# HANDBOOK OF METHODS IN CULTURAL ANTHROPOLOGY

H. Russell Bernard E D I T O R



### H. Russell Bernard editor



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#### **Preface**

Carole Bernard and I lived in Cologne, Germany, for a year in 1994–1995. Thomas Schweizer, director of the Institute for Ethnology at the University of Cologne, had invited me to be an Alexander von Humboldt research scholar there. It was the sort of opportunity scholars dream about: the chance to do nothing but read, write, and reflect, and to do all this while interacting with graduate students and colleagues. No classes. No committee work. Paradise.

This handbook is partly a product of that year. I have been interested in social research methods for as long as I can remember, so I took the opportunity in Cologne to read and read, and then read some more. My goal was to become better grounded in the range of methods used by scholars across the social sciences and to understand the role that anthropologists had played in the development of social research methods.

The idea for this handbook emerged in conversations about all this by e-mail, with Mitch Allen, editor of AltaMira Press. It seemed like a good time to take stock. The last handbook, edited by Raoul Naroll and Ronald Cohen, had been published in 1970. The content of the discipline of cultural anthropology and the demography of the profession had gone through big changes since then. In 1970, most anthropologists went into academic jobs. Today, most are in nonacademic jobs. Fewer graduate students do fieldwork in small, isolated communities now. They couldn't, even if they wanted to, for such communities are an endangered social species. In 1972, women received just 32% of the Ph.D. degrees in anthropology in the U.S. In 1995, women received 59% of the doctorates.

There was also the resurgence of the great epistemology debate that has so long pervaded the social sciences. Each side claims support from an indisputable observation: On the one hand, people construct their own realities, and the process

is dynamic, ever-changing; on the other, there are regularities in human behavior and human thought. While rhetorical energy is spent arguing that (a) the first fact renders impossible the pursuit of the second or that (b) the second fact renders irrelevant our worrying about the first, working scholars of all persuasions are out there doing empirical research. The core of the discipline, it seemed to me, was in the fact that nearly all cultural anthropologists choose from the same awesomely large kit of tools.

My goal, then, from the beginning has been to put together a handbook that would be useful to academic anthropologists and practicing anthropologists; to interpretivists and positivists; to idealists and materialists.

No project of this magnitude can be managed alone. Six colleagues graciously agreed to join this project and serve as a board of editors: Carol Ember (HRAF), Michael Herzfeld (Harvard), Jane Hill (Arizona), Roy ("Skip") Rappaport (Michigan; deceased), Nancy Scheper-Hughes (UC-Berkeley), and Thomas Schweizer (Cologne). When I thought about senior people whose work was respected by colleagues across the field, Rappaport's name came immediately to mind. Tragically, he didn't live to see the end of the project.

Right from the beginning, the members of the editorial board contributed ideas about chapters that needed to be included in the handbook and about who might write those chapters. They read the chapters and offered critical advice and support. Three of them (Ember, Herzfeld, and Schweizer) contributed chapters themselves. I am grateful to all.

Over the years, I have come to expect nothing less than the best of editorial guidance from Mitch Allen. He never disappoints, never holds back, never pulls punches. I am also grateful to the following colleagues (in alphabetical order) who read various chapters of the handbook in draft and provided detailed reviews: Devon Brewer, Karen Brodkin, Edward Bruner, Douglas Caulkins, Garry Chick, Victor De Munck, William Dressler, Darna Dufour, Robert V. Kemper, David Kertzer, Maxine Margolis, and Alvin Wolfe.

Special thanks go to Ronald Cohen, my colleague at the University of Florida. His was a pioneering effort in 1970 when he and Naroll put together that first handbook of methods in cultural anthropology.

My thanks also go to the Alexander von Humboldt Stiftung, Bonn, and to the College of Liberal Arts and Sciences at the University of Florida for support during 1994–95.

I know that I cannot thank sufficiently my partner, Carole Bernard, for her support all along the way and specifically for her work copyediting and producing the final product. But I can try.

H. Russell Bernard Gainesville, FL July 20, 1998



## Introduction On Method and Methods in Anthropology

This introduction has two parts. In Part 1, I offer some remarks about the history and scope of methods in cultural anthropology. Later, in Part 2, I describe in some detail what is in the various chapters.

#### Part 1: On Methods in Anthropology

Method is about choice—the choice of taking a *verstehen* or a positivist approach; the choice of collecting data by participant observation or in the archives, by direct observation or by interviewing; the choice of making quantitative measurements or collecting oral, written, or visual text. The authors in this handbook deal with all these choices, and more.

I will have a lot more to say later about the content of the chapters in this handbook. In this first part of the introduction, though, I want to make clear why this book is important—for all social scientists, not just for anthropologists. Conventional wisdom notwithstanding, anthropology has always been about methods, from the earliest days of the discipline right up to the present. Anthropologists have been prodigious consumers and adapters of research methods, and they have made important contributions to the big social science toolkit as well. I am going to document this and put it in perspective here.

There has always been a certain tension between those who would make anthropology a quantitative science and those whose goal it is to produce documents

that convey the richness—indeed, the uniqueness—of human thought and experience. Eric Wolf captured this in his wonderful aphorism that "Anthropology is the most humanistic of the sciences and the most scientific of the humanities" (1964:88). Students of cultural anthropology may be asked early in their training to take a stand for interpretivism or positivism, humanism or science, qualitative or quantitative research.

Readers of this handbook will find no support for this polarized vision of method. Instead, they will find scholars laying out the methods they use in practicing their craft—a craft rooted, for every author in this book, in one of the most essentially empirical traditions in all of science: participant observation fieldwork. Some authors are identified with interpretivist methods, some with quantitative methods for the collection and analysis of data, but none dismiss humanism or science and none ask their readers to choose once and for all between expressing their findings in words or in numbers.

#### Romancing the Methods

John Whiting and some of his fellow graduate students at Yale during the 1930s asked about having a seminar on methods. "Leslie Spier informed us disdainfully," recalls Whiting, "that this was a subject to discuss casually at breakfast and was not worthy subject matter for a seminar" (Whiting 1982:156). Try quoting Whiting at a convention of anthropologists. Chances are, you will discover that everyone chimes in with a favorite story of the same ilk.

It's all well and good for anthropologists to romanticize fieldwork—vulcanologists do it, too—particularly for fieldwork in places that take several days to get to, where the local language has no literary tradition, and where the chances of coming down with a serious illness are nontrivial. Social research really is harder to do in some places than in others. But the fact is, there is a long, noble tradition of concern with research methods in anthropology—quantitative and nonquantitative, humanistic and scientific.

Kathleen and Billie DeWalt quote at length in Chapter 8 what is surely one of the most-cited early discussions of methods in anthropology: Malinowski's introduction to Argonauts of the Western Pacific (1922). It is justly famous because, as the DeWalts say, it established the importance of long-term participant observation as a strategic method for field research on other cultures. No peering off the veranda at the natives for Malinowski.

Participant observation is an important method in anthropology but, as the DeWalts point out, it is one of many methods used in fieldwork. By the time Malinowski went to the Trobriands, *Notes and Queries on Anthropology*—the fieldwork manual produced by the Royal Anthropological Institute (RAI) of Great Britain and Ireland—was in its fourth edition (the first came out in 1874). The sixth

(and last) edition was published in 1951 and was reprinted five times until 1971. That final edition was edited by Brenda Seligmann and "a committee of the Royal Anthropological Institute" that included the likes of E. E. Evans-Pritchard, Daryl Forde, Raymond Firth, Meyer Fortes, and W. E. Le Gros Clark—and is must reading for anyone interested in learning about field methods. Strip away the quaint language and the vestiges of colonialism—"a sporting rifle and a shotgun are . . . of great assistance in many districts where the natives may welcome extra meat in the shape of game killed by their visitor" (RAI 1951:29)—and the book is still full of useful, late-model advice about how to conduct a census, how to handle photographic negatives in the field, what questions to ask about sexual orientation, infanticide, food production, warfare, art . . . The book is just a treasure.

In the 1920s, leading sociologists were concerned with moving their discipline away from an emphasis on social reform—away from the study of what ought to be and toward the study of what is. If the public were ever to trust social science, said Carl Taylor, then the emphasis had to be on "exact and quantitative expressions and measurements" (Taylor 1920:735). This, he said, required "technologies which will reduce observations to a comparative basis" (p. 753). The technology of choice, said Taylor, was the social survey, a method dating at least to John Howard's monumental, comparative study of prisons (1792).

Taylor's idea of what a survey should be was much broader than just questionnaires. "The survey method," he said, "is nothing whatever but the recognized and accepted comparative method of all science," and he concluded that "what . . . surveys can do and have done in the field of anthropology and ethnology, they can do and probably are destined to do for any body of knowledge or field of research to which they are applied (1920:752–753).

Taylor singled out the systematic study of vision, hearing, and pain that Charles and Brenda Seligmann (1911) had done on the Veddas of Sri Lanka. (Charles Seligmann was an ethnologist and physician.) Their 422-page ethnographic account covered family life, religion, the arts, property, and inheritance—and an 18-page report of the results of some psychological tests that they had used in their study of Vedda senses. Some of those tests had been devised by W.H.R. Rivers, an experimental psychologist who became interested in anthropology in 1899 when he was invited to join the Torres Straits expedition and saw the opportunity to do comparative studies of non-Western people (Tooker 1997:xiv).

Rivers, of course, developed the genealogical method—highly detailed, egocentered graphs for organizing kinship data. The genealogical tables he produced in his study of the Murray Islanders were singled out by Taylor as an example of "as perfect a scientific compilation as could well be imagined" (Taylor 1920:753. See Rivers's work in Volume VI of Haddon 1901–1935). These works by anthropologists, said Taylor, were examples of research to which all social science could aspire. Anthropologists continue to this day to work on improved methods for representing complex kinship structures (see White and Jorion 1992).

Rivers continued his work in anthropology and his development of the genealogical method with his research on the Todas of India (Rivers 1906). He developed what he called the "method of indirect corroboration." This method involves "obtaining the same information first in an abstract form and then by means of a number of concrete instances" (p. 11). Here is Rivers explaining the method with reference to his study of the laws of property inheritance:

I first obtained an account of what was done in the abstract—of the laws governing the inheritance of houses, the division of the buffaloes and other property among the children, etc. Next I gave a number of hypothetical concrete instances; I took cases of men with so many children and so many buffaloes, and repeating the cases I found that my informant gave answers which were consistent not only with one another but also with the abstract regulations previously given. Finally I took real persons and inquired into what had actually happened when A or B died, and again obtained a body of information consistent in itself and agreeing with that already obtained. (p. 11)

Rivers discussed his selection of informants, how he came to know that one of his informants had lied to him, the pros and cons of paying informants for their time, the need for getting information from many informants rather than just from a trusted few, and the importance of using the native language in field research.

Taylor must also have known about Lewis Henry Morgan's study, *Systems of Consanguinity and Affinity of the Human Family* (1870). It was a massive, crosscultural survey of kinship systems. Morgan collected a lot of the data on various Indian tribes himself, but he also sent questionnaires to missionaries and Indian agents. And Taylor surely also knew about Edward Burnett Tylor's key contribution to the literature on cross-cultural surveys (see Tylor 1889).

Contrary to popular wisdom, then, anthropologists have been keen survey methodologists from the earliest days of the discipline. Unlike sociologists, however, anthropologists studied small, remote groups of people. "These groups," observed Robert Lynd in 1939, "were 'primitive,' according to Western European standards, and therefore the older social sciences did not much care what anthropology did with them" (p. 14).

The point is, that by the time A Handbook of Method in Cultural Anthropology was published in 1970, the concern for methods in anthropology was already quite venerable. That volume, edited by Raoul Naroll and Ronald Cohen, was an enormous compilation—1,000 pages and 49 chapters by 46 authors (Naroll and Cohen wrote 6 of the chapters and participated in several others), including 5 that were reprinted from journal articles. The chapters in that handbook, as well as all the chapters on methods in Anthropology Today (Kroeber 1953), and in the Handbook of Social and Cultural Anthropology (Honigmann 1973), are as useful today as they were when they first appeared. The pioneering textbooks by Pertti