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Bias in the Biography: Bias and Subjectivity in Ethnographic Research

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This article is concerned with bias and subjectivity in ethnographic research. Since the research in ethnography cannot eliminate biographical determinants, the makeup of the researcher is critical to the quality of the work done. Looking at a number of factors that determine bias, the article suggests ways to move this "bias in biography" toward a new era in anthropology and education. ETHNOGRAPHY RESEARCH, BIAS, SUBJECTIVITY, ETHNOGRAPHIC INSTRUMENT

The discussion that follows is concerned with bias and subjectivity. These are subjects I have been concerned with for some time, and which have become of increasing importance in my writing and research. I would like to share my thoughts and tentative conclusions, because I feel that as ethnographers, these issues are perhaps most critical to us as scholars and to the credibility of the work we do, since we have no allegedly dispassionate mediator or "instrument" between us and the phenomena we observe.

The Ethnographic Instrument

If the researcher in ethnography is, indeed, the person of the researcher—and upon this there is little disagreement—identifying the sources of bias and subjectivity in the researcher's own makeup is critical to the quality of the work done. Researchers who use paper and pencil instruments, such as tests, interviews, and questionnaires, are required to make the biases in their data collection instruments explicit. Margins for error, reliability coefficients, known threats to validity—all are identified in the methods sections of good research reports. While ethnographers, or interpretive researchers (Erickson 1986) are held to the same standards, they are not held to mathematical explanations of bias. Rather, they are held to the highest form of disciplined honesty. Unfortunately, except for a rather good set of questions by Evertson and Green (1986) in the latest *Handbook of Research in Teaching*, I have found little in the way of a systematic model for identifying the biases in the ethnographic "instrument"—the human researcher. To do a better job of practicing "disciplined subjectivity" myself, I have begun a form of intellectual psychoanalysis or an "ethnography of the mind" in order to determine conscious and unconscious sources of

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bias in my own work. In so doing, I have been guided by the assumption that bias derives from personal human history and experience, and have based my inquiry into my own work and that of others in our field accordingly. I have been helped in this endeavor by a series of questions raised by Margaret Mead, who considered the following sources of influence when examining the impact of particular forms of schooling upon society (Spindler 1984):

1. Who did the work?
2. Where did they come from?
3. How were they selected?
4. How were they trained?
5. What was their history?
6. What was their value system?
7. What happened to them once they were out in the community?

These questions facilitate definition of what I have called "bias in the biography." For this discussion, I examined the historical record of my own research projects since high school, as well as reviews of the field of anthropology and education by respected practitioners of our discipline who, in looking at the history of the field, were attempting to define it. I hoped in so doing to develop a model for identifying my own biases as well as those in our collective intellectual biography, so as to guide our scholarship into under- and yet unexplored regions.

Sources of Bias in the Biography

Bias, it seems to me, comes from two sources—personal experience and professional training. Professional training is perhaps the easiest of the two to understand, though it consists of a number of factors that we usually do not consider seriously.

The History of the Discipline

A number of biases derive from our academic history. In other eras, scholars chose to focus upon different levels or units of analysis and have defined the field according to their particular interests. These apparently conflicting definitions can be resolved more easily when examined in historical perspective. As I reviewed the history of our field, it appeared to be divided into three eras. With apologies for rather unimaginative labeling, I have divided them as follows: (1) the wild or "feral" stage, (2) the stage of domestication or institutionalization, and (3) the stage of diffusion.

The wild or feral stage. During this period, which includes the period prior to the mid-1950s, inquiries into anthropology and education were informed by cultural or social anthropology and the techniques of ethnographic fieldwork. They were carried out by individuals who were, for the most part, formally trained in the discipline of anthro-

pology, and who worked in museums or university departments of anthropology. They were united by an interest in cultural transmission and its impact on personality, and they focused on the *context* of learning, as embodied in descriptions of the characteristics of cultural groups.

The domestic or institutional stage. This period lasted from the mid-1950s to the mid-1970s and includes the time during which the Council on Anthropology and Education was established. While the type of study carried out during the feral period continued to be done, a great many of the studies done during this time period were informed by learning theory and the data collection and analytic methods of linguistics. No longer were the practitioners of anthropology and education limited to the field of anthropology; now they included sociologists, curriculum specialists, evaluators, and psychologists, who often worked in colleges of education and public school systems. They were united by an interest in formal, usually public, elementary and secondary education, and their focus was not so much on the *context* of learning as it was on the *content*, both formal and informal, of teaching and learning, and upon individual teachers and learners. To some scholars, this may have seeded an unduly individualistic approach for cultural anthropologists, as well as one lacking in broader contextual linkages outside the interactional dyad or small group. However, to me the approach makes sense, because, as Erickson (1986) reminds us, educational anthropology is preeminently concerned with learning, and to have learning, you must also have learners.

The diffusion stage. Diffusion covers the mid-1970s to the present. It has been characterized by shortages of jobs and by movement of scholars out of academe. This has meant a change in the context of the discipline as well as in the questions it asks. In this period, we have been concerned less with the context and content of learning than with *process* and interaction, linkages with other cultures, culture brokering and boundary maintenance (Schensul 1981), and practice in applied settings. Some of the topics for investigation have included how learning takes place, how research gets done, how social life is negotiated, how boundaries between personal and professional life are brokered, how reality is to be depicted, and whose reality is to be portrayed. As anthropologists had to learn to negotiate outside academe, negotiation became a field of study. In other words, the intellectual experience of the field is recapitulated in the professional experience of its practitioners. The questions critical to the field are shaped by the era in which the fieldworkers live.

I present this rather cramped historical overview as a way to look at historical sources of bias in what we do. As a tool for identifying these biases in a systematic way, I suggest that we look at ourselves using the same modes of analysis that Judith Friedman Hansen (1982) suggested be used when developing an anthropology of learning. These

include the analysis of process, structure, context, history, and the interplay between biological and cultural variables. These are the ways in which good ethnographers analyze other people; should we not use the same care in analyzing ourselves?

The Literary Tradition

Ethnography. Bias also derives from the literary tradition of ethnography. It has only been in recent times that the impact on our research of our often overlooked and seldom appreciated literary tradition has been examined. My thoughts in this direction have been informed by the work of George Marcus and Michael Fischer (1986) and others who have used the tools of literary criticism to help them to understand better how to portray the realities that anthropologists study. For example, most anthropological research is informed by the literary metaphor of the travel diary. Most good ethnographies begin with descriptions of a ritual of encounter analogous to stories of "my first contact with the savages of . . ." (Pratt 1986). This is an historical artifact, in that much early anthropological data came from reports by amateur travelers who were reporting on the exotic sights they saw and the strange people they encountered. However, the genre is not only enshrined in studies of settings as unexotic as classrooms and corridors of American schools written by American scholars, but in research methods books, which, without exception, devote considerable space to the role of the researcher and entry to the field. We still hold it important to consider the face researchers put on and how they present themselves to the public. I am not criticizing this at all—only pointing out that it is a characteristic of our field that influences how we work and to what we attend.

False Generalization. Another literary habit is the tendency to generalize inappropriately from the particular. Malinowski spoke of "the" Trobriand Islanders, as if what he saw of those people at that point in time represented forever the whole reality of Trobriand life. Similarly, many descriptions of a classroom discuss the classroom as if what goes on there typifies activities in all classrooms. This is a poor substitute for the comparative analysis or ethnology of classrooms and schooling called for by Hymes, Harrington, Spindler, and others in the past. I think it often derives from an unconscious bias introduced by the way we traditionally have written, just as the habit of writing in the simple present tense in ethnography introduces a freeze-frame sort of fraudulent timelessness into ethnographic description, rather than depicting more accurately the dynamism of social life.

Technology and Intellectual Paradigms

Another influence from the history of the field involves the degree to which the level of technology available in the field and the sophis-

tication of the intellectual paradigms developed to date place important limitations on the kinds of questions that can be asked in the sciences. For example, just as large-scale opinion polling had to await the advent of card-sort machines, and sophisticated correlational analysis had to await the advent of computers, so also certain kinds of analysis required intellectual tools, including mathematical ones, for their execution and had to await new conceptual breakthroughs before they could be executed. I remember being excited upon reading John Collier's *Visual Anthropology* (1967), but being cowed by the expense, complexity, and breakability of video equipment. I chose still photography for my own work as more manageable. I know that there are many scholars even of my generation who still are ignorant of the use of computers and who are still using only the research tools that were available when they were doing their own dissertation research. I have a mea culpa in that direction, since it only has been in the last year that I have purchased my own microcomputer and only in the past three months that I have achieved any degree of comfort with its use. I know that I will write differently as a consequence of this new tool, and I think that the type of research I will do also will be influenced accordingly, simply because it is so much easier to do on the machine some of the things I found difficult—and hence avoided—doing by hand. That antitechnical bias in my own biography, by the way, probably is a consequence of having grown up female in 1950s America, when girls took home economics and weren't criticized for avoiding math. It is a bias against which I still struggle, and which still affects my work.

Mentors

What one is often is determined, at least in part, by the people with whom one studied and under whom one apprenticed. Now, with the wisdom of hindsight, I know that the reason that there is a "Chicago Style" of thinking is because, no matter how much we rebelled against it or simply were unconscious of the influences twisting our minds, students in my program at the University of Chicago all came out thinking, at least to some degree, in the same way as did our professors, for whom we developed a healthy ambivalence. Knowing that someone was a "student of Boas or Erickson, Dobbert or Goetz, Hymes or Wolcott, or Singleton or Spindler or Gearing," to name only a few of the more notable trainers among us, tells us a lot about how that person frames the world and the sort of questions he or she might be expected to ask—and equally importantly, how the answers might be sought.

Friends, Associates, and Colleagues

The herd instinct is strong among intellectuals, and one is known in part by the company one keeps. Originally, I had intended to do a sort of sociogram of the field for this article, cross-referencing citations

from all of the articles by those who had attempted to define what anthropology and education is. I thought that in that way I could define who belonged to what camp by seeing who cited whom. I didn't do it this time, though I still hope to. Part of the reason was that there were methodological problems, not the least of which was that the key articles I was using as sources were written at different points in time, and as I have pointed out, represented different historical eras. Also, some of our ancestors could not have cited important scholars from our younger generation, since in some cases, we hadn't even been born yet! However, friends, associates, mentors, and colleagues are influential in shaping how we define our intellectual world, and they tend to push us in certain directions, if only so as to facilitate maintenance of mutually interesting conversations. The presence of stimulating friends with whom to work and against whom to bounce off ideas often determines the direction of work, and it is demonstrated by the existence of research centers and work teams at a number of colleges and universities, which evolved from the mutual interests of a few individuals.

Environments

Professional environments have an effect similar to that of friends. I never would have written so much on research methods had I not first been housed in a college of education and then in a public school system for so long. In both settings, I constantly had to explain myself and to justify what I was doing. I finally committed my protests to formal writing. Similarly, this article is generated not so much from a mid-life crisis as it is in response to continued requests to clarify the notion of subjectivity in ways that are more convincing than counterattacks to the critics or simple assertions that I am trying to be as honest as possible.

Given environments also are conducive to particular kinds of research. People in colleges of education are encouraged to look at formal instruction, classrooms, and teaching; people in departments of anthropology may find more encouragement to look at cultural communities of learners, such as the work that John Singleton is doing among communities of folkcraft potters in Japan and the American Southwest. Practicing anthropologists may have to write about whichever population is most convenient to their workplace.

Paths of Opportunity

What this means is that the colleagues and environments among and within which one finds oneself have a profound, if unappreciated, impact upon one's intellectual career. Everyone's life thus consists of a sequence of opportunities taken and not taken, and these can be analyzed in the same way as economists examine the opportunity costs and benefits of decisions made in the marketplace. I have used the

metaphor of an archeological dig to understand the impact of opportunity costs in my own intellectual career. My dig was occasioned by a thorough housecleaning of my existing files—necessitated in part by several recent geographical moves and insufficient storage space in each of the places I found myself. This endeavor produced four categories of work ranging from:

1. Fruitful lines of inquiry
2. Consistent lines of interest that have yet to be followed up or brought to fruition
3. Hot new topics that haven't been explored
4. Dead ends and embarrassments

Like the potsherds and bones of an archeological kitchen midden, each of these categories has left its traces, and each of them is accompanied by a history that explains its current status and future destiny. The fruitful lines of inquiry have included my work on research methods and empirical research on socialization processes in classrooms. The embarrassment category includes a proposal to look at the relationship between food preferences and sexual behavior, which I called the "Food and Sex Study," and which somehow never got funded! The continuation or termination of these projects often hinged upon the presence or absence of supportive colleagues, funding agencies or institutions, and the degree of risk I felt able to undertake at the time.

What I have been trying to do in the past few months is to disentangle all of that history and to determine the conscious and unconscious motivations for my work. In that way, I hope not only to build most effectively upon what I have done in the past, but also to avoid being locked into doing over and over again what I already have done. I have, for example, begun to think—albeit with great trepidation—that I may have said all that I have to say, at least at this point in my life, about research methods. That, of course, means that I have to come up with something new, and that is a daunting prospect indeed.

It is at this point that I feel that our field may be—in need of examining what has been done already, what we have said well enough, what we hang onto because of old habits, biases, and resistances, and what might be new and exciting to pursue.

Personal Influences on the Biography

It is not so easy to delineate what influences from one's personal biography influence one's intellectual activities, because critical events obviously will vary from person to person. I think, however, that a number of common denominators need to be examined scrupulously by each scholar. I am reminded of the "Aha" experience I once had when I was trying to figure out why my childhood play experiences had evolved as they had. My mentor of the time reminded me that I not only had been the oldest child, but also was from the only Prot-

estant family in the neighborhood. As such, I was the only public school scholar on the block, and had no age-peer playmates in close proximity. This was plain demography, not psychology, but it certainly explained a lot of both my misery and sources of happiness during elementary school. Consequently, I would like to outline a few of the factors that I think influence intellectual development, and which we use as explanatory variables for the cultures we study, but which we often avoid building into discussions of bias and subjectivity in our own research.

History

Somehow, I always come back to the historical epoch. I remember that Plato became much more intelligible to the working-class freshman I once taught when I told them that *The Republic* was an attempt to define a better way of governing in a time of real political turmoil. As practical politics, it made more sense than it did as esoteric Greek philosophy. Similarly, some of our more arcane interpretations of data, as well as the questions we ask, may indeed be informed simply by the era in which we grew up. I am sure that my interest in social change derives in large part because I am a child of the '60s, a time when there was a great desire on the part of young people to break with the past and build a more humane social order. Similarly, the 1940s and 1950s, the time of intellectual development of most of my professors, were periods when people were desperately searching for a way to recover a stability, however illusory, which was forever ruptured by world war. Hence, the predominance of social theory that viewed conflict not as therapeutic, but as a real aberration in desirable social systems, and which focused instead upon a search for human equilibrium.

Geography and Demography

Feminists and social activists have begun to make scholars acutely aware of the impact of sex, social class, and ethnicity upon perception. As a consequence, the social sciences are being rewritten to include, and are even being written from the perspective of, nonwhites, non-Europeans, and nonmales. I think that this endeavor is salutary. It permits us to give real credence to the view that human experience is truly cross-cultural even within a culture, in that each person's apprehension of reality, however scholarly, is only one slice of that reality, and as such, is by definition incomplete. I would like simply to add a few more variables: age, geographical region, social background, religious training, birth order, family background, and prior cosmopolitan experience. I think, for example, that Californians may, in fact, think differently than Manhattan Islanders do, and that people who grew up in rural areas or distant suburbs think differently from those who grew up in the inner city.

In Conclusion

Where does all this leave us? As a broadly defined field, in good shape. My reading indicates that those we claim as our own cover many bases and contexts for culture and learning, and are moving into new areas in response to emerging cultural realities. However, as individual scholars, I find us to be more ahistorical, narrow, and contentious than the tolerant cultural ethos of our field would permit us to be. As an organization, I find us not inclusive of many whom we claim as fellow travelers, and in danger of excluding many who are not solely academics who would like to join us. We also are in need of examining some neglected topics. I will name only a few.

First, while we have learned to look at nonwhite, nonmale, and non-European ways of knowing and learning, we have tended to ignore nonhuman ways of learning and knowing. Since we are not so distant from being nonhuman ourselves, it is likely that were we to look at how animals think and learn, we would gain useful insight into processes of cultural acquisition used by human children and adults.

Second, I think that a return to the work of our "feral" scholarly ancestors, who looked at learning in a broader cultural context, would be useful. However, even our ancestors thought of learning primarily as a task for children and adolescents. This was perfectly all right when the life span was very short. However, the sociology and demography of contemporary life is different. Despite the fact that we live very long lives in the United States, and despite the fact that the majority of the population in the United States very soon will exceed what used to be the average life span, we still do not know very much about how adults and older people learn. More important, how do people learn to be old?

Third, educational anthropology is an American phenomenon, and since formal, public, universal, secular elementary and secondary schooling is the most visible context for learning in America, it isn't surprising that studies of this context dominate the field. However, there are other contexts for learning, and there are different kinds of learning, both cognitive and cultural. Can we not explore these, especially when our current learning institutions seem so out of step with current cultural realities? I find this increasingly imperative, particularly as I pursue my own research on dropouts, and find that the old incentive to study now so that you can have a better life in the future seems an increasingly pale inducement in a world where nuclear holocaust and genocide is an all-too-real part of everyday reality, and where the "job ceiling" on opportunity described by Ogbu (1978) seems to be generalizing to more and more kinds of young people.

Fourth, individual descriptive studies are useful, and are in fact the building blocks for the comparative analysis and ethnology of schooling that we have been urged to undertake. However, urging does not seem to have been all that effective. We still need new theory and new

definitions in cultural transmission and learning, and too many of the studies carried out in our field seem "merely descriptive" and devoid of a theoretical base.

I would like to close with a challenge: Can we not use an examination of the "bias in biographies," collectively and individually as a basis for the next era in anthropology and education? And can we not make it an examination not just to keep on doing again what we have done well before, but one that lets us consciously decide what the next act in the drama will be—and how it will be written?

Notes

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