Can Ethnographic Research Go Beyond the Status Quo?

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In the statement I wrote for the CAE nominations three years ago, I adopted from theologian Paul Tillich (1966) the concept of "boundary living" and suggested that both I and CAE live on the boundary between the worlds of academic scholarship and professional practice.[1] The goal of boundary living, it seems to me, is not to alternate between two worlds but to find forms of integration valid for both. Now, at the end of these three years, I want to raise a question about that integration: Can ethnographic research go beyond the status quo?

While teaching in Alaska last summer, I was told that somewhere in the Alaskan State Department of Education there is a sign that says:

WE DON'T NEED ANY MORE
ANTHROPOLOGICAL EXPLANATIONS
OF SCHOOL FAILURE

It doesn't matter whether there really is such a sign. It seems quite plausible that one could exist, and I want to pose the question of how we would respond to educators who nod in agreement at its idea, those who find our kind of social science unhelpful in their work.

One response is to attack the signmaker: The kind of research the signmaker wants is positivistic social science that can only be implemented in an authoritarian, manipulative, bureaucratic system. We don't believe in that kind of research, and we don't want to play that kind of role.

Another response, taken from an NIE committee report on "Fundamental Research and the Process of Education" (Committee on Fundamental Research 1977) argues for a broader view of research relevance. Exclusive concern with translating the outcomes of research into improved skills of the practitioner is too narrow; practitioners have not only skills, but also a view of reality, a vision of the achievable, and a commitment to act; and social science knowledge can influence all four. (Cf. Margaret Mead's [1957] plea for "more vivid utopias.") Moreover, says the committee, that influence takes place through a series of filters that are as yet poorly

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understood. Scholarship rarely goes directly to practitioners or policy makers. It more often enters a process of uneven diffusion through the filters of more popular writers and the media; political, legislative and legal debates and decisions; textbook publishers; and so forth. The leaves of our scholarship may be caught up in such breezes in a beneficial but untraceable way.

A third response, not incompatible with the first two, is more self-analytical. It poses questions:

- What is the balance in our work between description — even celebration — of patterns of life as we find it, and participation (as social scientists, not just as citizens) in imagining, trying out, documenting designs for change?

- Does our methodological emphasis on holistic description make it in principle wrong, or at least in practice difficult, to consider parts of a whole as potentially changeable — as "variables," to borrow a term from that other positivistic world view?

- Does our habit of painting an a-historical, synchronic portrait of the ethnographic present make it hard to shift to the kind of diachronic account of change over time that would be necessary not only to document learning, as Erickson (1982) has called for, but also to suggest alternative situations in which learning is more likely to occur?

- Does our preference for functionalist explanations — that all education "works" in some sense or it would not exist — make it seem a violation of our theories to say that some educational environments do not work in some other sense?

- Finally, what is the convincing argument or evidence for what seems to me our almost exclusive reliance on raising the consciousness of practitioners as the process by which our description of the status quo can lead to change?

To make my claim for the plausibility of that Alaskan sign more concrete, I will take one sense in which much education is not working and look at research relevant to its improvement. The problem is the need for greater equity, or decreased differential treatment. In Erickson's words:

> Without some considerable capacity of the teacher and learner to take adaptive action together in the mutual construction of learning environments, the species would not have survived and developed. Human groups can thus be seen as having a profound evolutionary stake not only in the capacity of the cultural

> neophyte to learn what is deliberately taught, but in the capacity of the cultural initiate to teach what needs to be learned. In institutions of schooling the mutual calibration and reciprocity between teacher and learner that is necessary for successful direct instruction in cognitive learning seem to occur only between some pupils and the teacher and not between other pupils and the teacher. This is at once the major policy issue for schooling in modern societies and a crucial issue of evolutionary adaptation for the species (1982:173).[2]

Of all the ethnographic/linguistic research on problems in achieving "mutual calibration and reciprocity" during the last 10-15 years, I know of only two clear examples that go beyond the status quo, two cases where ethnographers have not only described problems but have stayed to collaborate with teachers in designs for change: Shirley Brice Heath's work in Appalachia and the Kamehameha Early Education Program (KEEP) in Hawaii. An important contrast case, where there has been very impressive descriptive research but less participation in change, is the problem of dialect differences as exemplified by the recent "Black English" court case in Ann Arbor, Michigan.[3] A few words about each in turn.

Over a period of nine years between 1969-78, Heath was both an ethnographer in rural black and white communities, working at the request of parents who wanted to understand why their children were having problems in school, and a professor giving in-service courses for teachers; and she was able to combine both roles to great advantage. For example, following her own ethnographic research on the forms and functions of questions in the children's home settings, she encouraged teachers to observe the questions they asked their own children, at home and in school; and then worked with the teachers designing and trying out new curriculum, and new patterns of classroom discourse (Heath 1982, and in press).

The KEEP program also evolved over a number of years; but in contrast to solo Heath, it was the work of a large interdisciplinary team. Ethnographers, psychologists, reading educators and laboratory school teachers worked together for 10 years developing a program that has had dramatic benefit for Polynesian children's achievements in reading, at least in the primary grades (Calfee et al. 1982). What can be said about the contributions of ethnography to this program design? The anthropological insights are a matter of record. But it is not so easy to track the influence of those ideas in ongoing curriculum development work. The most extensive retrospective discussion available is by Jordan (n.d.).

In Jordan's account, the ethnographer had three roles. Direct application of ethnographic insight to KEEP program design seems to have been rare, but the involvement of children in setting up the classroom each morning was one such "planned intervention." A demonstration teacher conceived of this innovation after hearing Jordan talk about Hawaiian children's home responsibilities and observational learning. More often, the ethnographer participated in what Jordan calls "contribution to a consensus" — agreeing on a particular plan of action for cultural reasons while psychologists and educators agreed to the same action on other grounds. For example, Jordan contributed to the development of the learning center

social organization because of her insights into children's abilities and participant structures, while others favored the same plan as a way to increase children's time on task, or as a means to free the teacher for small-group instruction. With respect to still other program elements — for example, the "talk-story" patterns of interaction that evolved during the comprehension lessons (Au 1980) — existing anthropological research helped the team to understand and elaborate something that initially entered the program for other reasons.

Thus the particular relationship between anthropological insight and classroom practice varied from one program element to another. The single underlying characteristic seems to have been the presence of ethnographers throughout the research and development process, interacting continuously with teachers and other researchers, and willing and able to contribute to program design in these diverse ways.

To these two "success" stories, the Ann Arbor Black English case presents a much less optimistic contrast. Fortunately, we now have Labov's (1982) detailed presentation of the history of the expert testimony in that case. Parenthetically, as one who has also left the university for the witness chair and judge's chambers, I was especially interested in Labov's discussion of objectivity and commitment in scientific research. But what is relevant here is the relationship between expert knowledge and improved education for black children. Labov makes a strong case that in the 15 years between 1964, when research on Black English began, and 1979, when the Ann Arbor case was decided, the linguists got their act together. With the important help of young black linguists in the 1970s, they resolved the differences of fact and interpretation between the dialectologists and the creolists, and presented a united, coherent position on Black English to Judge Joiner, who found in favor of the 15 black children and accepted the Ann Arbor school system's plan for alleviating the language barriers by means of in-service education for teachers.

The record of linquistic research is impressive, and I do not want to diminish its importance in any way. But, with reference to that reputed sign in Alaska, we have also to acknowledge Labov's comments that:

> My own view, and one that I expressed in testimony, is that operations on attitudes alone [as in mandated in-service programs] will not be enough to make a substantial difference to the reading of black children. . . . It isn't clear that effective ways to implement this decision are on hand, or whether it can make a substantial difference to the education of minority children (1982:194).

At this point, isn't it true that we — the community of linguists and ethnographers — have explained educational failure without showing how it can be reversed? As long as this situation continues, the losers are not only the children but also our social science. Without examples of successful attempts at improvement based on hypotheses derived from our descriptive research, can we even be sure of our explanation of failure? With respect to

Ann Arbor, can we persuasively argue against Nathan Glazer, who believes Judge Joiner's decision should have been "case dismissed," when he says:

> Whatever the responsiveness of the Ann Arbor school authorities, the black children are still failing, and that, in some sense, is a failure of the system. But in what sense? And how is it to be corrected? What one finds here is a real grievance — my child is not learning, has not learned — and then a lawyer searches the law, interviews the experts, stretches his own imagination, and comes up with something that he thinks will help and that he can argue in addition is a right. The thing he comes up with is minor compared to all the factors that are leading to the grievance. After the winnowing of the court process, something even more minor may be granted to the plaintiffs. But whatever the causes of the failure, it is an act of ungrounded faith to believe that trying to heighten sensitivity to Black English can play any but the most insignificant role, if that, in mitigating it (1981:52-53).

As Erickson said in his past-president's address four years ago, "The ethnography of Malinowski and most other classic ethnography — mere ethnography — does not address such questions as 'How can we make this canoe better?' Thus classic ethnographers have been unable to learn what can only be learned when one gets involved in the action, and picks up one's own end of the log" (1979:186).[4]

In conclusion, I want both to agree and to disagree with McDermott and Hood's (1982) attack on the hegemony of educational psychology in educational, including ethnographic, research. They make a strong case for the necessity of researchers to consider displays of competence as a social, not an individual, accomplishment, and to go beyond assertions about the overall adaptability of children to show "how schooling is organized or how it might be better organized" (p. 236). Their case material is taken from Rosa, in the first grade reading group analyzed by McDermott, and Adam, a child labeled as learning disabled who was a participant in the after-school group established by the Laboratory of Comparative Human Development at Rockefeller University. "Both children were in great trouble in that they were far behind their peers in learning to read" (p. 242). But, after a lengthy argument for research that provides detailed analyses of the moments in which these children are called on for intellectual displays — paraphrasing Goffman, not "children and their moments," but "moments and their children" — the article ends with a final footnote:

> Adam actually was in a quite extraordinary private school in which less debilitating circumstances were often arranged for him, and he made much progress over the two years we followed him. But that is not relevant to the point being made here (p. 247).

Why is it not relevant? Wouldn't a description of those "less debilitating circumstances" that somehow added up to "much progress" be exactly what is needed to move from descriptions of failure to suggestions of how to achieve success?[5]

There are many problems in entering what Patricia Graham (1982) calls the "briar patch of school improvement," not the least of which is funding. Sarah Michaels (1982) and I experienced it firsthand when we tried to get money to expand our "Sharing Time" research (which at the moment also fits the "explanation of failure" category) into collaboration with teachers for change.

But unless more of us try to understand how to "make that canoe better," the educational psychologists whom McDermott and Hood want to unseat will stay in power. They are unabashedly in the business of design, as AERA President William Cooley has made clear (Cooley 1982). Unless we join them, our special ways of knowing about the relationship of actions to their contexts and their meaning to participants, and our more collaborative ways of working, will be ignored. And the message on that real or imagined sign in Alaska will remain all too true.

Endnotes

1. Since encountering Tillich's concept of boundary living in 1974-75, when I was living almost wholly on the classroom side of the border (Cazden 1976a), I have found other descriptions of professional identity in similar words. Gouldner speaks of himself as a "ridge rider: half sociologist and half Marxist, and rebel against both" (1976:xiv); Shulman calls fellow educational psychologists "border provincials" (1981:4-5). The details of border living depend, of course, on the adjacent worlds, the relationship between them, and one's stance toward both.

2. See Bredo and Feinberg (1983) for an analysis of positivistic, interpretive and critical theory research paradigms, with examples of each selected for their exploration of a single issue: "the differences in educational outcomes experienced by students of different social origins" (p. 10). See also Cazden (1982) for extended discussion of one kind of differential treatment.

3. In this year of Solon Kimball's death, I want to acknowledge his support for sociolinguistics as part of anthropology and education. He included *Functions of Language in the Classroom* (Cazden, John and Hymes 1972) in his Teachers College Press series.

4. Or, in another metaphor, "If school is a performance, how do we change the script" (Cazden 1976b)? Talk of making a canoe better or changing the school script may seem annoyingly "smart" remarks. They both attempt to convey an important idea: There can be no direct derivation of advice for change from any descriptive account — no matter how rich. There is a long history of controversy on this point, including a very interesting exchange between Malinowski and two colonial administrators, in the 1929-30 issues of *Africa*. This history was discussed by Eddy (1982), to whom I'm indebted for these references.

5. I realize that some colleagues consider attempts to "rewrite discourse" as "a failure of linguistics to consider the wider contexts of language use, let alone — in this case [referring to Cazden, 1979] — its failure to consider some of the implications of radical critiques of education" (Kress 1981:79-80). Of course there are wider contexts. See Heath (in press) for discussion of relationships between the innovations she describes and a particular period when desegregation of schools and employment created an openness to change. Social/political forces in Hawaii (which are not

discussed in KEEP's reports) may support or impede the extension of KEEP's work from its privately funded "laboratory beginning" out into the public schools. Social change of all kinds — from nuclear disarmament and removal of toxic wastes from the environment to more effective education in individual schools — requires some combination of the technical and the political. Asserting the importance of one does not negate the necessity of the other. To put the same issue another way, interactions in classrooms are not autonomous (Jules-Rosette and Mehan 1982:24-5), but they are not wholly determined either:

> Educators need to believe that they can make a difference, even in very difficult circumstances. Such a view finds support in the spirit of ethnographic research in discourse and education. The leading ethnographers of education are not determinists; they can not be. They can not assume that the efforts of individuals are unavailing against the forces that shape the economy and structure of society. Their own principles of research require them to assume that the situations in which people participate are in an essential respect created by the people themselves. That is why ethnography is necessary. If what people do, and the meanings of what they do, were entirely determined by demography, budget, administrative organization, and the like, there would be no continuing need for ethnography (Hymes 1982:6).

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