ASSESSING CULTURAL ANTHROPOLOGY

EDITED BY

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INTRODUCTION

The word “method” in anthropology, indeed in all of social science, has at least three meanings.

First, method refers to epistemology, to sets of assumptions about how we acquire knowledge. The phrase “scientific method,” for example, encapsulates a set of such assumptions (as well as some rules of practice). The assumptions are: (1) there is a reality “out there” (or “in there” in the case of ideas and emotions); (2) it can be apprehended, more or less, by human beings through direct experience (or through some proxy for direct experience); (3) all natural phenomena can be explained without recourse to mysterious forces beyond investigation; and (4) though the truth about phenomena is never known, we do better and better as old explanations are knocked down and are replaced by better ones. Competing epistemologies reject one or more of these assumptions.

Second, method refers to strategic approaches to the accumulation of actual data. Experimentalism and naturalism are two strategic approaches within the scientific method. Experimentalism involves the direct manipulation of variables under the most controlled conditions possible. Naturalism involves observing phenomena in their natural environment. Sea-going oceanographers, astronomers, wildlife biologists, and anthropologists are all naturalists. Participant observation is a strategic approach to data gathering used by some naturalists in the social sciences.

And third, method refers to techniques or sets of techniques for collecting and analyzing data. The survey questionnaire method is a set of techniques (involving sampling, construction of instruments, interviewing, and other things) for collecting data. Spot observation is another set of techniques for gathering data (for example, about how much time people spend in various activities). Content analysis, componental analysis, multidimensional scaling, and ethnographic decision tree modeling are techniques for analyzing (extracting meaning from) data.

Anthropologists receive little formal training in any of these three kinds of research methods. Many graduate programs in anthropology offer no required course in ethnographic methods (Trotter 1988; Plattner 1989). I believe that training in all three kinds of methods—epistemology, strategy, and technique—should be a major part of both the undergraduate and graduate curriculum in anthropology.

How much training in methods is enough? I’ll address this question at the end of the
chapter. First, I discuss method as epistemology and method as strategy.

**METHOD AS EPISTEMOLOGY**

**Positivism**

One might draw a broad distinction between two epistemologies in cultural anthropology—positivist and interpretivist. I will focus on the former here and leave others (in this volume) to describe the latter for, in my view, the positivist perspective has not always been understood. Positivists and interpretivists may disagree on matters of epistemology. But when we talk about methods at the level of strategy and technique, methods belong to all of us.

At its birth, positivism was simple enough. Here is John Stuart Mill in 1866 explaining "le positivisme" to an English-speaking audience.

> Whoever regards all events as parts of a constant order, each one being the invariable consequent of some antecedent condition, or combination of conditions, accepts fully the Positive mode of thought... (15)

> All theories in which the ultimate standard of institutions and rules of actions is the happiness of mankind, and observation and experience the guides... are entitled to the name Positive. (69)

Positivism in the mid-nineteenth century was science applied to the study of humanity, and done in the exuberant and optimistic spirit of service to human happiness.

The French social philosopher Auguste Comte didn’t just believe in science as an effective means for seeking instrumental knowledge, though. He envisioned a class of philosophers who, with support from the state, would direct all education. They would advise the government, which would be composed of capitalists “whose dignity and authority,” explained Mill, “are to be in the ratio of the degree of generality of their conceptions and operations—bankers at the summit, merchants next, then manufacturers, and agriculturalists at the bottom” (1866:122). Comte attracted admirers who wanted to implement the master’s plans. Mercifully, they are gone, but the word “positivist” still carries the taint of Comte’s ego.

**Logical Positivism**

The modern version of positivism was developed by members of the Vienna Circle, a group of philosophers, mathematicians, physicists, and social scientists who met from 1923 to 1936 in a series of seminars. The members of the circle were committed to empiricism and against metaphysics. Hunches could come from anywhere, but scientific knowledge, they said, is based only on experience, and scientific explanation is based only on logical, mathematical principles. Hence the label “logical empiricists,” by which they were known for a while.

For these positivists, logic was paramount. They understood that the assumptions of science are assertions. They also understood that the assumptions of science are not mere assumptions. The members of the Vienna Circle were committed to a scientific, that is logical, mode of thinking and to the benefits that this would bring to humanity. This was the component of positivism that they shared with Comte. It’s what motivates many social scientists today, including me, to count ourselves as positivists.

It is clear, of course, just how dangerous this commitment can be. My ideas about what is good may not be the same as yours. This is what makes the study of ethics a critical part of method as epistemology.

**Numbers and Science**

In the early decades of this century, many luminaries of cultural anthropology supported the collection of both quantitative and qualita-
tive data. They collected texts and counted things as necessary in their research.

For example, one of Tylor’s enduring contributions to anthropology was his paper “On a Method of Investigating the Development of Institutions” (1889). In that paper, Tylor described a numerical method for doing systematic cross-cultural comparisons. One of Boas’s great contributions was a monograph in which he demonstrated the relationship of nutrition to body size among immigrants, rendering useless much racist rhetoric of the day against allowing Eastern Europeans into the United States. Kroeber’s study of 300 years of women’s fashions is a landmark in the quantitative study of cultural trace materials (Richardson and Kroeber 1940).

When I was a graduate student at the University of Illinois thirty years ago, the tradition of using both qualitative and quantitative data was still strong. Oscar Lewis, for example, advocated the use of survey methods to complement ethnographic field studies. Joseph Casagrande taught cross-cultural hypothesis testing using the Human Relations Area Files. Both, of course, were ethnographers who had collected reams of qualitative data.

There was, to be sure, another tradition in anthropology, one opposed to the use of quantitative data. The cause against quantification in anthropology was eloquently taken up by one of Boas’s students, Paul Radin, in 1933. In The Method and Theory of Ethnology, Radin praised his teacher for criticizing Tylor’s and Frazer’s evolutionary doctrines. But he accused Boas of being “naturwissenschaftlich eingestellt” or science-minded (Radin 1933:10), of believing that cultural facts could be transformed into physical ones and counted. He railed at Boas for training a generation of students like Kroeber, Sapir, Lowie, Wissler, and Mead who, Radin said, sought to tag cultures and habitats, like animal and plant species, and compare them statistically (Radin 1933:10).

Radin was right about that crowd, but he had a different idea of what anthropology should be about. An ethnographer had to live a long time with the people whom he or she studied, and had to learn the native language fluently. Above all, said Radin, the ethnographer must provide readers with the original texts, the materials from which he or she makes observations.

Those texts, and the exegesis of the ethnologist, should show current cultural realities “as seen through the mirror of an actual man’s heart and brain and not through the artificial heart and brain of the marionettes with which Boas and Sapir and Kroeber operate” (Radin 1933:10). Radin didn’t mince any words. To demonstrate his method, Radin presented a meticulous analysis of a series of texts from three Winnebagos about the Peyote Cult. In one of those texts, John Rave discussed his conversion to the Peyote Cult. Radin drew on his knowledge of local history. He showed that Rave’s account reflected acceptance by Rave of particular components of Christian faith and particular components of earlier Winnebago beliefs. Rave failed to achieve a vision during his Winnebago puberty rite, and was denied membership in the Medicine Dance. This, said Radin, accounted for Rave’s proselytizing zeal.

You can object to all this as just so much pop-psych, but Radin was the consummate fieldworker: he knew the language, he spent years working with the Winnebagos and with these particular informants, and he had the texts to back up everything he said. If Kroeber and Boas and Sapir didn’t like his arguments, Radin said, “there still remains the document for them to interpret better and more profoundly” (1933:238).

Radin would not have wanted to be remembered as a scientist, but in my terms he was—and a gifted one, at that. He was empirical in the extreme; he built arguments only from observations; he was committed to the open-endedness of truth. With all his texts on the Peyote Cult, he had the data on which to postulate a theory of revitalization. That he left this nomothetic exercise to others who
would come later diminishes not one whit the contribution of Radin’s effort.

Many anthropologists today who share Radin’s views about primary data also want to contribute to the development of knowledge about regularities in human behavior. They may have some well-founded doubts about whether all the phenomena in which they are interested can be quantified accurately. But I believe that what keeps many cultural anthropologists (I haven’t counted them, but I’m guessing hundreds, not dozens) from conducting quantitative research is that they are just unequipped to do it when they leave graduate school.

METHOD AS STRATEGY:
PARTICIPANT OBSERVATION

Anthropologists are divided on epistemological issues, but almost all of us use the strategic method of participant observation to collect our primary data. Even anthropologists who use questionnaires in their research engage first in participant observation in order to build the questionnaire instrument.

Participant observation is what makes it possible for interpretivists and positivists alike to collect life-history documents, attend sacred festivals, talk to people about sensitive topics, map the landholdings of informants, trek with a hunter to count the kill, and interview women traders formally and informally about how they cover their losses in the daily market. Among other things, participant observation helps us build rapport. Rapport is what makes it possible for us to observe and talk to people and record information about their lives.4

Improving Participant Observation

Anthropologists developed the method of participant observation, and we have seen with satisfaction its acceptance by researchers in many other disciplines. But we haven’t followed up. After decades of experience with the method of participant observation, we are not much further along than when we started. We haven’t studied participant observation systematically; we haven’t improved it.

At least five things affect the kind and quality of data we can collect as participant observers: (1) personal characteristics, such as age and gender; (2) language fluency; (3) objectivity; (4) informant accuracy; and (5) informant representativeness.

Personal Characteristics. By the 1930s, Margaret Mead had already made clear the importance of gender as a variable in data collection. Gender has at least two consequences: it influences how you perceive others; it limits your access to certain information.

In all cultures, you can’t ask people certain questions because you’re a [woman][man]. You can’t go into certain areas and situations because you’re a [woman][man]. You can’t watch this or report on that because you’re a [woman][man]. Even the culture of anthropologists is affected: your credibility is diminished or enhanced with your colleagues when you talk about a certain subject because you’re a [woman][man]. (See Golde 1970; Whitehead and Conaway 1986; Scheper-Hughes 1983b; Altorki and El-Solh 1988.)

When she worked at the Thule relocation camp for Japanese-Americans during World War II, Rosalie Wax did not join any of the women’s groups or organizations. Looking back after more than forty years, Wax concluded that this was just poor judgment.

I was a university student and a researcher. I was not yet ready to accept myself as a total person, and this limited my perspective and my understanding. Those of us who instruct future field workers should encourage them to understand and value their full range of being, because only then can they cope intelligently with the range of experience they will encounter in the field. (1986:148)

Besides gender, we have also learned that being old lets you into certain things and shuts
you out of others. Being a parent helps you talk to people about certain areas of life and get more information than if you were not a parent. Being wealthy lets you talk to certain people about certain subjects and makes others avoid you. Gregarious anthropologists may be unable to talk to shy people.

Being divorced has its costs. Nancie González found that being a divorced mother of two young sons in the Dominican Republic was just too much. “Had I to do it again,” she says, “I would invent widowhood with appropriate rings and photographs” (1986:92). Even height may make a difference: Alan Jacobs once told me he thought he did better fieldwork with the Maasai because he’s almost 6 1/2 feet tall than he would have if he’d been, say, an average-sized 5 feet 10 inches.

**Language Fluency.** Thirty years ago, Raoul Naroll (1962:89–90) found suggestive statistical evidence that anthropologists who speak the local language are more likely to report witchcraft than those who don’t. His interpretation was that local language fluency improves your rapport, and this, in turn increases the probability that people will tell you about witchcraft.

Does the credibility of our data depend on control of the local language? In his 1933 diatribe against his science-minded peers, Radin complained that Mead’s work on Samoa was superficial because she wasn’t fluent in Samoan (1933:179). Fifty years later, Derek Freeman (1983) raised questions about whether Mead had been duped by her informants, perhaps because she didn’t know the local language well enough.\(^5\)

According to Brislin, Lonner, and Thorn-dike (1973:70) Samoa is one of those cultures where “it is considered acceptable to deceive and to ‘put on’ outsiders. Interviewers are likely to hear ridiculous answers, not given in a spirit of hostility but rather sport.” Brislin et al. call this the “sucker bias,” and warn field-workers to watch out for it. Presumably, knowing the local language fluently is one way to become alert to and avoid this problem.

If participant observers run the risk of being suckerized, then research is called for to determine (1) the conditions under which that’s most/least likely to happen and (2) how to recognize and avoid it. Research is called for on the kinds of data available to “indigenous anthropologists” that are not available to outsiders, and vice versa (see Messerschmidt 1981 for papers on doing fieldwork in your own culture). Research is called for on making participant observation the best strategic method it can be.

**Objectivity.** No one I’ve ever known in the social sciences has seriously thought that humans could become robotic, completely objective field researchers. But just because perfect objectivity is impossible, this does not let us off the hook. The economist Robert Sarlow is reported to have observed that, while a perfectly aseptic environment is impossible, this doesn’t mean we might as well conduct surgery in a sewer (cited by Geertz 1973:30). Objectivity clearly varies from person to person. Some people achieve more of it, others less. More is better.

Objectivity means becoming aware of one’s biases, and transcending them, not the lack of any biases. Some people do better at transcending, some do worse. Striving for objectivity is important even if perfect objectivity is unobtainable.

Laurie Krieger, an American woman doing fieldwork in Cairo, studied physical punishment against women. She learned that wife beatings were less violent than she had imagined, and that the act still sickened her. Her reaction brought out a lot of information from women who were recent recipients of their husbands’ wrath. “I found out,” she says, “that the biased outlook of an American woman and a trained anthropologist was not always disadvantageous, as long as I was aware of and able to control the expression of my biases” (1986:120).
The need for objectivity is recognized by even the most qualitative fieldworkers. Colin Turnbull says that the key to good fieldwork is "to know ourselves more deeply by conscious subjectivity" because in this way "the ultimate goal of objectivity is much more likely to be reached and our understanding of other cultures that much more profound" (1986:27).

Objectivity does not mean (and has never meant) value neutrality. No one asks Cultural Survival, Inc., to be neutral in documenting the violent obscenities against indigenous peoples of the world. No one asks Amnesty International to be neutral in its effort to document state-sanctioned torture. We recognize that the power of the documentation is in its objectivity, in its irrefutability, not in its neutrality.

Informant Accuracy. In 1940, C. Wright Mills wrote that "the central methodological problem of the social sciences" is the fact that what people say and what they do is so different (1940:329). He was right, and the problem remains central today.

Informants lie, of course. But lying appears to be a small part of the problem. Informants, like all of us, make honest errors of commission (they say they did things that they didn’t do) and of omission (they neglect to report things that they did do) in reporting their own behavior.

The problem was articulated clearly by Richard La Pierre in 1934. Accompanied by a young Chinese couple, he crisscrossed the United States by car. They stayed at and ate at 66 hotels and 184 restaurants. Six months after the trip was over, La Pierre wrote a letter to all those hotels and restaurants, telling the managers that he was planning a trip and would they mind serving Chinese people? More than 90 percent said they would not serve Chinese people. Irwin Deutscher picked up the theme in 1972, in his book What We Say, What We Do. He made it clear that the problem had not been solved, despite the fact that scholars from many disciplines had read La Pierre’s study and had recognized its importance.

My colleagues and I conducted a series of studies during the 1970s to find out if people could accurately report their social interactions. A lot of social science, after all, is based on data collected by asking people “who did you [talk to] [exchange memos with] [interact with] [call on the telephone] in the last [24 hours] [week] [month]?” In 1984, my colleagues and I reviewed the literature on informant accuracy (see Bernard and Killworth 1977, for example). A long list of studies, including our own, showed that a fourth to a half of what informants say about their behavior is inaccurate.

This finding shows up in studies of what people say they eat, how often they claim to have gone to the doctor in a given period of time, and in how much money they say they have in their savings accounts. It shows up in the most unlikely (we would have thought) places: in the 1961 census of Addis Ababa, 23 percent of the women underreported the number of their children. Apparently, people don’t “count” babies that die before reaching the age of two (Pausewang 1973:65).

Progress is being made in explaining informant inaccuracy. Thirty years ago, Cancian (1963) showed how informant errors conformed to expected patterns in prestige rankings in a Mexican village. D’Andrade showed in 1974 that there is a general pressure to think in terms of “what goes with what” even if this creates errors in reporting factual events. (Also see Shweder and D’Andrade 1980.) More recently, Freeman, Romney, and Freeman (1987) found that, for some behaviors at least, informants report typical behavior to anthropologists rather than specific behavior when asked to dredge up what they actually did and who they actually interacted with (see also Freeman and Romney 1987; McNabb 1990).

People round off, in other words, and report behavior according to rules of central ten-
dency. This may go for informants’ reports of their own behavior as well as for their reports of the behavior of others. (Cognitive psychologists, of course, have done careful investigations of how people store and retrieve information.)

Still, the question remains: When people report their behavior to us, do we get inaccurate answers, or answers that are accurate proxies for some other question about norms?

**Informant Representativeness.** Even if informants tell us accurately what they know, there remains the question of whether what they know is representative of the population we are studying.

Since the 1930s, cultural anthropology and sociology have gone their separate ways largely over this issue. Sociologists have focused on developing systematic instruments (like questionnaires) and applying those questionnaires to representative samples of respondents in a population. This places emphasis on reliability (whatever the instrument measures today it will measure tomorrow, more or less) and on external validity (whatever we find out from the sample can be extended to the rest of the population, within some known limits of error).

Anthropologists have focused on internal validity. What good is it to know that data are reliable or generalizable if they are inaccurate? Every social scientist knows horror stories of questionnaires that force people to produce nonsensical answers (because the questions are culturally inappropriate, for example).

On the other hand, why would we want to know what a handful of informants think or do, if the information cannot be generalized beyond those informants? This is one area where considerable progress has been made in anthropology. Jeffrey Johnson’s recent book (1990) on selecting ethnographic informants summarizes much useful information in this area. Romney, Weller, and Batchelder (1986) offer a technique called consensus modeling for selecting informants who are experts in particular cultural domains (like ethnozoology, local medical practice, or whatever). Romney et al. demonstrate that, when assumptions of their model are met, just six informants may be enough to achieve representative, valid information about a cultural domain. It is an intriguing line of research.

**METHODS TRAINING IN ANTHROPOLOGY**

I believe that training in methods should be a key part of both the undergraduate and the graduate curriculum in anthropology. How much is enough? Well, given the three meanings of the word “method” (epistemology, strategy, and technique), it takes a lot to be enough.

**Epistemology**

All anthropologists need a thorough grounding in the various approaches to knowledge that have characterized our discipline. This means exposure of all students, whatever their initial predilections, to the philosophical foundations of structuralism, symbolism, interpretivism, hermeneutics, phenomenology, positivism, and empiricism.

Training in ethics should be part of the training in epistemology. Plattner (1989:33) makes the case:

*Training in the ethics of research is as important as training in the techniques of research design, data collection and data analysis. Research consists of a series of decisions (where to study, what to study, who to interview, what questions to ask, when to leave the field), and every decision . . . has an ethical component.*

Every anthropologist has stories to tell about how his or her ethics were tested by circumstances in the field. We have accumulated a large collection of these cases, in the Newsletter
of the American Anthropological Association, for example, and in Cassell and Jacobs (1987). We ought systematically to use the wisdom they provide and not rely on the telling of stories informally to disseminate that wisdom. The case-study method is used in teaching law and business administration. A case-study course on ethics should be part of every graduate curriculum in anthropology.

Strategy

All anthropologists need formal training in participant observation. This means familiarity with the accumulating literature on the subject. It means studying what we already know about how good and bad participant observation is done—about culture shock, about gender roles in the field, about violence, disease, and other hazards of field research, about maintaining objectivity without the pretense of value neutrality.

This kind of training will give anthropologists the skills they need to do better fieldwork. It will also give them the means to contribute concretely to improving participant observation.

Technique

This is where it gets tough. College graduates in the social sciences these days simply must know how to use a computer—not just for word processing but for data entry, data management, data retrieval, and data analysis. Students of anthropology who manage to escape getting these skills before going to graduate school will need compensatory education before they can study formal data collection and analysis.

Once the basics are out of the way, anthropologists need training in research design, in data collection (structured and unstructured interviewing, for example), and in data analysis. Students who hope to do any serious work with survey data, for example, need at least two courses in applied statistics (through multivariate analysis). Students who want to work with a large corpus of field notes, or with life history material, need training in computer-based text management.

All anthropologists need formal training in the collection and analysis of both qualitative and quantitative data. Photography, videography, audio taping, stenography, and sketching are methods for collecting qualitative data. Coding, counting, and measuring produce quantitative data. People's responses to open-ended questionnaire items are qualitative. Coding and attaching numbers to the responses create quantitative data. Observing people and recording their behavior in words produce qualitative data. Counting the behaviors creates quantitative data. Transcribed texts are qualitative data. Counting the number of times people use a particular word or theme in a text creates quantitative data.

Pawing through field notes is qualitative data processing. Putting the notes on a computer and using a text management program to paw through them is still data processing. Doing a chi-square test with a hand-held calculator is quantitative data processing. So is putting thousands of numbers into a computer and having the machine calculate the chi-square value.

Much of what is called statistical analysis is, in my vocabulary, data processing, not data analysis. Thinking about the themes in a text, like field notes, is data analysis. Thinking about what a chi-square (or any statistical) value means is also data analysis. All analysis is ultimately qualitative. But we can't even get to that stage unless we've collected solid (valid, credible) data.

Besides learning to use research methods, we should be improving them. Every field trip provides information on how personal characteristics and language fluency affect data collection. Every field trip is an opportunity to test the relationship between what people say and what they do. We need to monitor and publish the results of all these naturally occurring experiments.
CONCLUSION

In the past, cultural anthropologists were more concerned with description than with explanation and prediction. A good description is still its own analysis; being right about what causes a problem is still the best contribution anyone can make to solving it; and good writing—telling a good story—is still the best method any anthropologist can acquire.

Many anthropologists today, however, are interested in research questions that demand explanation and prediction, questions like: Why are women in nearly all industrial societies, socialist and capitalist alike, paid less than men for the same work? Why is medical care so hard to get in some societies that produce plenty of it? Does cultural pluralism support or undermine the stability of states (as in Canada, Belgium, India, Azerbaijan, Yugoslavia, or Kenya, for example)?

Answering such questions demands greater sophistication in research methods than is now customary in anthropology. It requires skills in comparative statistical research and in gathering data from published sources including data that are only available on computer tapes. Anthropologists who are involved in multidisciplinary team research on complex human problems—in agricultural development, fertility control, health care delivery, and education—need more methods training than has been the norm to date.

Knowing about methods for collecting qualitative and quantitative data is more difficult in some ways today than it was forty years ago. There are simply more methods now than there were then. In some ways, though, learning research techniques is easier now. Microcomputers and efficient software make text management, statistics, and cognitive domain analysis less intimidating. No one can control all research methods. But those who control a certain fraction will be unintimidated about learning new methods that become available and that seem appropriate to particular research projects.

As I said at the beginning of this essay, anthropologists may disagree, even vehemently, about method as epistemology. But they share a common interest in and concern for method at the level of strategy and technique. Controlling many different methods for collecting and analyzing qualitative and quantitative data liberates us to investigate whatever theoretical questions attract us. And conversely: limited training in methods limits our ability to think about and solve many important theoretical questions. Knowing more is better than knowing less.

NOTES

1. Mill felt that the French word positivisme was not suited to English, but decided in the end to use Comte’s word, properly anglicized to refer to Comte’s concept. I’ve long wished Mill had decided otherwise.

2. See Lewis’s contribution to the 1953 volume Anthropology Today, edited by Alfred Kroeber. See also Lewis’s article on family studies in the 1950 volume of the American Journal of Sociology. In those days, family studies was still the province of sociologists. When Lewis wrote for anthropologists, he stressed the need for survey research as a complement to ethnography. When he wrote for sociologists, he stressed the need for ethnographic research as a complement to survey data.

3. I was inspired as a graduate by Radin’s work on the production and presentation of native texts. Radin followed Boas’s lead in native ethnography of indigenous North American peoples. Boas trained George Hunt, a Kwakiutl, to write ethnographic descriptions of Kwakiutl culture. Hunt produced over 5,000 pages of notes that served as the basis for much of Boas’s published work on Kwakiutl life. Radin trained Crashing Thunder to write in Winnebago, and Crashing Thunder wrote his classic autobiography of the same name.

Native ethnography has long held great promise as a means to develop an authentic emic data base about Indian and other indigenous cultures. I can only surmise that the physical difficulties involved must have kept native ethnography from becoming a major method in cultural anthropology.

In recent years, I have found that microcomputers make native ethnography much easier to do. I simply program a word processor to handle whatever special characters are needed in a particular language, teach
native speakers of previously nonwritten languages to use the equipment, and hand the equipment over to them.

For many years, my partner in this effort has been Jesús Salinas Pedraza, a Nahfiu (Otomí) Indian from the Mezquital Valley of Mexico. Salinas has written an extensive ethnography of the Nahfiu, which I translated and annotated (Bernard and Salinas 1989).

Since 1987, Salinas and Josefa González Ventura, a Nuu Savi (Mixtec) Indian from Oaxaca, Mexico, have run the Native Literacy Center at the Centro de Investigaciones y Estudios Superiores en Antropología Social in Oaxaca City. They have now trained 75 other Indians from Mexico, Peru, Bolivia, Argentina, Ecuador, and Chile to use microcomputers to write books in their respective native languages.

There are many social and ethical issues raised by this effort. I deal with these, and the history of the project, in Bernard 1992.

4. In this sense, participant observation, like everyday life, is also an exercise in impression management (see Berreman 1962:11).

5. Was Mead duped? Whether she was or not has nothing to do with her overwhelming contributions to anthropology. All ethnographers run the same risk.

6. How much, for example, can we rely on informants to tell us about their income? The size of their landholdings? Where their grown children are? Data on the nature and causes of informant inaccuracy would help illuminate the relation between cognition and behavior, between the internal and the external worlds of human beings around the world.

7. Methods do come and go, however. No one uses so-called “culture free” tests anymore to investigate cognition cross-culturally. Those tests were discredited and abandoned by most researchers thirty years ago (Brislin, Lonner, and Thordike 1973:109).

8. For handling relatively small amounts of text material (up to say, a thousand pages of field notes, transcribed interviews, etc.) GoFer is an excellent product. Write to Microlytics, Inc., Two Tobey Village Office Park, Pittsford, NY 14534. For handling large amounts of text material, ZylINDEX is highly recommended. Write to ZylAB, Information Dimensions, Inc., 100 Lexington Drive, Buffalo Grove, IL 60089. For coding and analyzing text, consider the following: (1) The Text Handler, by Gery Ryan. This package works with WordPerfect. Write to Gery Ryan, Dept. of Anthropology, University of Florida, Gainesville, FL 32611. (2) TALLY 3.0, by Jeffrey W. Bowyer. This program works with ASCII files. Write to Wm. C Brown Publishers, 2460 Kerper Boulevard, Dubuque, IA 52001. (3) DtSEARCH, by David Thede. DtSEARCH works with all of the top word processors, as well as with ASCII files. Write to DT Software, Inc., 2101 Crystal Plaza Arcade, Suite 231 Arlington, VA 22202. (4) THE ETHNOGRPH, by John Seidel. This program works with text from major word processors. Write to Qualis Research Associates, P.O. Box 2240, Corvalis, OR 97339. (5) ANTHROPAC, by Stephen Borgatti. This program helps researchers collect and analyze numerical data. It is particularly useful for multidimensional scaling, hierarchical clustering, consensus modeling and other methods that operate on similarity matrices. Write to Analytic Technologies, 306 South Walker Street, Columbia, SC 29205.

All of the programs mentioned here have been reviewed in Cultural Anthropology Methods from 1989 to 1993.

**INTELLECTUAL ROOTS**

H. Russell Bernard is Professor of Anthropology at the University of Florida. He has done field research in Greece, Mexico, and the United States and on ships at sea. Bernard was editor of Human Organization (1976-1981) and of the American Anthropologist (1981-1989). Recently Bernard has been working with Jesús Salinas and other Indian colleagues in Oaxaca, Mexico, to establish a center where native peoples can publish books in their own languages. Bernard's best-known contributions are Research Methods in Cultural Anthropology (1988); Native Ethnography (with Jesús Salinas Pedraza, 1989); Technology and Social Change (edited with Pertti Pelto, 2nd edition, 1987); and a series of articles on social network analysis (with Peter Killworth and others).

In the summer of 1959, as a junior at Queens College, I went to Mexico to study Spanish and came back knowing that I wanted to be an anthropologist. As an undergraduate, I studied with Ernestine Friedl, Hortense Powdertaker, and Mariam Slater. Late in my senior year, Powdertaker told me about a new Ph.D. program just opening at the
University of Illinois. Perhaps I could get in there, she said.

Illinois in 1961 was an intense, intellectual environment. I studied with Kenneth Hale and Duane Metzger for my M.A. in anthropological linguistics, and then with Edward Bruner, Oscar Lewis, Julian Steward, Dimitri Shimkin, and Joseph Casagrande for the Ph.D.

Metzger was part of the (then) new ethnoscience camp. The goal was to write the grammar of a culture—to learn what a native speaker of a language knows about, say, ordering a drink and to lay that knowledge out clearly.

Making cultural grammars turned out to be harder than anyone imagined. Metzger offered a hands-on seminar. With a few other students, I spent a semester working with one Japanese housewife, learning and mapping the implicit rules she used for deciding how to cut and arrange vegetables on a plate.

It was an enormous effort just to keep track of the data. One of the other students got the computer to sort and print the whole corpus every time we learned a new rule. The people over at the computer center thought this was pretty quaint, but this systematic approach to data gathering and the idea of using computers to make light work of complex data-management tasks have stayed with me ever since.

Ken Hale was Carl Voegelin’s student. Like Carl (and like Boas and his early students before him), Ken worked closely and collaboratively with Indian colleagues. The model was to help Indian colleagues produce their own texts, in their own languages, and then to use the texts for linguistic analysis and for cultural exegesis. Ken’s example, and the tradition it represented, led to my lifelong collaboration with Jesus Salinas, a Náhñu Indian from the Mezquital Valley in Mexico.

Jesus was my informant in 1962 when I did the research for my M.A. thesis on the tone patterns of Otomí (called Náhñu in those days). In 1971, I became Jesus’s informant, teaching him to write in Náhñu and, in the 1980s, to use a Náhñu word processor. I’m still working with Jesus, who now heads the Native Literacy Center in Oaxaca, where Indians from around Latin America train in using computers to write and print books in their own languages.

Much of my career, then, was shaped by my work at the M.A. level. I learned from that experience how important it is for students to become involved in research projects early and often.

During my Ph.D. studies, Julian Steward, Dimitri Shimkin, Joe Casagrande, and Kris Lehman encouraged me to pursue my interests in quantitative data analysis. In Casagrande’s seminar on cross-cultural research, I first learned to use the Human Relations Area Files and to test hypotheses using cultures as units of analysis.

I don’t recall anyone labeling all this “positivism” in those days, or worrying about whether my interest in scientific, quantitative research was unhealthy. I read works by Tylor, Boas, Kroeber, Driver, Wissler, Murdock, and Roberts and noticed that all of them did quantitative work and published reams of ethnographic work as well. I found this mix of qualitative and quantitative methods to be very sensible.

My major doctoral professor was Ed Bruner. Ed became identified with symbolic anthropology and I went in a different direction. But Ed taught me to write, and to understand that seeking knowledge was only half the battle. You have to be able to tell others what you have learned, to engage their attention, and to keep them from closing the book before you have finished your argument. This may be one of the few things that positivists and interpretivists fully agree on; but for my money, it’s the most important thing of all.

In 1972, I spent a year at the Scripps Institution of Oceanography, where I met Peter Killworth, an ocean physicist. We decided to study problems together that (1) neither of us could tackle alone, (2) both of use agreed were sheer fun, and (3) were not in the mainstream of research in either of our disciplines. We also agreed that we would not let our joint projects get in the way of our separate research careers. (He is in ocean modeling.)

We did, in fact, have a great time doing a series of papers on informant accuracy, and we are having just as much fun now testing a network model for estimating the size of populations that you cannot count (like the number of rape victims in a city). Peter has taught me a lot about data analysis.

I have also benefited greatly from my association with Pertti Pelto. We began the National Science Foundation Summer Institute on Research Methods in Cultural Anthropology in 1987. Stephen Borgatti joined the teaching team of the summer institute in 1988, and I have learned a lot from him about new analytic methods.
My intellectual biography is still being written. I can look back and see the influences of my professors clearly. But just as clearly, I see the influence of contemporaries, of junior colleagues, and of students. This is what makes anthropology so exciting for me. The learning never has to slow down.