4-1-2013

American Anthropology and Company

Stephen O. Murray

AMERICAN ANTHROPOLOGY & COMPANY

Critical Studies in the History of Anthropology

SERIES EDITORS

Regna Darnell
Stephen O. Murray

American Anthropology & Company

Historical Explorations

Stephen O. Murray

University of Nebraska Press
Lincoln and London

Library of Congress Cataloging-in-Publication Data

Murray, Stephen O. (1950–)
American anthropology and company : historical explorations / Stephen O. Murray.
pages cm. — (Critical studies in the history of anthropology)
Includes bibliographical references and index.
ISBN 978-0-8032-4395-8 (cloth : alk. paper)
1. Anthropology—United States—History.
2. Anthropological linguistics—United States—History. 3. Sociology—United States—History.
I. Title.
GN17.3.U6M87 2013
301.0973—dc23 2012050027

Set in Lyon Text by Laura Wellington.

In memory of
Robert A. Nisbet (1913–1986)
and Dell H. Hymes (1927–2009),
mentors of vast erudition,
who crossed disciplinary borders
with impunity

Contents

Illustrations

TABLES

FIGURES

Series Editor's Introduction

REGNA DARNELL

I have known Steve Murray since he was a University of Toronto sociology graduate student starting a network analysis of anthropological linguists. From research on anthropological linguists and linguistics—which culminated in his magisterial 1994 book *Theory Groups and the Study of Language in North America*—and without any institutional support, he branched out to examining other borders of academic anthropology, including the obvious one for a sociologist, sociology, plus (ethno)history, as well as psychological anthropology (in its "culture and personality" guise). Very few readers are familiar with the full disciplinary range of his work. Most of the papers published in this volume have appeared in earlier forms in print, directed to diverse audiences. It seems to me that his method and argument define a unique critical perspective on anthropology as institutionalized for the past century and a quarter in North America. Reflexive bookends at the beginning and end motivate the choice of papers and integrate Murray's preoccupations over his career. Few other historians of anthropology have written broadly enough on the subject for a career perspective to emerge.

Several of the chapters of this book look at histories of central institutions of American anthropology, including its core journals and geographical expansion. Familiar, canonized anthropologist "culture heroes" (Sapir, Kroeber, Lowie, Mead, Boas, Redfield) indeed appear, but they are juxtaposed with the likes of William F. Ogburn and W. I. Thomas, leading figures of American sociology whose history was interwoven with that of anthropology at least until the end of the Second World War. Murray unravels much of the tension behind disciplinary coexistence at major institutions, particularly the Universities of Chicago and California, Berkeley. At Chicago, an anthropology department split off from

its august sociology department. At Berkeley, the anthropologists, particularly Alfred Kroeber, effectively opposed the emergence of a department of sociology and relegated the work of demographer Dorothy Swaine Thomas to agricultural economics (thereby also excluding her from histories of sociology, though she was the first female president of the American Sociological Society).

Murray brings critical distance to the emergence of peasant studies as a rebellion against Boasian Native American salvage research; he foregrounds the sociological side of anthropology that attempted to move beyond the isolationism of North America in the interwar years. Murray is inclined to attribute the contemporary approach to urban anthropology and the ethnography of complex societies to Chicago sociologists rather than to the anthropologists in their midst and clearly shares a distaste he reports sociologists had for the pronouncements about "American culture" of 1940s anthropologists who had done little or no research on their own "home" society and culture. Questions of political suppression arise, for example, in the analysis of anthropologists' complicity in World War II Japanese internment and with the long-running martial law on Taiwan by the Kuomintang, purporting to be "the Republic of China." The history of anthropology and associated disciplines does not emerge as uniformly benign, and Murray's work challenges contemporary anthropologists to evaluate where they have come from as part of present practice.

Murray consults archival sources for professional correspondence and the published literature of articles and reviews for cues to anthropologists' networks and frames them in terms of institutional developments and cultural trends that were far from unique to anthropology. He is a tenacious archivist, following individuals and events from institution to institution and integrating widely dispersed sources. Trained as a sociologist, he counts things that can be counted—then tells his readers why the numbers explain what people were up to (citations, numbers of students, book reviews, etc.). He elicited memories and explanations from elders of the "tribes" (disciplines) who are now dead and triangulates these "native views" with archival records and published social science literature.

Murray is skeptical of the stories anthropologists tell about themselves for an audience within the discipline and seeks out alternative explanations and connections, particularly at disciplinary connecting points

where cross-fertilization is most likely to occur. He has a way of getting to the point and challenging readers to disagree, but only on the basis of historicist interpretation of concrete evidence. Sloppy generalizations about the history of anthropology annoy him and often stimulate him to undertake research complicating pat explanations.

In addition to ethnological analysis and theorizing, Murray has done (and published in refereed journals) ethnographic work in Latin America and Asia as well as North America, giving his voice an all-too-rare comparative ethnographer resonance. He deploys his insider-outsider status in anthropology to approach the preconceptions the anthropologist takes to the field in his collaboration with Keelung Hong on the latter's native Taiwan and emphasizes the explicit challenges this perspective poses to established anthropological wisdom. Questions are raised at the peripheries that tend to be glossed over at the center. This collection is provocative and needs to be taken seriously as both substantive historiography and methodological caveat for a critical history of anthropology.

Introduction

Collecting some of my writings about research on the history of American anthropology and its social science neighbors (history, linguistics, psychology, and at greatest length, sociology) provides the opportunity to reflect on how they came about and to see some relationships between a range of research projects aiming to answer questions about some things that happened and some that did not but seemingly could have along the relatively unfortified borders of twentieth-century American anthropologies.

Having had no undergraduate sociology or anthropology course but having read books about scientific communities by Herbert Butterfield and Don Price, I wandered into the history of social science stimulated by Thomas Kuhn's (1962) *The Structure of Scientific Revolutions*, which I read in 1972, during the summer after I graduated from college. At the University of Arizona two years later, Keith Basso stimulated my interest in social influences on language use. When my sociologist mentor Robert Nisbet told me he was leaving Arizona and that I needed to find a different PhD program, I migrated to the University of Toronto, intending to do dissertation research about power in spoken interaction, building on the work we did in Keith's seminar.

William Samarin took over my education about sociolinguistics, and I picked up some ideas about ethnohistory from him as well. The summer after my first year in graduate school I attended my first annual meeting of the American Sociological Association—at which I heard some very unimpressive presentations by many of the biggest names in the field. However, I also went to a sociology of science session in which I heard a paper by Nicholas C. Mullins (published in 1975) that provided the model of revolution-making in science that I would test and refine in my doctoral dissertation and one by Harriet Zuckerman (1974, never published, though the main example for it was, as Zuckerman and Led-

erberg 1986) about how lines of work can be postmature as well as premature, which bore fruits especially evident in chapters 10 and 12 of this volume.

I proposed a dissertation testing the bipartite (functionalist and conflict) Mullins model of group formation and rhetorics of continuity or of making revolutions. I quickly realized that context-free measures of the amount of discontinuity in practices or in theories did not exist and would be essentially contested were any proposed, whereas intercoder reliability about proclaiming continuity or revolution was obtainable. What I proposed eventually became *American Sociolinguistics* (Murray 1998), but the dissertation reached beyond anthropological linguistics to unanthropological linguistics (the purported "Chomskian revolution" in particular: see Murray 1980d) and back through centuries of North American work describing and attempting to explain language(s): what became *Theory Groups in the Study of Language in North America* (Murray 1994b).

The case studies (including theorizing that did not lead to "theory groups") contained a lot of detail, but were being deployed to test Mullins's model. I have included none of this line of work here. Indeed, only two chapters in this collection (2 and 10) deal with linguistic anthropology. Both focus on Edward Sapir's years (1925–31) in the University of Chicago Department of Sociology (of which an anthropology program was a junior partner before becoming a separate department in 1929). I feel that as a linguistic anthropologist (which I sometimes think I am) I am in the Sapir tradition, but am aware that I would probably not have gotten along with him had I been his student or colleague. I feel that opportunities for interdisciplinary integration were missed at Chicago of the late 1920s (for which there is plenty of blame to go around!) and that Sapir was the person there with the knowledge about languages to supplement Chicago sociologists' experiences doing ethnography.

I also think that anthropological research on peasantries by American anthropologists was late, if not fully "postmature." When it *was* done, it was done primarily by students of Robert Redfield at Chicago and those who had studied with Alfred Kroeber at Berkeley. This turn is explicable in expanding the subject matter as the supply of "tribal"/"primitive" people was rapidly waning. Kroeber supervised and encouraged work on peasants, though his own ethnographic work was "salvage anthropology" of severely disrupted California indigenous groups, along with

archaeology in Peru and analysis of cultural changes over centuries within large-scale civilizations.

It is difficult not to notice that anthropologists and sociologists who were trained at Berkeley and Chicago loom large and recurrently in my historical explorations (along with those, including Kroeber, Robert Lowie, Margaret Mead, and Sapir, who had been influenced by Franz Boas at Columbia, plus Columbia sociology PhDs such as William Ogburn and Elsie Clews Parsons). In part this focus on those sites is because they were important centers for social science research and theorizing. In part, it is that so much biographical work has been done on Columbia faculty and graduates that I did not see much point in undertaking research on them. Probably another part of the explanation is that very rich archives exist at the Bancroft Library (Berkeley) and the Regenstein Library (Chicago). I have spent a lot of time in both libraries, along with a week's time spent in the Boas Collection of the American Philosophical Society Library in Philadelphia.

Kroeber and Sapir play major roles in the stories told in several chapters, as do W. I. Thomas and his wife, Dorothy Swaine Thomas (who was already named Thomas before marrying him). I see tragic elements in the career (and inept careerism) of Edward Sapir. I see W. I. Thomas as something of a martyr who refused to consider himself a victim—Dorothy Thomas as a tragic heroine who also refused to consider herself a victim. The final two Berkeley-centered chapters can be read as cases of ingratitude by young and ambitious men (Morton Grodzins and Robert Nisbet) to accomplished and senior scholars whose sex made them vulnerable to marginalization (Dorothy Thomas and Margaret Hodgen).

I am not pushing a particular theory (about sexism, theory-group formation, or anything else) in the studies collected here. Most of my research projects were stimulated by doubts about particular claims. (Taking examples other than the historical ones examined in this volume, I was skeptical about the use of weak ties in getting job information or that interruption is a male monopoly in naturally occurring speech.) I think most of the chapters show that developments in (social) science are fitful and far more complicated than simpleminded "just-so" stories like those Derek Freeman and others have told. Zadie Smith wrote that "most of us have complicated back stories, messy histories, multiple narratives" (2009:42), and so do scientific "schools," theories, and methods. Kuhn (1962 and elsewhere) considered social sciences

"preparadigmatic" because of dissensus about such basic matters, though I have my doubts about any paradigm commanding the assent of every credentialed participant in any modern scientific field. I am interested in developments in anthropology and other fields but very skeptical of Development (or Evolution) in the singular. My dissertation research convinced me that there are both major continuities across the punctuations of "scientific revolutions" as well as major discontinuities within "normal science." Moreover, "normal science" (and, indeed, all academic disciplines) is often very rancorous, and even seemingly hegemonic theory groups are riven with internal controversies and personal antagonisms, though these may be cloaked in rationalizations of methodology and interpretations of doctrine and data.

Labels—even those such as "Boasian," "Freudian," or "Parsonian," which I sometimes use—often obscure nearly as much as they illuminate. I think that at least some of my work, especially the first four or five chapters in this volume, looks at practices, which to me include recurrent omissions as well as recurrent commissions in what social science practitioners do or did. Having entered the field looking at network clusters rather than genealogies, I wish that history of social science writing focused more on practices, less on pronouncements about affiliations to theoretical paradigms and apostolic successions.

Insofar as cultural anthropologists, linguists, and sociologists are "professionals," their "profession" is as "professors," not as researchers or writers living on fees for service and royalties.[1] Professors often seem more interested in professing to colleagues away from their institutional bases—whether in person or in keyboarding and subsequent publication—than to students at the university. When I speak of their "practices," this includes what they do inside their discipline (and subspecialty) more than what they do within a particular university or other institution. Jockeying for place and for quasi-ownership of particular research domains (places or topics) is a focus of many of these historical explorations. From my dissertation research on anthropological linguistics, my curiosity carried me from that borderland into unanthropological linguistics (see Murray 1994b) and unlinguistic anthropology (most of the chapters herein).

My first interest in Boasian anthropology was stimulated by Claude Lévi-Strauss, who was in the French sociological tradition of Émile Durkheim (and, especially, Durkheim's nephew and collaborator, Marcel

Mauss), as well as in the comparative social theorizing of Charles de Montesquieu and Jean-Jacques Rousseau. I've been tempted to include my first presentation at a professional meeting, "Lévi-Strauss in the State of Nature" (presented in El Paso at what was then called the Rocky Mountain Social Science Association), an honest-to-God structuralist analysis of the "states of nature" in Thomas Hobbes's *Leviathan*, Rousseau's *Social Contract*, and Lévi-Strauss's *Tristes tropiques* (and some articles by him).

My undergraduate background in social theory (in the ornately named major "Justice, Morality, and Constitutional Democracy" at James Madison College) allowed me to do well in the "pretest" that Robert Nisbet gave students on the first day of his social theory class. Nisbet had written two books on Durkheim and found my approach (through Rousseau, Montesquieu, and Durkheim) at least interesting. So, although my research on the history of anthropology followed interest in anthropological linguistics, that interest itself flowed from the French tradition of theorizing about society/ies.

I've already mentioned that I had no undergraduate sociology (or anthropology) courses, but I had read some Max Weber (not least, "Science as a Vocation") and was intrigued by the Weberian base of Berkeley sociologist Jerome Skolnick's (1967) police ethnography *Justice without Trial*. Weber is not viewed by as many anthropologists as having done ethnology as Durkheim, particularly in his last (1910) book, is seen as an ancestor of symbolic anthropology and (in earlier work) as a pioneer of structural analysis and of analysis of phenomena such as suicide with social variables. Durkheim's *Elementary Forms of the Religious Life* (1912) was (among other things) an ethnology of Australian aborigines. Weber's life project was comparative, comparing material on religions and economies; drawing data from historical texts and archival material along very long historical *durées*, Durkheim used then-recent ethnographies by others in *Elementary Forms* and official statistics in *Le suicide* (1897). I was once accused of being a "closet Weberian" but don't think that my comparativist interests were ever closeted. The comparativism among American social science traditions may not seem obviously Weberian, but in examining multiplicities rather than proclaiming a singular evolutionary trajectory, I feel that what I do is Weberian more than Durkheimian (or Marxist). I do not see the history of sociocultural research as teleological, progressing even unsmoothly to one ultimate

Truth (explanatory principle). Anyone seeking a teleological history can find it in Marvin Harris's *The Rise of Anthropological Theory* (1968), which definitely does not focus on practice(s).

This personal contextualization is included to establish that although my path into or toward sociocultural anthropology was through linguistic anthropology, once I arrived there, I had some relevant background. As a historian, I approached the anthropology-sociology border from anthropology, having approached sociocultural anthropology from linguistic anthropology and linguistic anthropology from the Durkheimian/Lévi-Strauss tradition. Perhaps my idiosyncratic peregrinations through social science disciplines did not matter to how I interviewed those with memories of the events and/or scholars in whom I came to be interested or how I worked through archival collections. I certainly learned some things about interpreting texts from my undergraduate (Justice, Morality, and Constitutional Democracy) and graduate (sociology and linguistic anthropology) classes and did some (Weber-influenced) content analysis in a social psychology methods course taught by William Crano at Michigan State. Part of my disillusion with the Arizona sociology program was the unconcern about data gathering I felt was prevalent there: the focus, particularly from Otis Dudley Duncan—my academic genealogy link to Ogburn—was on grinding quantitative data gathered by others; only a decade later did I find out that Duncan shared some of my concerns about the validity of the numbers he taught us to grind (Duncan 1984).

It might seem more-or-less natural that a sociologist who undertook research on anthropologists would take an interest in the sociology-anthropology border regions and that it was from having been a postdoctoral fellow in the Berkeley anthropology department that I would take an interest in earlier interdisciplinary projects and conflicts at Berkeley. Prefiguring an argument I will make in the conclusion, such "commonsense" inferences are misleading. It is from having been a student of Robert Nisbet's at Arizona that I explored the fate of the peculiar department in which he had been a student and junior faculty member. Similarly, my interest in the professional unpopularity of scientists who do popularizing work (chapter 3) came in part through Brandeis (where I've never been, but where Everett Hughes influenced some people who influenced me), in part through having lived in the Arizona-Sonora desert with connections through whom I reached and talked to Edward and

Rosamond Spicer, and in part from one of the theory groups of my dissertation research having been ethnoscience (see Murray 1982). On the other hand, despite my grounding in Freudian conceptions from undergraduate courses, the chapter (6) focusing on Freudian anthropology developed while I was in Berkeley, supposed to be working on linguistic anthropology, but having easy access to George Devereux's PhD dissertation on Mohave sexuality. If I didn't live in the San Francisco Bay Area, I might not have undertaken to write about Berkeley anthropology during the 1950s (that decade I chose because it seems less written about than earlier or later ones there). It was also while I was a Berkeley postdoc that Keelung Hong directed my attention at anthropological (mis) representations of Taiwan (chapter 8), including but not confined to Berkeley ones, with less-compromised work from people at my first alma mater, Michigan State (of whom I had been unaware when I was there).

Topics of Particular Chapters

The chapters within each of the two sections of this book are roughly in chronological order. The expanse of time covered in some of them makes for some temporal backtracking, however.

Divergences among "Boasians" are foci of the first and third chapters. I think that these chapters are concerned with practices in research and inference more than in theorizing. (I see Boas as antitheoretical, though many of his students were not, including Edward Sapir, a major character in the second chapter, one particularly focused on practices and how field practices and assumptions about a singular structure being recoverable from any speaker affected the descriptions and analyses produced.)

As the "salvage" project of American(ist) anthropology waned, some anthropologists got around to studying peasant communities. (Sociologists had been doing both rural and urban community studies for some time, though the 1930s was a "boom time" for community studies in sociology as well as in anthropology.) Anthropologists trained by Kroeber and Lowie at Berkeley were among the anthropologist pioneers of peasant studies (along with those trained at Chicago by Robert Redfield and A. Radcliffe-Brown), as is elaborated in chapter 4.

Chapter 5 shows that a geographic expansion of interest beyond U.S. territory was more gradual and less complete than some have suggested. In addition to expansion of geographic range, increased attention was paid to enculturation in early life, attention encouraged by Freudian as-

sertions about character structures being set in infancy and early childhood. In chapter 6 I look at the dissertation and predissertation fieldwork and training by Kroeber and Lowie of Georges Devereux, who became one of the most orthodox of Freudian anthropologists but was, I show, not one yet at the time of his Berkeley dissertation on the Mohaves.

Chapter 3 is unusually focused on a center of American anthropology research and training (Columbia/Barnard/New York's Museum of Natural History) rather than on anthropology borderlands (linguistic, psychoanalytic, or sociological). Its aim to correct the misrepresentations of the culture of Boasian anthropologists in Derek Freeman's shoddy, surmise-heavy "history" of American anthropology is obvious. Chapter 7 looks at a major department, the University of California at Berkeley's, which was somewhat becalmed during the 1950s with the retirements of its internationally known anthropologist superstars. This is the same department from which the psychological anthropology work discussed in the sixth chapter emerged (during its heyday) and one that reappears in chapter 13 as leading resistance to establishing a sociology department.

Chapter 8 turns to another periphery: doing fieldwork on Taiwan while American social scientists were banned from China and pretending to be studying "traditional Chinese culture," a more prestigious line of work than researching a hybrid culture under autocratic rule (that justified itself as restoring "traditional Chinese culture" to what had been a Japanese colony). We (the chapter was coauthored with Keelung Hong) show that there was variation by topical domain in how invisible in anthropologists' representations it was that the research was done on Taiwan. (The past tense is justified since American anthropologists fled democratizing Taiwan to work under the auspices of the autocrats in China once they could.)

Chapter 5 does not have a geographical base in terms of a particular anthropology department but looks at the field more generally in regard to field site locations. Although some American anthropologists looked beyond aboriginal America(s), looking at publication data shows that American anthropologist remained "Americanist" in the usual sense of American cultures and field sites.[2]

The ninth chapter shows that the pioneering empirical sociologist W. I. Thomas began as a Boasian ethnologist and ended as a behaviorist, although he has been read primarily as a voluntarist sociologist, especially by symbolic interactionist sociologists. (I do not think that he is

read at all by twenty-first-century anthropologists, though some may nod at his pioneering collection of life histories from Polish emigrants to the United States.)

The tenth chapter focuses even more than the second one did on Edward Sapir, again in his Chicago years (1925–31), and the failure of ethnography of speaking or any other kind of sociolinguists developing there and then. Though Sapir was brought in to bridge disciplinary boundaries, he was involved in the fission of the sociology department at Chicago into separate sociology and anthropology ones, and his strongest personal bond to a Chicago sociologist was to the least ethnographic of them, William F. Ogburn (who had been friends with Boasian anthropologists at Columbia and was data-oriented but was not interested in language).

The eleventh chapter—looking at the reception of anthropological books in the three core American sociology journals—shows that cultural anthropology was highly valued by American sociologists until pronouncements by Margaret Mead, Hortense Powdermaker, and Clyde Kluckhohn made sociologists wonder whether what anthropologists understood of distant/alien societies was as off-base (invalid) as the facile claims they made about "American culture" (and the inner workings of Hollywood).

The thirteenth chapter, like the tenth, examines a case of postmaturity. Although anthropologists were not the sole opposition to the University of California (Berkeley) establishing a sociology department, Kroeber in particular was involved in delaying establishment of one. Keeping Dorothy Swaine Thomas—wife of W. I., student of and collaborator with William F. Ogburn, and later the first female president of the American Sociological Association—marginalized in the Department of Agricultural Economics seems to have been a major motivation, not only for senior male scholars in other disciplines but also for Margaret Hodgen, pioneer of history of anthropology, who was the woman heading the Department of Social Institutions. That department was an ad hoc institutional home concocted for her mentor, Frederick J. Teggart, who retired in 1940 and died in 1946.

The twelfth chapter also shows Dorothy Thomas being kicked around, by both an ambitious former research assistant and by senior staff at the University of Chicago who she thought would understand her wanting to control research for hire done by a research assistant, Morton

Grodzins. The subject matter of the data (the forced removal of Japanese Americans from the U.S. West Coast) and some artful dodging of truth made it possible for Grodzins to publish project data . . . and to take the position of his primary Chicago advocate as head of the University of Chicago Press, despite having no other background in publishing than getting his PhD dissertation published over the vociferous opposition of his former supervisors—including his dissertation supervisor, Charles Aikin, and Thomas, who was on his PhD committee in addition to being the head of the Japanese-American Evacuation and Resettlement Study (JAERS).

As with many of the preceding chapters, the final substantive one aims to correct misapprehension, this one that editors of core anthropology and/or sociology journals profit by increased citation of their work in the journals they edit.

The concluding chapter reviews what I believe I have learned from doing history of social science: a set of maxims rather than a cookbook. I think that understanding past thinkers as they understood themselves is ultimately impossible but nonetheless is a worthy aspiration, which is to say I consider my work "historicist." I do not think that trying to figure out what people in the past thought they were doing disallows criticism of how they did—and failed to do—what they wanted to do. Moreover, I find deplorable some of the goals—notably herein those of the anthropologists who worked for concentration camp administrators (as the Berkeley Japanese-American Evacuation and Resettlement Study staff did not) and those eager for access to fields in Taiwan—and do not confine myself to observing the self-defeating conduct of social scientists. My research and analysis of the history of American social science(s) is primarily internalist,[3] but by no means would I proscribe externalist work, particularly of the World War II and Cold War work into which some anthropologists plunged enthusiastically (garnering much more financial support than British functionalists received from the British colonial establishment), which David Price (1998, 2002a, 2002b, 2008) has been exploring (see also Solovey 2001; D. Wax 2008). George Lucas (2009) extends such valuable inquiry into this century's combats.

AMERICAN ANTHROPOLOGY & COMPANY

1

Anthropology and Some of Its Companions

Introduction

Before the Boasians

Though the chapters in this book do not form a teleology or test any specific theory, some readers might find the particular topics addressed as beginning in medias res and/or readers may lack background in the prehistory of academic American anthropology—a process that began in the last years of the nineteenth century. In addition to recommending accounts in Bieder (1986), Bieder and Tax (1974), Darnell (1998a, 1999, 2001), Hinsley (1981), and Jacknis (2002), I provide a whirlwind overview below.

From the beginning of European settlement of the Americas, missionaries tried to learn about Native American cultures and languages. Proselytizing in native languages and translating the Bible into them was an early and persisting commitment, along with understanding Others the better to manipulate them ("applied anthropology" *après la lettre*). Many Christians were interested in scrutinizing the native people to see if they were some "lost tribe of Israel" (Hodgen 1964:303–25; Stocking 1968:42–68). Rationalists during the eighteenth century also sought information about the native inhabitants of the Western Hemisphere to build and assess models of "the state of nature."

Thomas Jefferson

Thomas Jefferson was an American rationalist with wide-ranging interests. Prior to his election as president of the United States of America, he worked on problems of Native American philology and speculated about where Native people(s) came from. As president, he promoted the collection of information on Native Peoples, especially through the (Meriwether) Lewis and (William) Clark expedition to the Pacific coast (1803–6). Jefferson himself prepared a research memorandum for them and

stressed the need to record languages and cultural traits. In his *Notes on the State of Virginia* of 1787, Jefferson set forth his own speculations about the ancestry of the American Indian(s), tabulated historical, descriptive, and statistical data on tribal groups, and reported on his own excavation of burial mounds.

In frequent correspondence with leading European and American intellectuals of his time, he "and others of his circle set an example by accumulating new knowledge regarding *Homo Americanus*. This was anthropology without a portfolio, pursued in our own frontiers" (Hallowell 1960:16).

Since Jefferson extended those frontiers, he also had less disinterested motivation for learning about the indigenous peoples. In his view, their cultures deserved respect. In the view of others, Native people were savages who happened to be in the way of what was later claimed to be the "manifest destiny" to supplant them with God's chosen northern European people (Anglo-Saxons) across North America. Once the new American government committed itself to the principle of recognizing native title to western lands, "it was of great practical importance for the government to have reliable knowledge about the Western tribes" (Hallowell 1960:18). Jefferson's own curiosity certainly extended beyond the practical needs of presiding over territorial expansion, but his wider humanistic motivations for inquiry were not necessarily shared by his successors, nor did they determine his presidential policies (see Wallace 2001). Jefferson institutionalized a connection between anthropological/linguistic inquiry, territorial expansion, and a responsibility for managing Native people within that encroachment.

Gallatin, Schoolcraft, and the American Ethnological Society

Together with Thomas Jefferson, whose secretary of the treasury and advisor on Indian affairs he was, Albert Gallatin (1761–1849) was one of the leading American Enlightenment figures. However, Gallatin "did not undertake serious ethnological studies until the 1820s, a time when Enlightenment assumptions about man were under attack" and German romanticism was increasingly influential (Bieder 1986:17).

Gallatin participated in the cultural, intellectual, and social institutions of the New York elite in the first half of the nineteenth century. John Bartlett, a fellow officer in the New York Historical Society, proposed to him "a new society, the attention of which should be devoted

to Geography, Archeology, Philology and inquiries generally connected with the human race" (quoted in Bieder and Tax 1976:12). Gallatin was elected president of the new American Ethnological Society (AES) in 1842. Its dinner meetings were held in his home until his death in 1849. "The active members tended to be gentlemen of some social standing in the New York community who knew each other well, and while they had some intellectual pretensions, they were not 'ethnological experts.' Nearly all were professional men. . . . Very few of the members, even in the early and more fruitful years of the AES had any ethnological experience" (Bieder and Tax 1974:16).

Through most of the nineteenth century, "descriptions [of native ways] were often interpreted by gentlemen and philosophers who themselves had not had contact with native peoples. . . . This period gave way to one of incipient professionalism in which individuals labeled their work as anthropology and submitted it to evaluation by their peers, but were not themselves trained as anthropologists" (Darnell 1976:70–71).

Rather than develop (evolve) toward professionalism, the AES foundered (degenerated). Its sponsor died, leaving only dilettantes behind. Also, "philosophical disagreements had an adverse effect on the Society's fortunes. Many members quarreled about what was pertinent and legitimate for discussion. The crux of the matter lay in the fundamental division between the atheistic polygenists . . . and the clerics. . . . As physical anthropology became a topic of common discussion in the Society, discord erupted. The polygenist approach to physical anthropology disturbed both the clergy and other monogenist members, many of whom were mainly interested in Near Eastern antiquities and insisted on a literal interpretation of the Bible" (Bieder and Tax 1976:17).

That is, the AES was split by "paradigm conflict" despite its "prescientific" status. The Bible served as a paradigm for the monogenists, who explained diversity as stemming from degeneration (due to the lapsing of God's law) from the original unity of descendants of Adam and Eve.[1] Those who believed in multiple origins of humanity, language, and cultures necessarily challenged the sufficiency of Genesis as a description and/or explanation (Hodgen 1964:225–94).

Gallatin argued that Indian languages were primitive against those who saw "polysynthetic" languages as residues of a higher ancestral civilization (usually assumed to be that of a "lost tribe of Israel"). Mayan, Aztec, and Inca civilizations, which Gallatin insisted were indigenous,

demonstrated the racial capacity to advance to urban, agriculture- and state-based civilization.

Henry Schoolcraft, now better remembered as the white discoverer and namer of the source of the Mississippi River (Lake Itasca, compounded from *veritas caput*) than as an expert on North American Native Peoples, was a geologist on an 1820 government expedition to Lake Superior and the Mississippi River led by governor/general Lewis Cass. In 1822 Schoolcraft was appointed Indian agent at Sault Saint Marie, Michigan. There he married Jane Johnson, an Ojibwa woman of mixed descent. Though spending more time in the field than most early ethnographers, Schoolcraft was typical in underacknowledged reliance upon the "tacit knowledge" of a long-term local resident (see Ogden 2011). Schoolcraft was less optimistic about Native Americans' racial capacity than Gallatin had been, often considering them "Oriental," which to him meant impervious to change, though he nevertheless believed the young could be Christianized and educated. Cass, a patron of Schoolcraft and of others studying Native Americans,[2] thought that Gallatin and others romantically overestimated the capacities of American Indians (see Cass 1826). Schoolcraft found—to his chagrin—that a "primitive language" (specifically Ojibwa, then called "Chippewa") was not as simple to master as Cass's theory of limited mental capacity of Indians ordained.

Frustrated by the difficulty of linguistic analysis, specifically with demonstrating the connections between Ojibwa and Hebrew, and by personal circumstances, Schoolcraft sought another, easier "royal road" to Native American history than comparative philology in folkloristics (Bieder 1986:158–173; Hallowell 1960:43). Despite the increasing dominance of polygenism (Horsman 1975), which made inroads even in the AES, Schoolcraft maintained the "Genesis paradigm."

Both by being innatist in science and by articulately criticizing genocidal policies, Schoolcraft and, later, Lewis Henry Morgan continued the Gallatin tradition, both in monogenist theory and in the practice of defending Native Americans from rationalizations for genocide. Indeed, his major ethnographic and ethnohistorical study of the League of the Iroquois was originally a series of "Letters on the Iroquois Addressed to Albert Gallatin" published in the *American Whig Review* in 1844–45. Although Schoolcraft and Morgan did extensive (if not intensive) fieldwork with particular peoples (Chipewyans and Iroquois, respectively), and

although all three collated published reports and questionnaires sent to those familiar with different North American peoples ("tribes"), they all treated American Indians as a single people at the stage of "barbarism" in the evolutionary rise from "savagism" to "barbarism" to "civilization" (see Hodgen 1964). All three defended the model of monogenesis against an increasingly dominant polygenism in which American Indians were considered a distinct species doomed to extinction in competition for territory with the "Anglo-Saxon race."

Brinton and Putnam at Pennsylvania and Harvard

A firm believer in the psychic unity of mankind, Daniel Garrison Brinton, a Philadelphia physician, was active in many local intellectual societies. I would include among the local organizations Philadelphia's American Philosophical Society, of which Brinton was president in 1869 and in the *Proceedings* of which he published many anthropological papers. Brinton was also president of the (more truly national) American Association for the Advancement of Science in 1882.

Brinton was "technically the first university professor of anthropology in North America," appointed at the University of Pennsylvania in 1884. However, the appointment was an honorary one without salary. Moreover, Brinton did not actually teach classes or have students at the university (Darnell 1970:82, 85). The grandiose structure of courses listed in university catalogs went mostly untaught for lack of students. At least, he trained no professional anthropologists (A. Kroeber 1939:178, 1960:4–5). He was "committed to the development of an academic framework for anthropology" (Darnell 1970:83) but was at odds with the local patrons of the university and museum, who were primarily interested in classical and Near Eastern antiquities. Reacting against "lost tribes of Israel" and other interpretations of superficial resemblances, especially in myths and especially across vast spaces, Brinton maintained the probability of independent invention. Brinton's work had little posthumous influence (A. Kroeber 1960:4), except as the unnamed object of some of Boas's polemics between 1896 and 1911—though Boas was certainly not seeking to renew grand schemes of diffusion such as those Brinton had combated.

Frederic Ward Putnam came closer to being an organizational leader than Brinton did. Putnam was trained between 1857 and 1864 as a zoologist by Louis Agassiz, the preeminent natural historian in the West-

ern Hemisphere. Putnam joined the Peabody Museum of American Archaeology in 1870 and held an honorific chair at Harvard similar to Brinton's at Pennsylvania between 1886 and his death in 1908. Putnam was not a Darwinian nor a Spencerian in any strict sense, albeit he was considerably more of one than was his patron Agassiz or his protégé Franz Boas. He "shared the prejudices and views of his contemporaries, although his commitment to a long historical perspective was tied to an advocacy of contact and borrowing more than in the case of his evolutionist contemporaries" (Timothy Thoresen:author, June 13, 1977).

As permanent secretary of the American Association for the Advancement of Science, Putnam had national ties in the world of science. His old New England family connections provided him access to the world of private philanthropy (Stocking 1968:279). Unlike Brinton, Putnam organized several enduring anthropological institutions, including the anthropological work for the World's Columbian Exposition in Chicago in 1893, which became the Field Museum, and for the American Museum of Natural History in New York (1894). He was also the nominal head of anthropological enterprises at the University of California beginning in 1900, although ill health kept him on the East Coast (A. Kroeber 1905). The Peabody Museum apparently did not pay him enough to live on, and he refused to give up his position there, but he could not realistically superintend day-to-day functioning of three institutions in three different cities. This overextension provided an opportunity for his protégés—Franz Boas in New York and Alfred Kroeber in California (see Thoresen 1975; Dexter 1989). Under their guidance, Columbia University and the University of California at Berkeley became two of the three preeminent institutions for training anthropologists in the twentieth century.

During the 1890s anthropology at Harvard produced fifteen PhDs—more than any other institution in the country. Following in the tradition established by Putnam, they were as a group predominantly interested in archaeology, without strong commitment to any major viewpoint in anthropological theory (Stocking 1968:279).

Putnam had no particular theory to push. Even in archaeology, where his own focus was, he did not found anything identifiable as a "theory group," nor did he provide distinctive intellectual leadership of the sort to found one. Even his importance as an organizational leader probably looms greater in retrospect because Boas and Kroeber, themselves im-

portant intellectual and organizational leaders, were among those Putnam sponsored and because archaeology continued at Harvard. He worked a number of years to place Boas and backed Boas's first (1901) Columbia PhD (Kroeber) more than Boas himself did (Thoresen 1975).

Major Powell and the Bureau of American Ethnology

The "gentleman amateur," engaged in fieldwork for a season or a shorter time, already encountered in the AES, was "most prominent in descriptive zoology and botany and in exploratory work in geography and geology," according to Randall Collins (1975:487). Although John Wesley Powell began his explorations of the Grand Canyon of the Colorado River as a professor of geology on a scientific expedition funded in part by his university, his early work fits that nonprofessional pattern, and the fruit of that first expedition was more journalistic adventure story than contribution to scientific knowledge (Stegner 1954:123). Powell was not primarily an anthropologist (and still less a linguist). He was a western explorer, "Washington scientific lion" (Scott 1976:27), and head of the Geological Survey of the Smithsonian Institution and, within it, of the Bureau of Ethnology, which he organized in 1879 (the name was specified to Bureau of American Ethnology in 1897). He was also president for the first nine terms of its existence of the Anthropological Society of Washington, which was later to become the American Anthropological Association.

As one of his successors recalled, of the collaborators he recruited to the BAE, "there's not one of them had training in anthropology, because there was no place they could train. . . . When I first went into the field, there wasn't a trained anthropologist in the whole lot. . . . Personal knowledge and interest gained them information" (Hodge 1955:80, 197). Although Powell lacked professional scientific training, built a staff of likewise self-taught scientists even in fields in which people with professional training were available, and was a major figure in amateur scientific circles in Washington DC, he was very oriented toward Theory. In his view, necessary observation could be made by almost anyone. Scientists such as himself then synthesized what amateurs reported (Hinsley 1981).

The self-made ethnologists whom Powell gathered together shared an evolutionary perspective. Morgan's (1877) *Ancient Society* and his view of unilinear evolution especially influenced Powell. While Social Dar-

winism was immensely popular in late nineteenth-century America (Hofstader 1944), Powell did not become a Social Darwinist. He preferred the progressivist-guided evolutionary doctrine of his protégé Lester Frank Ward.[3] While many of their contemporaries regarded the era of the robber barons as the inevitable pinnacle of human achievement, Powell and Ward stressed the role of intelligence and planning in human evolution—and were, therefore, implacable critics of Spencer and his view of mindless social forces (see Commager 1967:193–224; Hinkle 1980:184–213). Bureau ethnologists did not follow intellectual developments in Europe closely: "The subjects at home were so vast that it took all the time and research, the American Indians," as Hodge (1955:201) later rationalized.

The central problematic for BAE ethnologists was the observed "analogies and homologies" between human groups, particularly in North America. Since mankind was "distributed throughout the habitable earth in some *geological period* anterior to the present" one—and also "anterior to the development of organized speech"—what was common had to be accounted for by identical evolution (polygenesis) through fixed stages: "The individuals of one species, though inhabiting diverse communities, have progressed in a broad way by *the same stages*, have had the same arts, customs, institutions, and traditions *in the same order*" (Powell 1881:80). As a methodological principle, Powell asserted, "all sound anthropological investigation in the lower states of culture exhibited by tribes of men, as distinguished from nations, must have a firm foundation in language. Non-literate languages, representing a lower level of development, could profitably be studied with data from Native American tribes" (Powell 1881:xii–xv).

Powell and his subordinates were preoccupied with ordering the cosmos—and cannot be accused of gathering unconnected facts. Orderly classification of phenomena from the American "Wild West," such as Powell pressed for in geology, geography, hydrography, and ethnology, was useful to the government in Washington. Powell pioneered government science and tied it to a valiant attempt to plan development of the West based on understanding of its aridity.[4] Delineating Native Peoples was an administrative need of the federal government, responsible for their custody after the final expropriation of their lands (Holder 1966). Language was the obvious basis for groupings, so administrative needs and theoretical interests dovetailed.

The BAE classification of North American native languages bears Pow-

ell's name, and Powell (1891:218) claimed "full responsibility" (if not actual authorship) for it. The work of ordering the data on the premise that grammar relates to the stage of evolutionary development of the speakers of a language and that "the grammatic structure or plan of a language is forever changing" (Powell 1891:88) was done by Henry Henshaw, an ornithologist: "It was Henshaw who proposed and followed the biological method of linguistic stock precedence and nomenclature. . . . Powell was the moving spirit, and the final result, expedited by the approaching appearance of Brinton's *The American Race*, was published in 1891, under Powell's authorship but with credit to Henshaw in the Seventh Annual Report of the Bureau" (Darnell 1971:83–84).

Powell created the BAE and organized the research to be done by others whom he recruited. He also arranged for the dissemination of findings. With Otis T. Mason, he founded the Anthropological Society of Washington. "The Society met twice a month, except in the summer, to hear from two to four scholarly papers. The papers read at these meetings seldom languished unprinted, but usually appeared in the publications of the Smithsonian. . . . However, the need for a regular, official medium of publication was felt, and on Dec. 13, 1887, the Society became incorporated 'for the term of one thousand years' with the express purpose of bringing out a magazine, and the first number of the *American Anthropologist* appeared in January 1888" (Hallowell 1960:94).

If ever there was an organizational leader, it was Major Powell: "a genius at organization, [who] not only conceived a constructive program of research . . . but assembled the able men to carry it out" (Hallowell 1960:93), found positions for them, published and/or synthesized the resulting research, and "established a tradition which gave high priority to linguistic studies" (33). Powell designated what research was valuable and tied together the theoretical implications of the work done by subordinates into theoretically driven classifications.

The group that developed was not large. The number of full-time ethnologists in Powell's scientific empire was few, arguably none. Those involved were recruited in their maturity rather than trained by Powell. A nonstudent group with access to publication did not produce "revolutionary rhetoric." While rejecting then-influential Spencerism, Powell and his colleagues embraced the theoretical schema of Morgan. Celebrating their own lack of professional training as a virtue, Powell and his followers made no provisions to train successors. Despite successful

institutionalization—in government and in a "professional" society—the failure to train a new generation of workers resulted in the eventual eclipse of the Powell group and its paradigm by a university-based group.

Franz Boas

Though Boas and some of his students seem to have pushed the view that all was darkness until Franz Boas came to Clark University and said, "Let there be light," the material above shows this to be myth. Moreover, some anthropologists, Leslie White in particular, consider that Boas brought darkness (theoretical nihilism) and obfuscation of evolutionary patterns where there had been light (from Morgan in particular).

There is a vast literature on Boas, to which I have no interest in adding.[5] Not altogether advertently, he was central to relocating anthropological research from museums to universities. Quite advertently, he taught cohorts of cultural and linguistic anthropologists at Columbia University (where he was appointed in 1895) and established a paradigm of ethnographic particularism often focused on cultural traits (the infamous "shreds and patches"). The research in this volume begins with the first generation of Boasians in fairly secure positions—inside the BAE as well as in new academic departments (the Berkeley one not initially a teaching department but turned into an important one by Alfred Kroeber) and at least represented in the old one at Harvard.

There was resistance to Boas's domination of American anthropology, particularly from Harvard archeologists, and Boas was censured by the American Anthropological Association in 1919 for decrying U.S. spies posing as archeologists during the First World War, but his students ran the association's journal, the *American Anthropologist*, and were embedded in decisions by funders of anthropological research.

Racial pseudoscience did not vanish but little of it emanated from American university anthropology departments (Harvard again providing an exception with sometimes spy for the U.S. government Carleton Coon). Theoretical discussions were between Boasians, as in the instance examined in the first chapter, though in classifying languages there was ongoing conflict between the splitter Boas and the lumper Edward Sapir. The latter, Boas's most prominent linguistic student, was also dubious about Boas's exclusion of those without academic training in linguistics—and inclusion of some trained by Boas, matters I discuss in *Theory Groups* (Murray 1994b).

The "normal science" of trait distribution studies of memory cultures (salvage anthropology) was, in Boas's own opinion, largely done, and I believe that he had no interest either in comparing the culture particulars amassed (generally by "white room ethnography" elicitations, also called "debriefing elders") or in attempting to try to make sense of North American prehistoric movements of peoples, cultural complexes, or culture traits. Boas claimed, "When I thought that these historical methods were firmly established I began to stress, about 1910[!], the problems of cultural dynamics, of integration of culture and of the interaction between individuals and society" (1940:311).

Whether influenced by psychoanalysis or not (his students definitely were), a shift to the integration of culture in personalities was a paradigm shift not brought on by rebels or revolutionaries but encouraged by Boas himself (see chapter 3; Darnell 1977). Similarly, established anthropologists (Alfred Kroeber, Robert Redfield, Lloyd Warner) rather than rebels or revolutionaries also encouraged expanding from studying "primitives" to studying peasants (see chapter 3).

Eventually there was an evolutionist counterrevolution in the decade leading up to the 1959 celebration of the centenary of the original publication of *The Origin of Species*. Some leading Boasians (Kroeber and Margaret Mead in particular) made this transition (led by Kroeber's former student Julian Steward, who became chair of the Columbia anthropology department in 1946 and trained many of the GI Bill generation of graduate students at Columbia). Later still, dissatisfaction with the failure to provide satisfactory explanations of change in term of "cultural contact," which was also the basis for history/"dynamics" within British social anthropology, led to comparative history, in the work of Steward's student Eric Wolf (1969, 1982, 1999) and others, in terms of trade (including the slave trade) and conquests.

1

Historical Inferences from Ethnohistorical Data

Boasian Views

Particularly under the stimulus of Jan Vansina (1965, 1986), the possibility of using oral traditions to draw historical inferences regained legitimacy within anthropology (see, for instance, K. Brown and Roberts 1980). The earlier debate in which consideration of any historical value in such data shows a lack of agreement with the Boasian "band of sons," a phenomenon also evident in the shock to the older sons about what intellectual daughters of "Papa Franz" did, is discussed in chapter 3.

The Cultural Elements Paradigm

Franz Boas, the prime mover both in the institutionalization of American anthropology and in overthrowing the paradigm of nineteenth-century unilinear evolution theory, purported to view the distribution of cultural elements as not only a basis for reconstructing the history of societies without writing but as the **only** objective basis. During the first decade of the twentieth century, he directed his students to chart the geographical distribution of institutions, beliefs, and material objects from which to infer the migration of peoples and the diffusion of objects. He and they believed that the center of these scattergrams was the point of origin, that the peripheries where diffusion most recently had extended, and that the wider the distribution, the older the trait was. Sapir (1916) provided the most systematic account of the method for inferring age from area (l distribution).

Gathering data and refuting theories were more congenial to Boas than using data for the purposes for which they ostensibly were gathered. When his students began to draw inferences about prehistory, Boas did not support their efforts and shifted to another kind of particularistic study of single cultures, their psychic integration and reproduction. Nev-

ertheless, those already pursuing the first Boasian "normal science," the one in which they had been trained at the turn of the century, continued to try to solve the kinds of historical problems that were never seriously addressed by Boas or by his later students. Particularly in the project of salvaging memory cultures in California, Alfred Kroeber continued mapping cultural traits as reported by his students through the 1930s.

John Reed Swanton and Roland Burrage Dixon were the first Boasians to affirm some historical kernel of truth within folk traditions. Both received PhDs from Harvard, where Frederic Ward Putnam, a patron of both Boas and Kroeber, had established a center for anthropological research. Both had taken courses at Columbia from Boas. Swanton would later aver: "Whatever I have done is due to the inspiration of our teacher, Boas" (Swanton:Robert Lowie, July 30, 1957). Dixon, like Boas a veteran of the Jesup North Pacific Coast expeditions (see Freed and Freed 1983), was interested in language family reconstruction. As Kroeber noted in his obituary of Dixon, "Almost alone among their major contemporaries, he and Swanton maintained a sane and constructive interest in tribal and ethnic migration" (1923a:295).

This was the enduring interest that motivated Swanton and Dixon at least to consider whether folk traditions might contain grains of history. In their 1914 survey of the continent's prehistory they did not recommend uncritical acceptance of such traditions as offering transparent history; indeed, they cautiously suggested, "In investigating still existing people like the American Indian we can appeal in the first place to their traditions, which, although sometimes noncommittal and frequently misleading, gain weight when recorded with other data" (Swanton and Dixon 1914:402).

The far-from-wholesale endorsement was too much for the Machian positivist Robert Lowie, who was to succeed Swanton as editor of the *American Anthropologist* in 1923 and was, during the late teens, its book review editor. After claiming that "we are not concerned with the abstract possibility of tradition preserving a knowledge of events, we want to know what historical conclusions may safely be drawn from given oral traditions in ethnological practice" (Lowie 1915:597), Lowie appealed to the exemplification of sound practice provided by attorney-folklorist E. Sidney Hartland (1914). Lowie then proceeded to do nothing other than lay down his absolutistic ban of any even "abstract possibility" of such use of oral tradition: "I cannot attach to oral tradition any histori-

cal value whatsoever under any condition whatsoever. We cannot know them to be true except on the basis of extraneous evidence, and in that case they are superfluous, since linguistic, ethnological, or archeological data suffice to establish the conclusion in question. . . . From the traditions themselves, nothing can be deduced" (1915:598).

Dixon warned that "absolutely unqualified statements like that of Dr. Lowie's are usually dangerous" (1915:599), while Swanton defended folk traditions about movements as revealing at least of the direction of migrations. For Swanton (1915:601), an indicator that corroborated it in nine cases out of ten, when other evidence was available, could therefore be relied upon with some confidence in cases for which no other evidence was available. In Swanton's pragmatic view, some data were better than no data, but for Lowie, "it is our duty to doubt till the facts compel us to affirm" (Lowie:Paul Radin, October 2, 1920), which for Lowie, as for Boas, was never.

Boas's Columbia-Barnard colleague Alexander Goldenweiser, irritated by Lowie's attempted *reductio ad absurdum* in shifting from accepting reports of the direction of migration to crediting everything in creation myths, joined the fray, noting, "Dr. Lowie does not strengthen his case by citing creation myths as proof of deficient historical sense of the Indians. Commonly enough, the Indians themselves distinguish between a myth and a historical tradition. But even were that not so, who would doubt the word of a woman who tells of having witnessed a child being run over by a street car solely on the ground of his knowledge that the woman believes in ghosts?" (1915:764).

Goldenweiser also provided some less-colorful analogies:

> Poor evidence is poor evidence, and the extent to which such evidence can be trusted is determined by the probability of it being true evidence, which again may be estimated from the frequency of agreement between such evidence of an intrinsically higher merit. Just as the physician is guided to his diagnosis of a disease by vague and doubtful symptoms until a positive one is forthcoming, just as the detective follows illusive and contradictory clues before establishing convincing proof of the crime, so the ethnologist in the absence of better evidence follows the lead of tradition until data of higher evidential value serve to confirm or refute his preliminary conjecture or hypothesis. (1915:763–64)

Lowie returned to his battle against native legends having any value as history with more straw men in his December 1916 address as outgoing president of the American Folk-Lore Society (a publication he saw fit to reprint in his collection of his most important ethnological papers), such as "How can the historian beguile himself into the belief that he need only question the natives of a tribe to get at its history?" (Lowie 1960:204) and issued another blanket rejection: "Indian tradition is historically worthless, because the occurrences, possibly real, which it retains are of no historical significance; and because it fails to record accurately, the most momentous happenings" (207).

In 1916 Edward Sapir—who was, with Goldenweiser, a member of what Lowie considered the Boasian "super-intelligentsia" (1959:133)[1]—in laying out the age and area "hypothesis," rebelled against the methodological asceticism of Boas and Lowie.[2] As in his own work on language classifications and phonemics (Sapir 1921a, 1925), Sapir preferred using imperfect data to throwing up his hands and not making historical inferences. In regard to trait distribution, Sapir went beyond Dixon and Swanton to infer the sources (that is, which tribe originated the trait), not just direction of migration, from folklore.

Lowie did not want to review Sapir's book but, having failed to get either Boas or Kroeber to do it, undertook the task. When his evaluation reached Sapir's discussion of "native testimony," Lowie produced an odd introduction: "Dr. Sapir's position with reference to certain moot questions is of interest" (1919:76). Considering that Lowie's own position had recently been attacked in the profession's core journal by four of his elders, including the journal's editor, "moot questions" is quite bold a definition of the situation! Besides which, I wonder how any answer to a "moot question" can be of interest. (Perhaps the word was understood differently a century ago?)

Just as he used creation myths to dismiss limited inferences about population movement in attacking Swanton and Dixon, Lowie set up a Sapirian straw man. Rather than multiple tribes agreeing that some other tribe invented something, Lowie switched to a hypothetical case in which each tribe claimed to have originated the trait and sententiously concluded that "the fact that one of them must be correct does not establish the methodological validity of accepting native traditions as history" (1919:76).

This account, internal to American anthropology (and, indeed, to the

American Anthropologist), does not consider the extent to which both Boas and Lowie had been doing battle against German diffusionism theories, as most completely crystallized in Graebner (1911). Boas rarely cited anyone, but he not only reviewed Graebner's magnum opus very negatively but also reprinted his 1911 review from *Science* in his collection of his major publications, *Race, Language, and Culture* (Boas 1940:295–304). Lowie devoted a presentation at the 1911 annual meeting of the American Folk-Lore Society (published in its journal: Lowie 1913) to attacking the assumptions (and "premature classification" of similarities in culture traits) in Graebner's magnum opus of a conception of mechanical cultural transmission in positing historical transmission of cultural traits (also noted by Boas, whom Lowie did not mention in this connection).

Aftermath

Although Lowie's critique came down (at least in the lore of the field, at least as far as the 1970s) as refutation (in the full Popperian sense) of pseudohistorical methods, closer examination of the whole exchange (or two exchanges) shows that Lowie's dogmatic condemnation of ethnohistorical data fit with the antihistorical wave of the future (ca. 1920) of American anthropology (functionalism, structuralism, culture and personality). It is not that Lowie refuted Swanton and Dixon, only that interests turned away from history of any sort in general and trait distributions in particular (as Lowie 1955:120 recognized). The lack of attention to such evidence in the following decades has less to do with the cogency of Lowie's criticisms than with a shift from historical reconstruction to ahistorical work on modal personalities in intact cultures and synchronic fieldwork focused on social organization.

After completing his combination of linguistic stocks into protofamilies (see Sapir 1921a; Darnell 1971, 1990a), Sapir was at the forefront of "culture and/in personality" theorizing (see Darnell 1990a, 1998b; LaBarre 1958:280–81) and arranged for the main carrier to North America of ahistorical functionalism, A. R. Radcliffe Brown, to come to the University of Chicago (see chapter 10; Sapir:Louis Wirth, November 25, 1931). Lowie would eventually write a national character study (Lowie 1954) and a social organization textbook (Lowie 1948). Yet, at the end of his career, he would recollect, "It was the reconstruction of the ancient primitive life that interested me" (Lowie 1959:169), despite his part in mak-

ing such work seem neither possible nor worth doing (especially in Lowie 1937).

As already mentioned, staggering under the responsibility of salvaging knowledge about numerous California tribes (of considerable language family diversity), Kroeber persisted in collecting checklists of culture elements into the 1930s. By statistical analyses of data gathered by sometimes unenthusiastic students, Kroeber and Harold Driver sought to draw inferences about diffusion and to correlate cultural and environmental areas long after Boas had decreed the study of diffusion ended, and after even Clark Wissler had given it up.

Competing with his Berkeley colleague Frederick Teggart (see chapter 13), Kroeber continued historical correlations on a grand scale (A. Kroeber 1944 was his magnum opus); after World War II Kroeber (1955, 1961) championed Maurice Swadesh's development of a new mechanical discovery procedure for genetic reconstruction of languages, lexicostatisics (see Murray 1994b:207–11).

Above I quoted Kroeber writing that only Swanton and Dixon maintained an interest in tribal migration. Dixon's interests shifted from Native America to Polynesia (see Dixon 1916, 1928). Swanton continued careful sifting of whatever could be recovered from explorers' accounts (notably, Swanton 1932) and Native American traditions (Swanton 1930, 1942, 1952). Lowie recurrently used Swanton's work on the lack of clans in some American Indian tribes to refute evolutionary schema and presented it as the prime exemplar of Boasian ethnology in *The History of Ethnological Theory* (Lowie 1937:145).

And for my generation (and later ones), interest in the possibility of lore containing some valid knowledge (of history, or herbal remedies, and so forth) revived, along with scrutiny of accounts of vanished or greatly modified cultures made by nonprofessional (and still suspect) observers (the whole field of ethnohistory).

For the history of American anthropology, who won in either the short or the long run is less important than is revealing the quite heated disagreement within the fraternity (which it still was in 1915–20) of Boasian anthropologists. Boas's discomfort with Sapir's lumping of language families (from the profusion of the "Powell classification") and the discomfort of the first generation of Boasians with the methods of the later generation—most especially Margaret Mead, with her "vigorous omniscience" and lack of command of the languages of the people about

whom she pronounced—are better known (the latter is discussed in chapter 3). And almost immediately following the dustup over migration accounts, there was a better-remembered conflict between Kroeber (1917, 1918) advocating considering only "superorganic" phenomena and Sapir (1917) defending psychology (no particular psychology, but the individual as the locus of cultural analysis).

I think that "Boasian paradigm" is a meaningful locution, though those trained after the First World War were pointed at other problematics than those that their elders had been aimed at. Boas remained a nihilist about drawing conclusions and even making generalizations. He may have taught (old-fashioned for the day) statistical methods, but he strikes me as having been incapable of thinking statistically, continuing to search for a single counterinstance to "invalidate" any purported pattern across space or time.