Culture: Contested Realm in Research on Children and Youth

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A brief historical perspective on social scientists' treatment of culture precedes discussion of certain research methods generally taken to be acultural. Sociolinguistic and anthropological studies have shown that mainstream practices (e.g., interviewing) and dominant concepts (e.g., individual as independent agent) receive little attention as culturally biased, although researchers focus on the practices and beliefs of the "culturally diverse." Unquestioned are researchers' expectations of a linear path of development marked by stages of learning and achievement or adults as primary socializers. Research implications of both postmodernism and rejection of cultural labeling by a growing number of groups and individuals are set out. A broad approach is proposed by which disciplines can converge around key questions, integrate and combine methods, and rethink central dichotomies. The suggestion is made that, to the extent possible, research among groups identified as culturally diverse should be triangulated across methods and information sources and contextualized within existing knowledge relative to language, socialization patterns, and development.

Current enthusiasm over "multiculturalism," "plural cultures," and "cultural competence" often proceeds from essentialist categorizations of entire groups with labels based on racial and ethnic membership. Shared linguistic and social behaviors held in common by the group in daily practice, as well as group members' various identities played out in varying circumstances, rarely get addressed. References to *the* Hispanic culture (or *the* African American, *the* Southeast Asian, etc.) ignore individual differences in national origin, as well as strongly marked variations in religious beliefs and practices.

Labels offer shorthand ways of referencing others, to be sure, but what is it about *culture* as a key significance of these labels that has so caught the attention of researchers across a wide range of disciplines? What is it that enables social scientists to feel so free now to ascribe the behaviors and beliefs of others to cultural origins? Within anthropology, the discipline many consider homeland to the concept, *culture* has long been embattled and charged with edging off into theory only (Ortner, 1984). At one point, editors of *American Anthropologist*, the major journal of the American Anthropological Association, let it be known through informal circles that articles on culture would no longer be considered for publication. Since the 1980s, the utility and integrity of such a construct have been severely

challenged by theorists in anthropology of nearly every ideology. Though objections to culture differ in source, all those who would have social science be rid of it agree that researchers can no longer see the concept as viable in a world of volatile, situated, and overlapping social identities. Apprehension about the term is evidenced by encasing it within quotation marks or by "lexical avoidance behavior" that puts in its place terms such as "discourse," "praxis," or "habitus" (Brightman, 1995).

As early as 1973, in his widely read *The Interpretation of Cultures*, anthropologist Clifford Geertz seemingly gave permission for those outside anthropology to take up culture, so long as they understood it as embodied in public symbols. This strong focus on culture as "webs of significance" resonated with scholars previously inclined toward semiotics, and soon researchers from a wide array of fields zeroed in on meaning-making as the locus of culture. The field of "cultural studies" moved in as well, with many behaviors and artifacts previously indexed as culture now subsumed by the term *text* and surrounded by its lexical cousins, such as *genre, narrative,* or *textual practice*. More often then not, the resultant scattering of emphasis to both discourse and practice diverged from Geertz's original point, which centered around the *how* of symbols as they shape and carry culture around in people's ways of seeing the self in relation to others. Pierre Bourdieu's (1978) project of putting *habitus* out as a set of generative principles of internalized lessons or improvisations learned through socialization helped

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somewhat to refocus scholars on strategic deployment of these principles. Modes of practice amount to the expression of habitus.

Cultural criticism, particularly evident in fields such as education and sociology, meanwhile hammered away at the culture concept primarily on the grounds of its transmission of connotations of objectivity, discreteness, essentialism, and ahistoricism. Sociologists, on the other hand, took culture to be forms of modernity and thus contested ground of production, mass consumerism, and popular entertainment (Chaney, 1994). Literary theorists, poststructuralists, and a variety of postmodernists in the arts echoed similar concerns. Throughout what may be called the "human sciences," the move in the 1980s and 1990s was to break away from totalizing universalizing perspectives and to take up the local, particular, and fragmentary. Arbitrary constructions of almost any sort were set aside in favor of permeable membranes by no means predictable or deterministic.

Several advances in the "real" sciences helped disengage prior strict divisions between order and disorder, as well as fundamental notions of stability and the linear nature of order. Such physicists as Mitchell Feigenbaum in nonlinear dynamics came to see chaos not as a negative, but as a model of complexity and randomness within systems (Hayles, 1990). Soon, chaos theory became attractive to those in the human sciences, especially scholars focused on learning (Resnick, 1996). Psychologists began to apply chaos theory to numerous aspects of their field (Butz, 1996), and sociologists and economists applied variants of chaos theory to social issues, particularly in high-density populations (Crane, 1991).

But what are clinicians, policymakers, and researchers of a strong positivist bent to do with such slippery notions? Told on the one hand to attend to cultural differences, and on the other not to be too sure they will know culture if and when they see it, counselors, clinicians, and practitioners in a variety of fields wonder which position will win out in the long run. The majority of psychologists, for their own reasons, have generally ignored debates around culture and have stuck to their laboratory experiments and standard methods of data collection as well as analyses asserting universals. Too many of their test batteries, instruments of diagnosis, and remediation paths evolved out of older theories of maturation, social adjustment, and normal development. Hence, because practitioners in mental health, counseling, and education who use these assessment instruments see themselves as most closely tied to psychology, most have found justification for having few worries about culture and the differences it can or cannot make.

This article addresses the question of how social scientists, particularly those whose work relates to applied interests, can incorporate in their research designs an awareness of what cultural membership might mean and its capacity for portraying phenomena outside accustomed bounds. In particular, what is to be learned about behaviors normative within specific groups and deviant or dysfunctional to mainstream ideals? The focus here will be primarily on the socialization literature, especially with regard to language learning and identity development. These studies derive primarily from anthropology and incorporate microethnography, discourse analysis, and oral history. Drawing from cross-cultural studies of language socialization, comments here emphasize the need not only to expand methods of data collection, but also to recharacterize the role and contributions of those being studied in both data-gathering and analysis. Discursive practices traditionally employed in data collection merit special attention for their covert cultural biases (Briggs, 1986; Harkness, 1992).

Culturally Defensible Research

It is general practice within many branches of the social sciences to give considerable attention to instruments of research, such as interview protocols and surveys, and also to identification of variables, dependent and independent. Increased awareness of possible "interference" by culture has led some scholars to reframe their notions of *instrument*. At a fundamental level, in research and application, the individual carrying out the work is the primary instrument. Characteristics of this instrument—researcher or clinician—include epistemology as well as the primary tool kit of terms and procedures.

To take these features into account is to do more than nod at postmodernist acknowledgement of contingency and of the instability of language meaning. Recognizing that the age, gender, race, and status of researcher or clinician will affect data collection and skew evidence provides a first step. A second step is to reconsider just how the need to pin down discrete entities with labels dissects individuals into parts that must inevitably be closely intertwined and similarly sorts out from these parts particular disabilities or dysfunctions. For example, focus on pregnant teenagers as a "problem" almost invariably leaves aside holistic considerations of socialization history and current context. Similarly, centering on "conduct disorders" of young males from disenfranchised groups routinely ignores the positive survival roles some of these "disorders" serve in their primary socialization networks.

Cultural psychology, helped by social constructionists interested in how individuals' situated understandings engender social forms, has made strides toward examining the fluidity of influence from social interactions on microprocessual coordination or the lack thereof (Shweder, 1991; Stigler, Shweder, &

Herdt, 1990). The relation of this work to ethnography and also to human development has been particularly useful in making scholars aware that all members of a "community" will not have equivalent knowledge, nor will they necessarily have similar means of sharing information about their way of life (Jessor, Colby, & Shweder, 1996). This work holds considerable promise for moving scholars in human development to recognize that basic previously fixed labels, such as *adolescence, aggression, ethnicity,* and *identity* will not be fundamental explicators. Fixed definitions and expectations have little claim to either accurate or adequate representations of human behavior as close-up and long-term studies of groups and their socialization patterns for the young accumulate. What and when people learn, as well as how and where they distribute and display what they know, becomes far more complex than closed linear theories of development can possibly capture.

The sociohistorical theoretical movement deriving from the writings of L. Vygotsky as far back as the 1970s has accelerated its influence on concepts around learning (Vygotsky, 1978). This work, particularly in its implications for learning beyond early childhood, has received considerable impetus from activity theory, where the object of inquiry is the collective activity system embracing the multitude of situations in which individuals interact daily. These systems do not reduce to a series or sum of individual discrete actions for any specific situation but build toward integration of process and product (Engestrom, 1987; Leont'ev, 1978). Continuous construction in the moment of all the components—actors, tools, rules, divisions of labor—takes place to create every interaction as creative and novel. This work understandably throws open to question some of the usual means by which researchers judge reliability. Similarly, such studies indicate how performances of individuals in laboratory settings may bear little relation to their execution of similar tasks in other situations or settings.

Needed for culturally competent research then is disengagement of views that implicitly assume consistency in interactional behaviors, necessary stages of development, or fixed end points of maturity. This need is particularly critical for teenagers who not only come from increasingly diverse socialization networks, but also bear in most societies strong influence from what may be termed "global youth culture." Teens now frequently know much more than their elders, particularly their parents, about formerly taboo topics as well as have instrumental knowledge about access to services. Many assume parental responsibilities for younger siblings very early in life and negotiate financial management, childcare arrangements, and several means of employment on a daily basis. Different socialization contexts give various meanings to successful developmental trajectories, vary role expectations of adoles-

cents (as well as males, females, elders, etc.), and accelerate and intensify the assumption of responsibilities by the young. (For a review of studies supporting these points, see Burton, Obeidallah, & Allison, 1996.)

Thus, the stages, disorders, or dysfunctions assigned to individuals by counselors and educators representing mainstream institutional norms often have little meaning for those from different socialization paths. As these diversities increase through continuing need for adaptation, institutions that previously developed personnel and procedures for dealing with large numbers of people in the same way will have to create and adopt local and particular strategies. But to do so, professionals in these positions will need extensive background knowledge from social science about variation, socialization contexts, and bases of identity that matter to individuals (Tharp, 1991). This means that those who see themselves as providing basic research will need to make their studies available in accessible forms. In addition, such researchers must find more ways of working directly with practitioners to produce conceptual tools for rigorous tests of validity that institutional settings offer. These new alliances offer hope for breaking the dichotomy between basic and applied (Engestrom, 1996) as well as between normal and abnormal.

Shifting Research Methods

As cultural psychologists and others noted earlier as now taking a strong interest in human development push ahead to refine their theories, they also look to new methods of inquiry. At the heart of this search is ethnography, previously the genre that resulted from long-term fieldwork by cultural anthropologists, and now linked with a wide array of methods termed "qualitative." Epistemological framing for use of any of these methods is, however, crucial, for such work sees human consciousness as active and dynamic, mediated by symbols and systems of meaning and activity in which individuals are embedded. The individual can no longer be the unit of analysis, nor can the group, situation, or task. Instead, these in their interactional and interdependent flow call for the phenomenological and hermeneutical approaches of ethnographers.

What is often lost in the rush to ethnography, however, is the fact that "true ethnography" relies on comparative studies. These are usually of relatively small social units and depend on actual participation, whenever possible, as well as observation of naturally occurring events and interactions. (For a review of these points, see parts 1 and 2 of Jessor, Colby, & Shweder, 1996).

Often critical to such studies is involvement of those being studied. Some scholars simply work closely with particular members of social units to assist in analysis

and interpretation; others directly involve locals in collecting, transcribing, and analyzing data (Heath, 1983/1996). Concerns over ethics, as well as distribution of such participants within the social unit under study, must be addressed. However, as social science information becomes more widely disseminated around the world, groups previously isolated geographically, as well as through poverty and political oppression, have become more savvy about such work (Giddens, 1990). As such, they resist being characterized by temporary outsiders and may work to undermine particular areas of the research or to sabotage the entire effort. Bringing members of the social unit under study into the research in various roles can help to break down such resistance as well as greatly increase the validity of interpretation by the ethnographer.

These specific methods help, but by no means ultimately solve, the problem of fundamental dichotomies implicit in most research methods: subject–object, insider–outsider, native–foreigner. The question of just who is native becomes increasingly important as scholars using ethnographic methods study their own life histories or those of their own socialization. Some studies from "within" argue that outsiders can never "know" the life of the oppressed and stress the validity of narrative accounts by insiders. Other studies argue against the locus of researcher as either inside or outside a social unit. They emphasize instead fluctuating and multiple identities that must always play across certain bodily characteristics, such as age, gender, and race, that invariably influence membership (Narayan, 1993).

In addition, these methods leave researchers and clinicians with what can be seen as an excess of description and an abundance of terms from the postmodern lexicon ("plural," "fluid," "processual," "contingent," "situated"). Particularly for clinicians, and also for some basic researchers, the resultant push toward relativity is disturbing. Some wave this concern away by asserting that positivism remains strong. They further argue that interpretivist moves are not likely to insert themselves in significant ways into the many institutions and professions whose very existence rests on certainty and the authority of their professionals. Moreover, collecting knowledge through a variety of epistemological frames should persist and that because the very best of work termed "qualitative" may well require special gifts, the world is not likely to be overrun with such writings (Becker, 1996). Others worry about the "moral" nature of any social unit—the ultimate responsibility individuals bear to their reference group—and fear the easy blanket denunciations of "power" that mark some dominant theoretical approaches. Any moral authority requires power ordering. Researchers have to acknowledge that "the analysis of culture as a moral order and the analysis of culture as a power order" depend on each other (Shweder, 1996, p. 43).

Most of these concerns, divergent as they may seem, still unify in their challenge to scholars to let go of dichotomous thinking in which one of the pair of terms is always privileged over the other. Consider, for example, pairs in education such as *formal* and *nonformal*. The latter is defined by what it is *not* rather than by what it is, and the former is always implicitly therefore the norm. Similar dichotomous pairs embody particular theories of socialization; for example, divisions of teacher–student, expert–novice, and adult–child invariably rest on a transmission model of enculturation and privilege one particular body of skills and knowledge over another. Yet peer socialization is dominant in many parts of the world, and expertise achieves definition in accordance with situation and task. The very terms *expert–novice* often suggest that expertise is unified within an individual, whereas individuals and groups reflect continua of degrees of expertise with regard to almost an infinite range of domains. The pairs of teacher–student and adult–child place the elder as initiator and repository of information, when, in fact, students and children play these roles constantly.

Discursive Practices

Particularly troublesome to those with poststructuralist perspectives has been the fundamental faith that positivists appear to have in language. From specific terms taken as given and universal to their choice of verbal practices, oral and written, scholars in fields such as experimental psychology and social psychology take meanings as uniform and stable. A term so apparently simple as *family* holds a vast array of meanings within populations of the United States, where less than 10% of families are nuclear. *Clan* holds meanings for Hmong families that differ from those associated with clans claimed by Native American groups. We have already considered the problem of staged labels, such as *adolescent*, but it is important to note also omissions that come with assignment of terms along a developmental trajectory. *Child* does not cover those from 8–12 in nearly the same way it includes their younger counterparts; thus, the middle childhood years have suffered considerable neglect in studies by psychologists who either center their development studies on infants, toddlers, and preschoolers or focus on adolescents or those with particular maladjustments.

In addition to a worry about labels and their frequent irrelevance to the lives people lead come numerous warnings about long-standing methods of collecting data. Survey instruments, long a mainstay of social psychology, sociology, and other disciplines in the United States, generally presume monolingualism in English, as well as literacy and familiarity with question-and-answer formats for coding. Even when allowances are made by survey developers for translation to

other languages and oral administering of surveys, fundamental problems remain. Primary among these is the growing resistance of most members of complex societies to the impersonal bureaucratic intrusiveness of surveys. Whether by telephone (almost always during the dinner hour) or through the mail, surveys represent to many an invasion of privacy. Low return rates plague those who carry out surveys because their respondents, like informants in the earliest days of anthropological fieldwork, may well be marginal and atypical members of the population under study in part because of their very willingness to give information out to strangers.

Interviews or the collection of oral histories from individuals reflect similar difficulties. Foremost among these is the expectation that addressees must respond to questions as elicitations and that they will do so honestly or accurately. Many studies, as well as innumerable personal anecdotes exchanged at professional conferences of social scientists, indicate just how differently members of various groups respond to interview questions (Aunger, 1994; Briggs, 1986). The same kind of variation exists for narratives. Members of communities only a few miles apart may have vastly different estimations of what constitutes a story or narrative, as well as of appropriate information to tell a stranger (Heath, 1983/1996). Thus, even interviews labeled "ethnographic," in that they are carried out after the interviewer has become somewhat known to respondents, offer no assurance of adequate or accurate reflections of beliefs and behaviors. The same goes for collections of life histories or narrative accounts. In particular, if individuals eliciting responses to either surveys or interviews have little knowledge of the social unit under study, the usual safeguards for validity in ethnographic representation (e.g., close, careful, multiply recorded, extensive observation and possible participation) will be absent. Focus groups, a current favorite method from advertising research to human resources workshops, offer similar susceptibility to misinterpretation. Because consensus must be reached in these groups, rational moves toward agreement are taken for granted as generally shared among members. In addition, those who conduct such groups often fail to recognize that women or younger members from certain sociocultural groups may not disagree in public with men or their elders. Hence, focus group data cannot be relied on without triangulating data, such as fieldnotes on daily interactions and ritual gatherings, audiotapes of naturally occurring interactive group meetings, and an understanding of folk beliefs about dissent and agreement.

An even more fundamental difficulty lies in the fact that many individuals, as well as social groups, consider numerous means of displaying knowledge more appropriate than straightforward verbal accounts given by a single individual. Though most researchers take as

given that individuals can act as agents of decision and action, many sociocultural units around the world have no such concept. World health organizations meet this problem repeatedly when their units counsel women on birth control, prenatal care, and other issues considered to be "women's health issues." Yet in numerous sociopolitical and religious groupings, elder women, co-wives, and husbands make such decisions, not the younger individual females. Teachers make similar assumptions when they request parental visits to schools. Within many immigrant neighborhoods in the United States, for example, the exit of a woman alone from the community is unthinkable, and men work long hours and may be unavailable during appropriate times for school visits.

It is also the case that some individuals as well as social groups have no habit of reflecting on their behaviors and beliefs for verbal reconstruction and analysis of their behaviors or beliefs. In every society, automatic and routinized actions do not give themselves up easily to analysis. Yet researchers often appear not to question the validity of data they acquire by asking respondents to reflect, talk as they carry out certain actions, or offer meta-analyses of certain societal features. For example, protocol analysis has been widely used by researchers studying the writing process; writers are asked to talk about what and how they are thinking while they are in the actual process of writing. For many individuals, the simultaneity demand radically alters the writing, and what results is a new and different representation of what "actually" goes on in the head during solo writing. The 1990s penchant for reflection, particularly in education, represents another go at getting "inside" what is going on for an individual. Though written after a particular event or situation, such reflections vary in their validity and credibility in accordance with numerous factors that may be entirely extraneous to the topic of study. For example, for those who find writing, and especially writing about the self an easy task, reflections come easily; for those with strong insecurities about writing and distaste for what they may view as narcissism, such reflections prove strained and difficult. The danger for researchers is then to see such reflections across respondents as equivalent data sources.

Beyond these problems is one even more fundamental. Whereas positivist approaches take for granted that language is merely instrumental, conveying autonomous meaning, poststructuralist approaches suggest that systems of signs construct meanings and constitute sources of power as well as self-influence. Implications of this view for research methods bring us around to the need to realize in some highly practical way that traditional methods, especially those used in surveys, construct rather than reflect meaning. Little evidence exists that most social science researchers *admit and act* on

an understanding of the power of signifying practices to construct objects of knowledge.

All of these problems get magnified by the messiness of conditions of change that account for constant re-creations of habits, beliefs, and interactions. Child language research offers a good case in point. For several decades, scholars working on language acquisition studied firstborn children in homes of middle-class mainstream parents where the mother remained home most of the day with the preschool child. These studies were supplemented by laboratory experiments with these same children, brought to the laboratory by their at-leisure mothers who participated in experiments using books, blocks, toys, artifacts, and ways of behavior similar to those of their own homes (Snow & Ferguson, 1977 and many studies reported in Slobin, 1985). Omitted from such studies were not only worries over culturally biased forms and conditions of elicitation, but also children of households where leisure as well as extended access to mothers by a single child were nonexistent.

Cross-cultural long-term fieldwork among Pacific cultures (Kulick, 1992; Ochs, 1988; Schieffelin, 1990) and within groups of different historical backgrounds and classes within the United States (Heath, 1983/1996; Miller, 1982) has repeatedly demonstrated that certain aspects of parenting generally deemed basic in acquisition studies are by no means universal. Concepts such as a single child interacting at length with a single adult, commercially produced toys, and adjustment to local conditions through verbal directions appear as highly specific to certain types of social units with particular goals of human interaction. A comparative study carried out among middle-class European-derived families, middle-class Turkish families, and highland Guatemalans illustrates these points in detail (Rogoff, Mistry, Goncu, & Mosier, 1993).

It has been necessary to exercise great care in sorting through many particularities to identify the subtle small slices of human interaction surrounding child language acquisition that may be universal. Fernald, Taeschner, Dunn, Papousek, DeBoysson-Bardies and Fukui (1989) have specified features of intimate child and adult intonational interactions that may be universal. A growing body of "experiments in nature" (Tonick, 1992) delineates aspects of collaboration between toddlers and their caregivers while pointing out cultural variations in guided participation, goals of development, and nature of involvement between children and adults (Richman, Miller, & Levine, 1992; Rogoff, Mistry, Goncu, & Mosier, 1993). Particularly important is the attention in these studies to balance between description and theory-testing and to multiple locations of meaning-making. In addition, these studies attend to learning organized in large part through children's own observation and participation rather than through adults' direct verbal attempts to motivate and instruct.

Within this work, the process of socialization and children's development of language is constructed through individuals interacting with others in relationships across varying situations. Knowledge and meaning are reflected as structures fluctuate in their social construction at the very moments of practice. Shifts in audience, power relations, and often seemingly minuscule elements of the environment demonstrate the adaptive capacity of humans. Moreover, memories of the past and myths built around events and people shape ways in which knowledge is displayed, encoded, and applied and the extent to which information or facts may be defined and valued. It is also not possible to assume that the individual in every society is a unitary rational self who regards the self as central knower or even interpreter of the intentions of others. Moreover, the social dynamics of power shape and reshape knowledge and meaning as particular interests determine saliency at any given moment and dictate contexts, forms, and dissemination networks for information, goods, and services. In short, research that proposes to incorporate dimensions of diversity, variability, and particularities across social units will need to suspend "disbelief and to access hidden, unconscious, or marginalized aspects of [their] own subjectivity" (Shweder, 1996, p. 18). Ideally, such capabilities result from long-term participation and observation. Multiply situated perspectives gained by immersion within a group along with background knowledge of history, geography, and demography, produce a picture of the holistic array of shaping facts for social, linguistic, and cognitive change in groups and individuals. Short of such immersion and true participation and direct observation, clinicians and researchers must turn to the accounts of others and confront them with their questions and concerns. A vast amount of good will and earnest desire to learn has to take place by those of different epistemologies. In addition, alterations in regulatory proesses and assessment techniques used by large bodies of professionals have to begin to reflect findings from culturally defensible research.

Initial Steps Toward Understanding

Though no one will deny the difficulties involved in changing the ways of a field of science, specific suggestions for bringing about change rarely move from the programmatic level. More efforts are needed to bring researchers from a range of disciplines and theoretical traditions together to focus on content instead of defenses or criticisms of other traditions.

Numerous strategies for accomplishing communication about common concerns have been tried. Perhaps the most successful is that presented by Chaiklin and Lave (1993), the result of a conference that brought together scholars concerned about next steps

in understanding human practices located in social and societal interactions. Specific techniques used to keep authors from slipping into methodological defensiveness and thin presentations of data are reviewed in Chaiklin (1993).

Attempted here is a proposal of strategies with similar goals in bringing together those of radically different epistemologies—researchers and clinicians, especially those concerned with learning and human development. The underlying premise here is that such groups need to address what they have in common before they move to exaggerated views of their differences. But to do so, members of both groups may need to address specific basic questions together.

The first of these questions might pertain to the matter of what to do with "anomalous data." If events or responses do not fit previously noted categories or patterns, what do they indicate? Here, those from different epistemologies use such data, but often to varying ends. For example, in the study of child language, looking at phonological disorders of children can give very useful clues to ways that normal phonological systems work (Leonard, 1992). Recognition of this type of use of what may be called anomalous data can pave the way for understanding why ethnographers often study breaks in routines to discern how time, space, and expectations operate ordinarily within a particular group. In other words, to get at a recurrent pattern, scholars study nonrecurrent events and behaviors.

A second question that may draw groups from different epistemologies together is the matter of preferred times and periods of data collection. Many scholars agree that too few longitudinal studies exist and that funding considerations often drive the intense focus on initiation and short follow-through of treatment rather than long-term follow-up. For example, it is far more common for researchers to study the acquisition or onset of certain behaviors (ranging from language learning to reading) than the retention and development of these. Hence, researchers focus on universal patterns of acquisition rather than the wide variations that may show up over time. Relatively few studies ask what happens to reading once initially acquired. Similarly, scholars attend much more to learning among young children than to learning among older children and adults. Yet daily demands in workplaces require constant "upgrading" of skills and adaptation to new demands, especially technological and communicational. The widespread bias in research toward focus on acquisition and early stages may disadvantage members of certain groups (e.g., Native Americans) who frequently attest to skills and information that "click" long after the actual experience of being "taught." This has been particularly true with respect to literacy practices and habits of thinking identified as "literate" (Walker, 1981). Scholars of radically different epistemologies can join together to push for more long-term follow-through studies to benefit findings from several fields and areas of policy interest.

A further example of a question to bring those of different epistemologies together comes from the universally recognized but infrequently acknowledged fact that subjects can and do outwit researchers. Joint consideration of just when and how such occasions occur may help bring those within different paradigms together. Weisner (1996) makes a similar point when he calls for a rejection of "methodocentrism" and acknowledges that no important question can be answered through use of a single method. Complementary methods are increasingly necessary with "research-smart" individuals and groups able to second-guess just what the researcher is looking for or wants as a response.

Illustration of this last point comes from long-term fieldwork carried out among youth-based organizations that older children and teenagers identified as "effective" learning environments and safe places where they wanted to be (Heath & McLaughlin, 1993). In the first year or so of this decade-long project involving six major types of data (fieldnotes, audio recordings and transcripts, daily activity logs, informal interviews, sociodemographic and local statistics related to youth, and a national survey instrument), senior ethnographers indicated their desire to bring some youth onto the research team as junior ethnographers. Several members volunteered and were trained in the ethical and procedural requirements of taperecording young people talking about their activities carried out through their youth-based organization. They were also provided guidelines for conducting interviews with peers and other members of the community regarding the lives of young people.

In the summer of 1990, one junior ethnographer turned in audiotapes of interviews she had done. When Heath listened to the tapes, she realized that the four tapes were "false interviews" the young woman and her friends had staged in place of actual interviews with community members. When confronted, the young woman reported that she had been pushed for time, needed the money, and thought this substitution would be "all right."

Indeed, it was more than all right. These "non-data" in terms of the project's original definitions of data revealed much about what she as a new member of the research team thought that outside researchers wanted to hear from the four individuals (or "types") she had set up as community members. She chose first to interview "a very old lady" who had grown up in Mississippi picking cotton. Next, she fabricated a "gangbanger" who barely escaped jail and disdained school and family. Her third interview was with a "very good student" who toed the line and just "loved" her youth organization. The fourth interview was with a 13-year-old girl who portrayed the "typical" school girl/home girl on the verge of womanhood whose life centered around boys,

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fights with other girls over boys, and a sense of apathy about the future.

These created scripts fit neatly, but not too neatly, the range of interview questions outsiders are likely to ask of young African Americans in an inner-city environment. This young imaginative researcher raised questions such as "What is school like for you?" "What kind of hope do you have for the future of you and your friends around here?" "What was your childhood like?" The staged answers to these questions flowed easily and realistically, painting pictures of the expected: for example, the extreme poverty and persistent racism the older woman faced growing up in Mississippi, her disillusionment with her trip north, her troubles with men, and her acceptance of old age with a close sense of connection to God and gratitude for the blessings of her life. It is not possible to listen to these tapes prepared by a 16-year-old and her accomplices without having great admiration for their "insider" knowledge on how outsiders view them and go about constructing questions that will confirm the field's prior expectations of these subjects.

If these young people could so elaborately construct these "false interviews," complete with well-placed coughs, labored breathing, combinations of complete and incomplete sentences, hesitations, and overlaps, then it should be evident that they and others imaginatively create responses to surveys of epidemiologists, social workers, and so on. Repeatedly through the decade of the research project in youth-based organizations, the senior ethnographers heard from older children and teenagers their stories of fooling teachers, administrators, test administrators, social workers, police officers, and others about what they were "really like." Even after intense immersion in their daily activities, transcription of over a million and a half words of their language, and collection of an array of other types of data, no claim can be made to have a "full" ethnography or "real" picture of what goes on in their heads or of what "human consciousness" means to them.

What the project does have are sufficient data to illustrate ways in which these youth draw upon multiple symbol systems, engage several versions of themselves depending on circumstances, and call on multiple discourses according to need, motivation, and domains. These findings have made notions of fixed essences, intrinsic meanings, or the idea of universally necessary "truths" about young people increasingly suspect. It is clear that cultural and institutional forces specific to certain locales, historic forces, and current mixtures of populations have strong influences on the processes of psychological functioning and human development. This is especially the case for the young whose socialization and courses of learning in the late twentieth century differ greatly from those of their parents and grandparents.

Moreover, the work of this project, carried out in over thirty locations from Massachusetts to Hawaii, illustrates, as do other large long-term projects in the United States, just how unstable certain identifying labels can be (cf. Lamphere, 1992).

Primary among these is *ethnicity*, which varies in salience, definition, and representation by individuals according to perceived need. Depending on phenotypical characteristics, an individual born of a mother of Mexican origin and a father of Eastern European origin may be regarded as "black," "Greek," "Filipino," or "Latin," or any of an array of current ethnic labels. The individual's self identity may be none of these ascriptions but merely "American" with no particular label of ethnicity attached. Far more important may be neighborhood, school, or affiliated gang as identifying markers.

It is commonplace now to acknowledge that people who appear to be of the same ethnic background will greatly differ in their sense of themselves as "ethnic." Gendered identities can also be largely "made" (Ortner, 1996). In addition, the very occasions for claiming or making oneself to be a certain something will vary across age span, need, and with associates preferred at particular times and places (Heath & McLaughlin, 1993; Zentella, 1997). Shared socialization, similarities of customs, language, and history create a sense of membership that may alter as individuals move among groups in the same locale, from region to region, or from event to event. The "Mexican" background of the individual in the previous example may emerge at the funeral of the grandmother in Michoacan more than in any other situation or social context. Similarly, a trip to Eastern Europe or a Chicago suburb to visit relatives on the father's side will provoke a sense of connection not easily called up elsewhere. It is possible for those designated as being of one ethnic group to change their ethnic membership by adopting new customs, values, and language habits, and it is not uncommon for such changes to be prompted by a desire for political or social gain. Moreover, ethnicity is carried as only a part, and often only a relatively unimportant part, of the bundles of features that make up one's sense of self. Ethnicity is then ultimately very much embedded not only in the contexts that may call it forth to lesser or greater degrees, but also within numerous other features of self that achieve different salience depending on circumstances. If one works within a site or occupation traditionally or newly identified with a particular ethnicity, then ethnic self-identification may become useful or even necessary for employment and residence in particular neighborhoods. In the 1970s and 1980s, as high-rise public housing projects within inner cities increasingly included families from different linguistic and cultural backgrounds, the young were thrown together to face the streets as peers, and separation by

ethnic identity became threats to survival (Conquergood, 1992).

Studies among young people today indicate that in terms of self-identity, feeling good about oneself, and doing well converge more often around being able to *do* something that peers recognize, applaud, or fear than around one's ethnicity. This is confounded by the fact that marriages and relationships increasingly cut across ethnic, nationality, and linguistic lines, leading to "mixed" children who prefer not to claim one parent's ethnicity over the other's. Efficacy, not ethnicity, dominates identity among peers in many places throughout the United States. Self-concept and identity sit firmly within performance; esteem stems from achievement, not empty labels (Mecca, Smelser, & Vasconcelos, 1989; Rosenberg, Schooler, & Schoenback, 1989). The near-axiom of sociology that self-concepts come as reflections of the evaluations of others, implying a passive learner and conforming human being (Cooley, 1964), is gradually being replaced by the theoretical position that self-evaluation and future projections of self are based on efficacious action by an active learner. Inner-city youths make this message clear by forming groups with titles such as DWH—down with the 'hoods—and selecting members from across ethnicities assigned by school authorities when they insist students label themselves. Such groups insist that they select members on meaningful qualifications and not "just" by ethnicity. The young increasingly send clear messages that they want learning environments that enable both action and self-identity, and social responsiveness and autonomy in self-evaluation. Strong families, youth-based organizations, and cohesive athletic teams, as well as gangs, offer contexts that promote trans-situational learning in which young people play different roles and tie their self-evaluations to experiences of themselves as meaningful group members (McLaughlin, Irby, & Langman, 1994).

The Way Ahead

The search for similarities and differences within diversities will call increasingly for new combinations of researchers and inquiry methods. In the decades of the 1980s and 1990s, the John D. and Catherine T. MacArthur Foundation sponsored long-term interdisciplinary networks focused on single topics, such as middle childhood or adolescent development. Publications (e.g., Jessor, Colby, & Shweder, 1996) show progress in the efforts of scholars from different disciplines to work together. The Ford Foundation sponsored a similar coalescence of scholars studying new immigrants to urban centers after 1965 (Lamphere, 1992). Foundation grant programs designed for individual recipients increasingly indicate their willingness to support interdisciplinary work. National federally

funded research centers, such as the National Center for the Study of Writing, draw together researchers from several disciplines and encourage methods typically termed "quantitative" with techniques used for case studies and ethnographic study. Graduate programs, particularly those in education, call often for courses in a range of inquiry methods.

Through all of these efforts, more scholars recognize that the dichotomy between quantitative and qualitative methods means little. The fuzziness of these prior boundaries matches that now growing around categorizing labels for individuals and groups. Ethnicity and race, traditionally taken for granted as boundaries around behaviors and beliefs, bow to class, geographic, and individual life history factors that do not give way to easy classification. Variations within groups previously lumped as a single culture (e.g., African American, Mexican American, etc.) show up on television situation comedies as well as among evening newscasters. Ongoing processes of change create redefinitions by individuals and groups. Subversion of habituated expectations for members of an ethnic or cultural group now motivates social clubs as well as individual career choices. Gospel choirs purposefully perform music from across the centuries and around the world. From youth-based organizations to welcome-to-campus retreats for freshmen, young people defy what they often term "boxed identities"—the check-off spaces they find on bureaucratic forms.

All of these moves to ambiguate culture and blur lines will continue to influence research in a variety of ways. Undergraduate programs on many campuses now encourage study of issues surrounding ethnicity, race, gender, and sexuality, and ethnic studies units centered on a single group move away from the spotlight they held in the late 1970s and early 1980s.

Explorations across highly varying situations of social units in interdisciplinary collaboratives will have to be disorderly and unconventional (by past criteria of controlled studies). But tolerating this state are numerous new journals and publication outlets as well as centers and programs on university campuses that cut across usual departmental or disciplinary lines. Public service experience, now a staple in many high schools and colleges, exposes those who will be clinicians and researchers of the future to the lack of correlation between prior stereotypes of certain groups and realities of individual encounters.

Social units for study present possibilities of learning, teaching, adjusting, and innovating primarily in three ways. They do so first through the meaning-making systems their interactions construct to define individuals as group members. In addition, they exemplify these possibilities in the ways they shift identities and styles of presentation across situations, as well as ways they link themselves to the future. Finally, they handle possibilities that emerge in meaning making with avail-

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able tools (ranging from technologies of representation to those of production).

Past work in human development gave far too little attention to these three factors. They focused instead on socialization as transmission, barriers between "cultures," and convenient compatible groups for study. Scholars across disciplines now have the means to rethink conceptual models, ensure use of several methods of data collection and analysis, and create new theories of identity, motivation, socialization, and interaction. Collaborative research partnerships—across disciplines and in conjunction with those being studied, as well as within different sites of learning—will gradually permit a reshaping of the social science enterprise (Rogoff & Chavajay, 1995). Such partnerships will open a rethinking of categories, instruments, and discursive practices that have heretofore gone unquestioned. These integrated approaches offer a high goal for scholarship and a considerable challenge for clinical practice and education as well as hold promise for understanding much about sociocultural processes we have not yet begun to imagine.

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