

Chapter Title: Anthropology's Hidden Colonialism: Assistants and Their Ethnographers

Book Title: Ethnography in Today's World

Book Subtitle: Color Full Before Color Blind

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Published by: University of Pennsylvania Press

Stable URL: <https://www.jstor.org/stable/j.ctt5hjkgb.8>

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Chapter 5

Anthropology's Hidden Colonialism: Assistants and Their Ethnographers

This chapter concerns hidden issues of scholarly history, the interpersonal context of fieldwork, and intellectual colonialism in the study of “other cultures.” For more than a hundred years, members of the communities and cultures studied by anthropologists have been major providers of information, translation, fieldnotes, and fieldwork. Although professional ethnographers—usually white, mostly male—have normally assumed full authorship for their ethnographic products, the remarkable contribution of these assistants—mainly persons of color—is not widely appreciated or understood. In no major treatment of the discipline is it portrayed as a fundamental part of the history of anthropology.

Anthropologists remain as uncertain about what to call these locally recruited fieldworkers (research assistants, key informants?) as they do the people they study (natives, locals, informants, collaborators, respondents, subjects, “the other”?).¹ This is due in part to our failure to appreciate that anthropologist-assistant relationships have a history, a changing set of relationships that this chapter attempts to outline. It is organized in six sections, each dealing with a stage in the unfolding assistant-anthropologist relationship and each section also focused upon ethnographic research in a major world area. This circular-staircase analysis is not intended to suggest that fieldwork in each region developed along a unique course, still less to provide an exhaustive review of relations with local assistants, but rather to highlight particular cases and sequences in anthropology's collective history.

The Informant: Melanesia

In Bronislaw Malinowski's now famous diary, he recorded his first session with an informant in 1914.² This man was Ahuia Ova, resident of a Motu village near the colonial British New Guinea capital Port Moresby. Ova was already familiar with the role of the seated informant who would answer the ethnographer's pre-set questions, questions most frequently posed from printed guides such as the Royal Anthropological Institute's *Notes and Queries* (first edition 1874). Ova had served in 1904 as informant to C. G. Seligman, an alumnus of the pioneering 1898 Cambridge University Torres Strait Expedition, and was his principal source for the chapters on Koita culture, some hundred and fifty pages of Seligman's massive survey volume *The Melanesians of British New Guinea*, published in 1910.

With another expedition veteran, W. H. R. Rivers, Seligman had put on-the-spot fieldwork with informants at the center of an emerging British anthropology, and he served as mentor to Malinowski in his first extensive New Guinea fieldwork in Mailu in 1914–1915. Malinowski later broke decisively from *Notes and Queries* inquisition in his historic Trobriand Islands fieldwork in 1915–1918 and redefined the informant as someone to listen to and speak with in natural, everyday settings, as well as in arranged interviews.³ Malinowski would influence the development of anthropological fieldwork immensely, but he did not work again with Ova. Others did, however, and Ahuia Ova's autobiography, as edited by F. E. Williams, was followed by an update by Cyril Belshaw in 1951, the year of Ova's death, and research by Reg Shelley.⁴ These writings allow us to envision the career of a man caught up in the throes of colonial society, one who played an important role in working with a series of New Guinea coastal ethnographers.

Ahuia Ova is an example of the local cultural expert. There would be many more such men and women informing anthropologists in Melanesia and other world areas over the succeeding years. The dilemma for ethnographers who found it difficult not to acknowledge their dependence upon persons such as Ova has been addressed in various ways in Melanesian studies. Ralph Bulmer enlisted and credited a local collaborator, Saem Majnep, in a coauthored study, *Birds of My Kalam Country* (1977). Other anthropologists have made their Ahuia Ovas the subject of book-length life histories: Roger Keesing in *'Elota's Story: The Life and Times of a Solomon Islands Big Man* (1978) and Andrew Strathern in *Ongka: A Self Account by a New Guinea Big-Man* (1979).⁵

The Native's Point of View: North America

In his Northwest Coast fieldwork, beginning in the 1880s, Franz Boas envisioned his goal as capturing the native's point of view through texts that recorded myths and folktales or that described ritual and technological activities. His informants included the literate *metis* George Hunt, raised in a Kwakwaka'wakw (Kwakiutl) village, whom Boas taught to transcribe Kwakwala texts on his own. A stream of Hunt's materials arrived by mail in New York to be edited by Boas in several volumes, some of which listed Hunt as coauthor.⁶

Partnerships like this characterized the work of many of Boas's students. In his preface to *Havasupai Ethnography* (1928), the resolute Boasian Leslie Spier wrote, "My principal informant, Sinyella, was an energetic and well-informed old man, aged 71. So much material is directly autobiographical, that it would have been possible to present this sketch as an account of his life."⁷ This option was not taken, however, and the volume consists of topical chapters ranging from "Economic Life" to "Social Relations" to "Historical Tales," with Spier as sole author.

The Boas-Hunt and Spier-Sinyella collaborations were later surpassed in a monumental ethnographic trilogy on the Ingalik Indians of Alaska, a far better organized achievement than Boas's Kwakiutl corpus. These volumes list anthropologist Cornelius Osgood as author, yet he details carefully the role of Billy Williams, his Ingalik informant in 1934 and 1937. Williams came to understand fully the Yale anthropologist's objectives, and he was responsible, in Osgood's words, for "the great majority of my data," including "more than ninety-five percent" of the descriptions of ritual.⁸

The Boasian single-informant model clearly influenced many of those who became prominent figures as the discipline of anthropology achieved academic standing in the early decades of the twentieth century. Meanwhile, another ethnographic role for American Indians developed at the Bureau of American Ethnology (BAE) and other museum-oriented research centers. Here Native Americans became staff members, writing the ethnography of their own and closely related peoples. The Plains Indians Francis La Flesche, who was published in seven BAE volumes, and James Murie were two of the most industrious,⁹ but their careers were not without affronts of a racist and intellectually colonialist sort.

La Flesche normally wore tie and suit to his Smithsonian Institution office. Yet, as his biographer Margot Liberty has revealed, when he died in 1932, his *American Anthropologist* obituary photograph was retouched to

remove his usual attire, leaving only the buffalo robe he had once been persuaded to don over his shirt and tie for this single shot. We can compare the original with the altered version in the plates accompanying Liberty's two biographical essays.¹⁰ In death, La Flesche's Indian identity was seen as superseding his professional accomplishments.

Attached to the Chicago Field Museum, the BAE, and the American Museum of Natural History, Murie worked early in the century with white anthropologists Alice Fletcher, George Dorsey, and Clark Wissler, each of whom (to varying degrees) acknowledged his contributions. But few of Murie's own manuscripts were published in his lifetime: his two-volume *Ceremonies of the Pawnee*, completed in 1921, finally appeared in 1981. Ralph Linton, however, used Murie's fieldnotes (deposited at the Chicago museum) in writing five papers on the Pawnee, a group he had never worked among, and without any mention of Murie.

Murie died in 1921, and Linton's papers appeared between 1922 and 1926. In their biography of Linton, Adelin Linton and Charles Wagley imply that Linton acknowledged both Murie and Dorsey in his Pawnee publications. Douglas Parks, however, states that Linton drew upon and quoted Murie's unpublished materials but did not acknowledge him in any of his five Pawnee papers, although he did cite unpublished work by Dorsey. This is certainly the case in the two of Linton's five papers that I have examined; in each, "unpublished" material is attributed to Dorsey, and Murie's name appears nowhere at all.¹¹

More creditably to the discipline, the intimate informant-anthropologist partnerships that Boas had set in motion led one of his students, Paul Radin, as well as later generations, to find ways to acknowledge more fully the assistants upon whom their work depended. These assistants continued to be included in prefaces and introductions but also in fieldwork memoirs and, most tellingly in my opinion, book-length life histories. Beginning with Radin's *Autobiography of a Winnebago Indian* (1920), this genre had taken on a life of its own by the 1930s. It is ironic, perhaps, that the most detailed of these publications—Walter Dyk's *Son of Old Man Hat* (1938), Clellan Ford's *Smoke from Their Fires* (1941), and Leo Simmons's *Sun Chief* (1942)—like Radin's, vividly depict the clash and blend between Indian and U.S. or Canadian cultures that Boas studiously neglected.¹² In these volumes, the native's point of view overturned the cultural reconstruction and salvage goals that Boas had set for Americanist studies and that remained evident in many conventional ethnographic monographs.

The Assistant: Indonesia

With the blossoming of more fieldwork in the active Malinowskian style, and among living cultures as opposed to memory cultures, the seated query responder, text reciter, or life-history narrator began to recede in significance.¹³ For the new 1930s ethnographers in the Pacific, Asia, Africa, and Latin America, it was numbers of persons in daily and ceremonial activities who were now objects of observation and interlocutors in conversation. At the same time, it became common to enlist, and pay, one local cultural guide as a member of the ethnographic team. This person might translate, introduce, negotiate, gather facts, and even conduct interviews and write fieldnotes to facilitate the work of the professional ethnographer.

I Made Kaler, a Balinese who joined Margaret Mead and Gregory Bateson's field team in 1936–1939, filled this role par excellence, as Mead recounts vividly in her autobiography *Blackberry Winter* (1972) and her Balinese *Letters from the Field* (1977). A loose thread here, however, appears in the letter I Made Kaler wrote Mead in 1938 asking her if he could publish his own study of one aspect of Balinese culture: “Anyhow with this letter I do you a request. But when you think it will be bad for your Bali book, I won't do it. Do you think I can write a short article about the cockfight. But I tell you if you think this action will be a bit bad for your book, I won't do it, I don't want to make profit of any of the stuff we have collected. It belongs all to you.”¹⁴

I do not know what happened to this Balinese analysis of the Balinese cockfight. “The stuff we have collected belongs all to you.” We, anthropologists and assistants, lost something irreplaceable here. A much later analysis of this Balinese cultural focus by Clifford Geertz has become famous and has stimulated much commentary by other anthropologists.¹⁵ Geertz's fieldwork methods in Indonesia involved a broad range of informants and much less reliance on a key assistant than had Mead and Bateson's approach.¹⁶ Still, it is unfortunate that I Made Kaler's Balinese perspective has never been published in its own right as part of a more inclusive ethnographic record.

Collaboration: Mesoamerica

A more complex set of relationships between locals and ethnographers evolved during the 1930s research of American anthropologists in Mesoamerica. Robert Redfield worked first solely with his wife Margaret

Park Redfield in their aborted 1926–1927 study of the Nahuatl-speaking Mexican town Tepotzlan. They used informants in then-standard interview fashion but also drew upon historical and linguistic writings of Tepotztécans literate in Spanish.¹⁷ In Redfield's 1930–1933 study of Chan Kom, he collaborated with local schoolteacher Alfonso Villa Rojas, who spoke both Spanish and Mayan, and the resulting monograph appeared as a coauthored volume. Villa went on to study another Yucatan community on his own as part of Redfield's four-community research project, and his *Maya of East Central Quintana Roo* (1945) became one of many publications from this eminent Mexican anthropologist.¹⁸

Following the Yucatan project, Redfield and his American student Sol Tax worked in Guatemala from 1934 to 1941. Redfield continually prodded Tax to conduct more intensive fieldwork and to pay greater attention to natural conversations, an approach Redfield had not used in Tepotzlan and that he credited to Villa and not to Malinowski, with whose work he was certainly familiar. Tax had difficulty embracing this advice, preferring to develop close working relationships with Indian collaborators such as Santiago Yach, whose life history he recorded, and Juan Rosales, whom he employed to conduct research and record fieldnotes.¹⁹

Rosales also shared his fieldnotes with Charles Wagley, who did fieldwork in Guatemala in 1937, and some of these notes were appended to Wagley's *Social and Religious Life of a Guatemalan Village* (1949). But Wagley's principal fieldwork assistant was Carmelo San Jose, a Spanish- and Mayan-speaking Ladino. Earlier in the 1930s, Boas student Ruth Bunzel also worked in Guatemala, in Chichicastenango, a town Tax later studied. Bunzel used the single-informant text-transcription approach of her teacher. This variety of collaborative relationships offers an intriguing interpretive key to the body of Guatemalan ethnography from this coterie of researchers.²⁰

Post–World War II ethnography in Mesoamerica witnessed further evolution along the lines established in the 1930s. Oscar Lewis, with a large team that included Mexican researchers, restudied Tepotzlan in 1943–1947 and produced a major body of publications, some of which challenged Redfield's earlier conclusions. Relationships established by Redfield and Tax brought several Mexican and Guatemalan anthropologists to the University of Chicago. The extensive Chiapas research project launched from Harvard in 1957 employed old and new methods of using informants, including teaching them to use tape recorders and then to transcribe typewritten bilingual Mayan-Spanish texts, which they supplied for analysis to the U.S.

professional and student ethnographers. Later, James Sexton used written diaries and tape-recorded accounts to dramatic effect in portraying the turbulent change witnessed by one Guatemalan Indian informant: the results of Protestant evangelization, military rule, and endemic violence.²¹

Decolonization: Africa

In the early twentieth century, a body of works by African colonial subjects addressed issues that anthropologists would return to decades later—land tenure, politics, customary law, race relations, and “social change.” Books by Gold Coast (later Ghana) authors, such as *Fanti Customary Law* (1897) and *Fanti National Constitution* (1906) by John Mensah Sarbah and *Gold Coast Native Institutions* (1903) by J. E. Casely Hayford, and also by South African authors, such as *Native Life in South Africa* (1916) by Sol Plaatje and *The Black Problem* (1920) by D. D. T. Jabavu, watered the seedlings of African nationalism but not of anthropology. By the 1920s, as Robert Thornton has shown, a combination of missionary, academic professional, and political interests among European ruling groups had resulted in an anthropology of Africa that portrayed a primitive, timeless, and tribal present.²² This contrasted greatly with the themes that preoccupied contemporary African writers.²³

The opposed orientations of these two traditions of writing about Africa are evident in the ethnographic essays on the Gã published between 1910 and 1920 by A. B. Quartey-Papafio in the *British Journal of the Africa Society*²⁴ and the monographs two decades later by the English government ethnographer and Malinowski student M. J. Field—*Religion and Medicine of the Gã People* (1937) and *Social Organization of the Gã People* (1940). Both authors wrote about the indigenous inhabitants of Accra, the capital city of Ghana. While Quartey-Papafio, himself a Gã, dealt with kinship, political structure, and ritual, as did Field, he also described modern occupations, labor migration of educated Gã to other African colonies, and political maneuvers against the British occupiers by Gã leaders in collusion with Hausa and Yoruba immigrants. Field, who did not cite Quartey-Papafio, presented an altogether more primitive and tradition-bound portrait of Gã culture and society.

A major attempt to correct anthropology’s false start in Africa was evident in the work in Northern Rhodesia under the Rhodes-Livingstone Institute banner by British social anthropologists, many of leftward political

attachments, before and after World War II. Their urban research led to a stream of publications, notably from A. L. Epstein and J. Clyde Mitchell. For both political and practical reasons, the anthropologists depended upon African research assistants. These men went where whites could not and recorded extensive fieldnotes, which were used in the resulting analyses. Textual acknowledgments traced the assistants' accomplishments, but the most revealing and grateful of these was from American anthropologist Hortense Powdermaker, regarding Frederick Phiri, a Rhodes-Livingstone Institute assistant who worked with her in Luanshya in 1953–1954. Her books *Copper Town: Changing Africa* (1962) and *Stranger and Friend* (1967) describe her and Phiri's fieldwork in detail. Phiri later pursued higher education in Britain in community relations but not in anthropology.²⁵

Few Africans became anthropologists until the Decade of Independence in the 1960s. Jomo Kenyatta, who studied with Malinowski in the 1930s, was an exception; his *Facing Mount Kenya* (1938) returned to land tenure issues raised decades earlier by Plaatje, Casely Hayford, and others. The transition from assistants to professionals was not accomplished gradually, although a few publications by assistants at African research institutes staffed mainly by whites did begin to appear in the 1950s and 1960s. PhD-level careers were few, but the work of professional anthropologists Archie Mafeje, a South African, and Maxwell Owusu, a Ghanaian, exemplified the persisting concern with politics and decolonization begun by their countrymen early in the century.²⁶

The continuing reluctance of some white anthropologists to accord professional equality to their African counterparts has been described by Ugandan anthropologist Christine Obbo. Her own work from the 1970s onward has added to the rich heritage of African interests in Africa and matches the best urban ethnography by expatriates. Still, in a 1990 essay, she depicts the ungracious and uncollegial attempts of some white researchers throughout her career to surreptitiously appropriate and use her fieldnotes and analyses.²⁷

Application: United States

The desire to use anthropological research to address social problems in the United States led to the formation of the Society for Applied Anthropology in 1941 and to continuing work since by action, advocacy, and practicing anthropologists. Many of the first applied anthropologists were strongly

influenced by the studies of human interaction pioneered in the 1930s by a group of Harvard anthropologists, several of whom became leaders in the use of anthropology in industry and other fields.²⁸

In this group was William Foote Whyte, whose *Street Corner Society* (1943) justly became an ethnographic classic. Whyte's close relationship to Doc, an Italian American working-class Bostonian who collaborated in his fieldwork, is described in this book and in later writing that traces Doc's career.²⁹ Such an insider-outsider pairing is widely taken as an ideal by many ethnographers, even if the payoffs for each partner have not received much attention in anthropology's recurrent "rethinkings," "critiques," and "crises" since the 1960s.

Several of the founding applied group were among the twenty-one anthropologists who worked as researchers in U.S. government concentration camps for Japanese Americans between 1942 and 1946. Their job was to conduct studies of the culture and social organization of the internees. It was impossible for white anthropologists to move freely in the camps, and Japanese American research assistants, among them university graduates with social science training, provided the bulk of the data.

Despite a large body of resulting publications during the 1940s,³⁰ this formative period in the application of anthropology and in outsider-insider collaboration received little scrutiny through the 1950s or during the "radical anthropology" reorientations of the 1960s and 1970s. The first critical assessments of this period appeared in anthropology journals only in the 1980s.³¹ Few anthropologists even today understand how a large group of their colleagues, with several prominent names among them, applied their craft in the government camps. Nor, I venture, do courses in the history of anthropology devote much attention to their efforts.

None of the camp anthropologists continued research on Japanese Americans after World War II. Only one Japanese American was to be awarded a PhD in anthropology for work on the topic that had engaged so many of his senior colleagues during the war years—Toshio Yatsushiro, for *Political and Socio-cultural Issues at Poston and Manzanar Relocation Centers* (1953), a Cornell University dissertation.³² (A handful of Japanese Americans did produce sociology dissertations on the wartime experience.³³) The few other Japanese Americans who became professional anthropologists in the immediate postwar years studied such cultures as the Navajo and the Nias Islanders of Indonesia. Only in the 1970s did a second Japanese American anthropologist pursue PhD research concerning her own group.³⁴ A

rich literature of memoirs and novels about the concentration camp experience by Japanese Americans themselves, however, has emerged.³⁵ Qualities that mark these writings by “natives” about their own culture are ones that many anthropologists today view as enriching ethnography.

Conclusion

By the 1930s, anthropologists of diverse non-Western nationality and ancestry were making important contributions as professionals to the emerging discipline. They included Sir Peter Buck (Te Rangi Hiroa), Kenyatta, Fei Xiaotong, A. Aiyappan, D. N. Majumdar, Manuel Gamio, Julio de la Fuente, Villa Rojas, William Jones, Ella Deloria, Allison Davis, St. Clair Drake, and Arthur Huff Fauset.³⁶ In the United States, since the 1930s, their ranks have grown proportionately scarcely at all, and several notable such scholars have faced limited career choices.³⁷ The record of the discipline remains meager in terms of training and of according professional standing to ethnographers of color who study their own peoples. As of 1989, some 93 percent of anthropologists in full-time U.S. faculty positions were white, a figure even higher than the 89 percent white for all full-time faculty in U.S. institutions of higher education.³⁸ In the academy, one might conclude, the “native’s point of view” is still largely unwelcomed from the natives themselves.

Anthropology’s future must do better in this regard than anthropology’s past. If the discipline is to move into a phase of political maturity and responsibility in a color-full world, we need to face up to our historical antecedents. And as we do, we need to recognize that we have a richer history than some of our ancestors have acknowledged or wished to be recorded. It seems to me no longer possible that the history of anthropology should be taught without including in it Ahuia Ova, George Hunt, Billy Williams, Francis La Flesche, James Murie, I Made Kaler, Alfonso Villa Rojas, Juan Rosales, A. B. Quartey-Papafio, the Rhodes-Livingstone Institute African staff members (who, beyond Frederick Phiri, need to be named), Doc, the Japanese American internee fieldworkers (who, in addition to Toshio Yatsushiro, also need to be named), and other assistants and native scholars who were producers of our ethnographic heritage and history.

Globally representative voices are there—inadequately recognized, or disciplinarily silenced in various ways (like Murie, Kaler, and Quartey-Papafio), but still there. Ethnographers and assistants together made anthropology. We need to revise our textbooks and write new ones.³⁹