

Ethnography and Sociocultural Processes: A Symposium

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The Ecocultural Project of Human Development: Why Ethnography and Its Findings Matter

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ABSTRACT *Ethnography produces believable findings that matter both to those we study and to the social sciences. Ethnography is best suited to understanding human development as an adaptive project of individuals and communities—that is, what families and communities are trying to accomplish to meet their goals in their cultural world, and the cultural pathways that are available for children to achieve those goals.*

Ethnography is and should remain a preeminent method of inquiry in the social sciences because it produces findings that matter. In addition to ethical and personal-equation issues in fieldwork, what other reason is there for valuing any research method except that it assists us in achieving that goal? Ethnography certainly meets this test in the fields of psychological anthropology and human development. What a wonderful body of literature we have, crystallized in ethnographies, articles, chapters, and comparative studies, and how much bleaker and more partial would our understanding be if this literature were not available to us! Our ethnographic literature has come under attack from many sides in the last 15 years, and some of these critiques are useful and productive. But compared to *not* having a body of cross-cultural work to build on, the critiques pale. What is it about ethnography and fieldwork that produces findings that matter?

First, ethnographic fieldwork is centrally concerned with the adaptive projects of individuals and communities—that is, with what they are trying to accomplish to meet their goals in their cultural world. A project has a direction and a meaning; it involves struggle, internal and external

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conflicts, competing and collaborating with others' projects, failures and success in contingent circumstances. Human development and child rearing is a project of just this sort. To think of development as marked primarily by age, maturational change, or differential levels of stimulation is to underestimate profoundly the power of the cultural place and projects of parents and families that are the focus for ethnographic work. Ethnographic research is the preeminent method for understanding the ecocultural project of development. These adaptive projects defining development matter to our research participants and should always matter to ethnographers.

Projects, organized around goals, are lived out in an ecocultural world that provides a developmental niche for the developing child and an ecocultural niche for the family. Children and families develop in these niches, which provide culturally organized, everyday routines of life in communities (Super and Harkness 1986; Weisner 1993). Ethnography at least places the researcher in a position to understand that world. As long as this remains a central intention, however varied and incomplete actual ethnographic practice might be, ethnography is ready to assist in producing findings that matter.

A second reason ethnography produces findings that matter is that ethnography can and does have strong answers to the quite legitimate question regarding any method or any set of findings: Why should they be believed? Certainly one of the things that matters about findings is that they are believable, plausible, and true. Ethnographic work satisfies many criteria for being a valid method.

Ethnography should be used according to its producing findings that matter—but findings that matter to whom? This question is itself a part of the researcher's task: to examine the stakes held in the work, and to uphold the importance of having the findings matter to the scholarly and research community, as well as to others to the degree we can know how our research and findings will matter to others. The fact that ethnographic work intends to understand the adaptive project of our participants, does so in their ecocultural world, and takes seriously the charge to be believable, makes ethnography a method that matters.

ETHNOGRAPHIC STUDIES OF AN ADAPTIVE PROJECT: FAMILY CHANGE, MIGRATION, AND HUMAN DEVELOPMENT IN EAST AFRICA

First, I want to discuss an empirical illustration of understanding parenting and family change as an adaptive project in a cultural place, using a variety of methods including ethnography, invoking varying psychological processes. When I went to East Africa in 1968 to study family change and child rearing, I had a plan, expectations, a schema about what to do

and why. This plan arose from the usual mix: contemporary theories of change and child development in the social sciences; personal preferences and history; funding for fieldwork and travel; research access; and knowledge about Africa gleaned from readings, language study, and a few personal contacts. I chose the Abaluyia, a patrilineal, patrilocal, horticultural group of communities in Western Kenya, to work with. I was going to study Abaluyia 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 more formal and systematic assessments of children's and parents' competencies and responses to change. I went as a participant in a systematic cross-cultural research program in human development (Whiting and Edwards 1988; Whiting and Whiting 1975). After months of constant involvement with and interviewing of families (not to mention struggles with languages, gaining a role in the community, health problems, and so on) it gradually dawned on me that the ecocultural place and the adaptive project of Kenyan families did not always match my prior expectations.

The topics I brought with me to the field—modernization, stress, social behavior, mother-child stimulation—were themes in North American developmental work at the time (and still are), but there was a lot more going on in the families I knew. The Abaluyia family-adaptive project of child care had different goals and was differently conceived and managed. Child survival, compliance within a family hierarchy, deference, social competence, and socially distributed nurturance and support were important goals. The cultural system of caretaking—such as the rules of discipline, talking with children, evaluations of child competence, beliefs about gender, and so on—and the principles used in maintaining domestic life were complicated; I did not understand these. 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 often were not directly involved in care but rather managed and coordinated the family routine. Mothers and fathers talked with me about parenting not primarily in terms of dyadic interaction and stimulation with their children, but in terms of generations, pride, obedience and respect, social interdependence, economic survival, and other family-adaptive tasks. Mothers were more concerned with the complex adult relationships in their large households than they were with child rearing as a specialized activity. Levine et. al (1994) have described this as the contrast between “Pediatric” and “Respect-Obedience” cultural developmental models, which are concerned with child survival and contributing to family adaptation, rather than Pedagogical and Choice/Negotiation models, both of which are con-

cerned with early stimulation for literacy or cognitive skills, and independence and autonomy outside the home.

Furthermore, 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 research participants left their assigned sampling groups even while I was trying to identify or relocate them. I had to scrap that geographically based design and try to find a way to study family change using categories that were socially and culturally meaningful to Abaluyia. I settled on a study of the rural-urban family network itself, because that represented the shifting social world children lived in. The migrant network was the relevant social unit to try and understand, not city, country, and family homesteads alone. I eventually came up with a matched design, pairing rural and urban families together who knew each other and were members of the same lineage groups but who lived in city or country locations, since that was the only way to embed my study into their cultural place, as children and parents conceived and experienced it.

I had to figure out the system of sibling caretaking and socially distributed nurturance (Serpell 1993). Older children were responsible for caring for their younger brothers, sisters, and cousins. As a result, I found evidence of infant attachment to sibling caregivers parallel to maternal attachment. Whatever maturational readiness infants have for developing a fear of strangers and attachments to a primary caretaker at around 10–12 months, there is substantial variation due to ecocultural circumstances of child care, as Chisholm (1983) has found for Navajo, Takahashi (1990) for Japanese, and LeVine, Leiderman, and others for Gusii and Kikuyu in Kenya (LeVine et al. 1994). Due in part to sibling care, children's nurturance and social responsibility was high from about ages five to seven on, compared to Euro-American samples. I found that gender differences in social behavior sometimes favored girls (nurturance, directing other children, responsible for tasks) and sometimes boys (dominance, aggression and disruptiveness, proportion of nonsocial activity), reflecting the persisting socialization pressure placing girls and boys on different cultural pathways.

The older and younger siblings often were separated due to rural-urban migration, since the older children were needed in the rural homestead and usually were in school there. The cramped city living conditions made it difficult to bring older children along to the city. This produced many ramifying changes in children's social behavior and mother's adaptations in the city. I discovered that there was more disruption and aggression among children and with parents in the city, and that this was due to the splitting apart of the sibling group rather than because urban mothers

(at least as yet) had changed their cultural model of child care, or were under more stress. Indeed, mothers in rural-urban networks turned out to have lower levels of stress. Older siblings were not available as buffers between younger children and mothers, and their absence in the city changed the demographic and cultural scaffolding surrounding the socially distributed system of support. After all, they had both city and country bases for support, unlike mothers depending only on one or the other place.

I had to change some of my ways of measuring child competence, since Abaluyia parents' goals led to different cultural criteria for recognizing competence. Social and task competence were highly valued, more so than individual cognitive ability or verbal expressiveness. Abaluyia parents recognized cognitive cleverness and "quickness" in children, but cognitive abilities alone without accompanying competence in social and moral life made little sense to parents. Children also needed to be trusted to assist in family management and to be appropriately behaved representatives of their family to others. It turned out that children in Nairobi did somewhat better on standard cognitive tasks I adapted from Western tests—but the urban advantage was largely because city children gave more answers that were partially correct, and were more bold, exploratory, and responsive in the test situation. Actually, rural children did as well in giving wholly correct responses more often. They were more likely to wait until they were sure of their answers before responding. Furthermore, I found that children with higher participation in shared caretaking in the city or country did as well or better in local schools than children with low participation. Children "played school" outside of classrooms, emphasized socialization for responsibility, and valued the social skill to get and give assistance to other children regarding literacy and numeracy, all of which seemed to help children or at least not hinder them in school tasks.

I still used some of the various methods I had planned, and the comparative enterprise in which I and so many others participated has made lasting contributions to the systematic study of children and families. But the topics I studied were modified, the samples and designs were changed, and the questions were sometimes quite different as I constantly revised my prior schemas during fieldwork (Weisner 1976, 1979, 1987, 1989, 1996a; Weisner and Abbott 1977; Weisner et al. 1997). Ethnography's comparative advantage in the social sciences begins with this insistence on being there in the local culture, embedded in relationships in a human community, and therefore forced to make explicit the usually implicit prior schemas guiding our work. What mattered to Abaluyia families—their goals and hopes for their children, their struggles to adapt and survive in a delocalizing world economy, the cultural practices they use to

try and achieve these goals—are the figure, not the ground, in ethnographic work.

ECOCULTURAL THEORY OF DEVELOPMENT

It is useful to think of culture as shared cultural models lived out in practice, in the context of everyday routines of life. Everyday routines are made up of activities and practices, which in turn include five features as a minimum definition: goals and values, motives and emotions, tasks to be performed in that activity, a script for normative or appropriate conduct (the right ways to do that activity), and who the people are who should be participants. A child's participation in these linked activities in a local ecology is the single most important influence on development, and children are prepared to learn from and respond to activity settings.

This ecocultural theory of development suggests that the focus of ethnographic work on children should be at each of these three levels: (1) the ecocultural context (e.g., subsistence patterns and ecology, social supports and institutions, forms of family organization, demographic patterns, health, mortality concerns) that matters for families and children's development (e.g., Bronfenbrenner 1979; LeVine et al. 1994; Weisner 1984; Whiting and Edwards 1988); (2) the cultural models of development, parenting, the person, and so forth, held in the minds of parents, siblings, other family members, teachers, and others (D'Andrade and Strauss 1992; Harkness and Super 1996); and (3) a direct focus on the activity settings the child and others are engaged in—the agentic, interactional life of the child and family living out their everyday routines and activities (Gallimore et al. 1993; Ochs 1988). The Abaluyia child study moved across these levels: (1) the changing sociohistorical and subsistence patterns driving migration, rural-urban family arrangements, and so forth; (2) parental cultural models of parenting child competence or caretaking; and (3) systematic behavior observations of children in their natural daily routines, and informal field notes on everyday family life.

What matters for children and parents in this theory is the achievement of cultural well-being. Well-being in children is usually thought of as an intrapsychic state or individual attribute or resources available to the child. But well-being also is the ability of a child to actively and innovatively participate in the activities deemed important and valued by a cultural community. The culture provides activities for the child that require assessment regarding the goals, stability, safety, coherence, and so forth of the activities. These cultural activities should be embedded in child developmental measures of well-being and not seen as exogenous to the child. Ethnography is a crucial method of choice for studying activity settings, the developmental projects being lived out in them and the well-

being of children thought of as the achievement of active and innovative cultural participation in meaningful activities (cf. Cole 1985).

Each feature of an ecocultural theory of development (the idea of a developmental project and its goals; maturational changes in children; the power of the daily routine; activity settings that include goals, scripts, motives and emotions) includes a psychological component. Many psychological processes (attentional/perceptual, psychodynamic, cognitive/memory, sociolinguistic, choice making, emotional) contribute to action and can be found within the adaptive projects ethnography is good at understanding. In my view, the findings from cross-cultural work support the position that there are a multiplicity of psychological processes visible in human action and thought, that these processes are universally available in the human mind, and that their results are available to cultural study through ethnography, as well as other methods (D'Andrade 1992; LeVine et al. 1994).

WHY SHOULD ETHNOGRAPHY BE BELIEVED?

In spite of the varied and productive uses of ethnography in the study of children and human development, there are heightened anxieties, ironies, and concerns over ethnography today. Ethnographic fieldwork and its data remain plagued by epistemological concerns of all kinds. Ethnographic research and writing is vigorously debated and worriedly critiqued (Clifford and Marcus 1986; Geertz 1988; 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).

Is the ethnographic enterprise lost in a cloud of criticism, doubt, and confusion at the same time that I am suggesting that we need more of it? Quite to the contrary, an enduring ethnographic project remains for understanding human development: to write and speak about development in as clearly a plain-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—a project that indeed matters and makes a difference in the social sciences. Ethnography, in complement with other methods, has provided much of the basic data for the findings in the field of comparative human development (Cole et al. 1971; Edgerton 1971; Ember and Levinson 1991; Hewlett 1992; Leiderman et al. 1977; LeVine et al. 1994; Munroe and Munroe 1994; Whiting and Edwards 1988; Whiting and Whiting 1970; Whiting and Whiting 1975; Wozniak 1993). Ethnography should be used to get the news out, as Margery Wolf puts it, citing Watson, in order to try to understand why people around the world do what they do (Wolf 1992:1); and it should retain the core ethnographic commitments to empathy, scrupulousness, concre-

teness, fair mindedness, and hopefully “revelatory” discovery and understanding (Fernandez 1993:183, citing Feeley-Harnik). Ethnography should emphasize accuracy, precision, depth, and breadth as criteria for assessing ethnographic work—in counterpoint to criteria from quantitative methods like reliability or validity (Becker 1996:68). Ethnography, amongst 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 people are into endless connection, it is increasingly difficult to get out of each other’s way” (1988:147).

It is not that the critiques and fears about ethnography as method or field experience are not real and deserving of consideration in doing and evaluating fieldwork. It is, rather, that these critiques are not my greatest worry about methods, which is that I will not get close enough to what it is that I want to understand. The central concern in understanding human development is to get close enough to understand the cultural adaptive project of families and children, and to understand well-being in context.

As with any serious method, there are conventional (and hotly debated) ways available to assess the plausibility and believability of qualitative ethnographic data as well as texts offering professionally accepted ways of doing fieldwork and analyzing field data (e.g., Agar 1980; Bernard 1988; Miles and Huberman 1984, 1994; Pelto and Pelto 1978; Spradley 1979, 1980; Werner and Schoepfle 1987a, 1987b, and the journal *Cultural Anthropology Methods*). Although overtly out of fashion nowadays (but nonetheless still very often sought after for field use), there are standard topical lists and domains of interest in cultural research around which to orient general ethnographic fieldwork (e.g., Johnson and Johnson 1990; Murdock et al. 1961). There also are such lists focused more specifically on matters relevant to childhood and human development (e.g., Gallimore 1993; Hilger 1966, 1992; Weisner 1984; Whiting and Edwards 1988; Whiting and Whiting 1975).

These procedures and lists do improve the believability of fieldwork data, but the heart of ethnographic fieldwork is the process of systematically matching a prior schema in the mind of the ethnographer about the cultural world studied against field experience of that world. The Abaluyia friends and research participants, students, and research assistants I worked with in Kenya made this continuous matching and revision possible. There is always a prior set of expectations and schemas guiding fieldwork and brought by our fieldwork participants. Although openness and flexibility throughout fieldwork is crucial, the core question is: are prior expectations and schemas sufficiently clear, explicit, operationalized, and

available to the ethnographer for matching, confirmation, revision, and writing? This kind of reflexivity is one of the great strengths of ethnographic fieldwork; it is neither a burden hindering valid research, which we have to try fruitlessly to eliminate, nor a reason to abandon the method, treat it as only storytelling without empirical basis, or make an exclusive, specialized niche for it, protected from other methods in the social sciences.

There is a close connection in my view between good ethnography and the scientific method done well and in context. Our prior schema about what we expect or think should happen is our prior hypothesis. That schema may be implicit and may carry with it our own personal concerns and cultural history, but it is the role of a good scientist/ethnographer to make that implicit material explicit and examine it carefully, before and during fieldwork. Then the fieldwork and research experiences unfold and our prior schemas are matched against what happens in the world, causing us to revise and even abandon our prior schema and formulate a new one. Real ethnographic work obviously is messier and happens more in fits and starts than this abstract schema-matching paradigm, but good ethnographic work includes just this process, as does good science.

ETHNOGRAPHIC WORK AND A NONMETHODOCENTRIC EPISTEMOLOGY

Ethnographic methods are complementary to the family of methods in the social sciences. Yet prevailing discourse in the social sciences regarding methods does not help make this clear. The usual discourse is oppositional and dualistic. Qualitative research, in the everyday academic folk 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, impossibly, somehow culture free).

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 more accurately and usefully contrasted with nominal or categorical levels of measurement. 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 coherence, relationships, and patterns, which is what ethnographic work is best at.

Comparative studies have no opposite. 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. All methods have a context. The contrast then is between studies in which there are context-examined procedures, and those with little or none. Questionnaires done with a particular group have a context, usually unexamined. Ethnographic studies have a very visible context, at their best carefully considered if much less contrived or controllable than other methods. Using this kind of view of ethnographic and qualitative methods, one might ask a (highly exemplary) field researcher about his or her methods and hear this answer: “I did contrived as well as naturalistic studies, using nominal and interval levels of measurement, sometimes highly particularistic as well as holistic, across several levels of analysis, for the purpose of discovering both meanings, relationships, and cause in [several] cultural places.” This would be a productive and refreshing change from our current academic folk model: “Did you do quantitative or qualitative work?”

But ethnography stands for more than a method; it is also about discovering things in the world other methods cannot easily, if at all, get at. Shweder finds the continuing tension between quantitative and qualitative approaches not primarily about intellectual operations or false dichotomies per se, but rather finds the distinction of value because the world consists of “quanta” and “qualia” (1996:177–179). Quanta are objects, events, and processes in the world minus the subjective; they are things that have power independent of our experience and awareness of them. Qualia are things that can only be understood with or through the subjective—that is, by what they mean, signify, or imply to persons in a particular place. Quanta usually are studied using procedures such as pointing, counting, measuring, sampling, and calculating; qualia usually are studied using empathy, interpretation, thematization/emplotment, narration, contextualization, and exemplification.

Here is an example. Either divorce or mortality end a marriage in the United States. At a point in the late 1970s, marriages in the United States that ended in divorce equaled those ending because of the death of the spouse. This signaled the century-long consequences of the demographic trends of declining mortality along with recently increasing divorce rates (Skolnick 1991:156). But the meanings of the end of a marriage due to death versus divorce are very different—although perhaps not completely different. The demographic rates of marital dissolution and changing household composition due to death or divorce are kinds of quanta (but not entirely, being also socially constructed categories); the subjective meanings and emotional significance of the changes in household compo-

sition, cultural practices, and family activities and routines due to divorce or death are qualia (but not entirely, being partly understandable through the use of scales, sampling, and so forth). Both quantitative and qualitative methodological procedures are essential in order to understand—to get closer to—the impact of these family changes on children’s lives.

Ethnography places us in a position to capture the qualia of meaningful cultural adaptive projects lived out in a daily routine of activities. But as a part of a family of methods, ethnography also has a complementary role to play alongside quanta-like methods. If we act as if the world is made up of quanta and qualia, with a corresponding practical epistemology focused on matching our schemas to the world, it is my hypothesis that we will get closer to understanding human development as an adaptive cultural project, and that we will produce more interesting findings that will matter to families, children, and the social sciences.

NOTES

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Beyond What Are Given as Givens: Ethnography and Critical Policy Studies

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ABSTRACT *This commentary argues for the importance of a move from ethnography to theory to a critical stance on public policy and welfare reform. Two examples illustrate the vital role of ethnography in creating social policies that respect the variety of human experiences. One is a study of block grants in the rural south, the other of fast-food service workers in Oakland. These examples show the importance of using similar methods but with differences in ethnographic focus in order to address specific empirical problems and policy questions.*

My purpose as an anthropologist, working alongside many others in the field, is to propose that we configure our public policies around the social practices and everyday lives of people most deeply affected by these policies. By bringing ethnographic data into the resources of public reasoning, I hope to shift the character of the policy debate. I have written within three policy arenas over the past 25 years: disputes over who owns the child (best interests of the child, psychological parenting, child custody and foster care, permanency planning); entitlements (AFDC and household formation, 4D—parent locator bill, block grants and devolution); and housing and community impact. Working across these wide-ranging issues in family policy, I have used ethnography (arguing from the particular to the construction of theory) to attempt to convince policy makers about the need to consider extended kin as an essential ingredient in public reasoning. This is but one example of how to use ethnography to create social policies that respect the variety of human experiences.

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