

# Introduction Other Argonauts

## Chapters in the History of Pre-Malinowskian Ethnography

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Predating Malinowski's *Argonauts of the Western Pacific* (1922) by fifteen years, New Zealand frontier man and salvage ethnographer Elsdon Best (1856–1931) made a comparison between a scriptless Oceanic people and the heroic navigators of ancient Greek mythology:

And how came man to the land of the Maori and the *moa*? Whence came the Children of the Mist?

*Tena!* Far away across the dark, wild waves of the Sea of Kiwa, away beyond the parts where the sky hangs down, there floats a primitive vessel upon the surging waters. It is a hewn canoe, of great length, and decked with many a strange device, such as were used by Polynesian Vikings in the days when the world was wide. . . . How full of import is the progress of that primitive bark. . . . For it is the old, old story of the Argonauts, of happening on a new world, of the conquest of the earth by man. (Best 1925: 2, 4, 5)

Considering that the manuscript of *Tuhoe: The Children of the Mist* (1925) was completed by 1907, it is clear that Best did not borrow the idea from Bronisław Malinowski (1884–1942). In fact, Best had already compared Māori to the Argonauts of Greek mythology as early as 1894, in the *New Zealand Mail*.<sup>1</sup> This coincidence is symbolic as it tells us something about the two men's respective destinies in the history of anthropology: undisputed fame in the case of Malinowski, significant oblivion in that of Best—except in New Zealand/Aotearoa and within the specialized field of Māori studies. To be sure, the words of one of Best's informants were famously quoted by Marcel Mauss (1872–1950) in *Essai sur le don*—better known as

*The Gift*: “I will now speak of the *hau* . . .” (cited in Mauss 2016 [1925]: 70).<sup>2</sup> One would expect Elsdon Best’s name to be an indispensable reference for any anthropologist, but the fact that a small parcel of his ethnography has become a part of the canon, witness the periodical *HAU: Journal of Ethnographic Theory*, does not mean that his immense work is known, let alone read. Unlike Malinowski’s, his Argonauts are full of dust.

The present collection tries to recover other “Argonauts” or ethnographic accounts from the late nineteenth and early twentieth centuries. This provocative expression applies to forgotten or neglected monographies produced by ethnographers whose work, surpassed and overshadowed by that of later anthropologists, may both enlighten and question the dichotomy between canonic models of *writing culture* and “pre-Malinowskian” ones. At a time when anthropologists worldwide continually claim new fieldwork experiences and ethnographic results, from antipositivist to poststructural, from “gone native” to compassionate, a second chance should be given to older texts through a critical and creative combination of historicism and presentism. The disparate sensibilities of twenty-first-century practitioners reveal more than ever that once victorious criteria of professionalism are inadequate to assess the significance of previous ethnographic studies. Some deep-rooted assumptions are at stake, including the idea that intensive fieldwork in a single context by a single individual, with its corresponding output, the monograph, were twentieth-century inventions, and that nineteenth-century ethnographies were mere travelogues, expeditionary surveys or defective, fragmentary descriptions.

The present volume includes twelve case studies from a surprisingly large field. Shorter illustrations in this introduction and an appendix containing a selected bibliography of 365 ethnographic accounts, produced by 220 ethnographers, demonstrate how ethnography thrived during the fifty years between ca. 1870 and ca. 1922. The core idea underlying our project is that “pre-Malinowskian” ethnographies are a fundamental part of the history of anthropology, each ethnographic account containing several layers of meaning, style, and content that inspire open-ended readings and are projectable into the future.

## “Four Ways” and World Anthropologies

The search for “the first true fieldworkers” (Eriksen and Nielsen 2001: 26–27) involves several hazards, even when there is a nuanced

understanding of the complex historical processes underlying the development of ethnography as a scholarly field. One is the risk of Anglocentrism, viewing the history of the discipline mostly from an Anglo-American perspective. This is not to imply that historians of anthropology are unaware of this danger, but when scholars make it explicit that they are only addressing English-speaking countries, this does not necessarily prevent them and their readers from the temptation of generalizing from there, however subconsciously. Viewing British and North American anthropology as “major traditions” (Kuklick 2008) outshining less prominent ones worldwide affects the teaching of the history of the discipline. In the present volume, Anglophone monographs have been selected as case studies, but not to the exclusion of ethnographies from other settings conferring diachronic depth to lesser-known “minor traditions” brought to the fore by the world anthropologies paradigm.<sup>3</sup>

Anthropology has been portrayed as *One Discipline, Four Ways* in reference to the British, American, French, and German research traditions (Barth et al. 2005). But what about Brazilian, Italian, Chinese, Japanese, Indian, Nigerian, or Russian anthropologies, for example? The focus on four “major traditions” overshadows other important anthropological schools, in Europe and beyond. Even the history of German-speaking anthropology has fallen into oblivion and, due to two world wars, is no longer part of the anthropological canon (Eidson 2017: 49). Andre Gingrich refers to it as a “non-tradition of good anthropology” that has been “forgotten, repressed, and noticed only after tremendous time lags” (Gingrich 2005: 103; see Gingrich 2017). For example, the celebrity of Baldwin Spencer (1860–1929) and Francis Gillen (1855–1912) obfuscated the status of nonanglophone ethnographers, both in their own era and throughout the twentieth century, particularly of Carl Strehlow (1871–1922), their “rival” in the Arunta/Arrernte context.

In a 2019 special issue dedicated to “German-Speaking Anthropologists in Latin America, 1884–1945,” editors Han F. Vermeulen, Cláudio Costa Pinheiro, and Peter Schröder identify a similar case of “amnesia” (p. 79) concerning the pivotal role of ethnographers from German-speaking countries working among Indigenous groups in Brazil and elsewhere, mainly in the period between 1880 and the end of World War I.<sup>4</sup> They speak of a “Great Age” represented by ethnographers such as Karl von den Steinen (1855–1929), Konrad Theodor Preuss (1869–1938), Theodor Koch-Grünberg (1872–1924), and Max Schmidt (1874–1950)—up to their last representative, Curt Nimuendajú (1883–1945). Despite several other efforts to unveil their

ethnographic experiences, whether expeditionary or individual, and recent attempts to reassess their place in the history of anthropology as “forerunners of modern fieldwork,” these figures still have a “restricted visibility” (Vermeulen et al. 2019: 72, 80).

In France, a good example of obliterated ethnography is Arnold Van Gennep’s (1873–1957) monograph *En Algérie* (1914), which contains an important introductory section titled “Comment on enquête” (How to conduct field research): “I travelled through Algeria for five months: July–August 1911 and April–June 1912. That was too much, as now things that I thought were very simple are appearing to me with a distressing complexity. Or too little, because it would take years to untangle these complexities finally discerned” (Van Gennep 1914: 7–8). Christine Laurière highlights Van Gennep’s constant “pleas for direct observation, for fieldwork, for the attention to detail” (Laurière 2021: 10). Known internationally for his classic study *Les rites de passage* (1909), Van Gennep distinguished “one single method, equally applicable in all countries and from morning to evening.” Its cornerstones were empathy, respect, and conviviality: “It is only necessary to be introduced by individuals who have the confidence of the locals, to bend to the traditional rules of politeness, to lose track of time, to avoid any impatience, to avoid asking direct questions and to proceed by approximation” (Van Gennep 1912: 611; see also Van Gennep 1913).

When other European languages (not to mention non-European ones) are taken into account, the predicament of Anglocentrism becomes all the more obvious. At the same time, one should not lose sight of the extraordinary magnitude of the English-language ethnographic archive, namely, when the writings of amateur ethnographers are also considered. A prime example is *The Nuer of the Upper Nile Province* (1923), the first monograph on the celebrated Nuer, authored by army officer and colonial administrator Henry Cecil Jackson (1883–1962). First appearing in the *Sudan Notes and Records*, and subsequently published by El Hadara Printing Press in the Anglo-Egyptian Sudan, it is a rare book to this day and has never been properly analyzed by historians of anthropology. Both within and beyond the so-called major traditions, regionally focused studies on the history of anthropology often unveil long lists of ethnographers and ethnographies that require case-by-case considerations.<sup>5</sup> In *A Hundred Years of Anthropology*, T. K. Penniman faced this problem in lucid terms: “It is impossible to do more than mention the nature of evidence from the field without compiling an encyclopaedia” (1974 [1935]: 22).

## The Anthropological Canon and the Drama of Selection

Historians of anthropology are well positioned to sense the drama of drastic selections, as when George W. Stocking Jr. (1928–2013) stated, in *After Tylor*, that he was “acutely conscious that much has been scanted or excluded” (1995: xvi). In *Victorian Anthropology* (1987), Stocking selected only three cases—George Grey (1812–98), Thomas Williams (1815–91), and Francis Galton (1822–1911)—to evoke the providers of data for the evolutionist founding fathers who were the actual subject of his book. In *After Tylor* (1995), the missionary ethnographers Lorimer Fison (1832–1907) and Robert Henry Codrington (1830–1922) stood for nineteenth-century fieldwork, along with Spencer and Gillen and Alfred C. Haddon (1855–1940). For the early twentieth century, Stocking highlighted the case of eight trained anthropologists, British or connected to British academia and more or less fallen into oblivion, who undertook “the intensive study of a limited area before the Great War” (1995: 117–19), namely, Gerald C. Wheeler (1872–1943), Rafael Karsten (1879–1956), Gunnar Landtman (1878–1940), John Layard (1891–1974), Maria Czaplicka (1886–1921), Barbara Freire-Marreco (1879–1967), Diamond Jenness (1886–1969), and Robert S. Rattray (1881–1938), while developing, in particular, the cases of Arthur M. Hocart (1883–1939) and Edward Westermarck (1862–1939). Three of these ethnographic anthropologists, Czaplicka, Rattray, and Westermarck, return in individual chapters of the present book (see Kubica, McFate, and Shankland, this volume).

In his seminal paper “The Ethnographer’s Magic: Fieldwork in British Anthropology from Tylor to Malinowski” (1983), Stocking concluded:

Something more than delayed or institutionally marginal careers . . . would seem to be involved in the lapsed remembrance of . . . other academic ethnographers of Malinowski’s generation. Although some of them . . . are revealed in their field notes as extremely sensitive and reflective practical methodologists, their early monographs did not present them as self-conscious ethnographic innovators. (Stocking 1983: 31–32)

The recent tendency to recover forgotten or marginal figures, both within and beyond the four “major” traditions, is theorized in Richard Handler’s *Excluded Ancestors* (2000). In its introduction, Handler reminds readers that “the same processes of inclusion and exclusion that affect the discipline of anthropology also affect [the] history of anthropology as a subfield.” It would be impossible in any single

volume to address “all the categorical omissions (not to mention individual forgotten ancestors) we might imagine” (Handler 2000: 8). The need for a more inclusive picture has been a guiding principle since the history of anthropology affirmed itself as a field during the 1960s and 1970s, with Stocking and Adam Kuper (b. 1941) taking the lead. Sometimes, attempts to account for lesser-known figures have been justified by the need to contextualize more prominent ones, but prominence, whether living or posthumous, is a relative condition (see Bieder 1986: xii; Stocking 1974). Meanwhile, other formulas respond to the same goal, such as “Neglected Pasts” (Kuklick 2008) or “Missing Ancestors and Missing Narratives,” the title of a 2007 article in which Andrew Lyons addresses the problem in the following way: “We ask why some names, careers, and narratives are included in or excluded from histories of anthropology. These processes obviously influence our choices as to which books we shall read and which messages we shall heed. . . . A decision to omit someone from a historical survey may be overdetermined by many disciplinary and political traditions” (Lyons 2007: 148).

Have pre-Malinowskian ethnographers of the late nineteenth and the early twentieth centuries been excluded? The answer to this question depends on whether or not one considers the countless but scattered contributions to the reassessment of this or that particular case that may be found in biographies and monographic volumes, specialized journals and books within specific area studies or national traditions, online encyclopedias, and dictionaries. To name one example, Inuit studies specialists are fully aware of earlier ethnographies, such as the monumental descriptive volumes that were written in the 1870s, 1880s, and 1890s—mostly by natural scientists who, like Franz Boas (1858–1942), converted to anthropology (see Lewis, this volume). The title of the collective volume *Early Inuit Studies*, edited by Igor Krupnik in 2016, makes this historical sensibility explicit. The Danish administrator of South Greenland, Hinrich Rink (1819–93), is “widely recognized as one of the founding fathers of Eskimology” (Marquardt 2016: 35). Publishing extensively, both in Danish and in English, Rink’s “best-known” work is the two-volume monograph *The Eskimo Tribes* (1887–91). To nonspecialists, however, he is a complete stranger.

A glimpse of the ethnographic archive’s vastness has been made easier by the digital turn, and the present collection of essays aims at making pre-Malinowskian ethnography more visible. In view of the vast number of case studies that could have been considered for the present volume, our selection from the ethnographic archive is like the proverbial tip of the iceberg. In the process of reaching out

to the international community of historians of anthropology, the editors of this volume were confronted with all kinds of warnings concerning this or that forgotten ethnographer whose omission from consideration would be unfair. “Do you include Bronisław Piłsudski in your volume? Hope you did!”<sup>6</sup>

## Revisiting the “Revolution in Anthropology”

The history of anthropology in the nineteenth century is often equated with armchair anthropology because, as Thomas H. Eriksen and Finn S. Nielsen sustain, “the vast majority of anthropologists gathered their data through correspondence with colonial administrators, settlers, officers, missionaries, and other ‘whites’ living in exotic places” (2001: 24). This is only part of the story as the cases of two well-known founding fathers demonstrate. Lewis Henry Morgan (1818–81) was able to claim for himself the status of “witness” in the foreword to his ethnographic monograph on *The League of the Ho-dé-no-sau-nee, or Iroquois* thanks to his “frequent intercourse with the descendants of the Iroquois” (1851: x). The fact that this so-called armchair anthropologist was also a fieldworker is viewed as an exception—but the same applies to Edward B. Tylor’s (1832–1917) Mexican experience and the resulting volume, *Anahuac: or, Mexico and the Mexicans, Ancient and Modern* (1861).<sup>7</sup> As to the ethnographic encounters and monographic accounts of amateur observers, they are often omitted under the pretext of their “uneven quality” (Eriksen and Nielsen 2001: 24), amalgamated as “mostly prejudiced and always inadequate” (Hogbin 1958: 18) and therefore undeserving of much attention or, at best, briefly and selectively enumerated. Disciplinary pasts may be subject to an unconscious myopia or to deliberate forms of exclusion in power-imbued narratives. There is a prevailing, even “conventional” tendency, as Efram Sera-Shriar puts it, “to depict the history of anthropology as fragmented into divergent methodological epochs” (2013: 3). Likewise, David Shankland notes that anthropology “sustains itself in its popular discourse by dividing its past into a number of stages each of which may be regarded as having been safely surpassed” (2019: 51). The typical narrative “goes something like this,” he adds: “that anthropology contained a number of leading figures in the nineteenth century who proposed a form of evolutionism. These gave way to a ‘revolution’ in the 1920s headed by Malinowski, which created social anthropology and pioneered ‘real’ fieldwork” (ibid.).

According to Shankland, “Perhaps the archetypal work that sustains this understanding of the history, in its initial stages at least, of social anthropology is that by Adam Kuper” (ibid.: 52). Titled *Anthropology and Anthropologists: The British School 1922–1972*, Kuper’s classic has gone through four editions (1973, 1983, 1996, 2015) and several reprints. His first chapter, titled “Malinowski,” opens with powerful words: “Malinowski has a strong claim to being the founder of the profession of social anthropology in Britain, for he established its distinctive apprenticeship—intensive fieldwork in an exotic community” (1996 [1973]: 1). Kuper recalls the calls for professional ethnographic fieldwork by the generation preceding Malinowski. Despite the fact that “very little professional work involved more than a few days in any exotic area,” forcing ethnographers “to rely upon interpreters, or évolué informants,” Kuper added, “this represented a departure from the traditional system, whereby—as Marett described it—‘The man in the study busily propounded questions which only the man in the field could answer, and in the light of the answers that poured in from the field the study as busily revised its questions.’ It was now realized that the man in the field should be expert in the discipline, and that the European resident in the tropics was not generally a reliable informant. Some of these, particularly among the missionaries, had produced masterly ethnographies, but they were very much the exception; and even the best of them relied too heavily upon selected informants” (ibid.: 5–6).

While Kuper is right that Malinowski and A. R. Radcliffe-Brown (1881–1955) carried out intensive fieldwork and held important new chairs in anthropology at the London School of Economics (LSE) in 1927–38 and in social anthropology at the University of Oxford in 1937–46, respectively, his narrative does not accord enough attention to the so-called “exceptions” in the late nineteenth and early twentieth centuries, whether by professional or amateur ethnographers. His *Anthropology and Anthropologists* inadvertently influenced readers into formulating more simplified ways of dealing with the complexity of the discipline’s archive. These are frequent in textbooks on the history of anthropology that cover several research traditions, as well as in guides to the theory and practice of ethnography (see, for example, Robben 2007: 30; Madden 2010: 27).

An earlier candidate for archetypal work on the epistemic shift brought about by Malinowski was Ian Jarvie, whose influential book *The Revolution in Anthropology* (1964) played a major role in consolidating a discontinuous perception of the history of the discipline, according to which there was “a shift of attention from speculative



genetic theories of human society . . . to intensive, thorough and accurate field-work” (Gellner 1964: v; see Shankland, this volume).<sup>8</sup> Jarvie did so by emphasizing Malinowski’s coup de grâce to the era of armchair anthropology and by identifying James George Frazer (1854–1941) as the revolution’s symbolical scapegoat. Ironically, this echoed the slain godly kings of *The Golden Bough* (1890): “The first battle-cry of the revolution is ‘kill the chief-priest (or father) and his gang.’ Translated this reads: ‘overthrow the influence of these victorian intellectualist evolutionists’ . . . Bronislaw Malinowski plotted and directed the revolution. It was a genuine revolution, aiming to overthrow the establishment of Frazer and Tylor and their ideas; but mainly it was against Frazer” (Jarvie 1964: 43, 173; see also 1–2, 32–33). Jarvie’s idea that Malinowskian ethnography overthrew Frazerian anthropology is unfair in that it practically ignores pre-Malinowskian ethnography. Indeed, the transition toward a new, field-centered anthropology appears more clear-cut, if not sudden and personal, when previous ethnographies are left out of the picture.<sup>9</sup>

In his enthusiasm, Jarvie turned Malinowski’s critique of verandah ethnography into a metaphorical reference to the “verandah of western society,” of the evolutionists or, for that matter, the diffusionists who refused to go into the field: “Into the quarrel between these two schools of thought stepped Malinowski. ‘You both sit on the verandah spinning your theories and empty disputes,’ he seems to have said. . . . ‘Come down from the verandah of western society and look at men everywhere,’ he says. . . . speculation on the verandah is not science, science is observation and description” (Jarvie 1964: 11, 13). In fact, Malinowski made quite explicit that he had in mind the actual “verandah of the missionary compound, government station, or planter’s bungalow, where, armed with pencil and notebook and at times with a whisky and soda, he [the anthropologist] has been accustomed to collect statements from informants, write down stories, and fill out sheets of paper with savage texts” (Malinowski 1926: 122–23).<sup>10</sup> By disregarding the actual verandahs in the colonial settings where ethnography was done, Jarvie’s abstract verandah of armchair anthropologists may be taken as a metaphor for something else: a deeply ingrained disregard for nineteenth-century and early twentieth-century ethnography, as if it were negligible or irrelevant in the age of armchair anthropology.

Indeed, several ethnographers before Malinowski inhabited or were invited to the compounds, stations, and bungalows with verandahs where ethnographic encounters could happen. And this universe was certainly a target of his revolution, a crucial element if not the

key to Malinowski's own charter myth. When Malinowski claimed to have "found out where lay the secret of effective field-work" and further asked, "What is then this ethnographer's magic . . .?" he answered that living "without other white men" was "the most elementary" of the "foundation stones of field work." In one swoop, previous ethnographies, written by colonial agents or visitors from the metropolis, were affected and seriously put into question by Malinowski's alleged finding of the ethnographer's magic. As white residents had "biased and pre-judged opinions" in their "routine way of treating the natives" they could hardly be good company or good ethnographers themselves. Found "in the tone of the majority of white residents"—with "a few delightful exceptions"—such negative "features" were also identifiable "in the inferior amateur's writing," Malinowski stated (1922: 4, 5, 6), giving as illustration *Savage Life in New Guinea* (1902) by his former host on Kwato Island, missionary Charles William Abel (1862–1930), who portrayed the Indigenous people as "lawless" and only "governed by unchecked passions" (Abel 1902: 5; on Malinowski and missionaries, see Young 2004: 333).

In Malinowski's foreword to *Argonauts of the Western Pacific*, an opening statement made clear that the research "by men of academic training has proved beyond doubt and cavil that scientific, methodic inquiry can give us results far more abundant and of better quality than those of even the best amateur's work" (1922: xv; see also the conclusion in this volume).<sup>11</sup> Malinowski never gave any examples of such "modern scientific accounts," but it is unquestionable that he had in mind, at least, the production of his own mentors, the veterans of the celebrated Cambridge Anthropological Expedition to Torres Straits (1898–99), namely, Alfred Cort Haddon, William Halse Rivers (1864–1922), and Charles Seligman (1873–1940).

### **Academic versus Amateur Ethnography: A Fallacious Dichotomy**

The myth of Malinowski as "self-proclaimed inventor of modern fieldwork" (Stocking 1995: 13) has long been shattered by historians of anthropology highlighting the decisive role of Rivers, Haddon, and Seligman during the expedition to the Torres Straits. There is a debate, however, on whether their expedition was fertile from the ethnographic and methodological point of view. The variety of survey methods within the expeditionary and multidisciplinary model compromises the idea that it pioneered ethnographic intensive fieldwork.

And even the famed genealogical method of Rivers (1900), dating back to the expedition, was “an instrument of survey research, useful to investigators spending only a brief time in the field” (Kuklick 1991: 140). In his introduction to *Argonauts of the Western Pacific*, Malinowski himself associated that method with the initial, sterile phase of his Melanesian experience, when he was still to discover “the ethnographer’s magic.” In spite of its “iconic status,” the Cambridge Expedition’s legacies are “diffuse” and difficult to grasp in any consensual manner (Herle and Rouse 1998: 21). Also, the notion that the expedition paved the way to the modern blending of ethnography and theory is counterbalanced by contrasting dimensions in the work of its members, from warnings against theory-infected records (ibid.: 19) to the carefree adoption of historical standpoints that were quite distant from the synchronic perspectives of their successors and most famous pupils, Radcliffe-Brown and Malinowski. In this, Rivers, Haddon, and Seligman are no different from the numerous pre-Malinowskian ethnographers who incorporated theoretical views in their writing, often addressing issues and approaches that were later relegated to the margins of canonic trends in social anthropology, such as cultural diffusion, the origins of particular institutions, or precolonial and colonial history.

To what extent did the Torres Straits experience make Haddon, Seligman, and Rivers realize the “limitations” of the expeditionary model and channel fieldwork in new directions? They produced independent monographs afterward, such as *The Todas* (1906) by Rivers and *The Veddas* (1911) by Charles Seligman and his wife Brenda Seligman (1883–1965). Nevertheless, survey techniques, intended to systematically and rapidly collect data from various groups inhabiting a relatively circumscribed but large region, were not discarded. Rivers himself resorted to alternative methods that were to be denigrated because of a long-lasting antidiffusionist bias. *The Todas* “was to remain Rivers’ only attempt to produce an ethnography dealing with a single society,” since his later “gang plank ethnography,” James Urry writes, resulted in ethnographic accounts that were “fragmented in detail, lacking any sense of depth or internal coherence in their reporting” (Urry 1993: 50).<sup>12</sup> As Henrika Kuklick (1942–2013) put it in *The Savage Within*, “When anthropologists became fieldworkers, they did not necessarily become functionalists” (1991: 139; see Kuper 1996 [1973]: 8). So the idea of a unidirectional movement toward the modern monograph would be both misleading and teleological.

Moreover, Haddon and five of his Torres Straits colleagues were naturalists, trained in biological or biomedical disciplines, a circum-

stance that contributed to “a new sense of importance in the collection of data” (Urry 1993: 27; Urry 1984). The result of this “transfer of skills”—including the very word *fieldwork*, at the time spelled separately, as “field work”—may be disappointing. According to Urry, the influence of natural science and laboratory practice can be detected in their “dispassionate” reports, where facts are “pressed down, dried out or bottled in formalin” (Urry 1993: 7, 47). However determined these men were to professionalize fieldwork, they may be deemed “poorly qualified” under more humanistic criteria, namely for “lacking a background in languages, history or textual analysis”<sup>13</sup> (Herle and Rouse 1998: 7)—unlike the missionaries and government officials who were fluent in the vernacular languages and produced vernacular records that contrasted to Haddon’s folktales in Pidgin English.

During the nineteenth century, several observers worked for scientific institutions and/or were academically trained in various fields of knowledge, other than anthropology, so there is no reason to make a difference between their diverse background and that of the British pioneers of professional fieldwork, Haddon, Rivers, and Seligman, who were never taught to be ethnographers or anthropologists anyway. Henrika Kuklick showed that the process of making British universities “hospitable to anthropology” was complex and gradual (1991: 31; see also 51–53); and if we combine this perception with avoiding Anglocentrism, the picture becomes more complex, considering that other European countries implemented university studies in anthropology or ethnology (among other designations) long before Britain.<sup>14</sup>

On top of this, there is the danger of reproducing a center-periphery model that systematically underscores the role played in the history of ethnography by metropolitan figures traveling from North to South, be they Haddon or Malinowski, to the detriment of colonial ethnographers whose lives were more entangled with indigenous reality. In a special issue of *Oceania*, edited by Helen Gardner and Robert Kenny in 2016, six historians of Australian anthropology challenged the notion that the shift from armchair anthropology to fieldwork was “autogenetic,” that is, emanating from “those in the metropole”—a reference to Haddon and his teammates—“who realized that . . . anthropological research had to be hands-on and in place” (Gardner and Kenny 2016: 219). Highlighting the relevance of local ethnographic experiences predating the Torres Straits expedition (for example, the use of the genealogical method before Rivers), they deplore the fact that “standard historiography” has been “as heedless as the metropole itself” to the development of field practices

in Oceania by no single “iconic figure” at no “clear moment.” The fact that this was achieved in different ways by ethnographers with other primary tasks makes it difficult “to position colonial figures in the origins of the discipline.” The recurrent impact of new data on evolutionary and other theoretical discussions in Europe may then be interpreted as a symptom of malaise: the insufficiently acknowledged import of the South in the emergence of the ethnographic sensibility and in bringing the armchair paradigm “to the limits of its viability.” In sum, Gardner and Kenny claim that the history of anthropology “has been written upside down” (Gardner and Kenny 2016: 220, 223).<sup>15</sup>

If the line between trained and untrained observers is blurred, this is not just for reasons related to various academic pathways and institutional affiliations among nineteenth- and early twentieth-century ethnographers; it is also because, depending on various criteria, ethnographies produced by scientifically trained newcomers were not necessarily superior to amateur ethnographic texts.

## Franz Boas and the Dawn of the Americanist Tradition

Franz Boas’s ethnographic work, particularly along the Northwest Coast, has been the object of several in-depth studies (Codere 1966; Rohner 1969; Müller-Wille 1998; Müller-Wille and Giesecking 2011). Writing about his early explorations during the late 1880s, Rosemary Lévy Zumwalt (2019: 189) notes that Boas was “crafting innovative fieldwork techniques.” Resorting to the correspondence between Boas and the veteran linguist Horatio Hale (1817–96), who was then monitoring his fieldwork on behalf of the British Association for the Advancement of Science, Zumwalt unveils the tension between both men, as Hale explicitly told Boas that he did not want him to make “a minute account of two or three tribes or languages” but “a general synopsis of the ethnology of the whole of British Columbia.” Boas “disdained this approach” because it “must of necessity be very superficial” (Boas, quoted in Zumwalt 2019: 177). No wonder, then, that Boas kept complaining about the “senseless” instructions with which he had to conduct his ethnographic survey under Hale’s orders (*ibid.*: 178).

The other side of the coin was Boas’s tendency to overlook amateur ethnographies. It was with a grain of salt or as an exception to the rule that he acknowledged the work of this or that predecessor.<sup>16</sup> The main criterion in his consideration of previous ethnographies

was their utility as more or less reliable raw materials for the analysis of precontact cultures, and his ethnolinguistic standards were high. “Excepting the old missionary grammars,” he wrote as late as 1917, “there is very little systematic work, and we have no bodies of aboriginal texts” (p. 1). Other creative dimensions in the nineteenth-century ethnographic archive, whether literary, contextual, or theoretical, were basically neglected, and this also accounts for his severe scrutiny:

There are very few students who have taken the time and who have considered it necessary to familiarize themselves sufficiently with native languages to understand directly what the people whom they study speak about, what they think and what they do. There are fewer still who have deemed it worth while to record the customs and beliefs and the traditions of the people in their own words, thus giving us the objective material which will stand the scrutiny of painstaking investigation. (Boas 1906: 642)

According to Regna Darnell (1999), the “Americanist tradition” encompasses both Boasian and pre-Boasian ethnographies. In their collective volume *Theorizing the Americanist Tradition*, Darnell and Lisa Valentine challenged the ingrained perception of it being a “merely descriptive” and “a-theoretical” anthropological paradigm (Valentine and Darnell 1999: 5, 12).<sup>17</sup> Boasian textualism has been criticized as a strategy that disembodied Indigenous interlocutors, presenting their oral literature “as if unmediated,” and the same aversion befalls the historical precursors of that paradigm. “The idea of the text is little changed,” wrote Michael Harkin against Boas, “from that of the brothers Grimm, who saw folktales as the texts that would reveal *der Geist* (the spirit) of the *Volk*” (Harkin 2001: 98), i.e., the *Volkgeist*. Indeed, the production of vernacular records, giving room to the Herderian concept of *Volkgeist*, had a significant part of its roots in late eighteenth- and early nineteenth-century Germany. We cannot restate here the complex and diverse origins and developments of this broad tradition in the history of Western thought<sup>18</sup>—but no one will question that Boasian anthropology was one of its most flourishing branches. Historians of anthropology may choose to avoid overcritical readings of the pre-Boasian textualist tradition and search intellectual and spiritual idiosyncrasies reflected in the writings of Boas’s predecessors in the United States, particularly the Bureau of American Ethnology’s data collectors.

John Wesley Powell (1834–1902), the bureau’s founding director, had an eye for amateur geniuses who could do the job, and his

team included female ethnographers. In writing “Women in Early American Anthropology,” a contribution to *Pioneers of American Anthropology* (1966), Nancy Oestreich Lurie (1924–2017) led the way in rediscovering Powell’s female ethnographers, namely Erminnie Smith (1836–86), Alice Cunningham Fletcher (1838–1923), and Matilda Coxe Stevenson (1849–1915). From then on historiography followed its course, with biographies and other specialized studies on their legacies (see the conclusion in this volume). Some of these women were living legends at the end of her lives, but Lurie’s verdict on their overall oblivion still resounds today as a bold appreciation of exclusion for reasons other than gender: “As it turned out, early women have been relegated to no more obscurity than have many of their male contemporaries who were also remarkable pioneer spirits” (Lurie 1966: 32).

Under auspices of the bureau, Regna Darnell writes, “participant-observation fieldwork was carried out, long before Malinowski, by Frank Hamilton Cushing, James Mooney, James Owen Dorsey, and Francis LaFlesche” (2001: 10). Indeed, Powell’s team had several luminaries whose work was valued posthumously by twentieth-century anthropologists (see, for example, Lévi-Strauss 1958: 318) or brought to the fore by historians of the discipline. That is the case of the flamboyant Frank Hamilton Cushing (1857–1900) or the visionary ex-journalist James Mooney (1861–1921; see figure 0.1), whose status as excluded or included ancestors depends on the vantage point. Mooney’s “The Ghost Dance Religion and the Sioux Outbreak of 1890” (1896) may be an inescapable reference for scholars working on this prophetic messianic movement or correlating similar phenomena from a sociological point of view. In comparison with the stronger memory of the Boasians, though, there is a feeling that Mooney remains “a forgotten man” (Nader 2002: 52). The perception of nineteenth-century anthropology would be very different if he was present instead of absent from handbooks informing each new generation of anthropologists. In fact, many ignore the fact that Mooney was “a political time bomb” (Nader 2002: 50, 52), actually a forerunner of colonial studies, on a collision course with the United States Indian policies. His ethnographic work was revolutionary for several reasons.

Thirty years before Malinowski boasted of discovering “the ethnographer’s magic,” Mooney put participant observation into practice in an incomparable way. Whether he spent the night at the Mennonite missionary’s home or among the Cheyenne and Arapaho followers of the Ghost Dance is a minor question in the face of his first results



**Figure 0.1.** *James Mooney, undated. Courtesy of the Braun Research Library Collection, Autry Museum, Los Angeles; P. 36656.*

in Indian Territory. The myth of Malinowski's tent is meaningless in this context, as the tribal village gives way to the reservation. Not only did Mooney quickly understand that the traditionalist novelty, far from being a precolonial tradition, was related to oppression, he also participated in the dance, giving his hands to men and women for whom the new ritual embodied their faith in a better future. Produced in the postfrontier era, his collection of Ghost Dance songs transformed "salvage ethnography" into something new that should require the creation of a specific concept, since the sense of urgency



and attention to vernacular detail were combined with a recognition of the colonial dimension of the cultural traits in question. “This is,” according to Mooney, “the most pathetic of the Ghost Dance songs. It is sung to a plaintive tune, sometimes with tears rolling down the cheeks of the dancers as the words would bring up thoughts of *their present miserable and dependent condition*” (Mooney 1896: 977; our emphasis): “Father, have pity on me, I am crying for thirst, All is gone—I have nothing to eat.”

Boas’s harsh judgments on untrained ethnographers must be put into perspective since he considered, from the 1880s on and throughout his career, that they could positively contribute to anthropology. Ambiguity toward amateurs becomes more evident if we recall that the professional universe that Boas and his students wanted to consolidate for anthropology incorporated not only their Indigenous collaborators but also several amateur white ethnographers. Boas himself tried to “domesticate” them and influence their production and their ethnographic writings according to his methodological standards, but the outcome of this kind of rapprochement could be surprising (see Wickwire 2019).

The culturalists’ “men on the spot” might have much in common with those of the *evolutionists*, including ambiguous forms of humility or “disobedience” toward their big-town mentors, culminating in daring, independent monographs such as *The Sun Dance and Other Ceremonies of the Oglala Division of the Teton Dakota* (1917) by James R. Walker (1849–1926), physician at the Pine Ridge Reservation from 1896 until 1914. The fact that the names and individual statements of Walker’s informants were omitted in the published result—against the will of his supervisor, Clark Wissler (1870–1947)—arose the suspicion of Franz Boas, who asked Ella Cara Deloria (1889–1971) to confirm Walker’s data onsite in 1937, but her own informants were “particularly incensed at the suggestion that the shamans might have held back from the people secret knowledge that they as shamans shared in common” (DeMallie and Jahner 1980: 44). The issue of authenticity was resolved by the discovery of the original manuscripts in the late 1950s and their publication in the 1980s by DeMallie and Jahner, who, while identifying some “antiquated” dimensions in Walker’s monograph, risked the following statement: “That so much could have been lost in the twenty-four years since Walker left Pine Ridge is explainable only if some of the key parts of the information . . . were indeed secret. If this was really so, it places a truly enormous value on Walker’s work—in fact, the value that he insisted it had” (ibid.: 45).

Postcolonial theorists and polemicists have made trenchant deconstructions of white ethnographers' claims to be the vessels of a vanishing world, saved into print for posterity—mostly conceived of as a Euro-American audience.

## Beyond Postcolonial Anxieties

In their “Five Theses on Ethnography as Colonial Practice” (1994),<sup>19</sup> Peter Pels and Oscar Salemink tried to demonstrate the ethnographic nature of the colonial machine through the practice of late imperialism agents such as explorers, military officers, administrators, and missionaries. Assuming a political continuity between such amateur ethnographers and the later, professional ones, whose intellectual and academic status forged the illusion of a lesser participation in the system's violence, their accusatory historiography explicitly rejects the conventional genealogies of the discipline, focused on “the great thinkers of anthropology, those whom we think revolutionized its theories and methods, as the main carriers of the history of anthropology,” and proposes to “consider the history of anthropology from another angle,” redirecting our attention to more or less obscure “colonial ethnographers” (1994: 1–4). The parallelism with the present volume ends here. Following in the footsteps of historians of anthropology who stressed, after Thomas Kuhn's *The Structure of Scientific Revolutions* (1962), that paradigm shifts “are fuzzy, gradual, and partial” (Hinsley 1981: 151), the editors and contributors of the present volume admit connecting threads and continuities, more than stereotyped ruptures, between nineteenth- and twentieth-century ethnographies—except that colonial violence is not their criterion.

According to Pels and Salemink, the fact that various forms of “proto-relativism,” “proto-holism,” and “proto-functionalism” may be detected in the writings of colonial ethnographers is symptomatic if not proof that cultural relativism, holism, and functionalism “are as much products of colonial practice as they are theoretical innovations of academic anthropology.” In his countercritique of Pels and Salemink, Herbert S. Lewis (2004, 2014) called attention to the differences between professional anthropologists of the first half of the twentieth century and imperial agents acting as ethnographers, namely, from the point of view of colonial ethics. Focusing on the example of the United States, Lewis adds:

The doctrine of cultural relativism grew out of American anthropology and the ideas and teachings of Franz Boas, which were then adopted by

his students, and insofar as it had to do with colonialism it was a direct consequence of *opposition* to colonialism, cultural arrogance, and ethnocentrism! Johann Gottfried von Herder, and his predecessor Michel de Montaigne, were outraged by European overseas adventures and deeply troubled by ethnocentrism, and Franz Boas' genealogy includes these thinkers. (Lewis 2004: 253)

The present volume offers an alternative countercritique by admitting that the writings of amateur ethnographers can be imbricated, in varied and complex ways, in the Herderian as in other anthropological genealogies. What is of interest to us is the way in which ethnographic knowledge, in connection with observing and listening experiences that were never simply coinciding with colonial domination or ideology, is creatively transformed under text form. This perspective contrasts with a vast, critical literature that can hardly be listed here in any exhaustive way.<sup>20</sup>

If no idyllic portrait has been aimed at in the present volume, its contributors have tried to avoid the traps of radical postcolonial critique whenever it systematically puts “crime” and “horror” at the beginning of anthropology. This historiography of “hatred” (Lewis 2004: 247) or “self-aversion” (Singh and Guyer 2016: 199) is itself starting to be historically contextualized: “We need to question the motives of those who would burn the books” (Beals 2002: 225; Lewis 2014). New readings and positive ways of assessing the archive and its individual contributors are emerging. If our volume takes the decolonizing fever into account, it is by avoiding a triumphant gaze over anthropology's past and by joining other efforts in the same direction, as encapsulated, for instance, by Edvard Hviding and Cato Berg: “The factual contribution of our anthropological ancestors can now be appreciated in more generous ways than twenty years ago” (Hviding and Berg 2014: 30). Several titles speak for themselves, as representative of a recent tendency, following the postcolonial critique: “Acknowledging Ancestors” (Dureau 2014), “Voicing the Ancestors” (Handler 2016; Bashkow 2019), or “Exhuming the Ancestors” (Rosa 2019)—see also Karl-Heinz Kohl's “Plea for the Ethnographic Archive” (2014).

## Salvage Ethnography as Indigenous Archive

Armchair anthropologist Marcel Mauss gave the following advice to ethnographers applying the “philological method.” It was not enough to collect oral traditions: one should also “look for the ma-

gician who would supplement those formulas with the necessary comment.” Preferably this work should be done “by an authoritative Indigenous expert,” for “only the indigenous point of view matters.” Mauss added that “the ideal option would be to transform those Indigenous individuals into authors, not just informants” (Mauss 1989 [1947]: 210, our translation), as happened with Omaha Francis La Flesche (1857–1932), and the part-Tuscorora John Napoleon Brinton Hewitt (1859–1937). In their decision to become ethnographers, and eventually ethnologists—hired as such by the Bureau of American Ethnology—La Flesche and Hewitt were influenced by foreign visitors. Hewitt started his career as a collaborator with Erminnie Smith, and, as developed by Joanna Cohan Scherer in the present volume, La Flesche was like an adopted son to Alice Fletcher, with whom he collaborated for over a quarter of a century. For these men, who had a European-American education and lived in a cross-cultural world, the publishing universe, even the white academy or the museum, was a matter of course.

In his introduction to a series of essays dedicated to the Boasians’ fieldwork legacy from the point of view of the concerned Indigenous communities past and present, Ira Bashkow (2019) calls attention to the fact that salvage ethnography might be “co-constructed by the field researcher and the people researched” so that “it was not simply one-sided exploitation as one might imagine a colonial relationship.” He added that “a humanistic idea of ‘salvage’ developed . . . in the mid-twentieth century,” according to which “the record might someday have value for a descendant community of the people studied” (Bashkow 2019: 218, 216).<sup>21</sup> The present volume aims to add more depth of space and time to this challenge, as there is room for discovering varied illustrations of indigenous participation in premodern ethnographic projects, well before the Boasian paradigm took off and far beyond North America. In several contexts where salvage ethnography took place, Native individuals were aware that books constituted a legacy to future generations, both of white and Indigenous peoples. Instead of being powerless or passively manipulated, informants and collaborators could be committed to preserving their knowledge in print and getting involved in salvage ethnography as a project that concerned their own lives and their communities.

To be sure, in some cases informants were not properly informed about the salvaging project they were taking part in. Baldwin Spencer and Francis Gillen published material, including photographs, on the most sacred items of the Arunta/Arrernte that should never

be seen by women and noninitiated men. Analyzing the archive of the 1901–2 Spencer and Gillen expedition to the Northern Territory, Philip Batty (2018) unveils, however, the role of Aboriginal men, Erlikilyika and others, who not only were experts on the cross-cultural environment of that period but also acted as go-betweens in such a way that their dedication to the research goals influenced the shaping of the ethnographic field. Most meaningfully, Batty highlights the existence of an archive that involves Indigenous peoples, both in the past and the present (see Herle and Philp 2020). Therefore, it should not be reduced to mere Western imperial fantasies—and this is a powerful countercritique of the radical, postcolonial readings of Spencer and Gillen, in particular by Patrick Wolfe (1994), who equated ethnography with ethnocide.<sup>22</sup>

In their special issue of *Oceania*, “Before the Field,” the editors Gardner and Kenny write: “The burial of colonial ethnographers beneath the practice of metropole grandees has meant that the Indigenous experts of the colonial ethnographers have been even more thoroughly hidden than their colonial collaborators and in need of a specific archaeology.” Refusing to equate ethnography and colonialism in any simplistic or Manichaean manner, they “resist the efforts to bury Aboriginal agency and presence beneath this totalizing discourse” (Gardner and Kenny 2016: 222). Contemporary anthropologists estranged from the discipline’s ethnographic archive should take the alert from this ongoing movement into serious account:

The proof that these colonial records and texts are saturated with Indigenous knowledge can be found in their continued use by contemporary Aboriginal people, anthropologists, linguists, native title lawyers and historians who recognize the deep entanglement of Aboriginal peoples with those who wrote about them and the value of these documents for language, culture and the identification of boundaries. (ibid.: 222)

The value of early ethnographies can change according to present-day political realities, one important distinction being whether they relate to decolonized societies or to enduring settler colonial societies. These texts may be particularly relevant where First Nations further pursue their fight for acknowledgment in relation to current legislation on tribal boundaries and indigenous rights. Dichotomous views on the cosmopolitics of the ethnographic archive should, however, be avoided, considering the varied histories and historicities associated with political independence. Reassessments may also occur in decolonized societies where ethnographies pointing to the past are mined for culture and language for various reasons.

Exploring the intricacies of anthropological genealogy around figures claiming the title of pioneering ethnographers may be less significant for some readers than the here and now of the historical literature. One may ask if the sole legitimate historiography of anthropology is the one alert to contemporary political struggles echoing power relations in the colonial period. While we acknowledge the import of this trend, we are also inclined to embrace diversity among historians of anthropological and ethnological sciences.<sup>23</sup> Some of the contributors to this volume are entangled in present-day realities to the point of combining their archival researches and fieldwork activities, while others are more focused on disciplinary past per se and do not work as anthropologists in the field, let alone as activists. In this sense, this book echoes Regna Darnell's and Frederic W. Gleach's openness "to all approaches," as "all of these debates and perspectives are part of anthropology and thus of the histories of anthropology"—so they reaffirm in each editorial of the *Histories of Anthropology Annual* (2008: viii).

## Structure and Aims of the Present Volume

Each contributor to the present volume was invited to give prominence to a particular ethnographic text and to select descriptive, vernacular, theoretical, methodological, historical, literary, or other significant content from it. While keeping this selected textual content at the center of the analysis, with purposely long indented extracts, contributors were free to relate this content to other primary sources and to further, sometimes external, questions, such as intersubjective experiences in the field or colonial and sociopolitical contexts. The originality of the present volume is thus related to a focus on ethnography as a product, while the contributors never lose sight of ethnography as a process. Thus, each chapter in this volume varies in creative ways that flow from each contributor's scholarship within and beyond the historiography of the anthropological sciences, with multiple dialoguing perspectives in their respective reading of the texts selected for analysis. Ethnographic content related to specific aspects of the communities studied was, in several cases, a prime choice.

Part 1, "In Search of the Native's Point of View," is dedicated to pre-Malinowskian versions of this Malinowskian theme par excellence. The selected case studies reveal that professional and amateur ethnographers tried to apprehend the *Weltanschauung* of the people studied, both their core values and the contextualized meaning of the

smallest cultural traits, resulting in some form of relativism and a critique of Western prejudice. By putting Malinowski's way of grasping "the native's point of view" into perspective, the ethnographic monographs under analysis open the horizons of disciplinary history.

In chapter 1, Herbert S. Lewis combines a thorough analysis of Franz Boas's first ethnographic monograph, *The Central Eskimo* (1888), with a selection of other texts produced during his one-year stay on Baffin Island in 1883–84. Lewis reveals Boas's fieldwork among the Inuit as an immersive, collaborative experience based on participant observation and an effort to increasingly grasp "the native's point of view." Quoting extensively from *The Central Eskimo's* vivid accounts and detailed descriptions, Lewis questions the assumption that Boas "converted" from geography to anthropology and demonstrates how his intellectual and political background predisposed him to study and respect cultural difference in a fieldwork setting. Notwithstanding his faith in a revolution in ethnography, henceforth pursued by men of science to the detriment of amateurs, Boas believed in tutoring privileged collaborators in the field, a pattern he would continue to develop in the Northwest Coast but dating back to the period following his stay in Baffin Island.

In chapter 2, Barbara Chambers Dawson outlines how Katie Langloh Parker (1856–1940), a white settler in northern New South Wales for over twenty years, turned to the neighboring "Euahlayi" for companionship, learning their language and gaining their trust. This resulted in participant observation in ways that do not unfavorably compare with Malinowski, and Dawson quotes his formula—"to grasp the native's point of view"—in order to describe the core of Parker's ethnography. If her access to some of the knowledge of initiated men is "astonishing" (p. 94), Parker's intimate association with Indigenous women gave her unique access to the female native perspective, gaining insights into the agency of Aboriginal women that challenged the colonial stereotypes. According to Dawson, her book on *The Euahlayi Tribe* (1905) is a fundamental text in the history of anthropology by an unjustly excluded ancestor who, in her own words, "appreciated them [Aboriginal tribes] at their true value" (p. 108).

Chapter 3, by David Shankland, is dedicated to Edward Westermarck (1862–1939), professor of sociology at the LSE, close friend and supporter of Malinowski. Born in Finland into a Swedish-speaking family, Westermarck began ethnographic fieldwork in North Africa from 1898 on, with summer stays in Morocco for over two decades. His first ethnographic study, *Marriage Ceremonies in Mo-*

*rocco*, appeared in 1914. By analyzing a later two-volume monograph titled *Ritual and Belief in Morocco* (1926), Shankland sustains that Westermarck's ideas on the origins of ethical and moral behavior were applied to his ethnographic data in a relativist way. He believes that Westermarck's homosexuality was related to his understanding that "morals are rooted within different cultures' perceptions of behavior." Insisting on learning the vernacular, Westermarck concentrated on the meaning of folk Islamic religious concepts and "had no difficulty in assuming," Shankland concludes, "that the point of fieldwork was to give priority to the 'native's point of view.'" Shankland questions the motives why Westermarck was eventually excluded from the canon, being regarded as belonging to the nineteenth century.

Part 2, "The Indigenous Ethnographer's Magic," is dedicated to ethnographic accounts resulting from or unveiling particularly collaborative forms of ethnographic fieldwork. If the role of Indigenous informants and collaborators is transversal to the present volume, in part 2 they play the leading role of cultural experts, whether on an equal footing with the "white" ethnographers or assigning them a subordinate role as amanuensis or pupils.

In chapter 4, David Chidester proposes a vertiginous "deconstruction" of Anglican missionary Henry Callaway's (1917–1890) *The Religious System of the Amazulu* (1868–70), an ethnographic monograph that pioneered textual transliteration in two columns per page, namely in Zulu and English. While armchair anthropologists praised the authenticity of Callaway's raw ethnography for giving immediate access to the pristine beliefs of his Zulu informants, Chidester reveals that these men, including the monograph's chief authority, Mpengula Mbande (d. 1874), were figures from the margins, creatively struggling to adapt their traditional knowledge to Christian polemics and the disruptions of colonial encroachment. In conclusion, Chidester "reconstructs" Callaway's book, whose achievements are noteworthy from today's standards. The missionary recorded multiple voices to the detriment of his own and produced a text that voiced displaced and dispossessed individuals, "allowing the undercurrent of violence in the experience of his informants to surface in his monograph" (p. 180).

In chapter 5, Jeffrey Paparoa Holman explores the ten-year relationship between New Zealand ethnographer Elsdon Best and his foremost Māori collaborator Tutakangahau (ca. 1832–1907). Following a minute exegesis of *Waikare-moana* (1897), an ethnographic monograph under the guise of a tourist's guidebook, Paparoa Hol-



man unveils the covenant taking place by the Waikaremoana lake in 1896, when Tutakangahau revealed that he was “an *ariki taniwha* [Lord of the Dragons],” willing to impart the esoteric lore of yore to Best. In reality, Tutakangahau was not the backward-gazing sage of romanticism but a modernizer who saw the opportunity to commission Best as his amanuensis and thus preserve traditional knowledge for future generations. Notwithstanding the asymmetric power relations that prevailed in colonial New Zealand, Best was in the subordinate position of a pupil, with Tutakangahau the expert. “What is enacted here,” Paparoa Holman writes about the climax of their journey, “may legitimately be viewed as orality handing over its power to the written word” (p. 201).

In chapter 6, Joanna Cohan Scherer looks at the relationship between the Bureau of American Ethnology’s ethnographer Alice Cunningham Fletcher and her Omaha collaborator and protégé Francis La Flesche. Criticized in its time for classifying the collected material “according to canons of aboriginal rather than of scientific logic” (p. 219), their masterwork *The Omaha Tribe* (1911) resulted from a quarter of a century of joint and minute ethnographic fieldwork, as they both “lavished on details” (p. 229). A tribal member with privileged access to the lore of his people and pre-reservation cultural memories, La Flesche contributed to give the monograph “the true Omaha flavor” that Fletcher was searching for. Through the lens of archival correspondence and selected quotes from their book, Scherer brings *The Omaha Tribe* into our time as a monograph that was “a century ahead of other ethnographic publications,” indeed “a prototype of collaborative research” (p. 239).

If all case studies in our volume are related to colonialism in one way or the other, the chapters in part 3, “Colonial Ethnography from Invasion to Empathy,” offer a selection of ethnographic accounts whose colonial dimensions are particularly salient, albeit in contrasting ways. This section adds comparative depth to the volume by presenting one of the least “Malinowskian” among pre-Malinowskian ethnographies, under the form of invasive blitz ethnography. The section also discusses one of the most “Malinowskian” ethnographers, a surprising case of empathic “military anthropology” (McFate 2018). In between the two appears a missionary ethnography of the very type that Malinowski excoriated, resulting however from intimate observation of—and interference in—indigenous traditions. It can be taken as another counterexample of the volume’s main narrative, reminding us of methodological and political diversity in late nineteenth- and early twentieth-century ethnographic practices.

In chapter 7, Ronald L. Grimes presents a troubling case study in the sense that John Gregory Bourke (1846–96) was an invasive ethnographer who captured indigenous sacred scenes during a short stay among the Hopi and against their will. Moreover, in *The Snake-Dance of the Moquis of Arizona* (1884) he constructed a category, “ophiolatry,” that betrayed his culture-bound judgments. Grimes compares Bourke’s ethnography with that produced by other nineteenth-century observers of Pueblo Indians, particularly Frank Hamilton Cushing, who in spite of his long stay and “gone native” attitude shares one important feature with Bourke: they were both dramatic performers of their own research. With due attention to current predicaments of Hopi studies by non-Indigenous scholars, Grimes explores the ritual dimension of *The Snake-Dance* as containing the clue to a more benevolent reading of Bourke’s candid revulsion at the overwhelmingly olfactory sensations produced by the Snake Dance and the resulting vividness and sensuality of his writing.

In chapter 8, André Mary analyzes the case of *Chez les Fang, ou Quinze années de séjour au Congo français* (1912), written by the French missionary and ethnographer Henri Trilles (1866–1949). Combining monographic sections and missionary vignettes, *Chez les Fang* demonstrates the complexity of Catholic ethnography, from empirical soundness to literary fancy, from theoretical insights to ideological conundrums. On the one hand, Trilles practiced a manipulation of oral sources through his mytho-theological overinterpretation of native tales and legends. On the other, the narrative and dialogical style of *Chez les Fang*, the knowledge of and respect for Fang ritual specialists whenever he confronted them using their own codes and weapons, eventually reveal Trilles as a “genius of religious bricolage” (p. 283). Having fallen into oblivion, Trilles’s work “affected Fang and Gabonese cultural life and history, which only adds to its complexity and makes its reassessment all the more challenging” (p. 300).

In chapter 9, Montgomery McFate unveils an ethnographic monograph, *Ashanti* (1923), published one year after Malinowski’s *Argonauts of the Western Pacific* but resulting from fifteen years of intensive ethnographic fieldwork among the Ashanti in the British Gold Coast (now Ghana). A colonial officer eventually appointed government anthropologist in 1921, Robert Sutherland Rattray “was able to do what few anthropologists have done: stop what appeared to be an inevitable war between the Ashanti and the British” (p. 319). McFate describes Rattray’s work as an unsettling combination of imperial anthropology and strenuous defense of Ashanti culture, por-

traying Rattray as an “extremely progressive, if not radical” (p. 327) figure in his own time, as well as a “proto-feminist” who understood the political import of women—particularly the Ashanti Queen Mothers—in a way that exposed both British colonial prejudice and the shortcomings of Indirect Rule. Regarded with suspicion by his peers, Rattray participated in the esoteric world of the Ashanti “as a believer,” McFate sustains, while quoting inspirational passages from his monograph, such as: “I approached these old people . . . in the spirit of one who came to them as a seeker after truths” (p. 318).

Part 4, “Expeditionary Ethnography as Intensive Fieldwork,” challenges the assumption that expeditionary, extensive surveys preceded stationary, intensive forms of ethnographic fieldwork, the first being associated with the nineteenth century and the latter with the twentieth. The selected case studies reveal how both models intertwined with each other, resulting in sound methodological reflections, participation on the move, and empathic descriptions.

In chapter 10, Frederico Delgado Rosa analyzes the 1890 ethnographic monograph of the Portuguese explorer Henrique de Carvalho (1843–1909), *Etnografia e história tradicional dos povos da Lunda*. As Carvalho joined the exiled Prince Samadiamb, whom the courtiers urged to return to the Lunda empire’s capital, his expedition became like a traveling court if not the epicenter of local politics, with privileged conditions to pursue intensive ethnographic fieldwork. Carvalho stressed the importance of learning the native language and of experiencing “a close cohabitation and long-term work,” to the point of affirming “the necessity of going native” (p. 343). According to Rosa, the difference between expeditionary and intensive fieldwork is brought into question by this case study, which unsettles the history of anthropology for other reasons as well, since the monograph’s evolutionist and imperialist motives are related to relativistic and anti-racist dimensions. Carvalho’s reflections, Rosa sustains, “sound like a nineteenth-century version of Malinowski’s charter myth” (p. 366).

In chapter 11, Grażyna Kubica reassesses the work of Maria Czaplicka as head of an anthropological expedition to Siberia at the outbreak of World War I. By attentively reading her travelogue *My Siberian Year* (1916), Kubica highlights the intensive tones of Czaplicka’s expeditionary ethnography. If her experience was not stationary in comparison to that of her colleague and compatriot Malinowski, it was because she respectfully followed the nomadic rhythm of the people she studied, namely the Evenki (Tungus). Czaplicka’s reciprocity, tactfulness, and humorous attitude gave way to engaging and reflexive portrayals of various native institutions and

events, which can be considered “an early example of thick description” (p. 394). Following a feminist perspective, Kubica sustains that “*My Siberian Year* is no mere travelogue in the sense of continuing a nineteenth-century tradition, but a literary ethnographic text by a woman who had been trained in a male-dominated academic world to produce something quite different” (p. 394).

In chapter 12, Michael Kraus analyzes the ethnographic accounts of German ethnologists who participated in anthropological expeditions in the Amazon Basin during the late nineteenth and early twentieth centuries, particularly Karl von den Steinen, Konrad Theodor Preuss, Theodor Koch-Grünberg, Max Schmidt, and Fritz Krause (1881–1963). Their publications not only document indigenous cultures but also include methodological reflections on their increasingly intensive and stationary models of ethnographic fieldwork. The emphasis on learning the local languages, interacting with Indigenous people in their daily life, and trying to cover a complete annual cycle are some of the Malinowskian themes that this generation of German ethnographers anticipated. “It turns out,” Kraus concludes, “that a significant number of the methodological principles discussed by Malinowski had already been debated and practiced by the ethnographers of the Amazon Basin and thus were far from being genuine innovations” (p. 429).

## Conclusion: Ethnography, Ethnographers, and Empirical Anthropology

In *Before Boas: The Genesis of Ethnography and Ethnology in the German Enlightenment* (2015), Han F. Vermeulen concluded that before the eighteenth century ethnography existed only as “proto-ethnography” and demonstrated that “in a strict sense” ethnography and ethnology, including the coining of both terms in neo-Greek variants such as *ethnographia* (1767) and *ethnologia* (1783) or in German variants such as *Völkerkunde* (1771) or *Volks-Kunde* (1782), “were invented by eighteenth-century German-speaking historians,” particularly Gerhard Friedrich Müller (1705–83), August Ludwig Schlözer (1735–1809), and Adam František Kollár (1718–83).<sup>24</sup> Having forged a terminological cluster and a corresponding new academic field from the 1730s to the 1780s, their program for a worldwide comparative description of peoples and nations was adopted by nineteenth-century scholars such as Gustav Klemm (1802–67), Theodor Waitz (1821–64), and Adolf Bastian (1826–1905), who form but

one of the vertices in a triangle of intellectual diffusion from German-speaking countries to other European, American, and Asian centers of knowledge. While this intricate and thus far largely concealed genealogy concerns protagonist figures of the history of anthropology, from Edward Burnett Tylor to Franz Boas, it certainly implicates countless amateur ethnographers as well. Vermeulen states that “much of this process is unknown” (2015: 446, 449)—and perhaps it cannot be otherwise since the idea of ethnography was, by the mid-nineteenth century, on the loose, from Lithuania to Portugal, and beyond Europe, in a connected world in motion.

Thus, our volume deals with a period in which the term *ethnography* was fully established, meaning that it was self-evident and that its worldwide circulation no longer required justification or definition. In the late nineteenth and early twentieth centuries, the heydays of European colonialism, ethnography was a passion. Not only Boas and Cushing, or Bastian and von den Steinen, but scores of other scholars, travelers, and colonial agents tried their hand at describing peoples around the world (see the appendix to this volume). This means that it is virtually impossible to look for a unanimous, albeit tacit, understanding of the word *ethnography* in this period. As Urry noted, “The Greek suffix ‘ology’ or ‘logy’ means roughly ‘the study of’ with an emphasis on the scientific and the theoretical,” whereas “the ‘graphy’ in ethnography indicates something denoted or described, connected in turn to the notion of something written or inscribed (graphie)” (Urry 2006: 28–29).

How did ethnography become limited to a specific fieldwork method of enquiry? It was mostly Malinowski and the graduate students of his seminar at the LSE who would equate the description of a people with participant observation. But there were others, notably in Britain. According to Meyer Fortes (1953: 16), the “first serious attempt” to carry out an “intensive study of a limited area” was made by Haddon’s and Rivers’s pupil, Radcliffe-Brown of Trinity College, among the Andaman islanders in 1906–8. The “full demonstration of its possibilities” appeared in 1922, when both Radcliffe-Brown’s book on the *Andaman Islanders* and Malinowski’s *Argonauts of the Western Pacific* were published (see the conclusion in this volume). “They introduced field-work of a kind that can only be carried out by trained investigators” (Fortes 1953: 17). Thanks to their efforts, the term “field work” or “field-work” became the modern-day equivalent of what previously had been generally known as “ethnography.” Before 1900, *ethnography* referred to a descriptive and comparative study of a people, tribe or nation, a research program rather

than a method of enquiry—even if it was evident for ethnographers before Malinowski that describing implied observing and comparing. Echoing the original uses of the word in the eighteenth century, ethnography might also apply—interchangeably with other concepts, particularly ethnology—to a broad research program that currently can be circumscribed as empirical anthropology.

Relevant ethnographies certainly existed before the affirmation of sociocultural evolutionism from the 1860s/1870s on. Many ethnographic sources perused by armchair anthropologists predated the Darwinian, archaeological, and geological revolutions. Therefore, it will not be a surprise if the present study involuntarily rejoins other histories of anthropology written for more distant periods (Palerm 2010 [1974]; Blanckaert 1996; Fabre and Privat 2010; Sera-Shriar 2013; Douglas 2014; Vermeulen 2015). For chronological coherence, however, our intent is to explore ethnographic texts produced from ca. 1870 on, i.e., *after Tylor*, in creative ways that bring them nearer to the twentieth century up to the year when *Argonauts of the Western Pacific* and *The Andaman Islanders* were published.

Mostly dedicated to selected monographs resulting from intensive, stationary fieldwork, this collection is attentive to other genres that had monographic hues or allowed for the inclusion of monographic sections. We do not see a shift from travelogues and expeditionary reports to monographs but a co-occurrence, both before and after Malinowski. Initiated in 1897—and therefore preceding the Cambridge Anthropological Expedition to Torres Straits—the equally famous Jesup North Pacific Expedition (1897–1902), collective and individual at the same time, expeditionary by name but intensive in practice, is evidence of this. It is a well-known fact that in spite of its ambition to unveil cultural-historical relations between Asia and North America thanks to a multisited network of North American and Russian ethnographers gathered around Franz Boas, “the Jesup Expedition did achieve a more restricted goal of producing a set of ‘classical’ ethnographic monographs” (Fitzhugh and Krupnik 2001: 9). Moreover, alternative pre-Malinowskian ethnographic genres existed, such as the ethnographic novel, the oral literature collection, and the indigenous autobiography, as well as blurred genres *avant la lettre*. The perspective underlying the present volume runs counter to an earlier historiographic attempt by Robert Thornton (1983) who insisted, possibly too much, on the characteristic features of the ethnographic monograph genre as it emerged in the nineteenth century.<sup>25</sup> Therefore, instead of identifying one coherent, “pioneering” genre, we aim at the identification of multifarious configurations within the

monographic theme, as there were fluid boundaries between different genres that could have in common a monographic focus on a single group or various groups within a relatively circumscribed cultural region.

More than a paradigmatic shift, implying a consensus among anthropologists, what emerged with *Argonauts of the Western Pacific* was the notion of an ethnographic canon that manifested itself as a hegemonic trend within a diverse discipline. It relegated to its margins alternative ways of practicing it, such as expeditionary anthropology (Thomas and Harris 2018), with abundant but rather invisible illustrations in the twentieth century. The focus on participant observation has been productive and enriching but also reductive, ignoring the relevance of approaches such as comparative studies and ethnohistory.<sup>26</sup>

Therefore, it is time to render more visible the pre-Malinowskian side of the coin, by opening the ethnographic archive and bringing earlier ethnographies from the margins to the center of anthropology's history.

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books of 2016, awarded the ICAS Book Prize 2017 by the International Convention of Asia Scholars, and published in a paperback edition in 2018. He is a founding member of the “History of Anthropology Network” (HOAN) within the European Association of Social Anthropologists (EASA).

## Notes

1. Reprinted in the *Journal of the Polynesian Society* in 1901. As to the above quoted passage, it had appeared in an article published in the *Journal of the Polynesian Society* in 1913 (p. 153).
2. This quote was the transcription of a letter dated 23 November 1907, sent to Best by his Māori informant and collaborator Tamati Ranapiri (dates unknown; active 1872–1907).
3. The bibliography on world anthropologies is vast. See Ribeiro and Escobar (2006); Bošković (2008).
4. In the present volume, the words *Indigenous*, *Native*, and *Aboriginal* are capitalized when referring to Indigenous people or societies under settler colonialism and to individuals acting as experts of their own culture.
5. For Nigeria alone, see Jones 1974.
6. Email from Anna Sirina to the editors (30 September 2019). Reference is to Piłsudski’s monograph *Materials for the Study of the Ainu Language and Folklore* (Cracow, 1912).
7. “Was Tylor really ‘An Armchair Anthropologist?’” questions Maria Beatrice Di Brizio (2017) to challenge this perception (see also Sera-Shriar 2014). As to the nine research and acquisition “trips around the world” of Adolf Bastian, he “apparently spent less time in contact with the population than in the libraries of local scholars” (Fischer, Bolz, and Kamel 2007: 5). How much time Bastian actually spent with Asian, American, Oceanic, and African individuals or groups, and on what terms, deserves further attention in relation to his voluminous work (for Bastian’s research expeditions, see Fischer, Bolz, and Kamel 2007; see also Penny 2021, who explains that Bastian contacted many Germans living abroad, as well as local experts on culture).
8. In fact, Jarvie criticizes the scientism underlying Malinowski’s call to intensive fieldwork as a false religion comparable to cargo cults.
9. Jarvie made brief references to the forerunners of the “demand for direct observation,” Alfred C. Haddon, W. H. R. Rivers, and Charles Seligman; strangely enough, he included Baldwin Spencer and Francis Gillen in this group. Jarvie also defined their demand as “the weapon” later used by Malinowski to fulfill the revolution (Jarvie 1964: 2).
10. This passage, which hardly lends itself to a confusion between the actual and the metaphorical verandah, was quoted by Jarvie as “the manifesto of the revolution” (1964: 2).



11. The same anti-amateur bias can be detected in Malinowski's first Trobriand monograph, "Baloma: The Spirits of the Dead in the Trobriand Islands" (1916). On its import, see Alvarez Roldán (1995).
12. Ten scholars have recently reassessed the ethnographic achievements of the 1908 Percy Sladen Trust Expedition. The editors, Edvard Hviding and Cato Berg, recall that intensive ethnography and survey work were considered as complementary. The division of subjects between the three members of the expedition—Rivers, Hocart, and Wheeler—was counterbalanced by the geographical partition they eventually made. As a result, there were significant holistic dimensions in their writings. Hviding and Berg conclude that Rivers and Hocart were "true pioneers of fieldwork" who created "modern methodology on the spot," including participant observation. Even the "iconic tent" is not missing from the comparison with Malinowski (Hviding and Berg 2014: 13, 23, 37).
13. With the exception of Sydney Herbert Ray (1858–1939).
14. Such as the Netherlands, one of the first countries to make ethnography (*volkenkunde*) "a compulsory subject for those serving in the colonial administration" in the 1830s and 1840s (Vermeulen 2015: 413). Germany, where museums became "the nuclei around which anthropology was established as an academic discipline," is another case in point. "Very often," Karl-Heinz Kohl writes, "[museum directors and department curators] were accorded an honorary professorship at the nearby university" (Kohl 2018: 2651).
15. This new understanding is also inscribed in the title of Helen Gardner and Patrick McConvell's book on one of the most famous ethnographic monographs of the nineteenth century, *Southern Anthropology: A History of Fison and Howitt's Kamilaroi and Kurnai* (2015)—thus highlighting its value beyond the theoretical influence of Lewis Henry Morgan.
16. A case in point, highlighted by Curtis Hinsley (1981), is the work of the missionary ethnographer James Owen Dorsey (1848–95). Boas's appreciation of Dorsey's ethnographic work was ambiguous. On the one hand, there was nothing "in the whole range of American anthropological literature" that might be compared to his publications on the Ponka and the Omaha; on the other, they were also an instance—to be sure, "the best"—of "how utterly inadequate the available collections are" (Boas 1906: 643; see also Boas 1917: 1; Hinsley 1981: 174; Scherer, this volume).
17. In the United States, the expression "Americanist tradition" had an explicitly linguistic sense during the 1960s (Regna Darnell, personal communication), while there are broader uses of the word *americanist* to encompass anthropological traditions related to both North and South American ethnographic contexts.
18. Jacob Grimm's (1785–1863) dramatic call to "fellow-labourers" who could help him collect the remains of German "heathenism" is paradigmatic (Grimm 1882–83 [1835]: 1:12). The impact of his *Deutsche Mythologie* (1835) on the history of European ethnography can hardly

be overestimated. “It is impossible to list them all,” writes Hermann Bausinger about the collections inspired by Grimm’s work (Bausinger 1993 [1971]: 44–45). If the romantic search for folk authenticity has been deconstructed as an ideological blend of emotions and science that justified disembodied collections (Bendix 1997: 48–49), these are, in reality, very diverse ethnographic products that also require case-by-case analysis. Illustrations and variants of this trend are detectable in various countries in transformed ways, with or without proper attention to the performative dimensions of orality.

19. The editor’s introduction to a special issue of the journal *History and Anthropology*, followed by *Colonial Subjects: Essays on the Practical History of Anthropology* (Pels and Salemink 1999).
20. In association with other Africanist scholars, several African anthropologists warn against the risk of both denying colonialism and letting it “overdetermine intellectual debate” (Ntarangwi, Mills, and Babiker 2006: 12). Mwenda Ntarangwi, David Mills, and Mustafa Babiker propose to open an alternative dialogue with the past by foregrounding the early ethnographic work of African collaborators.
21. This benign understanding contrasts with more critical reassessments, namely by Margaret Bruchac (2018).
22. A similar warning may be found in Thomas (2011).
23. Including the right to avoid identifying oneself as a historian of anthropology.
24. In 1740, Gerhard Friedrich Müller coined the new research program “Völker-Beschreibung,” a “description of peoples.” The term *ethnographia* was used in Nördlingen, Swabia, as early as 1767, and *Ethnographie* in Göttingen, Hanover, in 1771–75. A first ethnographic journal titled *Allgemeines Archiv für Ethnographie und Linguistik* was published at Weimar in 1808 with the aim of examining the “physical, moral, and intellectual peculiarities of peoples, and their origins” (quoted in Vermeulen 2015: 344). With justice, Urry pointed out that some authors anachronistically applied the terms “ethnography” and “ethnographic” to earlier accounts and activities. These references to what was clearly proto-ethnography mostly come from outside anthropology, appearing in fields as varied as history, literary criticism, and cultural studies (Urry 2006).
25. Robert Thornton (1983) dealt with three monographs (presented as “the first” for Africa) by three missionaries: Callaway’s *The Religious System of the Amazulu* (1868–70), Junod’s “Les BaRonga” (1898), and Roscoe’s *The Baganda* (1911). Thornton maintained that these authors deliberately sought to “emulate the monographs of the natural sciences” and distinguish their texts from other kinds of writing containing ethnographic data (such as travelogues and missionary diaries). Admitting that the content of the ethnographic monograph was “not radically new in appearance” and that other genres contributed to its development, Thornton chose to underscore its cohesive “format and rhetorical conventions” (Thornton 1983: 503, 507; see also Thornton 1981).

26. The relevance of comparative studies and ethnohistory has been shown by Adam Kuper (1982) and Marshall Sahlins (1985), for instance.

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