics, to understand messages to children regarding self-esteem or autonomy. Horowitz argues for moving from laboratory to field situations in comparing ethnic groups' stereotypes. Corsaro administered questionnaires to his Italian teachers and tested the children, as well as hunkering

down on the preschool floor. He argues that quantitative and longitudinal data are better for assessing reliability and generalizability of studies in his field.

Many of these researchers intervened in their field settings. For instance, Saxe asks the candy wholesalers to track retailers' buying when they come in to restock. He intervenes on the street or in kids' math tasks in order to try to understand cognitive and memory processes "in flight." Saxe argues for the complementary use of specific structured techniques and sampling frames because of their precision and the history of our experience with them.

Newman and Saxe both develop the argument that careful sampling also can be crucial for ethnographic work. Newman samples particular groups of families for generational identity differences, and Saxe compares Brazilian street sellers who are adept and others who are novices in folk math tasks. Newman combines knowledge of the recent trends in employment from survey research and economic analysis, and argues for using longitudinal family samples as complementary to ethnographic data, even if there are only quantitative data available for some of those samples. Lock used large-scale social-survey data from Japan. Sullivan struggles with the tension between survey and "neighborhood" or census-tract data and the findings of participant observation in such neighborhoods. Heath, Miller, and Corsaro all use the interconnections between sociolinguistics and ethnography in their studies to generate their research findings. Miller uses information on reciprocal influences between her home observers and the participants as a type of sociolinguistic data. Corsaro uses differences in children's and teachers' perception of him and his Italian language abilities as indicators of their folk beliefs and the relational qualities among children and teachers. Heath combines intensive transcript-based sociolinguistic analysis along with her ethnographic work.

The prevailing discourse elsewhere in the social sciences regarding methods does not make the complementary nature of ethnographic work clear, as Don Campbell saw long ago. Nor does the prevailing academic discourse and the terms it uses help in debating the matter. Qualitative research, in the current model, is typically opposed to its presumed opposite, quantitative research. Naturalistic research is paired with its presumed opposite—experimental research. Cultural or comparative research is contrasted with its assumed opposite of monocultural work, which is somehow magically culture-free. It is apparent that the cultural categories and terms of discourse often used to define ethnographic as contrasted to other methods, are part of the difficulty in placing ethnographic and qualitative cultural research within the family of methods in the social sciences. These conventional methodological dichotomies, how-

ever common they are in everyday parlance, are neither accurate nor useful. Let's revise them.

Saxe's work on mathematics illustrates that the opposite of qualitative or holistic research is certainly not quantitative research, but rather *particularistic* or specifically focused research. Quantitative is not the opposite of qualitative, but rather has to do with the level of measurement available or appropriate for a study. Quantitative levels of measurement could be contrasted with *nominal* or categorical levels. Naturalistic studies contrast with research that is in some way *contrived* by the researcher or others. Experimental work, which attempts to infer cause, is usefully contrasted with *correlational* studies attempting to discover relationships and patterns. Comparative studies have no opposite, it seems to me. All studies have an implicit comparative frame of reference of some sort—a meaning in a context relevant to some cultural place, whether for the purpose of cultural comparison or not. In this sense, all studies have an “ethnographic” component embedded in them, even if ethnography was not done. If authors in this volume had been asked the standard-discourse question—Did you use quantitative or qualitative methods?—they collectively might have answered, We did contrived as well as naturalistic studies, using nominal and interval levels of measurement, sometimes highly particularistic as well as holistic, for the purpose of discovering both relationships and cause, in several cultural places.

Ethnographic Work Is Question Driven and Produces Findings

What did these studies discover; what findings did ethnography produce? Heath wants to understand how inner-city youth achieve a sense of place, a process involving a kind of familism and sharing among a trusted group; shared rules that matter; predictable structures (social, physical, emotional) in which to move; and a common value placed on achievement and hope for the future. More fundamentally, Heath searches for the mechanisms producing a “symbolic creation of place” and finds some of these in the language of rules shared in the youth organizations she studied. It seems to me that every one of these processes occurs in *all* cultures and communities adapting reasonably successfully; their absence is a sign of maladaptation in a cultural place (Edgerton 1992). So a more general question generated by these findings is, How can interventions at individual, family, and neighborhood levels assist in the developmental achievement of a meaningful sense of cultural place?

Corsaro asks whether children negotiate with adults, and thereby assist or resist the reproduction of their sociocultural circumstances, or whether they primarily “internalize” features of adult culture presented to them.

He finds negotiation common in everyday life in the preschool. He asks, To what extent are important cultural routines in which children participate over time a key set of activities driving child socialization? Recognizing the predictability and redundancy of such routines over time is crucial in understanding their force in his Italian preschool, as in most cultural situations. Longitudinal ethnography is probably the preeminent method for understanding this patterned activity, and Corsaro thus sets out to find these expectations and cultural routines. The Italian teachers he studied are valorized in their roles, and do not seem to need to, or be expected to, engage in “therapeutic” interactions vis-à-vis Italian parents. Nor are teachers asked by parents to demonstrate that they are treating each child as a unique individual whom they personally like, as some North American parents seem to expect (Tobin, Wu, and Davidson 1989). What is it about Italian life that produces such a preschool cultural world?

Miller argues for the importance of language as a “preeminent tool of meaning construction,” in which “talk is recognized to be not only reflective of meaning but constitutive of meaning.” Intensive ethnographic work involves describing language development and cultural practices each “in its own terms.” This leads to emergent questions based on sometimes troubling and painful experiences discovered in doing her fieldwork: “Why did [middle-class Chinese mothers in Taipei] routinely tease their daughters? Why did they talk about violent death, wife beatings, and child abuse in front of their children?” She also asks ethical and moral questions: Does ethnography require a certain kind of nonevaluative empathy and analysis of the reflexive relationships of ethnographer and participant?, Miller asks. If we want insiders to interpret their own practices for the researcher, does this thereby legitimate, justify, rationalize, the practices?

Miller provides insight into the parents' meanings and reasons for these practices, but does not move out into the wider cultural analysis of these families necessary to address such questions. Further, as Miller certainly recognizes, Taipei is not the only cultural place in which children learn that the same kin community who cares for you and protects you can also tease, dominate, and exploit you. This is a widespread kind of socialization experience across cultures. Hence, although the mechanisms of talk in this cultural socialization process are beautifully set out in Miller's work, other levels of analysis and complementary data (such as measures of self-esteem or identity, or ethnographic work on mothers' and fathers' cultural values and socialization goals) are also needed.

Saxe integrates findings regarding cognitive developmental abilities in math and the ethnographic context in which they are developed and exercised. He asks, What are the emergent goals for activities and actors in mathematics use? How do form-function patterns in math practices shift

over time in individuals as well as in *communities of use*? Saxe was into his third cross-cultural study by the time he arrived in Brazil to study child street sellers. His work among Alaskan Inuit (Eskimo) and then Oksapmin in Papua New Guinea persuaded him that communities of use and local apprenticeship knowledge were central in math practices. He asks about the interconnections of developmental abilities *across* different practices (for example, school arithmetic, street selling of candy, and negotiations with wholesalers). The striking situational competence of these children is matched against other views of street children as dangerous, failed, disabled, or wild. The social and family circumstances which provide a wider context for these Brazilian street sellers would have further enriched Saxe's account. I think the evidence suggests that most of these children *do* in fact face serious developmental problems and threats to their survival.

Sullivan explicitly frames his work around the underclass debate regarding inner-city youth, much as Heath does. The ethnographic enterprise and the underclass debate have largely *failed* to connect in his view. He found that in groups of youths followed through their teens, two or three out of a dozen persisted in delinquent and criminal activities, while most others seemed to begin a process of "maturing out" of some of these activities. Sullivan's groups and individuals *within* his neighborhood show that causal inferences regarding "inner-city neighborhood" effects require knowledge of individual developmental trajectories, peer-group developmental processes, and family influence, as well as of the larger social forces refracted into the neighborhood.

Horowitz, Stinson, and Milbrath's goal is laudable: to search for role-relationship models (RRM) and scripts, to relate these to different social situations individuals might face, and to search then for the multiple selves and identities these authors postulate are constitutive of the self. Although this cognition-to-social-situation matching is accessible to ethnography, and the transcripts of their subjects are susceptible to cultural analysis, I barely discern ethnographic work in this particular chapter. The question of the relationship of these RRM to behavior in everyday cultural activities remains to be explored.

Ethnography Should Be to the Developmental Sciences as Siblings Are to One Another

John Modell (in this volume) describes the relationships between developmental researchers and ethnography as that between "fascinated and mutually dangerous lovers." There is that delicious excitement and tension, and there are no doubt some unresolvable and unexamined conflicts, as Modell's metaphor implies and his comments point out. But the sib-

ling relationship metaphor is a useful alternative; it certainly seems to fit the chapters in this volume better, both their methods and their developmental findings.

We tend not to think of siblings as representing our culturally ideal relationship model, but many cultural places do (Marshall 1983; Weisner 1987, 1993). Certain kinds of cousins are also often classified as siblings—and marriage is sometimes encouraged between the appropriate cousins while being completely prohibited between others. I won't belabor the sororal/fraternal metaphor, but consider this: ethnography and human development share a common social science tradition, a common intellectual ancestry. We can appreciate this if we stand back from our local, sibling rivalries and consider the developmentalist ancestors who so often studied cases and their own children first (Sigmund Freud and Jean Piaget, for example), and our ethnographic ancestors, many of whom emerged from the natural sciences and psychology to do cultural comparison or to provide cultural interpretations of human variation in the face of the racist and Social Darwinist alternatives of the times (W. H. R. Rivers, Alfred Kroeber, Franz Boas, Margaret Mead). The rise of the social sciences as an autonomous set of disciplines in the twentieth century benefited developmentalists and ethnographers alike. Anthropology and developmental studies also share many cousins in the biological, psychological, and archaeological/historical fields, as well as in life history and biography. If we share the study of families, children, aging, and other topics, we often compete for the same sources of research funds. We certainly compete for resources within universities and research institutes. This competition could be valuable for ethnographers and developmentalists alike; exogamy with selective-cousin marriage may help insure continued innovation and vitality. But, like siblings, we should recognize our mutual interdependence.

Doing Fieldwork in Another Cultural Place Should Be an Expectable Part of Developmental Training

This interdependence should be institutionalized in training and our routine research practice. Imagine if every developmentalist (whether psychologist of one of the various persuasions, sociologist, pediatrician, anthropologist, or member of yet another discipline) had to have done fieldwork in another cultural place in order to receive a degree. Imagine that this was a routine, institutionalized, self-evident part of developmental training, a core requirement like statistics. Ideally, such fieldwork would be an apprenticeship experienced with members of that cultural place as well as with mentors and fellow students. This training would

involve firsthand ethnographic experience, not just readings as part of a course on the subject. Developmentalists from other countries would be coming to North America, because they would be engaged in the same training regimen in their countries, and important exchanges would occur with them in North America as well. Every nonconforming developmentalist would have to explain why he or she had not for some reason done fieldwork in another cultural place, or had not tested his or her findings from one place to another. How had they been able to (shamefully) "petition out" of such an obviously important research training experience? Those without such training would be at a disadvantage in the job market. To be sure, preferences for or against ethnographic work in another cultural place would not go away. Some would never do ethnography again; some (like the authors in this volume) would use it as one among many useful methods; some would embrace it fully.

If such an ethnographic requirement became widespread, methodocentrism would decline. Useful conventions would emerge for methods descriptions, along with frequent citations of cross-cultural material. The exploratory and discovery phases of research would become better reported, more valorized. A common body of cross-cultural experience would be brought to bear on important developmental problems. Communication across cultures and ethnic communities would benefit, and developmental researchers would be in a much better position to engage in that discourse in an increasingly tumbled world that so many see is upon us. Recognition of the influence of the cultural place in development, and of the power of ethnographic methods to study that influence, would come much more systematically, easily, and in a more useful, experience-rich form, to the developmental sciences.

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PART THREE

Ethnography and the Context
of Development