

Before and After Malinowski: Alternative Views on the History of Anthropology [A Virtual Round Table at the Royal Anthropological Institute, London, 7 July 2022]

Han F. Vermeulen

Max Planck Institute for Social Anthropology, Halle (Saale)

Frederico Delgado Rosa

CRIA / NOVA FCSH

2022

FULL REFERENCE

Vermeulen, Han F. & Frederico Delgado Rosa (dir.). 2022. "Before and After Malinowski: Alternative Views on the History of Anthropology [A Virtual Round Table at the Royal Anthropological Institute, London, 7 July 2022]" (avec la participation de Sophie Chevalier, Barbara Chambers Dawson, Thomas Hylland Eriksen, Michael Kraus, Adam Kuper, Herbert S. Lewis, Andrew Lyons, David Mills, David Shankland, James Urry, et Rosemary Lévy Zumwalt), *Bérose - Encyclopédie internationale des histoires de l'anthropologie*, Paris.

BEROSE Publisher: ISSN 2648-2770

© UMR9022 Héritages (CY Cergy Paris Université, CNRS, Ministère de la culture)/DIRI, Direction générale des patrimoines et de l'architecture du Ministère de la culture. (All rights reserved).

Your use of this article indicates your acceptance of the Bérose site (www.berose.fr) terms and conditions of use, available [here](#).

Visited on 22 March 2023 at 17:40

The present article is a special issue including fourteen short essays originally delivered at a virtual round table held on July 7, 2022, at the Royal Anthropological Institute (RAI) [1], London, to celebrate the centennial of Bronisław Malinowski's *Argonauts of the Western Pacific* (1922–2022) and the appearance of the edited volume *Ethnographers Before Malinowski: Pioneers of Anthropological Fieldwork 1870–1922* (Rosa and Vermeulen, Berghahn Books, EASA Series, 2022). [2] Chaired by David Shankland and Thomas Hylland Eriksen, with Andrew Lyons as discussant, this two-part event highlighted the history of ethnography before Malinowski's *Argonauts*, the genesis of British social anthropology in 1922, and its aftermath in Britain and beyond.

The resulting papers discuss the three theses that opened the round table: (1) In the fifty years before the publication of *Argonauts of the Western Pacific*, a growing number of ethnographers produced hundreds of ethnographic monographs worldwide, but much of their work was sidetracked or neglected by Malinowski and his followers; (2) Malinowski is still celebrated as the inventor of intensive fieldwork in a single society, despite the fact that he had many predecessors in other societies and continents pursuing the same goal; and (3) the success of

British social anthropology has been partly due to its marginalizing the relative importance of other approaches such as non-functionalist ethnographies, comparative studies and ethnohistory.

Worldwide interest in these topics is reflected in the attendance statistics of this webinar, with a representation of over 500 attendees from 70 different countries. [3] An online recording is available on YouTube:

- **“Before and after Malinowski: Alternative Views on the History of Anthropology – Part I”**: <https://www.youtube.com/watch?v=66IdnPyFc98>
- **“Before and after Malinowski: Alternative Views on the History of Anthropology – Part II”**: https://www.youtube.com/watch?v=ssx_cHwlZcg

Participants in the round table/contributors to the present special issue are (in alphabetical order): Sophie Chevalier, Barbara Chambers Dawson, Thomas Hylland Eriksen, Michael Kraus, Adam Kuper, Herbert S. Lewis, Andrew Lyons, David Mills, Frederico Delgado Rosa, David Shankland, James Urry, Han F. Vermeulen, and Rosemary Lévy Zumwalt.

The present special issue is complemented by “Opening the Archive: Selected Bibliography of Ethnographic Accounts, 1870–1922”, previously published as an appendix in Frederico Delgado Rosa and Han F. Vermeulen (eds.) *Ethnographers Before Malinowski: Pioneers of Anthropological Fieldwork, 1870-1922*. New York/Oxford: Berghahn Books (EASA Series 44), 2022, pp. 474-501. See also the interactive version titled “Online Interactive Archive: Ethnographic Monographs before *Argonauts of the Western Pacific (1870-1922)*” (in *HAR History of Anthropology Review*) introducing an expandable research bibliography of 365 monographs by 220 ethnographers working in the fifty years preceding the publication of Malinowski’s classic monograph, 1870–1922.

Table of contents

- INTRODUCTION
 - 1. Before, During, and After Malinowski: A Pioneer Ethnographer, His Predecessors, and Other Anthropologists, by Han F. VERMEULEN (Max Planck Institute for Social Anthropology)
 - 2. Projecting Pre-Malinowskian Ethnography into the Future, by Frederico Delgado ROSA (Universidade Nova de Lisboa)
- ROUND TABLE PART I
 - 3. Franz Boas’s Year Among the Inuit Of Baffinland And The Lessons He Derived From It For Anthropology, by Herbert S. LEWIS (University of Wisconsin-Madison)
 - 4. Katie Langloh Parker, A Female Ethnographer of Noongahburrah Women (Euahlayi Tribe), by Barbara Chambers DAWSON (Australian National University, Canberra)
 - 5. Westermarck and Social Anthropology, by David SHANKLAND (Director RAI/University College London)
 - 6. Henrique de Carvalho’s Intensive Fieldwork: A Nineteenth-Century Version of Malinowski’s Charter Myth, by Frederico Delgado ROSA (Universidade Nova de Lisboa)
 - 7. Developing Fieldwork and Analysing Historical Processes:

German Ethnographers in South America Between Recognition and Criticism, by Michael KRAUS (University of Göttingen)

- 8. Canons in Anthropology? Querying the Malinowskian Revolution, by Andrew LYONS (Wilfrid Laurier University, Waterloo)
- ROUND TABLE PART II
 - 9. Bringing Theory into The Field: Malinowski's Trobriand Ethnography, by Adam KUPER (London School of Economics)
 - 10. Anthropology After the *Argonauts*: Malinowski in France, by Sophie CHEVALIER (Université de Picardie Jules Verne, Amiens)
 - 11. Ethnographic Research, Ethnographic Writing, Anthropological Theory and Colonialism: Malinowski and His Early Students, by James URRY (Wellington, New Zealand)
 - 12. Franz Boas, His Students and Their Ethnographic Work, 1922–1942, by Rosemary Lévy ZUMWALT (Agnes Scott College)
 - 13. Why Disciplines Forget: Salvage Anthropology and The Politics of Historiography, by David MILLS (University of Oxford)
- AFTERWORD, by Thomas Hylland ERIKSEN (University of Oslo)
- REFERENCES CITED
- Exercise in Setting the Canon: Twelve Classic Ethnographic Accounts Between 1870 and 1922

INTRODUCTION

1. Before, During, and After Malinowski: A Pioneer Ethnographer, His Predecessors, and Other Anthropologists, by Han F. VERMEULEN (Max Planck Institute for Social Anthropology)

Published in June 2022, *Ethnographers Before Malinowski* is the result of over three years of efforts by two editors and a team of twelve scholars from ten countries on four continents to explore largely neglected aspects of the ethnographic archive and contribute to the history and theory of anthropology. The volume derived from our invitation to colleagues to write a chapter on one of their favourite ethnographers and “give prominence to a particular ethnographic text and to select descriptive, vernacular, theoretical, methodological, historical, literary, or other significant content from it” (Rosa and Vermeulen 2022: 22). Thus, the volume is related to a focus on ethnography as a product, while contributors never lost sight of ethnography as a process.

Focusing on key works in the history of anthropology, the authors introduced sixteen ethnographers working in African, Asian, North and South American, and Oceanian contexts. The result is a cross-section of an ethnographic archive that is apparently huge. During the fifty years between ca. 1870 and 1922 the editors found 365 ethnographic monographs written by 220 ethnographers (Rosa and Vermeulen 2022: 2, 463, 475). This is an amazingly large number: over two hundred ethnographers produced more than three hundred ethnographic monographs worldwide in only half a century! But it is the proverbial tip of the iceberg, as our sample of texts could never be exhaustive. Moreover, important ethnographic works also appeared before 1870 and the interest in writing ethnographies

seems to have increased after 1922.

Our survey makes clear that intensive, long-term fieldwork had been practised by many late nineteenth- and early twentieth-century ethnographers. Contrary to what some scholars suggest, the age of “armchair anthropologists” (such as James Frazer and E. B. Tylor) was in reality the era of ethnographic fieldworkers. Arguably, the late nineteenth and the early twentieth centuries were extremely productive periods for the creation of ethnographic accounts worldwide.

Unfortunately, many of these monographs are now largely forgotten. They are overlooked in the secondary literature and have fallen out of the anthropological canon. The inclusion and exclusion of ethnographic monographs from the anthropological canon is an overarching problem in the present special issue. The canon of anthropological texts, especially of ethnographic accounts, is a regularly shifting array of books, never really stable, always changing, but with some books being regarded as classics.

Argonauts of the Western Pacific by Bronisław Malinowski, published in London in 1922, is such a classic text. Malinowski and Alfred Radcliffe-Brown are seen as the founding fathers of social anthropology in Great Britain. Both published major ethnographic studies in 1922: Malinowski the *Argonauts*, Radcliffe-Brown *The Andaman Islanders*. Malinowski is often celebrated as the father of long-term fieldwork, developing the emblematic method of participant observation with which modern anthropology purportedly began (Kuper 1973, 1996, 2014; Barnard 2000).

So we have an immense corpus of ethnographic texts going largely unnoticed, while one particular specimen of this ethnographic archive, Malinowski’s *Argonauts*, is regarded as a seminal text. To be sure, our edited volume shows that Malinowski was not the inventor of intensive fieldwork – scores of other ethnographers practised that before him, such as Franz Boas, Frank Hamilton Cushing, James Mooney, Katie Langloh Parker, Henrique de Carvalho, Alice C. Fletcher, Francis La Flesche, Maria Czaplicka, Elsdon Best, Karl von den Steinen, Theodor Koch-Grünberg, Robert S. Rattray, and Edward Westermarck. We call them “pioneers of anthropological fieldwork”. Their work shows that ethnography was carried out on a large scale, worldwide. The collection in this book demonstrates this convincingly. The core idea underlying our project is that “pre-Malinowskian” ethnographies are a fundamental part of the history of anthropology (see Frederico Delgado Rosa’s summary introduction in the present special issue).

Moreover, the volume reveals an epistemological shift that occurred in or after 1922. Before Malinowski, ethnography encompassed diverse approaches to the description and comparison of peoples, tribes, and nations. As a systematic study, ethnography began during the early eighteenth century in Northern Asia with the German historian Gerhard Friedrich Müller (1705-1783). During a ten-year expedition (1733–1743) Müller’s research programme envisaged the compilation of detailed ethnographic studies of all Siberian peoples (*narodny*, *Völker*), followed by their comparison with similar studies of “the manners and customs of

the other Asian, African, and American peoples” (Vermeulen 2015: 170, 439; 2016; 2019a-b). In the English-speaking world scholars believed that “there were no students of ethnology during the eighteenth century” (Slotkin 1965: xiii). However, the material analysed in my book *Before Boas: The Genesis of Ethnography and Ethnology in the German Enlightenment* (Vermeulen 2015) shows that ethnography and ethnology did emerge during the Enlightenment in the context of research expeditions in the Russian Empire and reflections on history in both Germany and Austria (2015: 437).

It is hard to determine how many ethnographic monographs were written during the eighteenth and nineteenth centuries, but by 1800 more than a dozen such monographs existed in the Russian Empire, probably more than in any other nation. By the late nineteenth century, ethnography was fully established as a research programme, its worldwide circulation no longer required justification or definition, and its meaning had become self-evident: a description of one or more peoples.

In *Argonauts of the Western Pacific*, Malinowski described the institutions and customs of the Trobriand Islands off the coast of southern New Guinea. He dictated the *Argonauts* to his wife, Elsie Rosaline Masson, “an accomplished writer, who edited it before it was again revised by Malinowski” (Skalník 1995: 138). He added an introduction on how to conduct fieldwork, entitled “The Subject, Method and Scope of this Inquiry”. His biographer Michael Young wrote that “Malinowski’s explicit intention was to raise ethnographic fieldwork to a professional art, and his self-mythicizing Introduction to *Argonauts* – arguably the most influential twenty-five pages in the history of the discipline – prescribed a mode of research that would become a rite of passage for subsequent generations of anthropologists” (Young 2013: 509; 2018: 12).

Thus, Malinowski’s “self-mythicizing” introduction set an example “for subsequent generations”. This view was already expressed by Adam Kuper in 1973, who opened his history of British anthropology with the statement: “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). Another historian of anthropology, George W. Stocking, Jr. (1995: 13) called Malinowski the “self-proclaimed inventor of modern fieldwork”. And Edmund Leach (1957: 124) wrote that Malinowski “claimed to be the creator of an entirely new academic discipline”, namely social anthropology.

If this assessment is correct we have to evaluate how this came to be. To what extent did Malinowski’s research differ from that of his predecessors and what was so special about it? Why didn’t he credit his predecessors? And how did his students and associates respond to his work: what role did Audrey Richards, Edmund Leach, Phyllis Kaberry, Raymond Firth, Isaac Schapera and Evans-Pritchard play in this mythicizing?

Malinowski’s *Argonauts of the Western Pacific* and Radcliffe-Brown’s *The Andaman Islanders* initiated a new era, beginning British social anthropology’s *annus mirabilis*. Malinowski is

seen as the inventor of “holistic fieldwork” (Kuper 1996 [1973]: 49) and Radcliffe-Brown as the promotor of “comparative sociology” in the tradition of Emile Durkheim (ibid. 50, 63). However, the term *fieldwork* had been introduced by Alfred C. Haddon (Young 2004: 339) and Radcliffe-Brown’s mentor was William Rivers, director of the Royal Anthropological Institute and the instigator of “intensive work” in England (Rivers 1913). In 1901–02, Rivers had spent “several months” among the Todas in south-western India and in 1906 published a 755-page ethnography, *The Todas*, that was long cherished as a classic (Slobodin 1997: 28). In its preface, Rivers revealed that his book aimed to be a “demonstration of anthropological method” – an idea copied by Malinowski in his *Argonauts*. In his *History of Melanesian Society* (1914) Rivers distinguished intensive work from survey work, the “two chief kinds of ethnographical work” (Rivers 1914 I: 1; Slobodin 1997: 105). By that time, Radcliffe-Brown had returned from the Andaman Islands, where he conducted eighteen months of fieldwork in 1906–08, but his book appeared in 1922. “Intensive” was the research Malinowski pursued in the Trobriand Islands for about two years in 1915–18. Rivers died prematurely in June 1922, but his *Todas* was not credited by Malinowski.

Malinowski may have been a pioneering ethnographer but he created a myth that helped neglect the work of his male and female predecessors. Malinowski is considered to be an innovator who revolutionized fieldwork but participant observation was carried out by several ethnographers before him (see Rosa and Vermeulen 2022: 15). Moreover, the term “participant observation” penetrated anthropological discourse in the 1950s and saw its origins in sociology (Drucker-Brown 1985). By contrast, in the 1920s and 1930s social anthropologists spoke of “intensive fieldwork” to describe their practices of data collection. In 1922 Malinowski saw his project as “comparative Ethnology” and spoke of his own work as that of “an Ethnographer”. In his introduction he sketched a three-strata “goal of ethnographic field-work”: (1) “*The organisation of the tribe, and the anatomy of its culture*”; (2) “*Within this frame, the imponderabilia of actual life, and the type of behaviour*”; and (3) “*A collection of ethnographic statements, characteristic narratives, typical utterances, items of folk-lore and magical formulae*” that have to be given “*as a corpus inscriptionum, as documents of native mentality*”. These “three lines of approach lead to the final goal, of which an Ethnographer should never lose sight”. In his own words, this goal was “*to grasp the native’s point of view, his relation to life, to realize his vision of his world*” (Malinowski 1922: 24–25; cf. Young ed. 1979).

In the 1920s and 1930s, Malinowski declined to credit German scientists, which is understandable as their work had fallen into international disrepute due to World War I. However, he also failed to credit other contemporaries. He did acknowledge C. G. Seligman, “my friend and teacher” (to whom he dedicated his *Argonauts*), Sir James Frazer, who wrote the preface, and Professor L. T. Hobhouse, who read the proofs (xvii–xviii). He mentioned Rivers’s work on genealogical tables (Malinowski 1922: 14), but wasted no words on Westermarck, his teacher at the LSE in London. They were both dismissed at the end of the book in two short sentences on “the classical school of British Anthropology (Tylor, Frazer, Westermarck, Sydney Hartland, Crawley)” and “the ethnological school (Ratzel, Foy,

Gräbner, W. Schmidt, Rivers, and Elliott-Smith)” (Malinowski 1922: 515–516). These sentences served to introduce the study of “the influence of the various aspects of an institution” and “the study of the social and psychological mechanism in which the institution is based” – a new type of theoretical studies (Malinowski 1922: 516) that would soon be defined as functionalism.

Functionalism derived from Wilhelm Wundt’s investigation of ethics and was developed by Emile Durkheim, who had studied at Marburg, Berlin and Leipzig in 1885–87. Malinowski, who had studied with Wundt in Leipzig in 1908–10 (Hann 2012), borrowed the concept of social function from Durkheim (Leach 1957: 123). In 1922, Malinowski announced “a new type of theory” but in *Argonauts* came no further in defining it than “the manner in which two aspects of culture functionally depend on one another” (1922: 515). In 1940 Radcliffe-Brown, regarded as one of the fathers of functionalism, denied being a leader of a “Functional School of Social Anthropology” because “this Functional School does not really exist; it is a myth invented by Professor Malinowski”, who had bestowed “the magnificent title of the Functional School of Anthropology ... by myself, in a way on myself, and to a large extent out of my own sense of irresponsibility” (Malinowski 1932: xxix cited in Radcliffe-Brown 1952 [1940]: 188). Self-mockery indeed, but Leach was right that this nevertheless reflected “Malinowski’s belief in himself as the prophet of a new creed” (1957: 124 note 1).

Robert Lowie has observed that Malinowski’s “field technique conforms to Boas’s standards: he learnt his Trobrianders’ tongue, tried to live their life, garnered concrete rather than abstract statements from his informants, and recorded them in the vernacular” (Lowie 1937: 230). The parallels between Boas’s and Malinowski’s field research are striking, as the citations of Boas in Herbert Lewis’s chapter in our volume and his essay in the current issue demonstrate. Obviously, Malinowski refrained from making this explicit. As Rosemary Lévy Zumwalt makes clear in her contribution to this special issue, Malinowski and Boas had good relations personally, but not publicly.

The main subject of discussion in our round table event was exclusion and inclusion in the ethnographic canon (see also Andrew Lyons in the present special issue). This is the overarching problem: the canon of anthropological classics, especially ethnographic accounts. Our volume demonstrates the selectiveness of the anthropological record. While an amazing amount of ethnographic texts have been published, only a selection has escaped oblivion. Still, our aim is not to dethrone a culture hero such as Malinowski; it should be to contribute to understanding how canons of scholarly knowledge are built and guarded. If our subject is the exclusion and inclusion of texts in the anthropological canon, which ethnographic accounts are or should be part of the canon?

While George Stocking found Malinowski the “self-proclaimed inventor of modern fieldwork” (Stocking 1995: 13), James Urry nuanced this in his statement in the current special issue: “Malinowski encouraged his students to think that he had established an epistemological break with the past. (...) Modern ethnography began with Malinowski”. Countering the claim by Malinowskians such as Raymond Firth and Edmund Leach that

Malinowski had founded ethnography, Adam Kuper consistently placed Radcliffe-Brown next to Malinowski as the second founder of British social anthropology, arguing that British social anthropology began with intensive fieldwork and theory. In this he was followed by Jack Goody (1995) and Alan Barnard (2000).

Thus, even if Malinowski was not the inventor of intensive fieldwork he brought its merits into the open and instructed his students to put the method into practice. Later followers routinized the idea of Malinowski as the prophet of modern anthropology; many of them found positions in social anthropology departments as universities expanded after World War II (see Shankland 2019 and Shankland in Rosa and Vermeulen 2022: 463–464). His students created a myth that helped neglect the work of his male and female predecessors. They introduced the idea that intensive fieldwork in a single community had been a twentieth-century invention.

With hindsight it is clear that the establishment of social anthropology as a discipline from the 1920s and 1930s on was an attempt to position a specific version of anthropology, based on fieldwork and a social theory, vis-à-vis preceding variants such as cross-cultural ethnology and cultural anthropology. After WWII most European anthropologists defined themselves as social anthropologists rather than as members of an international community: the totality of anthropological and ethnological sciences.

During the twentieth century, ethnography changed course. As the result of a shift from an all-embracing anthropology to social anthropology, ethnography was limited to intensive fieldwork and participant observation in a single society. This shift was productive but also reductive: fieldwork became the equivalent of ethnography. Historians of anthropology should study these developments: from worldwide ethnography before Malinowski, and his “self-mythicizing” ideas during the 1920s and 1930s, to the mythicizing by other anthropologists after Malinowski.

2. Projecting Pre-Malinowskian Ethnography into the Future, by Frederico Delgado ROSA (Universidade Nova de Lisboa)

The core idea underlying our volume *Ethnographers Before Malinowski* is that the so-called age of armchair anthropology was also an era of ethnographers. “Pre-Malinowskian” ethnographies are a fundamental part of the history of anthropology, but most accounts from the fifty years preceding Bronislaw Malinowski’s *Argonauts of the Western Pacific* (1922) have fallen into oblivion. There is a deeply ingrained disregard for this literature, as if it were of lesser value or irrelevant. This is because many scholars assume that earlier accounts were either infected by speculative evolutionism or purely descriptive materials with no consideration for the interdependence between social institutions. In fact, our volume reveals that these accounts are part of an enormous archive comprising hundreds of ethnographic monographs from 1870 to 1922, *after Tylor and before Malinowski*. Many of them are fascinating and remain relevant for various reasons, whether as irreplaceable sources on

cultural history or as records of intellectual, literary, and human experiences of their authors in dialogue with important partners during ethnographic encounters.

In the search for ethnographic pioneers before Malinowski, there is a risk of overestimating, for Britain alone, the so-called “Cambridge School” of Alfred C. Haddon (1855–1940), W. H. R. Rivers (1864–1922) and Charles Seligman (1873–1940), who conducted ethnographic research in Oceania, Asia and Africa, to the detriment of fieldworkers academically trained by armchair anthropologists, namely by Edward Tylor (1832–1917) and his successor at Oxford, Robert R. Marett (1866–1943). Besides, as Henrika Kuklick has shown (1991), the process of making British universities hospitable to anthropology was complex and gradual, so that a dichotomous view of periods with and without academic institutionalization should be avoided. In our volume, the line between trained and untrained observers is blurred, and deliberately so, since the once supreme criterion of professionalism is no longer adequate to separate the wheat from the chaff. Following an opening chapter on Franz Boas (1858–1942), the founding father of modern American anthropology who conducted fieldwork in the Canadian arctic, we have a chapter dedicated to Katie Langloh Parker (1856–1940), a white settler in Australia who turned to the neighbouring Aboriginal people for companionship (Lewis, Dawson, in Rosa and Vermeulen, 2022). This juxtaposition of Boas and someone like Langloh Parker, who was anything but an academically trained observer, is a statement. Moreover, several Victorian observers 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 alleged pioneers of professional fieldwork who were never taught to be ethnographers or anthropologists.

The way Malinowski himself obfuscated amateur accounts also enters the equation. Let us recall that when he famously asked “What is then this ethnographer’s magic?” (1922: 6), his answer was that isolating oneself from other white men was a foundation stone of good ethnography. In this way, in one fell swoop, previous ethnographies written by colonial agents or visitors from the metropolis were pushed aside. White residents, he said, had “biased and pre-judged opinions”, so they could hardly be good company or good ethnographers. Malinowski’s anti-amateur bias can also be detected in his first Trobriand monograph, “Baloma: The Spirits of the Dead in the Trobriand Islands” (1916), in which he disparaged German missionary ethnographer Carl Strehlow (1871–1921) for denying the nescience of physiological paternity among the Arunta. Ironically, it was Malinowski, not the amateur ethnographer, who claimed the existence of so-called “supernatural birth”, a category strongly connoted with evolutionist armchair anthropology – and one of his most controversial discoveries in the Trobriand islands. In a footnote to the conclusion of our volume we quote Malinowski’s own words on Strehlow’s incapacity: “Here the explanation lies in the insufficient mental training of the observer [Strehlow]. You can no more expect good all-round ethnographical work from an untrained observer than you can expect a good geological statement from a miner, or hydrodynamic theory from a diver” (1916: 415; cited in Rosa and Vermeulen 2022: 467, n16). Moreover, since the 1990s, Strehlow’s work has been reassessed by Walter F. Veit, and more recently by other Australian scholars and Indigenous

experts who consider his ethnography a fundamental record providing “some of the earliest insights into the true sophistication of Aboriginal cultures” (Kenny 2013: 10).

Diversity is a fundamental keyword when addressing the “pre-Malinowskian” ethnographic archive. If some ethnographers knew and followed the available queries, including the *Notes and Queries on Anthropology* (five editions between 1874 and 1929) many others did not. The notion ethnographers might have of contributing to science, even explicitly to anthropology or ethnology, varied from humble subordination and flat empiricism to sheer criticism or theoretical ambition. The way they organized and presented their data could be creatively intuitive and personal; it could not, however, lack any form of systematization. If the table of contents of a pre-modern monograph gives the impression of scattered topics, obliterating their holistic dimensions may be a false reading, as fragmentation and holism were never mutually exclusive, but related intellectual forces in the history of anthropology. Notwithstanding the fact that anthropometry was fashionable in several European and American countries, not all ethnographers of the period under consideration were interested in physical anthropology, nor did their ethnographic observations necessarily have racist undertones. In many cases, no chapters or sections were dedicated to that subject at all; in other cases, racist thought could be mitigated or even suppressed by the ethnographic encounter. Thus, taken-for-granted dichotomies, such as ethnocentrism versus relativism, may be challenged by new readings. Affected by diffuse epochal influences and by unforeseen incidents and inner transformations following culture contact in the field, male and female ethnographers might express their ethnographic and human experiences in many different ways.

Also exciting about pre-Malinowskian ethnographers is that the way they incorporated theoretical views in their writings – whether evolutionist or related to other schools, with bibliographic references that seemed often eclectic or heteroclitic – did not necessarily prevent them from grasping or trying to grasp “the native’s point of view”. Accordingly, it may come as a surprise that E. E. Evans-Pritchard’s (1902–1973) *The Nuer* (1940) was not the first monograph dedicated to the (anthropologically speaking) celebrated Nilotic people but, in fact, the third. The first one, *The Nuer of the Upper Nile Province*, was authored by army officer and colonial administrator Henry Cecil Jackson (1883–1962). Appearing in the *Sudan Notes and Records* of 1923 and published in the same year by El Hadara Printing Press in the Anglo-Egyptian Sudan, this rare book has never been properly analysed by historians of anthropology. Despite the fact that real-life details were permeated with Frazerian overtones, including cross-cultural comparisons that explicitly referred the reader to *The Golden Bough* (1890), Jackson revealed the intimate relations between men and cattle – “they live for, by, and with the cattle”, as everything seemed to indicate the existence of “an almost physical and spiritual bond between the Nuer and his cattle” (1923: 59, 94). “Savage” beliefs could only sound absurd and incredible to those ignorant in matters of anthropology, he added. The second ethnographic study of the Nuer, *Nuer Customs and Folklore*, written by missionary Ray Huffman, was published in London in 1931.

One side effect of ignoring this vast archive has been the neglect of women ethnographers doing fieldwork before Malinowski. Our volume includes analyses of the work carried out by three such women: Katie Langloh Parker (mentioned above); Malinowski's friend and colleague Maria Czaplicka (1884–1921), who committed suicide at the age of 37 but left a remarkable ethnographic legacy; and Alice C. Fletcher (1838–1923), whose work is inseparable from that of her Omaha collaborator and “adoptive son”, Francis La Flesche (1857–1932) (Kubica, Scherer, in Rosa and Vermeulen 2022). In fact, Indigenous individuals engaged in ethnographic projects in the period from around 1870 to 1922 should be acknowledged in more consequential ways, whether as interlocutors, collaborators, or ethnographers in their own right. Post-colonial theorists have made trenchant deconstructions of white ethnographers claiming to be the vessels bearing knowledge of a vanishing world, saved in print for a Euro-American posterity. In our volume, we have avoided radical views on the cosmopolitics of the archive and have chosen to pursue decolonization in other directions, namely by reassessing salvage ethnography as being an Indigenous archive in fundamental ways. Rather than being powerless or passively manipulated, Indigenous and mixed descent individuals could commit themselves to preserving their knowledge in print and being involved in salvage ethnography as a project that concerned the past, present and future of their communities. Thus, there is room for discovering varied illustrations of informed, negotiated or proactive Indigenous participation in pre-modern ethnographic projects in which the allegedly predatory dimension of ethnography gave way to the collaborative one.

The place of Indigenous ethnographers in the history of anthropology is also a matter of perspective. Linda Tuhiwai Smith regrets the fact that, for reasons that relate to the enduring political power of Western science, the name of Elsdon Best (1856–1931) still outweighs that of Māori individuals who generously permitted his job to be done (Smith 2012 [1999]: 88). She portrays him as a someone who took advantage of his Indigenous collaborators, but there is a strong case for the inextricability between Māori and “white” projects of salvage ethnography in New Zealand/Aotearoa in that period, as Jeffrey Paparoa Holman claims in Chapter 5 of our volume. In his reconstitution of their passage between communicable worlds and interests, he highlights the fact that Tutakangahau (1832?–1907), who had witnessed cultural transition in his youth, saw the ethnographer Elsdon Best as an opportunity and not as a threat. In fact, Tutakangahau invested himself and Best, respectively, with the halo of master and disciple, of ultimate donor and legatee of the vanishing knowledge.

Living in post-structural times, with anthropologists claiming all kinds of new fieldwork experiences and new ethnographic results, we should give a second chance to pre-structural views incorporated in the writing of earlier ethnographers, as they addressed issues and approaches that were later relegated to the margins of canonical trends in social anthropology. Whether as pre-colonial or colonial history, attention was given by several pre-Malinowskian ethnographers to ethnic, cultural, and linguistic variety and mobility in the same context or within relatively circumscribed regions. One case in point is James Mooney (1861–1921), who put participant observation into practice thirty years before

Malinowski, by giving his hands to Cheyenne and Arapaho ghost dancers, both men and women, for whom the Ghost Dance embodied their faith in a better future. Mooney understood it as a traditionalist novelty whose diffusion from reservation to reservation was related to colonial oppression and made possible by new means of communication. It is unfortunate that Mooney and his 1896 monograph, *The Ghost-Dance Religion and the Sioux Outbreak of 1890*, are systematically absent from history of anthropology handbooks informing new generations of students. Current perceptions of late nineteenth-century anthropology would be very different if Mooney's work was taken as an illustration of relevant ethnography during the so-called age of armchair scholarship. [4]

In celebrating the centennial of *Argonauts of the Western Pacific* in 2022, let it be clear that we are not questioning Malinowski's undeniable merits and the innovations he brought to the monographic genre. We are certainly not the first to shatter the myth of Malinowski as "self-proclaimed inventor of modern fieldwork" (Stocking 1995:13), as there are countless but scattered contributions to the reassessment of this or that ethnographer that may be found in biographies and monographic volumes, specialized journals and books within specific area studies or national traditions, online encyclopaedias, and dictionaries. Nevertheless, we hope that our volume will help open the historical horizons of the twentieth-first century anthropological community by unsettling some deep-rooted assumptions – including the idea that intensive fieldwork in a single context by a single individual and its corresponding output, the monograph, were twentieth-century inventions. Before arriving at the hasty judgment that earlier accounts were mere travelogues, expeditionary surveys or defective descriptions containing miscellaneous or dry compilations of odds-and-ends, we should get back to reading them with an open mind. Our volume includes twelve case studies from a surprisingly large field, each ethnographic account containing several layers of meaning, style, and content that inspire open-ended readings and are projectable into the future.

ROUND TABLE PART I

3. Franz Boas's Year Among the Inuit Of Baffinland And The Lessons He Derived From It For Anthropology, by Herbert S. LEWIS (University of Wisconsin-Madison)

Franz Boas undertook his first fieldwork among Inuit in Baffinland in 1883, almost the same time as Karl von den Steinen went to the Xingu, and Alice Fletcher and Frank Cushing began their fieldwork among the Omaha and the Zuni. [5] Clearly the idea of immersive fieldwork was in the air at that time. But when Boas settled in the United States in 1886, he consciously and explicitly set about to lead the development of a new discipline in his new country, with very new theoretical and ethical perspectives, three decades before Malinowski's efforts at the London School of Economics. Boas's anthropology had a biological component, and encompassed prehistory and museum work as well, and was much broader than the discipline that Malinowski and Radcliffe-Brown later created in Britain. Its cultural or

ethnographic writ went far beyond the production of texts, as consideration of the corpus produced by American ethnographers readily attests, and culture history was but one significant element in a much broader enterprise.

Boas was quite clear that his anthropology was to deal with all cultures. In 1905 he wrote to one of his patrons, “...the principal use of our anthropological work is to make students in our country familiar with the value of foreign cultures and civilizations, and to make our people more appreciative of the achievements of foreign nations. It so happens that my own personal work is directed primarily to the study of primitive tribes, but I consider my own work as merely a phase of this more general problem; and I have been working towards the introduction and extension of studies relating to the civilizations of foreign nations, such as the Latin countries and also to those of Asia” (see Lewis 2020).

Franz Boas was remarkably well prepared to undertake ethnographic research in the Arctic as a result of his thorough grounding in physical sciences and human geography. His approach was also underlain by the humanitarian and ethical values he absorbed from his politically progressive German-Jewish home and contemporary liberal movements in Germany. The following passage from his “letter-diary” to his fiancée has often been cited as central to his transformation from a geographer to an ethnologist: “I believe that if this trip has a significant impact on me as a thinking person, then it is the strengthening [note] of my notion of the relativity of all *education* and the conviction of how the value of people lies in their *Herzensbildung* (education of the heart), which I find or miss here, just as at home, and that thus all service which a person can render to humanity must depend on the furthering of *truth*...” (Lewis 2022:68). And by the time he had finished his year among the Eskimos, 26-year-old Franz had apparently formed the central ideas he would transmit to the incipient anthropological community in the United States, his new homeland.

The essay on Boas in this volume deals with his fieldwork and his major ethnographic monograph, *The Central Eskimo* (1888), and it also draws on the newspaper articles that he published in Berlin and New York City. In these short pieces we can see the fundamentals of his approach to the understanding of peoples of other cultures that he would impart to his students over the next five decades. His “savages” (a term he used in quotation marks indicating irony) were both rational actors *and* were as “shackled by tradition” as were his own people and the students he taught in New York City.

The Central Eskimo and his letters to his fiancée firmly demonstrate that “intimate, yet authentic picture of aboriginal life” that his student Robert Lowie mistakenly thought he had not attempted. Here are excerpts from one of those articles:

We recognize more and more that a scholar, who studies a primitive people, must not be content [to] simply [study] the obvious customs and traditions of foreigners. He needs to become thoroughly familiar with the thinking and point of view of a people . . .

From this it follows that the collection of ethnographic material cannot be a task to be simply performed by any traveler ... The researcher has to live

entirely like a member of the tribe which is the object of his investigation. As long as a foreign people look upon a traveler as an intruder, or worse, as an enemy, the characteristics of its thought and of its emotional life will never be revealed to him ... Many of its customs will completely elude the researcher; others will remain incomprehensible...

The designation of so many primitive people as “savages” became popular and widespread only because travelers looked at the behavior of foreigners shortsightedly from the point of view of our European social institutions. They lacked a thorough knowledge of their languages, of their emotions, of their thinking, of their religious ideas, and of their traditions. Without this knowledge, the lifestyle of a primitive people, so far removed from our own thinking and our own way of life, may look absurd and unworthy of any kind of human society. Only because of such misunderstandings could the use of the term “savages” come about for tribes whose way of life is governed by strict religious rules, and whose rich imagination has created a plenitude of legends, songs, and inventions. (Boas 1885) [6]

He concluded another article this way:

After all the many little adventures, and after a long and intimate intercourse with the Eskimo, it was with feelings of sorrow and regret that I parted from my Arctic friends. I had seen that they enjoyed life, and a hard life, as we do; that nature is also beautiful to them; that feelings of friendship also root in the Eskimo heart; that, although the character of their life is so rude as compared to civilized life, the Eskimo is a man as we are: that his feelings, his virtues, and his shortcomings are based in human nature, like ours. (1887: 402)

These sentiments were not travelogue clichés for Boas. He was speaking to an incipient field of study whose amateur proponents were generally under the spell of both racial determinism and evolutionary thought in which “the savage”, “primitive man” was believed to be lacking such virtues as aesthetic enjoyment and friendship. From the moment of his immigration to the United States in July 1886, he started to demonstrate the falsity of those notions, as he would continue to do throughout his career. This was the message he brought home from his first venture as an ethnographer.

Franz Boas came to the United States at the time when university education and academic disciplines were moving to a new professionalism, and the model for many was the development of science and graduate education in Germany. He came with the prestige of German science as well as his own drive and energy, to impress his knowledge and understandings on the nascent field in the United States. He had to fight entrenched individual and institutional interests, racist and racialist attitudes, anti-Semitism, and the ethnocentric perspectives of the day, but his abilities were undeniable and were soon recognized. His approach to human biology, culture, and language, would come to be the basis of most of American anthropology in the twentieth century.

4. Katie Langloh Parker, A Female Ethnographer of Noongahburrah Women (Euahlayi Tribe), by Barbara Chambers DAWSON (Australian National University, Canberra)

From 1879 to 1901, Katie Langloh Parker (1856–1940) lived among the Noongahburrah (Nhunggabarra) of the Narran River region – members of the larger Euahlayi (Yuwaalaraay) tribe – whose country stretched across forty-six hundred square miles in far northern New South Wales. [7] In 1905 she published *The Euahlayi Tribe: A Study of Aboriginal Life in Australia*, Archibald Constable and Company Ltd, London. She documented a significant body of knowledge from Noongahburrah oral tradition and was confident that her study of the “manners, customs, beliefs, and legends of the Aborigines of Australia was an important contribution to ethnographic enquiry” (Parker 2018 [1905]: 17). By 1922, the study of human societies and their cultures was beginning to be recognized as the discipline of social anthropology. Parker’s vital contributions to anthropological knowledge places her within the annals of a discipline to which she deservedly belongs.

Born Catherine Eliza Sommerville Field on 1 May 1856 at Encounter Bay, South Australia, Katie understood from an early age that Australia was made up of many different Indigenous nations, living on their own separate country and with their own language and culture and customary laws, beliefs, and stories. Her pastoralist father Henry Field farmed on the country of the Ramindjeri. Although she briefly attended a girls’ school in Adelaide when she was sixteen, after the death of her mother in childbirth, Katie was otherwise “home-schooled” (Papers of Marcie Muir relating to Katie Langloh Parker; [8] Muir 1990). Intelligent and well read, she had been taught by parents with a high educational standard: her maternal grandmother could read the Old Testament in Hebrew and the New Testament in Greek. Her father, who had had a classical education, taught his children about Greek life and culture and also read to them Aboriginal legends. In 1859 the family moved to Marra Station, ninety miles north-east of Wilcannia, on the Darling River, to Paakantyi country. Katie also had opportunities to grow close to Aboriginal people. As a child of almost six years, she was saved from drowning in the river by the family’s Aboriginal nursemaid “Miola”, who was unable to save Katie’s two sisters (Parker 2018 [1905]: 17; Dawson 2012). Miola subsequently joined the surviving Field children in the school lessons provided by their mother.

In January 1875, aged eighteen, Katie married Langloh Parker (1840–1903) and they moved in 1879 to Bangate station, a sheep and cattle station, in the remote outback regions of Australia near Goodooga in far northern New South Wales, 530 miles north-west of Sydney. By 1887 it stretched across 215,408 acres (336 square miles), with more than 100,000 sheep and several thousand head of cattle (Muir 1982: 34–35). Because her husband was a popular and well-known colonial pastoralist, being much older than Katie, she adopted her husband’s first name “Langloh” for her publications, to give her books a notable profile.

Bangate was a huge property typical of others “taken up” by Europeans on Indigenous ancestral land. It had a large, predominantly male workforce, including Indigenous workers. Occasionally a white female cook or laundress might be employed on the station but, as

lower-class workers, they were considered unsuitable companions for the wife of the owner. A fall from a horse soon after Katie's arrival at Bangate precluded any further riding activities and prevented her from visiting neighbouring females of her own class, who might live hundreds of miles away on distant properties. As the daughter and wife of colonial landholders, who had settled on huge tracts of Aboriginal territory at a time of pastoral expansion and as the mistress to young Aboriginal women who worked in menial household jobs in a manipulative power imbalance, Katie Parker was complicit in colonization (Evans, Grimshaw, and Standish 2003: 15). Childless, in a male-dominated and isolated environment, she turned to the local Noongahburrah people to compensate for loneliness, and for companionship. It was through the friendly, accommodating and kindly attitude of the Noongahburrah that they accepted her. She grew close to them and learned to know them as individuals. A mutual trust engendered personal conversations between the white woman and the Noongahburrah.

Knowing about Greek and aboriginal mythology, Katie Parker approached the Noongahburrah, curious to hear their traditional stories and eager to gain an intellectual outlet in her solitude. Her opportunities for carrying out "fieldwork" were as close as the area outside her front door. Living alongside the Noongahburrah, sharing life on their traditional lands for over twenty years, she writes that she gained their trust, learned their language and associated dialects, and established close relationships. She could speak and understand the Noongahburrah language and she recorded and transcribed it. The Aboriginal women generously incorporated her into Noongahburrah society, giving her the Aboriginal name "Innerah", which meant "a woman with a camp of her own" (Parker 2018 [1905]: 100).

Parker collected her data primarily through interactive conversations and by listening to and observing the tribal rituals and ceremonies. She used the same ethnographic methods as Malinowski's, "watching [the Trobriand Islanders] daily at work and at play, conversing with them in their own tongue, and deriving all ... information from the surest sources – personal observation and statements made to [the ethnographer] directly by the natives in their own language" and, importantly in ethnographical practice, by grasping "the native's point of view", (Malinowski 1972 [1922]: vii–viii, 24–25). Through these means she was able to produce an account of Noongahburrah culture as she observed it, elements of which she could compare with other ethnographic findings and relate to anthropological theories of her time.

For her information, Parker was utterly dependent on Noongahburrah cultural experts, relying on four main "informants": Peter Hippi, Hippitha, Barahgurrie and Beemunny. The chief one was Peter Hippi to whom she dedicated her collection of *Australian Legendary Tales* (1896). Through patient repetitions from the Noongahburrah, she assiduously transcribed their legends. All the ethnographical findings were checked and double-checked with the Aboriginal collaborators, and were unsullied by any of her own interpretative additions that might have allowed her own ideas or prejudices to intrude. By her own assertion, she was scrupulous in reproducing the exact translation of Euahlayi knowledge. At the end of *Australian Legendary Tales*, she wrote down the story of Dinewan Boollarhnah

Goomblegubbon in Aboriginal language (Parker 1998 [1896]: 126–28), and in *Australian Legendary Tales*, *More Australian Legendary Tales* (1898) and *The Euahlayi Tribe*, she provided three different glossaries.

In particular, as a female ethnographer, Katie Parker's closeness to and the affection shared between her and two key Indigenous female elders – Beemunny and Bootha – gave her access to arcane feminine knowledge. She recorded the Noongahburrah women's experience and cultural stories surrounding female initiation, betrothal, marriage, childbirth and the raising of children. These beliefs, customs and practices formed a field of study, left largely unexamined by male ethnographers. She recorded the agency, authority and identity within the group of these older women. Through their interaction with Parker, the women allowed her direct and personal participation in customary practices, such as burial rites. Her friendship and empathy with the women enabled her to write of their personalities and life events with sympathy and warmth; her descriptive accounts, with their intricate details, were told with sensitivity.

Although somewhat dismissed after publication in 1905, *The Euahlayi Tribe* is still relevant today, not only within the ongoing history of social anthropology – through the depth of the Euahlayi cultural knowledge (their ceremonies, rules and behaviours) – that Parker recorded, but also from her warm depictions of the generosity of the Noongahburrah, who welcomed the white woman onto their country, befriended, and essentially cared for her over their time together.

Parker wrote at a time in Australia's history when the forces of the assimilation of Indigenous people (into white society) were in danger of changing, even obliterating, Indigenous customs and belief systems. At this time – at the end of the nineteenth century – there was a popular idea that the Aborigines were “a race fast dying out”. Parker's ethnographical work of recording Noongahburrah culture and practices rescued this cultural knowledge.

First Nations people now find their contribution to modern Australia increasingly acknowledged. After the 2022 Australian Federal Elections, there were eleven Australians of Indigenous descent serving in the Australian Federal Parliament. Academic interest in Aboriginal history – and the benefits that can be applied to Australian society from Indigenous culture – is increasing. Parker's validation of Indigenous cultural strength can be seen as a foundation for this growing appreciation.

5. Westermarck and Social Anthropology, by David SHANKLAND (Director RAI/University College London)

Edward Westermarck (1862–1939) flourished and died just before the establishment of social anthropology as we now know it, which arguably reached maturity with the founding of the Association of Social Anthropologists (ASA) in 1946. [9] The ASA quickly assumed a leading role in shaping the way the university departments developed. Equally, its members wrote

textbooks defining the new discipline, and were responsible for teaching successive generations of students. In doing so, they covered a great deal of ground very quickly, as a glance at say Beattie's *Other Cultures: Aims, Methods and Achievements in Social Anthropology* (1964), one of the most popular of these books, illustrates. Busy then, with the exploration of the new possibilities of their fresh approach, how did they treat those who helped to bring about the sweeping success that they achieved? Or, to put this another way, how did they treat the memories of those who were soldiers, revolutionaries to the cause, along the way?

Westermarck, from this point of view, is an excellent case in point. There is no doubt that he had struck many heavy blows: he had throughout his career written against the careless use of survivals; he had conducted a great deal of field research; he had written extensively on moral relativism; he had been widely plied with prizes, and above all he was the teacher of Bronisław Malinowski, the acknowledged leader of the new movement, whose followers were now firmly in place. This, then, might have assured him a place on the plinth of the Great Founders. Alas, though (from the point of view of Westermarck's memory), the exact opposite happened. He was firmly excluded from disciplinary memory, regarded as not being fit for the new incarnation.

What reasons were given? There were, at various points, many. His fieldwork was regarded as being diffuse, his underlying philosophy too close to evolutionism, his writings redolent of the scissor and paste of the previous generation. Institutionally, his position could also be regarded as slightly uncertain. His chair at the London School of Economics was in sociology, rather than in anthropology as Malinowski's came to be. For many years, he taught in London only one term a year, his remaining time spent in Åbo, Finland, or in Morocco, where he was conducting fieldwork. All this is certainly true. There is, however, a further potential difficulty. Though he was extraordinarily discrete throughout his life, he was homosexual. As he made no mention of this in his written work it is tempting to set this to one side when considering his oeuvre. Yet I now think that it is central to his life's work, and indeed his motivation to become an anthropologist in the first place. In turn, I think that an appreciation of this helps to explain – at least in part – why he was eclipsed so thoroughly in the historiography of anthropology.

To consider the obvious motivation first; the question as to whether his sexuality impeded his professional career in a direct way. As I have been able to find nothing in his papers, published or unpublished about this, we can only go by the external signs of advancement. Here, one can only conclude that there appears to have been no such obvious prejudice; he was appointed to professional positions very early in both Helsinki and London; he was supported by the established great figures in the field at the time (particularly A. R. Wallace and J. G. Frazer), he received the Huxley medal from the Royal Anthropological Institute, and he was published widely. At Åbo he was invited to take up the position of university rector, and though there were various disputes and dissensions during his time there, there is nothing to suggest that these were not the usual, recognisable academic tussles. Over and above this, in as much as he sought to obtain a role in public affairs, he did so in support of

Finland, and he appears to have been respected by his country for doing so. Certainly, he had enough influence to obtain a knighthood from the Finnish state for his Berber fieldwork assistant, Shereef 'Abd-es-salam El-Baqqali in return for the many decades of help he had given Westermarck in the field (Shankland 2014).

Even if, however, his sexual orientation did not affect his academic career in any immediate way, there does appear to have been some doubt. Adam Kuper (personal communication) has remarked that Raymond Firth told him that he had no desire to acknowledge Westermarck's historical contribution because of his activities in North Africa. The difficulty then becomes to disentangle the mores governing homosexuality between the wars: certainly discrete homosexuality appears to have been little remarked upon in Fabian or academic circles such as those which Westermarck lived in, but, even if it is difficult to approach the matter more than circumstantially at this distance, perhaps there was also a sense that those who preferred to desport themselves overseas in North Africa in the fashion of Oscar Wilde, Bernard Bosanquet, and André Gide should be disapproved of even within this wider tolerance (Gellner 1981 touches on these issues in his discussion of *Si le grain ne meurt*).

In the absence of more information, it is difficult to be clearer. Yet, there is a different way to approach the matter, which is to look at the totality of Westermarck's writings, from his first work until his final writings in order to appreciate the motivation that drove him to become an anthropologist in the first place. This is rarely done today. Westermarck, when he is not ignored, seems to be acknowledged piecemeal: his early theories concerning marriage, kinship and incest avoidance are occasionally referred to, as indeed is, amongst specialists at least, his later work on Morocco. His approach to morality, however, which is key to an understanding to his life's work is more or less ignored, and certainly almost never considered within the context of his other writings. Yet, it frames his work or, to put it another way, Westermarck appears to have spent his intellectual life working through a particular set of ideas that struck him very early on in his career, and his different books can be seen as consistent explorations of his returning to these central themes.

The underlying motivation appears to be this: why is it that homosexuality (or any other custom) should be disapproved of from the ethical point of view? This thought took him to considerations of the nature of Christianity, the social roots of ethical and moral systems, the comparative exploration of customs in various societies around the world, and highly detailed fieldwork in Morocco. His exploration, repeated in many different ways, consists of an analysis of the way that that individual actions are constrained by societies' collective understandings, and correspondingly to the conclusion that there is no essential reason why such constraints should exist. The conclusion I reach then in my chapter in *Ethnographers Before Malinowski*, upon which these brief reflections are based, is that Westermarck was above all a kind of Henrik Ibsen, an intellectual stimulated to explore and expose by the – to them – stultifying conventions of high bourgeois life in Helsinki or Oslo at the end of the nineteenth century.

In a short summary of Westermarck, one can hardly do such a rich and varied thinker

justice, but it may suffice to discuss several of his major works briefly in turn to illustrate this: his *History of Human Marriage* (1891) can be seen as an attempt to work out what rules governing social life are essential, and what contrived. Here, taking marriage as the foundation of society, he concludes that some kind of pair bonding appears to be part of the biological make-up of all humans, and certain other species, even if the way that marriage is conceived is subject to multiple interpretations. This is later expanded to a wider consideration of morality, in his *Origin and Development of the Moral Ideas* (1906–08), where he assumes that given the biological capacity to empathize or to be sensitive to approbation or disapprobation, a moral system, however varied, can emerge that is profoundly effective in assuring social control. Again, what *kind* of social control may emerge is not foreshadowed; it can and does take an immense plurality as he documents in some detail.

In his most lengthy exploration of his Moroccan fieldwork, his two volumes *Religion and Ritual in Morocco* (1926), he attempts to set out these ideas in more ethnographic detail. Through extensive discussion of the way that actions may be proscribed or permitted from the religious point of view, he provides a fascinating insight into the constraints that operate on everyday behaviour, both from the point of view of an individual's present actions and in evaluating their past (or indeed possible future) consequences. Then, in two final monographs completed before his death, he returns to his intellectual beginnings: in *Ethical Relativity* (1932) he writes out his ideas concerning the social context of ethical systems and morality in a clear and coherent way, perhaps acknowledging in doing so that the scissors and paste presentation of their first publication made it unnecessarily difficult for the reader to follow, and concludes with a final critique in *Christianity and Morals* (1939).

Of course, Westermarck wrote a great deal, in more than one language. I think, however, on balance that the coherence is there, and not imposed by my reading. If so, it helps us to explain why it was so difficult for later generations to grasp the importance of his contribution. Already, indeed, they were grappling with the different style of fieldwork that he conducted and the way that he wrote it up: a curious middle-ranged inquiry, with many comments on different customs and where they are found, but without wider description of the communities concerned. Further to this, Westermarck's insistence on following so consistently his line of inquiry makes it very difficult for the reader to pause, to take a mental breath, and simply absorb their impression of life amongst the peoples where Westermarck lived. It might be argued, as indeed, Gellner (1995) did, that the social anthropological texts which came after Malinowski lacked obvious coherence. But they had the great saving grace of being profoundly informative, of providing an enormous amount of detail that the reader could absorb without the authorial voice being over obtrusive.

All this means that Westermarck could not easily become known as the founder of modern anthropology. He taught for many years at the LSE and encouraged his students to conduct fieldwork, but did not codify any general practice that ethnographers should follow: he has no chapter in *Notes and Queries*, nor did he have anything approaching the first chapter of Malinowski's *Argonauts of the Western Pacific*. His ethnography, in spite of its unusual

presentation, is of the highest quality. But it had few emulators in the now established social anthropological tradition that was already becoming crystallized as his career was coming to an end. He certainly should be known as a pioneer of fundamental importance in the anthropology of morality and ethics, but by the time that this topic became of general interest, his work was more or less forgotten. And those specialists who mined his work for its insights, vital but incidental to his main intellectual preoccupation, could do so without needing to take on board his motivation for writing in this vein in the first place.

There is yet a further reason. Even if a reader is not put off by the distinctive presentation, by the need to read different parts of his life work to understand the whole, and the sheer amount of text that he produced, his theoretical conclusion as to the way that communities realize social control is firmly based in the individual and their interaction with the rules and norms that govern behaviour. Yet, precisely at this time the core ethos of the Association of Social Anthropology and the Oxford Institute from which it emerged was the study of social structure, one that minimized the role of the individual in approved Durkheimian fashion. An analogy would be that of a careful reader who thoroughly absorbed all the necessary apparatus to become an expert in a particular school of philosophy, only to find that it contrasted with the, at that time, hegemonic paradigm. Even if they found it of interest, and had the training to follow it, there would have been little incentive and certainly no encouragement to do so.

My interpretation of Westermarck is, then, to sum up a man who is puzzled. He wants to know why he has been unable to express his sexuality in the way that he pleased. Politically he is a liberal, equally a supporter of women's rights, and writes in a fashion that tries to show that social constraints, even if they must inevitably be based in the biological entity that we all inhabit, are not directly drawn from it or any other essential rules. Instead, by his account, they come about through the collective, emotional social interaction that is part of being human. Yet, we should not conclude from this that he is a romantic, seeking relief in the Orient, or in the ancient Greeks, or in the supposed primitive tribes from the coldness of Scandinavian high culture. He certainly did not see himself as a Rousseau seeking after a relief from rationality in strange cultures. Though he himself did move restlessly from Finland, to England, and to North Africa in a continuous triangular journey, it is clear that he felt that any human society gave rise to similar constraints. His answer was not to flee like Paul Gauguin but instead to seek to investigate in the most rational way he could, to dissect calmly and thoroughly, these seemingly universal but wildly varying constraints in the hope of alleviating them through exposing them to inquiry.

For Westermarck, it is therefore through rational argument that one may develop the possibility of societal change. He pales against the colourful forcefulness of Malinowski exhorting his followers to adventure, escaping from their fellows to encounter a real world unobscured by contrivance and convention. But arguably, when we do grasp his project, we can see that he is none the worse a thinker for it.

6. Henrique de Carvalho's Intensive Fieldwork: A Nineteenth-Century Version of Malinowski's Charter Myth, by Frederico Delgado ROSA (Universidade Nova de Lisboa)

Friendship is a recurrent theme in various chapters in *Ethnographers Before Malinowski: Pioneers of Anthropological Fieldwork, 1870-1922*. [10] Herbert S. Lewis quotes Franz Boas on his "Arctic friends" (74). Barbara Chambers Dawson tells us that Katie Langloh Parker joined the Aboriginal women and girls when they went swimming in the river – a vignette allowing us to imagine a wider picture of empathy that facilitated intensive ethnographic experiences. The theme of friendship reappears in my chapter on Portuguese ethnographer Henrique de Carvalho (1843–1909), titled "From Savages to Friends: Henrique de Carvalho and His *Etnografia e História Tradicional dos Povos da Lunda* (1890a)". Human closeness is at the core of Carvalho's experience in Central Africa – namely in the country of Lunda, where he lived between December 1884 and October 1887 – as abundantly illustrated in his manuscript diaries and the resulting four-volume travelogue (1890b), but also in passages of the ethnographic monograph that he published, such as: "During a serious illness in which I lost all consciousness (...), I was treated by them with the most remarkable affection. (...) I returned to myself after about eight days, so they told me. Then I found myself surrounded, not by savages, but by dedicated friends who manifested their joy and felt truly happy to have saved my life" (1890a: 45).

This important dimension of Carvalho's ethnographic encounter is inseparable from the meticulousness of his observations, in the sense that he totally immersed himself in local politics and became a close friend of important political figures, in particular the exiled prince Samadiamb (dates unknown), whom the courtiers urged to return to the Lunda empire's capital. What is interesting about Carvalho is that his expedition became like a travelling court if not the epicentre of local politics when he decided to join Samadiamb. This forced him to follow the (very) slow rhythm of Lunda royal protocol and etiquette, with endless stops, audiences, even the construction of provisional stately buildings, but gave him privileged conditions to pursue intensive ethnographic fieldwork. The tension between expeditionary and intensive fieldwork is clearly brought into question by this case study.

Eventually Samadiamb gave up his claim to the Lunda "throne". The two men separated and Henrique de Carvalho went on his way without his Lunda friend and without the two other Portuguese officers who made up the expedition, whom he simply ordered to withdraw. Carvalho agreed that about twenty African companions whose loyalty was not affected by the trials of the expedition would remain at his side. "I will never forget my debt to these companions", he wrote, "I don't care about their humble position, their social status and where they came from! I know that they are men of feelings (...)" (1890a, IV: 12). The sincerity of such a proclamation seems to be attested by another important dimension of Carvalho's work: the photographic album of the expedition. [11] If the photos were taken by one of his subordinates, it was Henrique de Carvalho who wrote the handwritten captions, which he completed with biographical information and comments on each individual, including the

humblest, such as the expedition's porters. Along with surprising psychological insights, the unparalleled wealth of photographs and their respective captions allow us not only to know the faces of Carvalho's African interlocutors, but also to get an idea of their personal feelings – so asserts Beatrix Heintze, an anthropologist and historian of anthropology specializing in the region (1990; 2011).

Heintze goes even further and states that “in terms of the number and variety of details presented there, as well as its multiple dimensions, the monumental work of Henrique Dias de Carvalho has so far been not only insufficiently recognized, but completely ignored outside the Portuguese-speaking world” (2011: 97). As his staunchest defendant, Heintze writes: “Anyone who is not particularly interested in details will soon feel overwhelmed by Carvalho's accounts. And even scholars of late nineteenth-century eastern Angola and southern Congo (...) seem to have capitulated at some point in the face of his writings” (Heintze 2011: 96). Heintze laments the fact that the all-too-brief references to his work only concern only a small fraction of the single-volume *Etnografia e História Tradicional dos Povos da Lunda*.

In fact, excerpts of this monograph were translated by Victor Turner (1920–1983) and published in 1955 in *The Rhodes-Livingstone Journal* under the title “A Lunda Love Story and Its Consequences: Selected Texts from Traditions Collected by Henrique Dias de Carvalho at the Court of the Mwantianvwa in 1887”. Thanks to Turner, the foundational love story of the Lunda empire is the only (relatively) famous passage of Carvalho's work, as this corpus (along with Belgian colonial ethnographies and other sources on Lunda and related contexts) nourished a scholarly debate of significant proportions, especially in the 1970s and 1980s. Historians of Africa gathered around tutelary figures such as Jan Vansina (1929–2017) and Joseph C. Miller (1939–2019), who used these ethnographic materials to reconstruct the formation of Central African kingdoms, namely those of Lunda origin; while structural anthropologists led by Luc de Heusch (1927–2012) short-circuited the historical reading of what they considered to be a symbolical construct whose variants occurred within a geographically and ethnically broad mythological cycle: that of the foreign hunter who marries the autochthonous princess and establishes sacred kingship. Further divergences and rapprochements within and between the two camps have complicated the debate.

But Carvalho's monograph only appears in this literature as a footnote reference (e.g., Vansina 1966: 60–75; De Heusch 1972: 182). Let us also recall that the Ndembu, made famous in anthropology by Turner, are southern Lunda/Aruwund and historically descendants of the northern Lunda. For this reason, Turner's monographs include occasional references to Carvalho. In my chapter, I try to demonstrate that Carvalho's *Etnografia e História Tradicional dos Povos da Lunda* presents a challenge to twenty-first-century readers for two reasons. Carvalho's monograph contrasts with Malinowski's *Argonauts of the Western Pacific* (published thirty-two years later) in terms of structure and of style. It lacks explicit holistic connections. But we may say without a fear of presentism that its opening pages sound like a nineteenth-century version of Malinowski's charter myth, thanks to passages in which Carvalho

advocated the need to live the life and use the language of the local people: “These [words] should bring to our brains the images and sensations of local objects according to the same psychic emotions and comparisons that these objects provoke in the minds of that people. Only then can we feel these emotions or understand them as they feel them, as they conceive them” (Carvalho 1890b: xxii).

George W. Stocking, Jr. (1928-2013) issued a warning against the risks of anachronism when appreciating nineteenth-century uses of the word *culture*. He called attention to how evolutionary status was “frequently argued in racial terms” (1966: 870). But when Carvalho wrote that so-called savages also had “their culture” – in Portuguese, “*sua cultura*” (1890: v) – the plurality behind this phrasing is implied. Carvalho discarded anthropometry and the pertinence of taking the “biological conditions” of the peoples of Lunda into account, and even refused to plunder graves, considering it a shameful activity. Moreover, Carvalho’s notion of culture also dialogued with folk perceptions of it in Africa. One of the local proverbs that Carvalho collected encapsulates the greatest lesson he brought from Africa: “When among the Lunda, do as the Lunda do; when among the Cokwe, do as the Cokwe do” (Carvalho 1890b, II: 667).

In our volume, Han F. Vermeulen and I have tried to avoid an excessively Anglocentric perspective on disciplinary history. Anglophone monographs have been selected as case studies, but not to the exclusion of ethnographies from other settings, representing lesser-known anthropological traditions. This is the case of Carvalho’s ethnography. If he had not belonged to the minor tradition of a peripheral country, he might have had a better chance of being recognized as an important ethnographer whose work unsettles ingrained dichotomies, such as evolutionism versus particularism. A colonial figure and a man of his time, Carvalho did, however, anticipate certain aspects of the modern anthropological sensibility – and he pioneered a later theme of Malinowski’s anthropology: culture contact in colonial Africa.

In his monograph, Carvalho never uses the word *taboo*, whose international repercussion is associated with *The Golden Bough* (1890), the celebrated bestseller published in the same year as his own ethnographic historiography. In the following, expanded editions, James George Frazer (1854–1941), ever the polyglot, might have perused Carvalho’s ethnography – if only he had known about it – for its abundant data on sacred kingship. A comparison between the two men and their respective works would be meaningful. Who does not know Frazer’s *The Golden Bough*? But the invisibility of Carvalho’s *Etnografia e História Tradicional dos Povos da Lunda* also stands for the oblivion of ethnographers before Malinowski. If their rightful place in disciplinary history has been overshadowed, this is because of the disproportionate spotlight given to nineteenth-century armchair anthropology to the detriment of ethnography.

7. Developing Fieldwork and Analysing Historical Processes: German Ethnographers in South America Between Recognition and Criticism, by Michael KRAUS (University of Göttingen)

What most chapters of *Ethnographers Before Malinowski: Pioneers of Anthropological Fieldwork, 1870–1922* make abundantly clear is that so-called modern methods of ethnographic fieldwork had been developed long *before* 1922 – and *not* by one person, one school, or in one country, but by many people in many different places. [12] What I try to show in my own chapter “Developing Fieldwork in the South American Lowlands: Debates and Practices in the Work of German Ethnographers (1884–1928)” is how a number of German ethnographers – such as Karl von den Steinen (1855–1929), Theodor Koch-Grünberg (1872–1924), Konrad Theodor Preuss (1869–1938) and others – had already implemented the principles presented by Bronislaw Malinowski (1884–1942) in the methodological introduction of *Argonauts of the Western Pacific* (1922).

For example, like Malinowski more than 30 years later, Karl von den Steinen emphasized the qualitative difference that arises when one stays alone in a village, not in a large group. Theodor Koch-Grünberg not only described the life in the villages he visited in his diary, but regularly put his pencil aside and participated in hunting, cooking, dancing, playing, smoking, singing, telling stories, sharing things and information. Konrad Theodor Preuss did not trust the information provided by translators any more than Malinowski did. With his extensive collections of myths recorded in the native languages he created a *corpus inscriptionum* as Malinowski called for in the famous introduction to his *Argonauts*. Already before World War One, Preuss – who lived in Cora, Huichol, and Mexicanero villages in Mexico from 1905 to 1907 and conducted ethnographic research among the Uitoto and Kogi of Colombia in 1914 and 1915 – pointed out that fieldwork should ideally take at least a year, as this would allow one “to observe the cycle of annual festivals” (Preuss 1912: iv). Doing field research, the ethnographer should “feel fully like a learner and record as precisely as possible the worldview passed down in the indigenous language” (Preuss 1926: 11). Preuss also criticized the attitude of merely wanting to be the first to explore a region. What was needed, he said, were thorough studies, not supposedly new discoveries. And he not only demanded this of others; he put it into practice himself.

The list of examples could easily be multiplied. In fact, since the beginning of its professional institutionalization, and thus long before World War I, ethnology in Germany had been anything but an armchair science. Instead, it was often characterized by researchers who aimed – mostly, but not exclusively – at establishing a longer-term social relationship as the basis for describing indigenous living conditions, recording words, stories and myths, and collecting and documenting material culture.

It is important to consider that the scholars discussed in my chapter already practised ethnology as a profession. On the one hand, they conducted ethnographic field studies in Latin America; most of them even carried out multiple research trips. On the other hand, these scholars were employed in one of the newly founded ethnological museums in

Germany that emerged at the end of the nineteenth and the beginning of the twentieth century. In addition, all of the individuals mentioned gained their *Habilitation* – their *venia legendi* or teaching credentials – with an ethnological treatise at a university. In this way, they contributed both to the development of research methods in the field and to the institutionalization of ethnology as an academic discipline in Germany.

In writing this, I do not mean to say that Malinowski did not make an important contribution to the summarizing, further developing and propagating of this research method. And I would also like to point out that the scholars whose experiences and texts I describe in my chapter of the edited volume did not invent fieldwork either. They contributed to a much larger, international development. That is exactly why it is questionable to consider a single person as the founder or inventor of a methodology that has been developed and refined in various places by many people over several decades.

From the complex field of analysing the debates on methods of a former generation, I want to pick out here just one further point. What has been said “On Ethnographic Authority” does also apply to the authority of criticism. I will only hint at this here by adding three words to a passage from a famous essay: “The development of [the critique of] ethnographic science cannot ultimately be understood in isolation from more general political-epistemological debates about writing and the representation of otherness” (Clifford 1983:120).

In numerous critiques we often find a mixture of methodological, political and moral arguments that are not always clearly named. Therefore, “modern” analyses also run the risk of becoming blurred with conceptions that are uncritically assumed to be timeless and universal. Studying the history of science, only rarely do authors make their own perspectives explicit. A fine example was Mark Münzel, who in his study of the ethnologist and Africa traveller Leo Frobenius (1873–1938) self-critically stated how the respective time – and the respective state of the scientific discussion related to it – coloured his view:

I have been reading him [Frobenius] for several decades now. In the 1950s, he helped me out of the humanistic Eurocentrism that only knew culture around the Mediterranean. In the 1960s, he turned out to be a colonialist. In the 1970s, he became a museum ethnologist and media-savvy stager of prejudice criticism, and in the 1980s, a postmodern carnivalist. Basically, I did with his texts what he did with the cultures of Africa: read them out in the spirit of my time. (Münzel 1993: 23)

In contrast to Münzel’s self-critical reflection, many critics of past generations fail to disclose their own premises.

This consideration can be connected with a point of perspective. In anthropology, it is well known that an important aspect in the analysis of foreign social systems is the elaboration of the emic perspective; or, as Malinowski put it, “to grasp the native’s point of view” (Malinowski 1922: 25). This is also necessary when considering the past. We make great efforts to question and overcome a “Eurocentric” or “ethnocentric” perspective. In the case of dealing with the past, it is equally important to avoid a “chronocentric” interpretation (Streck

1997: 42, 51), that is, an assessment of the past based exclusively on the standards of the present. The norms of our “own” society of more than a hundred years ago are also comparatively foreign to us. Economic structures and legal systems have changed in many ways, as have gender relations or scientific theories. Someone who grew up in the German Empire was not only socialized differently than someone who grew up in central Brazil at that time but also than someone growing up in Germany at the beginning of the twenty-first century. In order to understand and judge the behaviour of the respective situation, we have to understand the respective backgrounds and contexts – which of course can vary greatly again between social groups living in the same region or at the same time. “The past”, so the oft-quoted first sentence from G. P. Hartley’s novel *The Go-Between* (1953), is also “a foreign country”. One did “things differently there” – because one was a representative of a different culture and set of values.

I am not opposing the need for criticism at this point. Behaviours cannot be excused only because they were common at a particular time. My point is merely to clarify more clearly the premises on which criticism is based. What I argue for is a more anthropological approach to history (cf. Hallowell 1965).

Looking back at the ethnographers analysed in my chapter, there is much to criticize. Noticeable, for example, is their adherence to evolutionist thought patterns, even if the encounters in the field led to a relativization of these convictions. The same is true of the fatalism with which the destruction of indigenous cultures was perceived. It was described and lamented, but considered to be inevitable.

However, in order to understand complex settings, we should not reduce our analyses to those aspects that increase our own authority as critics. In doing so we run the risk of constructing “savages” again, albeit this time in our own past. We also have to ask ourselves if these forefathers and foremothers were progressive compared to the views of their contemporaries. Which views did they construct or confirm? And which views did they contradict or help to overcome?

Already at the end of the nineteenth century, for example, Karl von den Steinen wrote in the preface to one of his books “that there is no such thing as the ‘savage,’ because everywhere a social order is present as well as a linguistic and cultural bedrock that is already furnished with the most essential elements of higher development”. And he added: “Savagery, as it really is, still rests deep in our minds and hearts [...]; the culture of the savages is much higher on average, ours is much lower than commonly thought” (Steinen 1897: vii-viii.). Likewise, Koch-Grünberg increasingly put the then common expression “savages” in quotation marks, and regularly remarked on social behaviour encountered among the indigenous people he visited in South America: “tout comme chez nous” – just like at home.

As stated, in every book we will find passages that can be criticized. And that is exactly what we have to do then. But we should also ask which contemporary beliefs certain authors have questioned in order to begin to change them. In the section “Reducing the Distance” of my

chapter in the book mentioned, I try to show how the investigated scholars critically revised common beliefs of their time themselves and thus contributed to an improved perspective on other societies – although we also find convictions in their books that we reject today. However, some statements about earlier generations of our discipline seem to virtually congeal into new topoi, thus leading once again to the reproduction of homogenizing ideas about social groups presented in an ethnographic present. They were developed on the basis of specific examples, but are now repeated unquestioningly as if they were correct representations of entire generations. What definitely cannot be found in the described generation of researchers in Amazonia is, for example, the idea of indigenous peoples without culture and history. Here, too, the critical analysis of individual cases is important in order to avoid generalizations and stereotypes.

Dealing with the past is important to understand the genesis of the present. In the first section of this short essay, I emphasized how little the development of the complex methods of fieldwork can be reduced to individuals. In the second section, I outlined two aspects as prerequisites for a deeper understanding of the situations analysed: the necessity of the elaboration of an emic perspective, also when looking at the past, as well as the disclosure and reflection of our own moral and political criteria that are often implicitly set as valid. In my mind, the consideration of these aspects is linked to qualitative epistemological progress. It does not make criticism any less necessary, but it should challenge us to combine criticism with self-reflection – or even self-criticism. Whereby it may be necessary to point out that being self-critical means something else than simply moving the activity of “Othering” into one’s own society.

8. Canons in Anthropology? Querying the Malinowskian Revolution, by Andrew LYONS (Wilfrid Laurier University, Waterloo)

That this landmark volume *Ethnographers Before Malinowski* (Rosa and Vermeulen 2022) appears in the centennial year of Malinowski’s *Argonauts of the Western Pacific* is appropriate. According to Ian Jarvie (1964), the publication of this monograph marked the “Revolution in Anthropology”, the writing of ethnography’s charter myth. It is now fifty-five years since the publication of Malinowski’s diary (1967) informed us that he did not always do the things he said anthropologists should do, and that both he and the Trobrianders were much less isolated than he pretended. It is fifty years since Dell Hymes’s *Reinventing Anthropology* subjected American anthropology to radical political critique in the light of the Vietnam War, and forty-nine years since a parallel critique, Talal Asad’s *Anthropology and the Colonial Encounter* first appeared in Britain. Later critiques not only raised issues of colonialism and post-colonialism but also reflected on manifestations of power, textual form and the legitimacy of anthropology as a discipline. *Writing Culture* (1986) was but one landmark of the “post-modern critique”. There were other perturbations too from other theoretical and moral standpoints. In the United States, controversies erupted in 1983 over Derek Freeman’s attack on Margaret Mead and, nearly two decades later, over the fieldwork and ethnography

of Napoleon Chagnon. Ethnographic texts have been understood as forms of rhetoric, not transparent lenses through which to observe unadorned facts.

During the *longue durée* of the critique of anthropology there have been other significant developments. Not only does anthropology now study globalization, but it has also itself globalized. When I studied anthropology at Oxford in the 1960s, we read very little American ethnography. When I started my first teaching job in the United States in 1970, my colleagues and their students read very little British ethnography. British students like me learned precious little about culture and personality or neo-evolutionism. We did read Mauss, Durkheim, Van Gennep, Dumont and Lévi-Strauss, all of whom were little read in many American universities. Few of us read any German anthropology.

The authors in this volume teach or have taught in Portugal, Germany, France, Poland, Britain, South Africa, New Zealand, the USA and Canada. Rosa's and Vermeulen's vision for this volume focused on exclusion and inclusion in the ethnographic canon. Each writer was asked to select one author and one ethnographic monograph that should have been deemed a strong candidate for inclusion in the canon but has apparently been absent from it.

As I prepared my discussion of this volume it occurred to me that the *acknowledged* existence of an ethnographic canon was coincident with the beginning of the critique of anthropology. A related point is that books dealing with the history of anthropology over fifty years ago were in the main part histories of *theory*. A little was said about armchair versus fieldwork anthropology, about participant observers, surveys and salvage anthropology, but the writing of ethnography itself was rarely problematized. We did not talk about ethnography as a genre. This does *not* mean that a canon did not exist – it was not a central focus.

We are informed that, if indeed there was a revolution in anthropology, its supporters followed the precepts outlined in the early chapters of *Argonauts*. Participant observation was essential. Anthropologists must live in one village or camp with their subjects for a period of several months at a time, not on the veranda of a colonial HQ for an occasional afternoon. Contact with their own society must be minimal. Full, linguistic competence in the subjects' language must be attained. When field notes are transformed into a scientific article or a monograph, the language must be detached. The ethnographer should outline the critical norms of the society studied; all the customs and behaviours that supposedly express or reflect those norms (the *imponderabilia* of daily life); texts of myths, folklore, spells and prayers should be recorded. Built into all these requirements was the expectation that the anthropologist would be properly trained, a professional in a newly established discipline. In Malinowski's view the anthropologist must ensure a proper, theoretical focus for both the research project and the resultant ethnography. For him this meant functionalism, implying that observed customs had practical utility and avoiding assertions about social evolution or primitive irrationality. There was an assumption too that the ethnographic monograph would focus on a single theme, e.g. ceremonial exchange, social control, marriage and sexuality, religion and magic.

Perhaps it is important to say who could not write ethnographies and what should not be included in that genre. The reports of missionaries and administrators with no anthropological training, travelogues, autobiographies of both men and women in contact zones and journalism, might be food for the anthropologist, but they were not ethnography and often reflected ethnocentric perspectives. Informal, personal accounts of fieldwork were not deemed to be ethnography, nor were the writings of amateurs. Much of this template for ethnography was accepted by Radcliffe-Brown's students, and a similar model was also adopted in the USA, albeit with a less strict insistence on synchrony. In France, under Griaule and others, expeditionary anthropology flourished through the 1930s. Nationality, language and location had a lot to do with the readership or influence of an ethnographer. If one had the wrong contacts, the wrong networks or worked in the wrong country, one's work would be less noted. Personality and serendipity all played their roles.

The critique of ethnography focussed first on the political circumstances of ethnographic production, namely that anthropology was a tool of colonialism, involved in the pacification process and subsequently in British strategies of indirect rule and parallel processes elsewhere, and that it seldom involved the proper consent of subjects. Secondly, in *Writing Culture* (1986), James Clifford, Vincent Crapanzano, Michael Fischer and Renato Rosaldo also focussed on issues of rhetoric and language, including the invocation of such creations as the "ethnographic pastoral" that denied realities of dominance, conflict and change. Correction of these faults would require strategies of multivocality and proper collaboration, including co-authorship, with subjects. After Clifford Geertz had asserted that blurred genres were everywhere (Geertz 1980), his epigoni in the *Writing Culture* school rejected the former ideals of scientific or scientific purity and insisted that popular ethnographies, writings of skilled amateurs, some pre-functionalist writings, even fictional or autobiographical accounts by indigenous writers, could be considered for admission to the canon. Biases of gender and race had to be corrected. For example, Zora Neale Hurston, once disdained, has long since been admitted to the anthropological canon.

Ethnographers Before Malinowski presents us with a reaction to the critique of anthropology. Nobody is defending the abuses of colonialism, but three contributors, Frederico Delgado Rosa, Montgomery McFate and Jeffrey Paparoa Holman argue that good anthropology could be produced by servants of colonial governments and that some of it predated Malinowski. In one important way, the critique is endorsed in this volume – most essays concern candidates for inclusion in a less rigid canon.

Frederico Delgado Rosa's chapter is the story of a soldier and explorer: Henrique de Carvalho, a skilled amateur ethnographer who even used the word "etnografia" in the title of the main work under discussion, *Etnografia e historic tradicional de povos da Lunda* (1890). He empathized strongly with the Lunda people whom he credited with saving his life, and was in many ways a relativist, ahead of his time. His expedition into and through the Lunda empire of Angola and the Congo took place in the mid-1880s, a time of political decline and ethnic conflict, when the kingship itself was under dispute. The work contains an account of

Carvalho's expedition to the capital and his stay there. Carvalho describes the royal complex, explains the workings of the institution of divine kingship, and narrates its charter myth. Victor Turner (1955) translated that myth, and the "Lunda love story" was one of the set of myths discussed by Luc De Heusch in his book *The Drunken King* (1982). Unfortunately, because of the language barrier, Carvalho has been ignored. Rosa tells us that the explorer/ethnographer never doubted the rectitude of the Portuguese colonial mission. His trip was motivated by both intellectual curiosity and a vision of restored colonial glory. We might well disapprove of some of his motives today, but his very fine work was stimulated by them.

Montgomery McFate's chapter on Rattray concerns a scholar who was, strictly speaking, Malinowski's contemporary rather than his predecessor. Rattray was one of those members of the colonial civil service who received training in anthropology at Oxford in the days of Robert Marett. McFate deals with one of his three main ethnographies, *The Ashanti* (1923), in accordance with the editors' prescriptions, although I think that she might have improved a strong chapter by dealing with all of them. *The Ashanti* does not have a single focus, but its discussions of matriliney, the rôle of the Queen Mother and the significance of the Golden Stool are indeed memorable. McFate demonstrates that Rattray was a good anthropologist who respected Ashanti institutions. She claims that his advice to the British authorities in the Gold Coast to stay clear of the Golden Stool prevented the recurrence of hostilities. The message given in this chapter is that such advice from trained anthropologists may have prevented the misrule that can be engendered by ethnocentric misunderstandings and thereby saved lives, though some anthropologists will object to all forms of participation in colonial rule, particularly the giving of advice relevant to the so-called pacification process. Northcote Thomas, whom McFate mentions, wrote a brilliant book refuting theories of primitive promiscuity and group marriage in Australia before accepting positions as a government anthropologist in Nigeria and Sierra Leone. In other words he was an anthropologist *before* he took a job in the colonial service on a series of temporary contracts. He was a solid fieldworker, as his reports on the Edo make clear, but in his case his sympathy and understanding of African societies proved a handicap with his employers, because some of them could not understand *his* culture.

In another stimulating chapter, Ronald Grimes compares and contrasts the work of three nineteenth-century writers who visited the Hopi in Arizona: John Gregory Bourke, Walter Fewkes and Frank Hamilton Cushing. Fewkes was a thorough, patient worker. Cushing was a pioneer of participant observation, an adopted member and advocate of the Hopi nation. Bourke, the main subject of the chapter and best known for his cross-cultural work on scatology, was a tourist with an excellent eye and no knowledge of the language, who disregarded all boundaries, intruded on Hopi territory and ritual places, and produced a sensationalist fiction of snake worship: *The Snake-Dance of the Moquis of Arizona* (1884). Grimes is hardly suggesting the inclusion of Bourke in any canon – but many of us might include one of Cushing's books. However, Grimes argues that in his own, much more subtle way, Cushing intruded more effectively than Bourke on Hopi culture. By way of contrast, Grimes

draws attention to the more collaborative research of the contemporary anthropologist, Peter Whiteley.

Collaborative research is not a creation of post-colonial ethnography, nor merely a response to the so-called crisis of representation. Three chapters deal with collaboration in pre-Malinowskian writings. Elsdon Best is known to most anthropology students as the author from New Zealand whom Mauss quoted in his discussion of the *hau* of the gift (Mauss 1952: 9). Best had an interesting and varied life. He was an autodidact who ended up employed as an ethnologist on the staff of a museum, but before that he was a farmer, a worker in a sawmill, briefly an emigrant to the USA, and a quartermaster on a roadbuilding project in North Island that crossed Maori Tuhoe territory. Some Maori writers today have described Best in unflattering terms as a colonialist, even a racist. Jeffrey Paparoa Holman demonstrates that Best's ethnography relied on the willing collaboration of indigenous informants. His ethnography, *Waikare-moana* (1897), was published as a travelogue, containing vivid descriptions of beautiful countryside, but it also encapsulates a wealth of anthropological detail, supplied by Best's aged informant, Tutakangahau, concerning Tuhoe cosmology, myths, legends, genealogies and history. Both parties to these dialogues achieved their purpose. Best made a contribution to anthropology, and Tutakangahau, a literate veteran of the Te Kooti rebellion who had of necessity made some compromises with colonial power and socio-economic change, was able to ensure that there was a written record of Tuhoe traditions that might otherwise vanish.

Joanna Cohan Scherer offers us an interesting account of the long-term collaboration of Alice Fletcher and an indigenous Omaha, Francis La Flesche, which eventually resulted in the publication of *The Omaha Tribe* (1911). The ethnography is particularly remarkable for its description of the traditions relating to the Sacred Pole. Fletcher and La Flesche were close companions, enjoying what Scherer describes as a "mother-son relationship". La Flesche, who was a law graduate, became the first Native American to secure an appointment at the Bureau of Ethnology. Robert Lowie, notes Scherer, gave a somewhat hostile review to the book because it gave inadequate credit to the deceased James Owen Dorsey, with whom La Flesche had also worked. The reason for this omission was that La Flesche knew that Dorsey had made some errors. Was there here a whiff of what we would now call "white male privilege"?

The editors do not include Barbara Dawson's excellent chapter on Katie Langloh Parker in their section on indigenous collaboration, but in her book, *The Euahlayi Tribe* (1905), Parker makes her debt to indigenous women like the elderly Bootha patently clear. On an isolated farm in New South Wales near the Queensland border, Parker depended on the indigenous population for general assistance, companionship and information. She came from a very literate family and had received some schooling, but she had no formal training in anthropology or folklore. She produced popular books on aboriginal legends as well as academic articles on religion and folklore. She was not academically isolated; she corresponded with Andrew Lang, who encouraged her work. *The Euahlayi Tribe* is written in

simple, clear English, and demonstrates an extensive knowledge of all recent anthropological writings on indigenous social organization, marriage, initiation rites, foraging and folklore. Parker explains the moiety system, the four section system (similar to the neighbouring Kamilaroi), the betrothal of young girls, female puberty practices, and the male initiation rites, as well as the belief in the high god, or Baiame (whose worship contradicted the theories of Spencer and Tylor respecting animism). Dawson tells a remarkable story. The *Euahlayi Tribe* can be downloaded on Kindle.

It is interesting to contrast what Parker tells us about indigenous beliefs in a high god in Australia with what Callaway, the subject of David Chidester's chapter, and Father Trilles, the subject of André Mary's chapter, have to say about high gods in Africa. Katie Parker tells us that there simply is no evidence of any correlation between belief in a high god and perceived cultural complexity or the lack of it in aboriginal societies. Both missionaries in Africa, however, thought that a pagan belief in a high god was better than pagan animism. Callaway objected to Bishop Colenso's use of the Bantu word, *unkulunkulu*, for high god or God in translated Christian texts. Among the Zulu the word appeared to mean "first ancestor". Another word was used for "Creator God", but Chidester's Zulu had primarily been animists. Callaway, and his aide, Mpengula Mbande, who was responsible for many key texts in *The Religious System of the Amazulu* (1868–70), were as missionaries appropriately obsessed with conversion, and their attempts to comprehend Zulu religion were focussed in that direction. In both their writing and that of Trilles, animistic systems such as those of the Zulu and the Fang are seen as degenerate religious forms (monotheism has degenerated into animism). Chidester thinks Callaway was correct in viewing himself as an anthropologist as well as a missionary, noting Tylor's praise of his writing. However, I think that his work is best used by anthropologists as a rich secondary source to be explored with due caution. Trilles referred to Fang fetishism as "a caricature of a religion". Mary notes his neglect by later writers. Perhaps they were wise. To include Trilles's *Chez les Fang* (1912) in a revised canon would involve the total erasure of Malinowski's criteria for good fieldwork and ethnography. The editors do not suggest this, quite the reverse.

Two papers, besides Rosa's, deal with expeditionary anthropology. The Polish ethnographer, Maria Czaplicka has recently received overdue attention in two major publications, a biography by the author of our chapter (Kubica 2020) and a chapter in Frances Larson's *Undreamed Shores* (2020). Czaplicka was the author of a work on *Aboriginal Siberia*, which was based on library research, as well as *My Siberian Year* (1916) which is discussed in Kubica's remarkable chapter in this volume. After two members of the expedition returned home at the outbreak of the Great War, Czaplicka and an American colleague, Henry Hall were left to complete the bulk of the work, inland on the Yenisei in the winter, and upriver in the summer. They encountered the Tungus, the Samoyed and the Sibirians (the European settlers). Her intrepid fieldwork in harsh conditions was described in *My Siberian Year*, an informal, popular narrative which we may now happily include in the genre of ethnography. In her chapter for this volume, Kubica focusses on Czaplicka's vivid account of the *munyak*, or Tungus native court. Regrettably, Czaplicka did not survive long enough to complete an

ethnography similar to her friend's book, *Argonauts*. She committed suicide in 1921 at the age of 37.

Michael Kraus's chapter differs in many ways from others in the collection. He focusses on the German tradition in anthropology with which many of us are unfamiliar. Secondly, he discusses five anthropologists rather than a single one. All of them led expeditions to collect artefacts for museums as well as information about Amazonian (or in one case, Preuss, Mexican) cultures. All were influenced by Adolf Bastian and possessed some interest in the study of thought patterns in other cultures. They also held positions in museums. Personally I had come across references to Karl von den Steinen and Theodor Koch-Grünberg in various sources in years of reading both for classes I taught and for pleasure. I do not recall reading anything before about Konrad Theodor Preuss, Max Schmidt or Fritz Krause. In his analysis of the work of this generation of ethnographers, Kraus makes it clear that they represent a transitional point in the development of fieldwork before Malinowski, between travelogues and scientific monographs, between quick surveys and participant observation over longer periods, between thin and thick description. Koch-Grünberg often stayed in one place for weeks at a time; Preuss stayed at some sites in Mexico for a few months. These scholars were not ignored. For example, in *The Cubeo*, Irving Goldman (1963: 12) tells his readers that he was unaware of Koch-Grünberg when he went to the field. When he did discover Koch-Grünberg's work, he found it invaluable, but it would have been even richer if the author had stayed longer in each place. In *Amazonian Cosmos*, Gerardo Reichel-Dolmatoff (1974:7) made a similar remark, noting that Koch-Grünberg had much to say about the material culture of Tucanoan peoples but much less on social organization and myth. It is a pity that the scope of Kraus's article, and doubtless limitations of space, did not allow him to discuss any work by these five authors in detail, but he has done many of us a great service by bringing their work to our attention. After 100 years or more it is never too late.

Last but not least, two of the twelve chapters deal with one extremely important figure and another major figure in the history of anthropology, Franz Boas and Edward Westermarck respectively, neither of whom is commonly regarded as an exemplar of the ethnographic art. Despite his primacy in American anthropology, Franz Boas is best known as an anthropologist who campaigned against racism, a consummate linguist, a collector of valuable texts, an individual who exhaustively collected valuable data on Northwest Coast cultures, and a teacher of many good fieldworkers and ethnographers. However, critics have noted his over-reliance on an intermediary, George Hunt, and have stated that he did not ever immerse himself in any one Northwest Coast culture for any period of time. Furthermore, they say, there is no connected monograph by him exploring a particular theme in Kwakwaka society and culture. All very well, but look at what there is to read, enough to keep Regna Darnell and her remarkable team of co-researchers busy for years. Boas's oeuvre is immense, not to mention a plethora of excellent books, collected papers and articles by and about him, including Rosemary Lévy Zumwalt's biography in two volumes (2019, 2022).

Before Boas ventured to British Columbia, he spent a year on Baffin Island. Herbert Lewis reminds us of the importance of his monograph, *The Central Eskimo* (1888). For large parts of his stay, Boas was isolated in ways Malinowski never was and may have undergone privations that Malinowski never suffered. There was no alternative to learning the language. The resulting monograph reflects the transition in Boas's career, because, when he wrote it, he was betwixt geography and anthropology. It is imbued with a nascent spirit of relativism and admiration for the endurance and decency of his hosts. Lewis says little about large portions of the book that might interest geographers more, but rather discusses the small but significant, extremely "meaty" part of it that discusses marriage, homicides, the egalitarian band structure, and, in particular, religion, shamanism, arts, and warfare. Together, these short, pithy chapters constitute a mini-ethnography.

A couple of themes emerge clearly: first, that Boas is at pains to stress that, in the harsh Arctic environment, humans are not passive, that they usually cope with adversity, and that the environment does not totally determine social organization. He noted the alternation between dispersal in the caribou hunting season and aggregation in the autumn and winter sealing season, which was the preferred time for ceremonials. Taboos reinforced observations on hunting rules, e.g., no caribou flesh in the sealing season. It was these features in the accounts of Boas and others that drew the attention of Marcel Mauss and Henri Beuchat, resulting in an essay, published in 1906, that has had some influence in France, Canada and Britain, and, curiously, less influence in the USA. Significantly, their review of Boas influenced Evans-Pritchard in his chapters on transhumance, aggregation and dispersal among the Nuer. I would argue that to some at least *The Central Eskimo* already qualified as a canonical work *avant la lettre*.

Most anthropology students may know of Edward Westermarck only with respect to his views about the incest taboo. In a book we wrote a couple of decades ago, Harriet Lyons and I noted Westermarck's significant role in demolishing assertions that primitive promiscuity was a universal stage in social evolution (Lyons and Lyons 2004). We also noted his application of a form of relativism to the study of same-sex sexualities. Later these ideas were developed into a full-blown ethical relativism. At the time, very few people were looking at Westermarck, who taught Malinowski as well as being his colleague in the years before his retirement. Over the last decade David Shankland has done more than anyone else in the English-speaking world to restore Westermarck's reputation not just as a theorist but as an ethnographer. The reader of Westermarck's ethnographies can easily get impatient with their old-fashioned presentation that still recalls the more irritating features of evolutionists like Frazer, what somebody once called the "Amongtha" school. What Shankland does in his elegant chapter is to demonstrate just how good an ethnography *Ritual and Belief in Morocco* (1926) is. The comparative method Westermarck deployed served a purpose, because he travelled throughout the country and knew about nearly every cultural variation in it and elsewhere in the region in key rituals and beliefs. His linguistic command was excellent, the result of years of fieldwork. He gave explanations of concepts and supposed entities like *baraka*, *jinns*, the evil eye and 'ar which demonstrated the ways in which Moroccans spun and

were entangled in webs of significance. The Geertzian reference is significant, because, whereas the continuity between Westermarck and Gellner may not be evident, Westermarck's Morocco is recalled in the worlds described decades later by Geertz and Crapanzano. Shankland also reminds us that Westermarck was a gentleman scholar who honoured his key informants. I might add that Westermarck was a determined Darwinian atheist, yet he appears to demonstrate an extraordinary grasp of ritual thought and action.

Just one day after our round table, I downloaded a new popular history of anthropology by a British author, Lucy Moore (2022). *In Search of Us* focusses on fieldwork and ethnography before 1940. The authors discussed are Boas, Haddon, Rivers, Westermarck, Malinowski, Daisy Bates along with Radcliffe-Brown, Margaret Mead, Ruth Benedict, Zora Neale Hurston, Audrey Richards and Claude Lévi-Strauss. In each case a field site is detailed, and in the case of Boas the focus is on the Inuit. In the case of Westermarck, now receiving deserved credit eighty-three years after his death, it is all of Morocco. The fieldwork of Daisy Bates in Australia is valorized at the expense of that of Radcliffe-Brown. In other words, this popular book reflects changes in the canon as well as its globalization in the last fifty years, all those changes which made our RAI global round table possible along with the new volume that was the reason for holding it.

ROUND TABLE PART II

9. Bringing Theory into The Field: Malinowski's Trobriand Ethnography, by Adam KUPER (London School of Economics)

Modern anthropology emerged in the 1840s. The first learned societies were formed in Paris, London and New York. Travellers, administrators and missionaries began to submit ethnographic reports to the new journals. The most distinguished contributions were often by missionaries. They had to learn local languages and find synonyms for "god", "spirit", "sin" and "hell" when they translated the Bible, so they did research on beliefs and customs, and collected texts of myths. Some of the nineteenth-century theorists did (limited) field research on their own account. William Robertson Smith, who was an accomplished Arabist, visited Bedouin camps; Lewis Henry Morgan collected materials in Iroquois reservations. At the same time, the first dedicated museums of "ethnography" were established. They promoted "ethnographic" collections, and for the next century most ethnographic expeditions were financed by museums.

Some of the museum ethnographers undertook long-term field studies. In 1879 the Smithsonian Institution in Washington D.C. recruited Frank Hamilton Cushing to take part in a collecting expedition to Hopi and Zuni pueblos in the Southwest. This was an inspired appointment. The frail, artistic, original, largely self-educated young man had been fascinated by Indian artefacts since childhood. Now Cushing was captivated by his first exposure to an Indian way of life. He spent two and a half years in a pueblo, learned the Zuni language, adopted traditional Zuni dress, and collected information on myth, religion and

custom. “Cushing can think as myth-makers think”, said John Wesley Powell, the director of the Bureau of Ethnology: “he can expound as priests expound, and his tales have the verisimilitude of ancient lore” (Darnell 1998: 76). When Cushing required a scalp to take part in a Zuni initiation ceremony, the National Museum of Natural History sent him one from its holdings (Darnell 1998: 75).

Franz Boas described Cushing as “a very able man” but, he added, “I’m afraid his work will have to be done all over again” (Lowie 1956). Yet if his ethnographic reports were unsystematic, his collections of artefacts were remarkable. “Within a 25-year period the Smithsonian Institution alone collected at least 41,000 objects from the Southwest, of which more than 34,000 were accessioned into the permanent collections”, Nancy Parezo writes. “Two-thirds of this extraordinary exercise ... was completed in less than 6 years, primarily when Cushing was at Zuni” (Parezo 1985: 769). And for the first time, ethnographic finds were carefully documented. Wherever possible, “knowledgeable Zuni informants were asked to provide the native term for the object, describe its use and function, record its previous history, identify the design and its meaning, tell how it was made, discuss the history of the craft, state whether it was rare or common, and comment on the quality of the workmanship. Construction techniques were recorded in minute detail and raw materials identified” (Parezo 1985: 765).

Adolf Bastian was the best-known museum ethnographer in the second half of the nineteenth century. He described Alexander von Humboldt as the “hero of our age”, who “provided the platform on which to erect the temple of the harmonious cosmos by inductive research” (Bunzl 1996: 50). On graduation, Bastian set off to become a world traveller in the mould of Humboldt, sailing around the world for eight years as a ship’s doctor. On his return he published a ponderous, abstract three-volume treatise on humanity, which he dedicated to Humboldt. Then he travelled for five years in east Asia, where, among other adventures, he passed several months in the compound of the Mandalay Palace as a reluctant guest of the King of Burma, obliged to spend several hours every day in the study of Buddhism. This Asian journey resulted in a six-volume compendium, *Die Völker des östlichen Asien*. Bastian eventually published thirty volumes on his travels. The books sold rather well, but were perhaps not widely read. A later generation found them impenetrable. “Who now reads Bastian?”, asked Robert Lowie. “Two factors invested Bastian with a comic halo in the judgment of irreverent posterity – his determined opposition to Darwinism and his style”. Darwin’s German disciple, Ernst Haeckel, described Bastian as “*Geheimer Oberkonfusionsrat*” – secret upper confusion counsellor.

Unlike Bastian, most of the early ethnographers did not aim to be theorists. There was a recognized division of labour. As R. R. Marett, the doyen of Oxford University’s anthropology programme, explained the system in 1927: “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” (Marett 1927; see also Kuper 1980). The ethnographers generally felt that their role was to send ethnographic reports back

to the synthesizers in the metropolis. Four editions of *Notes and Queries on Anthropology* were published between 1870 and 1920. These laid down field methods for “men in the tropics” and provided a checklist of questions (Urry 1973). The N&Q tradition was still largely about collecting material culture but it added a range of questions, for purposes of cross-cultural comparison. Soon James George Frazer, Morgan and others began to send out their own questionnaires, adapted to their particular theoretical concerns. Henri Junod’s magnificent *Life of a South African Tribe* (1912) was largely structured as a response to Frazer’s questionnaire. More often the framework was provided by *Notes and Queries*. This was the case for Malinowski’s first field studies, reported in *The Natives of Mailu* (1915).

From the 1890s, professional anthropologists began to do fieldwork on their own account, but they still respected the basic division of labour. The fieldworker produced facts. The theorist inserted them in a comparative framework and produced explanations.

The university-trained fieldworkers typically worked in teams, making surveys of large culture areas – most notably, the Jesup North Pacific Expedition (1897–1902) and the Torres Straits Expedition (1898–9). There followed individual field studies, notably by Franz Boas in British Columbia and Richard Thurnwald in New Guinea. Veterans of the Torres Straits undertook solo research work, though for short periods. Charles Seligman, for instance, did studies in Melanesia, in Sri Lanka, and later in Sudan; William Halse Rivers went on to work among the Todas in India and then in Melanesia. Rivers’ student Alfred Radcliffe-Brown spent several months in the Andaman Islands in 1906 but in his monograph he drew (as he said) very heavily on ethnographic observations of E. H. Man, who had been brought up in the Andamans. Edward Westermarck’s fieldwork in Morocco was one of the most thorough in this period, but it seems to have surprisingly little impact at the time, as David Shankland remarks in his contribution to this volume.

Soon methodological critiques were formulated. R. R. Marett in 1912 and W. H. Rivers in 1913 insisted that it was time to move on from the sort of survey work that characterized much of the research in the generation before World War One. Marett argued that intensive immersion in the life of a community would make the ethnographer conscious of individual variation and changing values and practices. Rivers criticized the conventional research of the day, which he termed “survey work”. He advocated “intensive work”:

A typical piece of intensive work is one in which the worker lives for a year or more among a community of perhaps four or five hundred people and studies every detail of their life and culture; in which he comes to know every member of the community personally; in which he is not content with generalized information, but studies every feature of life and custom in concrete details and by means of the vernacular language... It is only by such work that it is possible to discover the incomplete and even misleading character of much of the vast mass survey work which forms the existing material of anthropology. (Rivers 1913: 7)

The first person to come close to following the programme was Bronislaw Malinowski. This was in part by chance. Malinowski had to spend the First World War years in the Australian

area, and he had permission and funds for fieldwork. The first chapter of *Argonauts of the Western Pacific* laid out a new methodology for immersive long-term ethnographic research, and set out a new theoretical goal. *This was not historical reconstruction*. In the final pages of the monograph Malinowski announced, “it seems to me that there is room for a new type of theory... The influence on one another of the various aspects of an institution, the study of the social and psychological mechanism on which the institution is based, are a type of theoretical studies which has been practised up till now in a tentative way only, but I venture to foretell will come into their own soon or later. This kind of research will pave the way and provide the material for the others” (Malinowski 1922: 533).

Malinowski rejected the division of labour between metropolitan theorists and fieldworkers. He put theory into the field. So far as the functionalists were concerned, only a properly equipped fieldworker could understand how a particular society operated. It followed that the trained ethnographer was a privileged theorist. This went against the positivist doctrine of the day. Boas once told a student, “there are two kinds of people: those who have to have general conceptions into which to fit the facts; those who find the facts sufficient. I belong to the latter category” (Kluckhohn & Prufer 1959).

Overstating the case, but making an important point, Ernest Gellner remarked that the Malinowskians “insist that anthropology differs from ‘mere’ ethnography by also having theory; on investigation of this theory, called Functionalism, it turns out to be in large part the doctrine that anthropology should be nothing but ethnography. Or rather—good ethnography” (Gellner 1973: 98).

But there was more to it than that. For the functionalist, what mattered was the way in which a society worked in the here and now. The institutions of faraway peoples should not be explained as hangovers from olden times or as mindless obstacles to progress. Customs and beliefs served necessary purposes. They were apps for life. Even apparently irrational rites make sense when their *use* is appreciated: “in his relation to nature and destiny”, Malinowski wrote, “whether he tries to exploit the first or to dodge the second, primitive man recognises both the natural and the supernatural forces and agencies, and he tries to use them both for his benefit” (Malinowski 1948: 32).

That implied another functionalist principle: people, everywhere, are pragmatists. They look after their own interests. “Whenever the native can evade his obligations without the loss of prestige, or without the prospective loss of gain, he does so, exactly as a civilized business man would do”, Malinowski wrote (Malinowski 1926: 30). He was the first anthropologist to treat the “savage” as a rational actor.

The chapters of the *Argonauts* were presented in draft form in the LSE seminars that Malinowski was beginning to run from 1920. These seminars formed the first generation of “functionalist” anthropologists. So Malinowski’s new paradigm – immersive field work over an extended period to find out how a society worked, and how it was understood by the actors – became the model for a school of ethnographic research. Boas was doing the same

work at Columbia University, but his paradigm of ethnographic research was very different. The major element was the production of texts, and the theoretical framework was culture history. As Robert Lowie remarked, Boas was, nevertheless, “especially appreciative of men who had achieved what he never attempted – an intimate, yet authentic, picture of aboriginal life. I have hardly ever heard him speak with such veritable enthusiasm as when lauding Bogoras’s account of the Chukchi, Rasmussen’s of the Eskimo, Turi’s of the Lapps” (Lowie 1947).

There were many other capable ethnographers at work before Malinowski: the volume under discussion (Rosa & Vermeulen 2022) documents their contributions. However, Malinowski introduced a challenging, original paradigm for ethnographic research and he created a distinctive school whose students were taught to synthesize a new method and a new theoretical perspective.

10. Anthropology After the *Argonauts*: Malinowski in France, by Sophie CHEVALIER (Université de Picardie Jules Verne, Amiens)

Ethnography in France developed not only through exotic anthropology abroad, but also through ethnology at home. Bronisław Malinowski made an impact on the latter field through participating in a long-term collective project, but Malinowski’s work was introduced to French audiences earlier through Marcel Mauss. Connected to these developments was another important figure in the history of French anthropology, Arnold Van Gennep.

All three founders of social anthropology promoted the ethnographic method, but only two of them employed it; and they all explored relations between their nascent ‘science’ and literature. Malinowski first entered French anthropology through Marcel Mauss, especially in lectures to students at Paris’ Institut d’Ethnologie. Malinowski also participated in the ethnology of France at home, where Arnold Van Gennep was instrumental in transforming folklore studies into regional anthropology. I conclude by asking how Malinowski and the ethnographic method is taught in French universities today (Chevalier 2015a). In France, fieldwork has often taken the form of collective surveys, unlike the British norm of individual fieldwork projects.

Malinowski entered French anthropology primarily not as a methodologist, but through his *kula* ethnography as mediated by Mauss’s *Essai sur le don* (The Gift, 1925). Thus, students in introductory courses first learn about him through interpretations of his ethnography by others. Only later do they hear about Malinowski as a pioneer of anthropological field research. But their first contact links two individuals who are generally thought to be the founders of modern anthropology in Britain and France.

Mauss corresponded with Malinowski and admired his work for its contribution to the ethnology of Melanesia and especially for how it fed his own theoretical and political thinking about social exchange. Their letters did not address the ethnographic method itself

even if Mauss later recognized that ethnography was a very important subject. He considered Malinowski, Franz Boas, and himself to be the founders of three national schools of anthropology in France, Britain and the USA. Mauss's interaction with Malinowski, and with Boas, concerning their common interest in what was then called "potlatch", was very important for his personal development after the war. This included Mauss's theory of exchange, his comparative perspective, and his democratic insistence on giving priority to ordinary people in politics (Mauss 1997, Fournier 2015 [1994]). In the early 1920s and especially when composing *The Gift*, Mauss came to share Malinowski's view of ethnography's revolutionary potential, but initially more as a form of writing and reading than as fieldwork. Only later did he turn to training fieldworkers.

Mauss was at first less interested in Malinowski's commitment to long-term fieldwork, his British "trademark", than in *Argonauts* as a decisive contribution to understanding the significance of potlatch, now in the Pacific region and not just on the Northwest Coast. He was even less interested in Malinowski's "functionalism", a judgment that history has confirmed. Mauss conveyed to his French audience the immense significance of Malinowski's ethnographic substance, but not his methods. In *The Gift* he also expressed doubts about Malinowski's comparative approach to markets and money (Mauss 2016 [1925]: 91-93 n. 29).

We will not understand how Mauss mediated the French reception of Malinowski if we stick to the conventional stereotypes of the French as theoretical and the English as empiricist. Malinowski pioneered an ethnographic method based on long-term fieldwork and, as a consummate writer, made his ideas appear as descriptions from life. Marcel Mauss never did any fieldwork; for him ethnography was a form of writing. But he recognized that ethnography does not impose ideas on its readers, leaving them rather to make personal selections of what they find significant. This is why he devoted his last decades to teaching ethnographic fieldworkers and instructed them to combine theoretical ideas with meticulous empirical descriptions.

There are major differences that genuinely distinguish the history of French anthropology from its British counterpart. Prominent among them has been the central role played by museums and their collections in the development of the French discipline; Mauss was very sensitive to this phenomenon, Malinowski perhaps less so (L'Estoile 2017). Thus, the contrast between Mauss the armchair anthropologist and Malinowski the fieldwork-based ethnographer is false, confusing French openness to history, which their British counterparts rejected, with a preference for theoretical conjecture over empirical analysis. They both shared a passion for building a new discipline through the transmission of knowledge. Both trained cohorts of ethnographers with varying commitment to field experience and to reading.

After writing *The Gift*, Mauss devoted his life to educating graduate students of ethnography as director of the Institute of Ethnology in Paris. These included Denise Paulme, who wrote down Mauss's fieldwork advice for his *Manual of Ethnography* (Mauss 2007 [1947], as well as

Germaine Tillion, Marcel Griaule, and Jacques Soustelle (Laurière 2019). Ten expeditions to Africa (1928–1939) were organized by students of the institute. Exotic fieldwork or, better, leading an ethnographic mission was an essential step in a student’s professional career. And this is still the case today, when anthropology theses are usually based on long-term fieldwork.

But beyond methodology, Mauss and Malinowski shared an interest in the descriptive writing of social reality, focusing on the relation between scientific writing and literature. Both valued ethnography as writing, but Mauss preferred oral communication – hence his choice of teaching over research in his later career.

Vincent Debaene (2014 [2010]) has shown how complicated the relation was between nascent French anthropology and literature. This cannot be seen just as an opposition between objectivity and subjectivity. For example, Malinowski’s field diary (1988 [1967]), the writing of which was allegedly inspired by his love of the novels of Joseph Conrad, whom he knew personally (Clifford and Marcus 1986), is said to be an expression of his personality, while for the *Argonauts* this is generally overlooked. It is also forgotten that these founders of modern anthropology had to find new ways of writing to reflect their subject matter and methods, and sometimes found inspiration in literature and even in Greek myths. The *Argonauts* is a model of contemporary literature in its monographic form (Clifford 1985). While Malinowski felt an affinity with literature, he did not divide his publications between scientific treatises for professionals and literary texts for the public – as did Michel Leiris and Claude Lévi-Strauss, for example. Instead, he sought to integrate narrative and rhetoric in his writings, while making clear what he considered to be a practical model for the discipline’s future (Debaene 2014 [2010]: 103ff).

This is what makes Malinowski’s books still readable by anthropology students today. He is celebrated both as the “champion” of intensive fieldwork and as a storyteller. [13] *Argonauts* is a canonical and highly readable book, along with Eward Evans-Pritchard’s *The Nuer*, Gregory Bateson’s *Naven*, Oscar Lewis’s *The Children of Sanchez* and Philippe Descola’s *The Spears of Twilight*.

Regarding Malinowski’s part in the ethnology of France, the issues of research methods and writing up were the same in exotic anthropology as for ethnology in France. Arnold Van Gennep was a great modernist in transforming folklore into regional anthropology (Belmont 1979; and above all Fabre and Laurière 2018 and the dossier on Van Gennep in BeroSe, 2022). [14] He admired anthropology’s trajectory in Britain; but was marginal to Durkheim’s monopoly in Paris which in the name of sociology also took a significantly anthropological line. This overlap between Durkheimian sociology and anthropology has meant that the two disciplines are closely aligned in France under the hegemony of the sociologists.

National territory is the prime focus of anthropology at home. In France and Britain, home was contrasted with empire. France used both exotic and home anthropology for its national project, whereas Britain privileged exotic anthropology (Chevalier et al. 2007; Chevalier

2015b). The national question interested Durkheim and Mauss, but Van Gennep addressed it squarely in *Traité comparatif des nationalités* (1995 [1922]). This focus was familiar to Malinowski who brought his conception of ethnography from participation in Polish nationalism.

Van Gennep rejected any barrier between exotic anthropology and European ethnography, including the division between 'them' and 'us' practised by the Institute of Ethnology. Van Gennep was a fervent ethnographer and defended it all his life. Unlike Mauss, "his enemy brother in ethnography" (Laurière 2021), Van Gennep made fieldwork expeditions before 1914 in various regions of France, especially Savoy, and in Eastern Europe, even in Algeria (Pouillon 2018). He was also seeking a writing model to report on his fieldwork trips; in the introduction to his *Le folklore français* (1998–99, I: 55), he recommended that the ethnologist follow the "great descriptive writers" such as Flaubert and Maupassant (quoted in Adell 2022).

His marginal situation doesn't allow him to be involved in developments in French ethnology elsewhere, especially the impetus given by Georges Henri Rivière and the National Museum of Folk Art. In moving away from folklore, this museum launched a number of large-scale field exercises, including one in Sologne in the Loire valley during the period 1937–38, in which Malinowski and Louis Dumont participated. [15] Rather than use correspondents, researchers were sent into the field; and this became the academic norm after 1945. These "fieldwork camps" were a platform for an ethnographic revolution in home ethnology similar to Malinowski's programme in Britain for sending out students to carry out exotic anthropology abroad.

What about how Malinowski's espousal of long-term fieldwork came to shape global anthropology? In *The Gift*, Mauss claimed that "total social facts" drawing on many aspects of society at once can only be studied and communicated through ethnography. In other words, fieldwork is necessary to generate more realistic written descriptions. This soon became the rule in France for both exotic anthropology and anthropology at home.

Today teaching fieldwork to French anthropology students is most often conducted by teams and funded by universities to take place in their local regions. [16] Students and teachers can thus combine research and teaching during a week of cohabitation. These university courses were pioneered in 1947 by André Leroi-Gourhan of the University of Paris. His title for this course was "collective annual initiation into the field", clearly identifying the pedagogical moment as a "rite of passage" in Van Gennep's sense and experienced as such by the students. These courses spread to many anthropology departments (Chaudat and Leservoisier 2022) [17] and to other disciplines like sociology and geography.

French students usually encounter Malinowski first through Mauss in lectures on the *kula* featuring Mauss' theory of social exchange. Then they read the *Argonauts* which allows them to appreciate the quality of his ethnographic descriptions and writing.

While Malinowski is known for his advocacy and practice of long-term fieldwork, I have tried to show that his legacy in France is always seen in the context of a wider range of ancestors, especially French. This gallery includes Mauss, of course, but also Van Gennep, the founder of ethnology at home in France. The colonial emphasis of ethnographic research in Britain makes the strong French focus on anthropology at home as well as in the empire a significant contrast between the two countries. Mauss shares with Malinowski a search for a writing model built around an agreed approach to ethnographic research. If the traditional British approach was once satirized as “a lone ranger in a pith helmet”, French ethnological training has focused on collective fieldwork. These collective experiences are “rites of passage” in the teaching of ethnography in French universities today.

Malinowski’s legacy, in France and globally, is his role in the formation of the British school. But against a big man theory of history, I have suggested here, using the French case, that for students of anthropology in any country, the role of famous leaders is always mediated by a network of local actors whose influence is often more significant.

11. Ethnographic Research, Ethnographic Writing, Anthropological Theory and Colonialism: Malinowski and His Early Students, by James URRY (Wellington, New Zealand)

This essay will examine the development of ethnographic research and writing following the publication of Bronislaw Malinowski’s *Argonauts* (1922). Four issues will be considered through the recollections of some of Malinowski’s students and their comments on their experiences in constructing ethnographic monographs and the external influences that affected ethnographic research and writing to the early 1940s:

- Were Malinowski’s students taught how do ethnographic research?
- Did ethnographic fieldwork shape ethnographic accounts?
- How did the development of anthropological theory influence ethnographic research and the writing of monographs?
- How did colonialism influence ethnographic research and the writing of ethnography?

It is usually assumed that the first generation of Malinowski’s students received formal instruction in field methods following the so-called “revolution” he initiated in ethnographic methods. But one of Malinowski’s first students, Audrey Richards in the 1980s when asked whether she had received any training in methods, replied in unequivocal terms: “We never had any teaching on fieldwork”. [18] Richards’ use of the word “we” clearly refers to what she called the “very small” group of Malinowski’s first students. [19] Richards suggested that whatever members of this group learned about methods was gained informally mainly from each other, and not through any instruction, least of all from the master himself. She did relate, however, an amusing story of Malinowski rushing down the railway platform as she was about to depart for Africa clutching a set of coloured pens and cards. The pens were intended to help classify cultural categories in her notes and the cards to record different

aspects of language. Apparently, Malinowski's gifts only proved to be a burden. Her story undoubtedly reflects one aspect of Malinowski's own research including his use of charts to organize ethnographic material which his students helped him order by crawling around the floor of his room. So, no doubt these charts exposed his students informally to one aspect of his methods.

E. E. Evans-Pritchard's experience was similar and he later complained neither Malinowski nor C. G. Seligman provided much in the way of fieldwork training. He recalled Seligman advised him to "take ten grains of quinine every night and keep off women". When he asked Malinowski for advice on field methods, he was told not to be a "*bloody fool*". [20] According to Richards, Evans-Pritchard eventually also rejected the idea that his students needed to take a course on field methods, and it was better they simply immersed themselves in the lives of their people in the field. A. R. Radcliffe-Brown, Evans-Pritchard's predecessor at Oxford, syllabus for social anthropology in the Diploma in Anthropology, did not include a course on method. [21]

When I wrote on the history of field methods for a book concerned with ethnographic methods, I included American anthropologists, especially Boas and his followers, Raymond Firth – one of Malinowski's first students – suggested to the editor my discussion of American anthropologists be removed because they did not have field methods (Urry 1984). [22] In her interview Richards reported that at the LSE: "There were certain straw men who appeared like the terrible stories of American fieldworkers paying four dollars an hour to Indians to give them material, and we resolved immediately without saying anything that we would never pay anyone for anything". [23]

One might assume that for Malinowski's first students, ethnographic fieldwork preceded the submission of their PhD thesis which would later form the basis of a published ethnographic monograph. This was not so. Most of the first generation of students, Firth, Ian Hogbin, Hortense Powdermaker, Richards and Isaac Schapera, carried out library research for their doctorates at the LSE. [24] As Schapera later noted, they first had first "*to find and then to evaluate documentary sources*". [25] Only Evans-Pritchard proceeded directly to the field. Firth's first monograph *Primitive Economics of the New Zealand Maori* (1929), written before he left for fieldwork, attempted to establish the "traditional" economic life of Maori based on existing literature. [26] Schapera's thesis became the basis for his monograph *The Khoisan Peoples of South Africa: Bushmen and Hottentots* (1930), which was similarly based on ethnographic literature. [27] Therefore, despite criticism of the research methods of American ethnologists, a number of Malinowski's students first published ethnographic accounts were not based on field research. [28]

On their return from the field all the ethnographers faced the challenge of writing up their research into ethnographic monographs. Firth later quoted from a letter Malinowski sent to him after his return from the field in which he urged Firth to "proceed at once to a full straightforward account" of Tikopian culture and not to write it up "piecemeal" as he had done with his Trobriand material (1957: 10). [29] Malinowski revealed he originally had

intended to write a monumental monograph but eventually abandoned the idea and wrote the *Argonauts*. [30] For Malinowski's first students, his opening statements to *Argonauts* provided a very general guide and justification for fieldwork. The structure of his monograph suggested that once fieldwork was completed, their ethnographic monograph need not be a full, straightforward account like those published earlier, for example Rivers's *The Todas* (1906), the Seligman's *The Veddas* (1911), and Radcliffe-Brown's *The Andaman Islanders* (1922 but completed earlier). Firth in his *We the Tikopia* (1936) did not take Malinowski's advice and he obviously struggled with the book as it took seven years to complete and ended up as a study focussed on kinship. Later he produced separate monographs on other aspects of Tikopian life. [31]

While conducting ethnographic research, Malinowski's students quickly discovered, as Richards expressed it, "the mass of 'concrete documentation'... grew alarmingly" and the researcher "seemed to become a person who tried to find out more and more about more and more" (1957: 28). This was a reflection of what Edmund Leach characterized as Malinowski's "obsessional empiricism", and his insistence that every minute detail be recorded, however unimportant (1957: 120). Unsurprisingly, most of his students felt overwhelmed by the prospect of producing "whole" accounts from their ethnographic material. The situation was made worse because Malinowski's functionalism also required that accounts show the "interdependencies" of "institutions". (Kaberry 1957: 72) Consequently, Malinowski's insistence on the detailed gathering and the integration of ethnographic material forced his students to write focussed ethnographic studies, counter to his own stated views concerning a single ethnographic account. Other forces, however, were involved in choosing to write more focussed ethnographies.

Written accounts needed to be more than an display of ethnographic methods and mere description. It needed theory to give it substance and if anthropology was to be recognized as a legitimate discipline and its practitioners recognized as scholars in academia, they needed to justify themselves on more than a method and colourful accounts of exotic cultures. It should be remembered that the subtitle of *Argonauts* described it as an "adventure", and a rollicking tale of "native enterprise". [32]

Kaberry later stated that Malinowski set "a standard for intensive field-work and rigorous documentation of theory that few have achieved since" while his "functional approach has become so much a part of the texture of our thinking that we are apt to forget its first full formulation and demonstration occurred only a generation ago in 1922". (1957: 72) For Kaberry, modern ethnography and anthropology began with the *Argonauts*. She had studied under Malinowski but while some of her contemporaries probably agreed that Malinowski had set a "standard" for ethnographic fieldwork, not all would acknowledge his contribution to theory, or that "rigorous documentation" was a suitable basis for anthropological theory.

In the same year *Argonauts* was published, Malinowski wrote a review of Sir James George Frazer's abridged *Golden Bough*. He boldly stated that "a sound theory must be the forerunner of empirical discoveries, it must allow us to foreshadow new facts not yet ascertained by

observation” (1923: 659). He appeared more than willing to subordinate himself to other writers of theoretical studies, declaring “I derived constant inspiration and benefit from the works of Westermarck, Karl Buecher, Ratzel, Marett, Hubert and Mauss, Crawley and Rivers, some of which I actually have re-read while in the field, others again in the intervals between my expeditions”. However, he apologized for not taking with him the “twelve volumes of the *Golden Bough*”, too “heavy and costly a burden to carry across sago swamps, to paddle over lagoons in an outrigger always ready to capsize, or to keep in a tent or thatched hut by no means rain- and insect-proof”. Now, following the publication of an abridged version of Frazer’s volumes a “more fortunate field-worker” could “constantly refer” to Frazer’s ideas (ibid: 662). Frazer was the theoretician who would guide people like himself, a “field worker” in “unexplored countries”.

Later Malinowski did attempt to formulate theoretical statements in order to justify his new anthropology but apparently untroubled whether to refer to himself as an ethnographer, ethnologist, or anthropologist (social or otherwise). Malinowski’s theoretical vision for anthropology though was decidedly weak. Radcliffe-Brown, Malinowski’s contemporary, was much clearer. He identified himself as a “social anthropologist” and “social anthropology” as the proper name for the emergent scientific discipline. [33] For Radcliffe-Brown, ethnographic research was just a means to an end, not an end itself and all accounts had to be informed by theory. In 1931 he wrote “intensive studies must ... be inspired and guided by theory” and anthropological theory “must rest on the comparison of different cultures one with another” (1958 [1931]: 76). [34] Such studies were useful only if they contributed to larger, comparative studies; as an ethnographic study of a “single society may provide materials for comparative study ... or may afford occasion for hypotheses, which then need to be tested by reference to other societies; it cannot give demonstrated results” (Radcliffe-Brown 1940: 5). [35] In several and often varied ways, Malinowski’s students started to turn to Radcliffe-Brown for “theoretical” direction. In time Radcliffe-Brown’s ideas started to shape ethnographic writing and also the direction of ethnographic research as fieldwork became more focussed even if a broad range of information still was collected. How this influenced ethnographic writing was noted by Kaberry in a section entitled “Malinowski’s effect on the writing of ethnography” where she devoted more time criticizing Radcliffe-Brown’s impact than discussing Malinowski’s influence. She suggested the “new” ethnographies, many written by Malinowski’s own students, were too “rigid”, that resulted in the “neglect” of “other aspects of social life” and a “high level of abstraction” where “people, in the sense of a group of personalities, are conspicuous by their absence” (1957: 88-89).

Ethnographic research and the writing of ethnography occurred in the context of colonialism but often in different situations. Malinowski and Radcliffe-Brown had greater success than anthropologists before the First World War in convincing central and colonial governments of the value of ethnographic research and anthropological knowledge. Malinowski’s calls for a “practical anthropology” and Radcliffe-Brown’s for an “applied anthropology” helped their students to gain access to field sites as well as financial support. Colonial administrations,

however, wanted ethnographic research to produce concrete results. An example of how research was influenced by a colonial administration is Evans-Pritchard's work among the Nuer and the structure of his monograph, *The Nuer* (1940). British administrators of the Sudan were concerned with Nuer raiding and other aspects of Nuer society and Evans-Pritchard provided important information on such matters while also using his ethnographic material to write an influential ethnography that reflected in part the value of Radcliffe-Brown's approach. Evans-Pritchard's first ethnographic research a decade earlier among the Azande clearly showed the influence of Seligman and Malinowski. [36] *The Nuer*, however, illustrated the importance of colonial support, the value of focussed ethnographic research and writing, and the usefulness of all these factors in the development of anthropology as a discipline.

Malinowski encouraged his students to think he had established an epistemological break with anthropology's past, marked by a new standard of ethnographic research, the writing of ethnography and to a lesser extent new theories. Meyer Fortes later wrote of Malinowski that he viewed himself "as the leader of a revolutionary movement in anthropology", who presented his "ethnographic discoveries in the form of an assault on the *ancien régime*" (1957: 157). In the same volume, Edmund Leach claimed Malinowski had "created a theory of ethnographic field-work" while transforming "ethnography from the museum study of items of custom into the sociological study of systems of action" through producing "vivid" accounts that replaced the "dry record of 'old style ethnography'" (1957: 119). For many of Malinowski's students, the break had been established: modern ethnography began with Malinowski.

12. Franz Boas, His Students and Their Ethnographic Work, 1922–1942, by Rosemary Lévy ZUMWALT (Agnes Scott College)

The volume, *Ethnographers Before Malinowski*, co-edited by Frederico Delgado Rosa and Han F. Vermeulen, provides a powerful antidote to the misconception that ethnographic fieldwork started with Malinowski and the *Argonauts of the Western Pacific*, as does Herbert Lewis's paper on Franz Boas's fieldwork in Baffin Island, published in this edited volume. I have written on Boas's ethnography in *Franz Boas: The Emergence of the Anthropologist* (Zumwalt 2019) and will now focus on his students' ethnographic work post-1922, a subject also discussed in my second Boas biography, *Franz Boas: Shaping Anthropology and Working for Social Justice* (Zumwalt 2022). In the period, 1922 to Boas's death in 1942, Boas had 32 students who conducted ethnographic fieldwork. These are listed with date of publication of their PhDs. [37]

- Ruth Benedict, 1923
- Gladys Reichard, 1925
- Melville Herskovits, [1923 completed] 1926
- Isabel Gordon Carter, 1928
- Erna Gunther, 1928

- Margaret Mead, 1928
- Ruth Bunzel, 1929
- Alexander Lesser, [1929 completed] 1958
- Thelma Adamson, [1929, completed but not published]
- Gene Weltfish, [1929 completed] 1950
- Melville Jacobs, [1928 completed] 1931
- Reo Fortune, 1931
- Zora Neale Hurston, [no PhD]
- Ella Cara Deloria, [no PhD]
- Louis Eugene King, [1932 completed] 1965
- Manuel J. Andrade, 1933
- Bernard W. “Burt” Aginsky, 1935
- Ethel G. Aginsky, 1935
- Jules [Blumensohn] Henry, 1935
- Edward Allan Kennard, 1936
- George Herzog, 1936
- Ashley F. Montagu, 1937
- Ruth Underhill, 1937
- William Whitman III, 1937
- Ruth Landes, 1937
- Clara Hilderman Ehrlich, 1937
- Marian Wesley Smith, 1938
- Viola E. Garfield, [1935 completed] 1939
- Edward Adamson Hoebel, [1934 completed] 1940
- Irving Goldman, 1942
- David Efrón, 1941
- Amelia Louis Susan [Schultz], [1941 completed] 1943

During this period of 1922 to 1942, Boas’s women and men students penetrated the icy exterior with which his earlier students were familiar, and they gentled his image. In his early work with his students, Boas had cultivated a cool, steely exterior. In tribute to his mentor, Alfred Louis Kroeber wrote, “The only motto he ever proclaimed, and that sparingly, was probably derived from Virchow: ‘icy enthusiasm.’” Boas maintained that “icy enthusiasm” was necessary for good scientific work (1943: 22). [38] For the later students, Franz Boas, the esteemed, aloof professor of anthropology became “Papa Franz”, a fictive kinship term that they also extended to Marie, who became “Mama Franz”. Even Bronislaw Malinowski used the fictive kinship term. In a letter of May 4, 1933, Malinowski wrote Ruth Benedict, “I was extremely glad however to have seen you and to have had some time together with Papa Franz whom I got really to like very much personally, as in fact I always did”. [39]

In *Voyage to Greenland*, Boas student and archaeologist Frederica de Laguna wrote of her 1929 spring term at the London School of Economics where she had studied with Bronislaw

Malinowski: “The course with Professor Malinowski was an unpleasant disappointment, for he regularly spent the first hour of the weekly two-hour seminar in attacking the United States and the nasty habits of Americans... I was his particular butt, since he had conceived a violent hatred of Dr. Boas and lost no opportunity to reveal my ignorance as an illustration of Boas’ poor teaching” (1977: 23).

This image of Boas as Malinowski’s nemesis was belied by their correspondence. Boas and Malinowski wrote frequently between 1933 and 1934 with respect to their efforts to rescue scientists from the Nazis. Boas wrote Malinowski in 1933, “I ought to have written to you a long time ago but I have been very hard pressed for time, very largely on account of our endeavors to help the ousted German scientists. We have followed the example of the London School of Economics, and our faculty in Columbia University has raised enough money to bring a few people here”. [40] They drew rather close in their shared efforts. Malinowski wrote Boas in November 1933, “Things have not been going very well with me personally, my health and above all, that of my wife, being a constant source of worry. As to the world in general, you probably feel as I do, that it is not getting a nicer place to live in. I often think of you when new horrors come from Germany or elsewhere, since I think your point of view is very much the same as mine. Remember me to your entourage, please”. [41]

By “entourage”, Malinowski was referring to the group of students he had spoken with when he visited Columbia University in March 1933. Boas had invited Malinowski to “speak . . . to our students, perhaps in our Seminar”. [42] Boas’s reference to the seminar was to one of the crucial courses that he taught. The seminar accommodated student presentations, visiting lecturers, and students who had returned from the field to talk about their fieldwork. And if one renders “entourage” as “cohort”, one might refer to David Browman and Stephen Williams and their work on *Anthropology at Harvard* in which they point out that cohorts form around “contemporaneous networks of scholars who attended the same classes and scientific meetings, worked on the same projects, and debated issues of common interest, regardless of their subfield specialization” (2013: x).

Adding to the camaraderie were the parties that Boas gave at his home. Psychologist Otto Klineberg blended his recollection of Franz Boas with the social milieu. He recalled other students talking about Margaret Mead who had been away in Samoa conducting fieldwork in 1925: “She came back, I think, during that academic year, and I remember meeting her at one of the parties that Boas had in his home on the New Jersey side of the River, where all of us congregated once in a while to be with our dear ‘Papa Franz.’ All of his students called him that and I was quickly accepted into the fraternity, and he became my Papa Franz as well”. [43]

As recorded in her diary, Anita Brenner also recalled a gathering at Boas’s home: “Friday evening at Dr. Boas’, with students and anthro notables dripping brains all over the place. Most animated and fantastic gathering, with hours of talk about science”. She typified Boas as “a grand-old man”, who was “extraordinarily considerate”, and one from whom she gained “intellectual humility” (1988: 114). [44]

The sense of the connection with students is very clear in Boas's correspondence. In 1930, he wrote to George Herzog,

Anita Brenner is married and has sailed for Europe. She has a Guggenheim Fellowship this year and expects to go in the fall to Mexico. Ruth Bunzel also has a Guggenheim Fellowship and is going to Guatemala to study Kiche. Lesser and Gene are in the field continuing their Pawnee work. Gene has a National Research Council Fellowship and we are paying Lesser from the linguistic fund. I believe you also know Miss de Laguna, who spent last summer in Greenland. She is now doing archaeological research work in British Columbia. Gladys is in New Mexico working with the Navajo. Gladys and Jacobs are both going to the Americanists Congress in the fall. I believe this is all the news I can tell you. [45]

Boas's letter to Herzog conveys the closeness of his graduate students: they all kept track of what each other was doing; and the exciting nature of their fieldwork.

Boas worked successfully to expand fieldwork sites beyond the borders of the United States. This had always been Boas's concern, that fieldwork should not be limited to the boundaries of the United States but should truly be international. So, through Boas's efforts, the ethnographic work moved south, to Central and South America (David Efrón, Marcus Goldman, and Ruth Landes), to Africa (Marian Wesley Smith, George Herzog, and Melville Herskovits); and to the South Pacific (Margaret Mead and Reo Fortune).

Boas was also instrumental through his students in the emergence of new sub-fields in ethnography. He always stressed the importance of folklore and of collecting folklore texts. The master of this was his student Melville Jacobs who developed an approach to the study of narrative that stressed the oral literary style and grew into performance study, which is crucial in the field of folklore. Through his work, Edward Adamson Hoebel developed the field of legal anthropology. David Efrón studied the hand gestures of Russian Jews and Southern Italians in New York City. His work developed into gesture studies or non-verbal communication. Ruth Bunzel took Boas's interest in the individual creativity of the artist and shaped it into the study of the *Pueblo Potter*. Ruth Landes's work and Gladys Reichard's formed the basis for work on women's studies.

Franz Boas was dedicated to the field of ethnography and to training his students to carry on and to branch out, both geographically and conceptually. He was an innovator in his own fieldwork and encouraged innovation in the work of his students. He always stressed the need to capture the native view of the world and he used new technologies to capture the evanescent. With respect to the latter, Boas used 16 mm motion picture film in his 1930–31 fieldwork in Ft. Rupert, British Columbia, at a time when this technology was less than ten years old. He filmed dances, gestures, and games, all that was non-verbal and kinaesthetic. "In my opinion", he wrote, "the recording of Indian life by means of motions pictures . . . is a most valuable undertaking". Only by using film could one obtain information for the study of "dance and similar matters" about which "a detailed knowledge of the movement of the body

and rhythm is necessary”. [46]

Throughout his work with his students and in his own ethnographic research, Boas insisted on the complexities and variabilities within a culture. In his 1933 review of G. W. Locher's *The Serpent in Kwakiutl Religion, a Study in Primitive Culture*, Boas questioned the emphasis on “the unity of culture”, and the focus on discovering “the ‘function’ of each and every act or thought in the cultural system”. In thinly veiled recognition of criticisms against his approach, Boas wrote, “The complaint has been made often and by every earnest student that the stereotyped ethnographical description provides us only with disconnected fragments of the living culture”. He continued, “However, it is not justifiable to conclude from the defects of the available descriptions which do not reveal a unity of culture, that the whole culture must be a compact unity, that contradictions within a culture are impossible, and that all features must be parts of a system. We should rather ask in how far so-called primitive cultures possess a unity that covers all aspects of cultural life”. Boas queried, “Have we not reason to expect that here as well as in more complicated cultures, sex, generation, age, individuality, and social organization will give rise to the most manifold contradictions?” Recognizing the complexity and diversity within a culture, Boas was loath to attribute simplicity to any cultural system. By extension, he offered a critique of the functionalist approach as exemplified in Malinowski's work that emphasized the interrelated connections of all parts of a social system (1933: 418). Boas offers us complexity, variety, and continual surprises in all his ethnographic work through his own work and through that of his students.

13. Why Disciplines Forget: Salvage Anthropology and The Politics of Historiography, by David MILLS (University of Oxford)

About twenty years ago I found myself learning the skills of an archivist. I had been sent a large battered tin trunk, full of neat piles of committee papers, along with a jumble of receipt books and bank statements. This Pandora's box of disciplinary secrets was the mobile archive of the Association of Social Anthropologists (ASA), established in 1946 as an organizational rival to the Royal Anthropological Institute (Mills 2008). The tradition had been that each office holder sent on their own papers to their successors, adding new pages to the files. Archivists emphasize the importance of “provenance” and the need to preserve the “original order” of the deposited materials (National Archives 2016: 7). But this logic was hard to impose on this live disciplinary palimpsest, as each office holder added their own notes and queries. The trunk was also full of cheque book stubs, applications for membership, bank statements and expenses receipts. What should I keep and what, if anything, should I throw away? What would prove historically significant, and what was administrative ephemera? Knowing how to unpack the trunk exemplified the challenges of unpacking and accounting for an intellectual tradition, and the many different stories that could be told about its emergence and pasts.

I began to read between the lines of the archived committee minutes, trying to piece together the tense tenor of early ASA business meetings. The association was the brainchild of Edward

Evans-Pritchard, who had set himself the task of institutionalizing Radcliffe-Brown's vision of a theoretical "natural science of society" (Mills 2017), an idea hatched during Oxford pub conversations before the war (Stocking 1996). A new discipline needed an intellectual infrastructure – conferences, journals, papers. It also needed to have the right people in the room. As Jack Goody recalled, "the closeness of the fraternity was one way in which the highly amorphous subject of anthropology ... was given some manageable bounds" (Goody 1995: 83). The corollary also applied: the association devoted much time to discussing who to invite to join, and who to keep out. At their inaugural meeting in Oxford 1946, the ten founding members agreed to create a "register of anthropologists in the British Empire", with membership to be restricted to "teachers and research workers in Social Anthropology in Great Britain, the Dominions and the Colonies" (Mills 2008). The intention was to promote the interests of "professional" social anthropology. Nominations had to be approved and so could also be rejected. In practice those invited were largely colleagues and ex-students of Radcliffe-Brown and Malinowski, many of whom were teaching in universities across the British empire. The new discipline's networks continued, at least briefly, to sustain an "empire of scholars" (Pietsch 2013). On the other hand, colonial administrators, as well as those working in other branches of anthropology, along with most US scholars, were rather less welcome. The strategy enabled a coherent intellectual focus. Yet this was at the cost of becoming increasingly inward-looking and self-referential (Stocking 1996). Since the 1970s there have been many further discussions about membership (Grillo 1994, Spencer 2000), and disagreements about whether the association should be more inclusive continue to this day.

1922 was "year zero" for modernism. It was also the year Bronisław Malinowski published *Argonauts of the Western Pacific*. The conjuncture was a powerful one, and it partly explains why social anthropology keeps returning to the romantic figure of Malinowski strutting the Trobriand shores (Hutnyk 1998, Manganaro 2002). Malinowski liked nothing more than portraying himself as a revolutionary Conradian figure, championing his "new science". He was a skilful bricoleur, struggling to reconcile literary rhetoric and positivist science (Payne 1981). But he tended not to foreground his own scholarly heroes – including Nietzsche, Mach, Westermarck and Freud – in his writing. His talent at myth-making ensured that methodological continuities were similarly effaced. For Edwin Ardener, Malinowski's self-coronation as the leader of the "Functional School of Anthropology" was the perfect example of a "typically modernist act" (1989: 135–139).

Intellectual histories, especially those that anthropologists such as Adam Kuper (1973) began to tell about this new field, foregrounded ideas rather than institutions. There was relatively less attention to the disciplinary infrastructures, political conjunctures and economic forces that shaped and enabled their ambitions. In Malinowski's case, this included the impact of the First World War, and the patronage of influential funders, universities and the colonial state (Mills 2008). During the late-colonial period, social anthropologists like Raymond Firth and Audrey Richards held influential roles within the Colonial Office committee that funded much social anthropological work. Colonial Office patronage ensured that university

appointments, grants and research fellowships helped reinforce the centrality of this new disciplinary paradigm, sidelining other approaches and perspectives (Mills 2005, Steinmetz 2013)

Malinowski's legacy was far from uncontested: he made many enemies amongst colleagues and former students. Post-war social anthropologists sought to distance themselves from his theoretical ideas, even as they mythologized his ethnographic fieldwork and the LSE seminar. Some years later, contributors to Firth's *Man and Culture* helped re-establish his methodological reputation (Firth 1957). Yet the disciplinary paradigm that he and Radcliffe-Brown had helped create – with its focus on society as a 'going concern' – was soon to come under attack. In 1951 George Murdock dismissed what he called the "British" school, attacking the overweening influence of Radcliffe-Brown on its intellectual concerns. He was particularly critical of its "complete neglect of history" and a "lack of interest in the processes by which culture changes over time" (Murdock 1951: 469–472). In his view this work was comparable with "sociological schools of an earlier generation". Whilst Firth and other "British" social anthropologists politely challenged this analysis, the growing American dominance of the social sciences was occurring alongside the crumbling of imperial academic networks (Firth 1951). Gradually the debates that had sustained this community of scholars were sidelined by the theoretical revolutions led by Talcott Parsons and Clifford Geertz in the USA (Mills 2017).

It is common to think of scholarly disciplines as benign intellectual fora, curating intellectual traditions, stewarding debates and socializing new generations. But discipline, as the word's etymology reminds us, is always about power. Knowledge orders are unstable, and constantly need shoring up: through professional associations, journal editorships and tenure decisions. Hidden under the bonnet of intellectual respectability, disciplines are also machines for forgetting. From a Kuhnian perspective, in order to make history, one has to let go of history. It is no surprise that disciplines disown troublesome ancestors and efface inconvenient pasts, remembering origin stories accordingly.

The first histories of post-Malinowskian social anthropology were largely celebratory, though Talal Asad's (1973) *Anthropology and the Colonial Encounter* and Dell Hymes' (1972) *Reinventing Anthropology* began to unsettle this. Today there are many more subaltern histories of anthropology, as well as ethnographies of its coloniality and racial exclusions (e.g. Harrison and Harrison 1999, Simpson 2014). For Anand Pandian (2019: 8) "deep histories of inheritance remain essential, as are neglected and forgotten resources for reinvention". The patient work of historical recovery could be described as "salvage anthropology". It questions singular histories and disciplinary retellings of the "anthropology of the winners". Its excavation of hidden genealogies echoes the decolonial and feminist critiques of exclusionary disciplinary canons. Disciplinarity and historiography are entangled, reminding us to read the history of anthropology as a contested sociology of knowledge.

So what do we remember, and how? The archive cannot speak for itself. As Jacques Derrida notes, "every archive ... is at once institutive and conservative ... revolutionary and traditional

... there is no political power without control of the archive, if not of memory” (1995: 7). For Derrida, “effective democracy can always be measured by this essential criterion, the participation in and access to the archive, its constitution and its interpretation”. Just as important as the archive’s “custodians” are its interpreters and guides, the historians. As the academy wrestles with the recursive power of coloniality, the discipline of history itself is increasingly under scrutiny. As Priya Satia points out in *Time’s Monster* (2020: 6), “historical discipline helped make empire – by making it ethically thinkable”.

A critical attention to disciplinary historiography opens up difficult questions about the politics of creating usable pasts, and the conflicted role of anthropology’s historians in these narratives. Even George Stocking became “one of us” in the end. Disciplines can be sites of intense loyalty and belonging, as well as of exclusion: identities that are at once generative and destructive. Can disciplines be trusted to tell their own histories?

AFTERWORD, by Thomas Hylland ERIKSEN (University of Oslo)

1922 is the pivotal year around which the present collection of short essays gravitates, as the subtitle of the book under discussion confirms: “Pioneers of Anthropological Fieldwork, 1870-1922”. As Rosa and Vermeulen and their collaborators suggest, the past can alter quickly in a time of rapid change. A very anthropological insight this is, and one which did not sit well with the alleged inventors of modern social anthropology, Radcliffe-Brown and Malinowski, who had their disagreements, but agreed on the futility of studying social change when self-reproducing societies were at hand. They had their reasons for dismissing the historical dimension. Speculative cultural history, in extreme cases tracing all human civilization back to Egypt, was thriving at the time, and well into the 1930s, lecturers in some universities had to give courses on the diffusion of Megalithic cultures.

Apart from being the year in which *Argonauts* and *Andaman Islanders* were published, 1922 was also the year in which William H. R. Rivers died prematurely and suddenly of a strangulated hernia. A towering figure in British anthropology and president of the Royal Anthropological Institute at the time of his death, Rivers represented a broader, more historically grounded (if sometimes speculative) and psychologically inclined anthropology than his successors, who sought to narrow the field of enquiry in order to make a “real science” of social anthropology. Just a couple of years later, a third text of lasting significance (some might even venture to say *the* third defining text of social anthropology) was published in 1925 (in the 1923-24 issue of *L’Année sociologique*), namely Marcel Mauss’s “Essai sur le don”.

It was not just in anthropology that the year 1922 signalled a new beginning. Europe had finished licking its wounds after the devastating war of 1914–1918, it had just left a debilitating and deadly pandemic behind, and it was a time for renewal and fresh air. The portly gentleman with a cigar and waistcoat gave way almost unnoticed to athletic, slim men driving convertibles and frivolous women speaking into telephone mouthpieces, just as the Victorian ponderousness of the academy was replaced by a lighter, more playful and more

experimental touch. Indeed, by pure coincidence, a massive tome on my desk, catching my eye at this precise moment, is the centenary edition of *Ulysses*, densely annotated and explained with learned essays and commentaries.

1922 saw the publication of T.S. Eliot's *The Wasteland* as well as James Joyce's *Ulysses*, and modernism was flourishing. Marcel Proust was in the middle of his huge roman-fleuve, and the first translation (of *Swann's Way*) appeared in English in 1922. Virginia Woolf's masterpieces would be written later in the decade, but she was already a central thinker and active writer (who, incidentally, disapproved of *Ulysses*). The Bloomsbury group had been established around Leonard and Virginia Woolf, and one of its members, John Maynard Keynes, had begun to redefine economic science. Perhaps more interestingly in this context, Keynes was open about his homosexuality. Edward Westermarck, twenty years his senior, was not. Westermarck had already been in his late thirties at the time of Queen Victoria's death in 1901 – David Shankland in the present special issue speaks of the “stultifying conventions of high bourgeois life” – whereas Keynes was still a teenager at the time.

The early 1920s, thus, marked a watershed in several ways. Arnold Schoenberg was by now composing twelve-tone music. Albert Einstein received the Nobel Prize in Physics in 1921. Robert J. Flaherty's pathbreaking ethnographic film *Nanook of the North* was made in 1922. On the continent, Dadaism was morphing into a broader surrealist movement not least thanks to the efforts of André Breton (who claimed Picasso as one of their own). When Han Vermeulen in the opening statement of the present special issue speaks of an “epistemological shift that occurred in or after 1922”, he could refer to a much broader movement than the change in anthropology.

There were also exciting changes taking place in science and technology. Medical insulin, which has since saved millions of lives (including mine), was applied successfully in Canada in January 1922. It was the year when waterskiing and the electric blender saw the light of day, and the year when macromolecules were discovered which would soon make it possible to produce plastic.

In anthropology, vast, pompous edifices like Frazer's *Golden Bough*, a twelve-volume monster in its largest edition and – in fairness – Rivers's *History of Melanesian Society* (1914) were superseded by more concise, focused, readable books such as *Argonauts*; and it bears mentioning that an abridged, single-volume edition of *The Golden Bough*, was actually published in the fateful year of 1922 (see Frazer 2002 for a later single-volume edition). However, as James Urry indicates in this round table, Malinowski had something of the transitional figure about him, in that he advised his students to write fully detailed, holistic accounts of their societies almost in the Victorian style. They nevertheless deviated from his plan, as he did himself, a trickster from Joseph Conrad's country, producing a series of thematically delineated books rather than a massive volume covering every institution and their interrelationships.

One outcome of Rosa and Vermeulen's book and this round table is the realization that

whether deservedly or not, the idea of a Malinowskian revolution in social anthropology around 1922, in some versions of disciplinary history coupled with an equally important Radcliffe-Brownian one, is deeply ingrained one hundred years later. Perhaps the two and their contemporaries were just lucky to be in the right place at the right time. 1922, “year zero” in David Mills’s words, was beyond doubt an auspicious year for adventurous and bold intellectual projects.

The most important follow-up question concerns the enduring legacy of this fieldwork revolution, a hundred years later. Has Malinowski’s “scientific methodology”, an ideal and blueprint for generations of anthropologists, by now become a straitjacket to be discarded, or does it still offer sensible guidelines for systematic research on social organization and cultural worlds? Certain lacunae and shortcomings of Malinowski’s original programme have often been remarked upon, notably the lack of a strong comparative methodology, the indifference bordering on hostility towards historical accounts and the nearly total lack of interest in the most fundamental criterion of social differentiation in human societies, gender.

At this historical moment, no less a pivotal time than the early 1920s, the most glaring shortcoming of the classic ethnographic method consists of its myopic bias. It encourages the social scientist to crawl on all fours with a magnifying glass to study the relationships between the grains of sand on the beach, but lacks a complementary principle forcing the researcher to raise their gaze and scale up both temporally and spatially. In *The Gift*, Mauss shows how Malinowski’s analysis of the *kula* would have benefitted from a stronger historical and comparative perspective; and doing anthropology in the 2020s, at a time of accelerated, runaway, overheated globalization, requires a theoretical perspective which encompasses both the long now and the big here. Ethnography is a superb methodology for generating high-octane empirical material of unsurpassed quality, but in itself, it cannot tell you much about the world. It can only teach you about the human condition (including its imponderabilia).

Compared to the anthropology established in the 1920s, the social anthropology of the 2020s is definitely more gender-sensitive and reflexive; it has stronger interdisciplinary leanings both methodologically and theoretically, takes a more positive view of historical explanations (which were, incidentally, never dismissed in mainstream American anthropology), and represents a break not only with Radcliffe-Brown and Malinowski, but also with other pioneers such as Boas and Mauss, in that it sees humanity through a broader ecological lens. This shift may represent the most radical development in anthropology since the so-called fieldwork revolution. Research questions and priorities change, interdisciplinary collaboration with new academic fields becomes necessary, and innovative theoretical ideas proliferate, not least those aiming to reconceptualize the relationship between human beings and everything else.

At the same time, it is well known that not all parts of a culture change at the same speed, and the toolbox of anthropology has been supplemented, not replaced, in the century since the

publication of *Argonauts*. One of the most valuable resources anthropology can bring to the table in the interdisciplinary endeavour to come to terms with environmental destruction, climate change and Anthropocene effects more broadly, consists of thick descriptions (see Eriksen 2016, 2018, 2020). Like in Kiriwina during the First World War, ethnography focusing on local responses to overheated globalization is recorded painstakingly, slowly and meticulously, usually with the local language as a medium, with an emphasis on understanding “the native’s point of view” but also connecting values to practices and practices to social organization. These are skills that can be traced back to where we started, namely the *annus mirabilis* of 1922, when the professional methods of social anthropology, which had accumulated and been refined over a long period, were finally crystallized, summarized and focalized in the introduction to *Argonauts*.

REFERENCES CITED

Adell, Nicolas, 2022, “Manières ethnologiques de faire avec de la littérature”. *Anthropologie et études littéraires*, Special Issue, *Contextes, Revue de sociologie de la littérature*, n° 32 [online]

Ardener, Edwin [and Malcolm Chapman]. 1989. *The Voice of Prophecy and Other Essays*. Oxford: Basil Blackwell.

Asad, Talal (ed.) 1973. *Anthropology and the Colonial Encounter*. London: Ithaca Press/Atlantic Highlands, NJ: Humanities Press.

Barnard, Alan. 2000. *History and Theory in Anthropology*. Cambridge: Cambridge University Press. 2nd ed. 2022.

Beattie, John. 1964. *Other Cultures: Aims, Methods and Achievements in Social Anthropology*. London: Cohen & West.

Belmont, Nicole. 1979. *Arnold van Gennep: The Creator of French Ethnography*. Chicago and London: University of Chicago Press.

Boas, Franz. 1885. “Below the Polar Ice”. *New Yorker Staats-Zeitung*, 18 February. In Franz Boas, *Arctic Expedition 1883–1884: Translated German Newspaper Accounts of My Life with the Eskimos*, edited by Norman F. Boas and Doris W. Boas. Private edition, 2009.

Boas, Franz. 1887. “A Year among the Eskimo”. *Journal of the American Geographical Society of New York* 19: 383–402.

Boas, Franz. 1888. “The Central Eskimo”. In *Sixth Annual Report of the Bureau of Ethnology Presented to the Secretary of the Smithsonian Institution 1884–’85*, 409–669. Washington, DC: Government Printing Office.

Boas, Franz. 1933. Review of *The Serpent in Kwakiutl Religion: A Study in Primitive Culture* by G. W. Locher. *Journal of American Folklore* 46, no. 182: 413–21.

- Browman, David L. and Stephen Williams. 2013. *Anthropology at Harvard: A Biographical History, 1790–1940*. Cambridge, MA: Peabody Museum Press.
- Bunzl, Matti. 1996. “Franz Boas and the Humboldtian Tradition: From Volksgeist and Nationalcharakter to an Anthropological Concept of Culture”. In George W. Stocking, Jr. (ed.) *Volksgeist as Method and Ethic: Essays on Boasian Ethnography and the German Anthropological Tradition*. Madison, WI: University of Wisconsin Press, pp. 17–78.
- Bunzel, Ruth. 1929. *The Pueblo Potter*. New York: Columbia University Press.
- Burton, John W. 1983. “The Ghost of Malinowski in the Southern Sudan: Evans-Pritchard and Ethnographic Fieldwork”. *Proceedings of the American Philosophical Society* 127: 278–289.
- Carvalho, Henrique Augusto Dias de. 1890a. *Etnografia e História Tradicional dos Povos da Lunda*. Lisbon: Imprensa Nacional.
- Carvalho, Henrique Augusto Dias de. 1890b [1890–94]. *Descrição da viagem à mussumba do Muatiânvua*. 4 vols. Lisbon: Imprensa Nacional.
- Chaudat, Philippe and Olivier Leservoisier. 2022. “De l’apprentissage du terrain au terrain comme apprentissage”. *Émulations*, n° 39–40 [online]
- Chevalier, Sophie, Jeanette Edwards, and Sharon Macdonald (eds.). 2007. “The Anthropology of Britain: a growing discipline.” *Ethnologie française* XXXVII (2): 197–211.
- Chevalier, Sophie (ed.). 2015a. *Anthropology at the Crossroads: The View from France*. London: Sean Kingston Publishing (The Royal Anthropological Institute Country Series, 1).
- Chevalier, Sophie. 2015b. “Anthropology at Home”. In: James D. Wright (editor-in-chief), *International Encyclopedia of the Social & Behavioral Sciences*, 2nd ed., vol. 1. Oxford: Elsevier, pp. 751–757.
- Clifford, James. 1983. “On Ethnographic Authority”. In: *Representations* 2: 118–146.
- Clifford, James. 1985. “De l’ethnographie comme fiction. Conrad et Malinowski”. *Études rurales*, n° 97–98, pp. 47–67.
- Clifford, James and George E. Marcus (eds.). 1986. *Writing Culture: The Poetics and Politics of Ethnography*. Berkeley, CA: University of California Press.
- Clifford, James. 1986. “On Ethnographic Allegory”. In: *Writing Culture: The Poetics and Politics of Ethnography*. Berkeley: University of California Press, pp. 98–121.
- Comaroff, Jean and John L. Comaroff. 1988. “On the Founding Fathers, Fieldwork and Functionalism: A Conversation with Isaac Schapera”. *American Ethnologist* 15(3): 554–565.
- Darnell, Regna. 1998. *And Along Came Boas: Continuity and Revolution in Americanist Anthropology*. Amsterdam: John Benjamins.

- Debaene, Vincent. 2014 [2010]. *Far Afield: French Anthropology between Science and Literature*. Chicago: University of Chicago Press.
- De Heusch, Luc. 1982. *The Drunken King, or, The Origin of the State*. Bloomington, IN: Indiana University Press.
- De Laguna, Frederica. 1977. *Voyage to Greenland: A Personal Initiation into Anthropology*. New York: Norton.
- Derrida, Jacques. 1995. *Archive Fever: A Freudian Impression*. Chicago, Ill: University of Chicago Press.
- Drucker-Brown, Susan. 1985. "Participant Observation: A Social Anthropologist's Use of the Label". *Cambridge Anthropology* 10(3): 41–73.
- Efrón, David, John M. Efrón and Stuyvesant Van Veen. 1972. *Gesture, Race and Culture*. The Hague: Mouton.
- Eriksen, Thomas Hylland. 2016. *Overheating: An Anthropology of Accelerated Change*. London: Pluto.
- Eriksen, Thomas Hylland. 2018. *Cooling down the overheated Anthropocene: Lessons from anthropology and cultural history*. 2018 Gutorm Gjessing lecture, Museum of Cultural History, University of Oslo. Working Paper No. 1.
- Eriksen, Thomas Hylland. 2019. "Afterword: Genealogies of Social Anthropology". *Zeitschrift für Ethnologie* 144(1-2): 199–209. Special issue: "One Hundred and Fifty Years of the Zeitschrift für Ethnologie – a Look Back and Ahead", edited by Peter Finke. Berlin: Dietrich Reimer Verlag, 2020.
- Eriksen, Thomas Hylland. 2020. "Climate Change." *Cambridge Encyclopedia of Anthropology*, <https://www.anthroencyclopedia.com/entry/climate-change>
- Evans, Julie, Patricia Grimshaw, and Ann Standish. 2003. "Caring for Country: Yuwalaraay Women and Attachments to Land on an Australian Colonial Frontier". *Journal of Women's History* 14 (4): 15–37. Bloomington: Indiana University Press.
- Evans-Pritchard, Edward E. 1969 [1940]. *The Nuer. A Description of the Modes of Livelihood and Political Institutions of a Nilotic People*. New York, Oxford: Oxford University Press.
- Fabre, Daniel and Christine Laurière (eds.) 2018. *Arnold Van Gennep, du folklore à l'ethnographie*. Paris: Éditions du CTHS (Le regard de l'ethnologue series).
- Firth, Raymond. 1929. *Primitive Economics of the New Zealand Maori*. London: George Routledge.
- Firth, Raymond. 1951. "Contemporary British Social Anthropology". *American Anthropologist*

53(4): 474–489.

Firth, Raymond. 1957. “Introduction: Malinowski as Scientist and as Man”. In: Raymond Firth (ed.) *Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*. London: Routledge & Kegan Paul, pp. 1–14.

Firth, Raymond (ed.). 1957. *Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*. London: Routledge & Kegan Paul.

Firth, Raymond. 1990. “Encounters with Tikopia over Sixty Years”. *Oceania* 60(4): 241–249.

Fortes, Meyer. 1957. “Malinowski and the Study of Kinship”. In Raymond Firth (ed.) *Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*. London: Routledge & Kegan Paul, 157–188.

Fournier, Marcel. 2015 [1994]. *Marcel Mauss: A Biography*. Princeton: Princeton University Press.

Frazer, James George. 1890. *The Golden Bough: A Study in Comparative Religion*. London: Macmillan & Co.

Frazer, James George. 1922. “Preface”. In: Bronislaw Malinowski, *Argonauts of the Western Pacific*. London: George Routledge & Sons, pp. vii–xiv.

Frazer, James George. 2002 [1906–15]. *The Golden Bough: Abridged Edition*. Garden City, NY: Dover.

Geertz, Clifford. 1980. “Blurred Genres: The Reconfiguration of Social Thought”. *The American Scholar* 49(2): 165–179.

Gellner, Ernest. 1973. *Cause and Meaning in the Social Sciences*, London: Routledge.

Gellner, Ernest. 1981. *Muslim Society*. Cambridge: Cambridge University Press.

Gellner, Ernest. 1995. *Anthropology and Politics: Revolutions in the Sacred Grove*. Oxford: Blackwell.

Glusker, Susannah Joel. 1998. *Anita Brenner, A Mind of her Own*. Austin: University of Texas Press.

Goldman, Irving. 1963. *The Cubeo Indians of the Northwest Amazon*. Urbana, IL: University of Illinois Press.

Goody, Jack. 1995. *The Expansive Moment: The Rise of Social Anthropology in Britain and Africa, 1918-1970*. Cambridge: Cambridge University Press.

Grillo, Ralph. 1994. “The Application of Anthropology in Britain, 1983-1993”. In Chris Hann (ed.) *When History Accelerates: Essays on Rapid Social Change, Complexity and Creativity*. London:

Athlone Press.

Hallowell, A. Irving. 1965. "The History of Anthropology as an Anthropological Problem". *Journal of the History of the Behavioral Sciences* 1(1): 24–38.

Hann, Chris. 2012. "Universalismus hinterfragen, Eigentumsbegriffe hinterfragen: der Wirtschaftsethnologie im Leipziger Raum". *Comparativ – Zeitschrift für Globalgeschichte und vergleichende Gesellschaftsforschung* 22(2): 126–136.

Harrison, Ira E. and Faye Venetia Harrison. 1999. *African-American Pioneers in Anthropology* Urbana, Ill: University of Illinois Press.

Hartley, Leslie Poles. 1998 [1953]. *The Go-Between*. London: Penguin in assoc. with Hamish Hamilton.

Heintze, Beatrix. 1990. "In Pursuit of a Chameleon: Early Ethnographic Photography from Angola in Context". *History in Africa* 17: 131–156.

Heintze, Beatrix. 2011. "A Rare Insight into African Aspects of Angolan History: Henrique Dias de Carvalho's Records of his Lunda Expedition, 1880–1884". *Portuguese Studies Review* 19(1–2): 93–113.

Hogbin, Herbert Ian. 1934. *Law and Order in Polynesia: A Study of Primitive Legal Institutions*. London: Christophers.

Huffman, Ray. 1931. *Nuer Customs and Folklore*. London: Oxford University Press,

International Institute of African Languages and Cultures.

Hutnyk, John. 1998. "Clifford's Ethnographica". *Critique of Anthropology* 18(4): 339–378.

Hymes, Dell H. (ed.). 1972. *Reinventing Anthropology*. New York: Pantheon Books.

Jackson, H. C.[Henry Cecil], 1923. "The Nuer of the Upper Nile Province", *Sudan Notes and Records*, 6(1): 59-107; 6(2): 123-89 [also published as *The Nuer of the Upper Nile Province*, Karthoum, El Hadara Printing Press, 1923].

Jarvie, Ian C. 1964. *The Revolution in Anthropology*. London: Routledge & Kegan Paul.

Jones, Gwilym I. 1974. "Social Anthropology in Nigeria during the Colonial Period". *Africa: Journal of the International African Institute* 44(3): 280–89.

Kaberry, Phyllis. 1957. "Malinowski's Contribution to Field-work Methods and the Writing of Ethnography". In Raymond Firth (ed.) *Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*. London: Routledge & Kegan Paul, pp. 71–91.

Kenny, Anna. 2013. *The Aranda's Pepa: An Introduction to Carl Strehlow's Masterpiece Die Aranda- und Loritja-Stämme in Zentral-Australien (1907–1920)*. Acton [Canberra]: ANU

Press.

Kluckhohn, Clyde & Olaf Prufer. 1959. "Influences during the Formative Years". In Walter Goldschmidt (ed.) *The Anthropology of Franz Boas*. American Anthropological Association Memoir, no. 89, pp. 4–28.

Koch-Grünberg, Theodor. 1909-1910. *Zwei Jahre unter den Indianern. Reisen in Nordwest-Brasilien 1903/1905*. 2 vols. Berlin: Wasmuth.

Kroeber, Alfred Louis. 1943. "Franz Boas: The Man". In *Franz Boas, 1858–1942*. Memoir of the American Anthropological Association 61, supplement to *American Anthropologist* 2: 308–36.

Kraus, Michael. 2022. "Developing Fieldwork in the South American Lowlands: Debates and Practices in the Work of German Ethnographers (1884-1928)". In: Rosa, Frederico Delgado and Han F. Vermeulen (eds.) *Ethnographers Before Malinowski: Pioneers of Anthropological Fieldwork, 1870-1922*. New York/ Oxford: Berghahn, 411–448.

Kubica, Grażyna. 2020. *Maria Czaplicka: Gender, Shamanism, Race*. Lincoln, NE: University of Nebraska Press.

Kuklick, Henrika. 1991. *The Savage Within: The Social History of British Anthropology, 1885–1945*. Cambridge: Cambridge University Press.

Kuper, Adam. 1973. *Anthropologists and Anthropology: The British School, 1922-1972*. London: Allen Lane/New York: Pica Press. (Reprint London: Penguin Books, 1975). 2nd revised and expanded edition London/New York: Routledge, 1983.

Kuper, Adam. 1980. "The Man in the Study and the Man in the Field: Ethnography, Theory and Comparison in Social Anthropology". *European Journal of Sociology* 21(1): 14–39.

Kuper, Adam. 1996. *Anthropology and Anthropologists: The Modern British School*. 3rd revised and enlarged ed. London/New York: Routledge.

Kuper, Adam. 2014. *Anthropology and Anthropologists: The British School in the Twentieth Century*. 4th revised and enlarged ed. London/New York: Routledge.

Landes, Ruth. 2005. *The City of Women*. Albuquerque: University of New Mexico Press.

Larson, Frances. 2021. *Undreamed Shores: Hidden Heroines of British Anthropology*. London: Granta Books.

Laurière, Christine. 2019. "Le jeune musée de l'Homme à l'épreuve de la guerre et de l'Occupation (1938-1949)", in *BEROSE International Encyclopaedia of the Histories of Anthropology*, Paris. URL BEROSE : [article1681.html](https://berose.org/article1681.html)

Laurière, Christine. 2021. "L'ethnographie pour raison de vivre : un portrait d'Arnold Van Gennep", in *BEROSE International Encyclopaedia of the Histories of Anthropology*,

Paris. URL BEROSE: [article1899.html](#)

Leach, Edmund R. 1957. "The Epistemological Background to Malinowski's Empiricism". In: Raymond Firth (ed.) *Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*. London: Routledge & Kegan Paul, pp. 119–138.

L'Estoile, Benoît de. 2017. *Le goût des autres. De l'exposition coloniales aux Arts premiers*. Paris: Flammarion.

Lewis, Herbert S. 2020. "Who's Who in the Age of Boas: The Sponsors of *Anthropological Papers Written in Honor of Franz Boas (1906)*", in *BEROSE International Encyclopaedia of the Histories of Anthropology*, Paris. URL: [article2087.html](#)

Lewis, Herbert S. 2022. "Adapt Fully to Their Customs": Franz Boas as an Ethnographer among the Inuit of Baffinland (1883–84) and His Monograph *The Central Eskimo* (1888). In Rosa, Frederico Delgado, and Han F. Vermeulen (eds.). *Ethnographers Before Malinowski: Pioneers of Anthropological Fieldwork 1870-1922*. New York: Berghahn Books.

Lowie, Robert H. 1947. "Franz Boas, Biographical Memoirs". *National Academy of Science*, vol. 25, 303–322.

Lowie, Robert H. 1956. "Reminiscences of Anthropological Currents in America Half a Century Ago". *American Anthropologist* 58(6), new series, pp. 995–1016.

Lyons, Andrew P., and Harriet D. Lyons. 2004. *Irregular Connections: A History of Anthropology and Sexuality*. Lincoln, NE: University of Nebraska Press.

Macfarlane, Alan, Jack Goody, Jean La Fontaine, and Frank Kermode. 2021. *Creative Lives and Works: Raymond Firth, Audrey Richards, Lucy Mair, Meyer Fortes and Edmund Leach*. London: Routledge.

Malinowski, Bronisław. 1916. "Baloma: The Spirits of the Dead in the Trobriand Islands". *Journal of the Royal Anthropological Institute* 46: 353–430.

Malinowski, Bronisław. 1922. *Argonauts of the Western Pacific: An Account of Native Enterprise and Adventure in the Archipelagoes of Melanesian New Guinea*. With a Preface by Sir James George Frazer. London: George Routledge & Sons.

Malinowski, Bronisław. 1972 [1922]. *Argonauts of the Western Pacific*. 8th impression. London: Routledge & Kegan Paul.

Malinowski, Bronisław. 2010 [1922]. "The Subject, Method, and Scope of this Inquiry" [*Argonauts of the Western Pacific*, pp. 1–25] In: Paul A. Erickson (ed.) *Readings for a History of Anthropological Theory*. 3rd ed. Ontario: University of Toronto Press, pp. 202–217.

Malinowski, Bronisław. 1923. [Review of James George Frazer's] "The Golden Bough: a Study in Magic and Religion". *Nature* 111: 658–662.

- Malinowski, Bronisław. 1926. *Crime and Custom in Savage Society*. London: Kegan Paul, Trench, Trübner and Co.
- Malinowski, Bronisław. 1929. *The Sexual Life of Savages in North-western Melanesia*. New York: Halcyon House.
- Malinowski, Bronisław. 1932 [1929]. *The Sexual Life of Savages in North-Western Melanesia*. 3rd ed., with a special foreword, pp. xix–xliv. Preface by Havelock Ellis. London: Routledge.
- Malinowski, Bronisław. 1948. *Magic, Science and Religion*. Glencoe, Ill.: The Free Press.
- Malinowski, Bronisław. 1988 [1967] *A Diary in the Strict Sense of the Term*. Preface by Valetta Malinowska. Introduction by Raymond Firth (editor). Transl. [from the Polish] by Norbert Guterman. London: Athlone Press. London: Routledge & Kegan Paul. Reprinted Stanford, CA: Stanford University Press, 1988.
- Mancuso, Alessandro. 2021. “Before and After Science: Radcliffe-Brown, British Social Anthropology, and the Relationship between Field Research, Ethnography, and Theory”. In Vincenzo Matera and Angela Biscaldi (eds.) *Ethnography: A Theoretically Oriented Practice*. London: Palgrave Macmillan, pp. 51–80.
- Manganaro, Marc. 2002. *Culture, 1922: The Emergence of a Concept*. Princeton, NJ: Princeton University Press.
- Marett, Robert R. 1927. *The Diffusion of Culture*. Cambridge: Cambridge University Press.
- Mauss, Marcel, with Henri Beuchat. 1979 [1906]. *Seasonal Variations of the Eskimo*. Translated by James J. Fox. London: Routledge & Kegan Paul.
- Mauss, Marcel. 1952 [1925]. *The Gift*. Translation of *Essai sur le don*. London: Cohen and West.
- Mauss, Marcel. 2016 [1925], *The Gift: Expanded Edition*, ed. Jane I. Guyer. Chicago, University of Chicago Press.
- Mauss, Marcel. 2007 [1947], *The Manual of Ethnography*. Oxford: Berghahn Books.
- Mauss, Marcel. 1997, *Écrits politiques*, edited by Marcel Fournier. Paris: Fayard.
- Mills, David. 2005. “Anthropology at the End of Empire: The Rise and Fall of the Colonial Social Sciences Research Council, 1944-1962”. In: Benoît de L’Estoile, Federico Neiburg and Lygia Maria Sigaud (eds.) *Empires, Nations, and Natives: Anthropology and State-Making*. Durham, NC: Duke University Press.
- Mills, David. 2008. *Difficult Folk: A Political History of Social Anthropology*. Oxford: Berghahn Books.
- Mills, David. 2017. “Anthropology”. In: Mark Bevir (ed.) *Modernism and the Social Sciences: Anglo-American Exchanges, c.1918-1980*. Cambridge/New York: Cambridge University Press.

Mooney, James. 1896. "The Ghost-Dance Religion and the Sioux Outbreak of 1890". In *Fourteenth Annual Report of the Bureau of American Ethnology 1892-'93: Part 2*, 641–1110. Washington, DC: Government Printing Office.

Moore, Lucy. 2022. *In Search of Us: Adventures in Anthropology*. London: Atlantic Books.

Muir, Marcie. 1982. *My Bush Book: K. Langloh Parker's 1890s Story of Outback Station Life, with Background and Biography*. Adelaide: Rigby.

Muir, Marcie. 1990. "Stow, Catherine Eliza (Katie) (1856–1940)". *Australian Dictionary of Biography*, National Centre of Biography, Australian National University. Retrieved 29 July 2022 from <http://adb.anu.edu.au/biography/stow-catherine-eliza-katie-8691/text15205>.

Münzel, Mark. 1999. "Frobenius kennen wir nun so ziemlich. Über das Unabgeschlossene in der Ethnologie". *Paideuma* 45: 9–29.

Murdock, George Peter. 1951. "British Social Anthropology". *American Anthropologist* 53(4): 465–473.

National Archives. 2016. 'Archives Principles and Practices: An Introduction to Archives for Non-archivists.' Online resource.

<https://cdn.nationalarchives.gov.uk/documents/archives/archive-principles-and-practice-an-introduction-to-archives-for-non-archivists.pdf>

<https://cdn.nationalarchives.gov.uk...>

Pandian, Anand. 2019. *A Possible Anthropology: Methods for Uneasy Times*. Durham, NC: Duke University Press.

Parezo, Nancy J. 1985. "Cushing as Part of the Team: The Collecting Activities of the Smithsonian Institution". *American Ethnologist* 12(4), pp. 763–774.

Parker, K. Langloh. 1896. *Australian Legendary Tales: Folk-lore of the Noongahburrahs as Told to the Piccaninnies*. London: David Nutt/Melbourne: Melville, Mullen and Slade.

Parker, K. Langloh. 1898. *More Australian Legendary Tales*. London: David Nutt/Melbourne: Melville, Mullen & Slade.

Parker, K. Langloh. 1905. *The Euahlayi Tribe: A Study of Aboriginal Life in Australia*. London: Archibald Constable.

Parker, K. Langloh. 1982. "My Bush Book, Based on the Notebooks of an Old-Time Squatter's Wife, 1879–1901". In M. Muir, *My Bush Book: K. Langloh Parker's 1890s Story of Outback Station Life, with Background and Biography Part Two*, 45–141. Adelaide: Rigby.

Parker, K. Langloh. 1998 [1896]. *Australian Legendary Tales*. Middlesex: Senate, Tiger Books International.

- Parker, K. Langloh. 2018 [1905]. *The Euahlayi Tribe: A Study of Aboriginal Life in Australia*. Indianapolis: Alpha Editions.
- Payne, Harry C. 1981. "Malinowski's Style". *Proceedings of the American Philosophical Society* 125(6): 416-440.
- Pietsch, Tamson. 2013. *Empire of Scholars: Universities, Networks and the British Academic World, 1850-1939*. Manchester: Manchester University Press.
- Pouillon, François. 2018. 'En Algérie : le détour exotique d'Arnold Van Gennep.' In : Daniel Fabre and Christine Laurière (eds.) *Arnold Van Gennep. Du folklore à l'ethnographie*. Paris: CTHS, pp. 133-166.
- Preuss, Konrad Theodor. 1912. *Die Nayarit-Expedition: Textaufnahmen und Beobachtungen unter mexikanischen Indianern. Erster Band. Die Religion der Cora-Indianer*. Leipzig: B. G. Teubner.
- Preuss, Konrad Theodor. 1926. *Glauben und Mystik im Schatten des höchsten Wesens*. Leipzig: C. L. Hirschfeld.
- Radcliffe-Brown, Alfred Reginald [Brown, A. R.]. 1922. *The Andaman Islanders: A Study in Social Anthropology*. Cambridge: Cambridge University Press. 2nd ed. 1933.
- Radcliffe-Brown, Alfred Reginald. 1958 [1931]. "The Present Position of Anthropological Studies". In: M. N. Srinivas (ed.) *Method in Social Anthropology: Selected Essays by A. R. Radcliffe-Brown*. Preface by Fred Eggan. Chicago: University of Chicago Press, pp. 42-95.
- Radcliffe-Brown, Alfred Reginald. 1940. "On Social Structure". *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 70: 1-22. Reprinted in 1952, and in Adam Kuper (ed.) *The Social Anthropology of Radcliffe-Brown*, London 1977: 25-41.
- Radcliffe-Brown, Alfred Reginald. 1952. *Structure and Function in Primitive Society: Essays and Addresses*. Foreword by E. E. Evans-Pritchard and Fred Eggan. London: Cohen & West/Glencoe, Ill.: Free Press.
- Reichard, Gladys. 1971. *Dezba, Woman of the Desert*. Glorieta, New Mexico: Rio Grande Press.
- Reichel Dolmatoff, Gerardo. 1974. *Amazonian Cosmos*. Chicago, IL: University of Chicago Press.
- Richards, Audrey. 1932. *Hunger and Work in a Savage Tribe: A Functional Study of Nutrition among the Southern Bantu*. London: Kegan Paul.
- Richards, Audrey I. 1957. "The Concept of Culture in Malinowski's Work". In Raymond Firth (ed.) *Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*. London: Routledge & Kegan Paul, pp. 15-31.
- Rivers, William Halse Rivers. 1906 *The Todas*. London: Macmillan and Co.

- Rivers, William Halse Rivers. 1913. "Report on Anthropological Research Outside America". In *Reports on the Present Condition and Future Needs of the Science of Anthropology*, presented by W. H. Rivers, A. E. Jenks & S. G. Morley. Washington, DC: The Carnegie Institution of Washington.
- Rivers, William Halse Rivers. 1914. *The History of Melanesian Society*. Percy Sladen Trust Expedition to Melanesia. 2 vols. Cambridge: University Press.
- Rosa, Frederico Delgado and Han F. Vermeulen (eds.). 2022. *Ethnographers Before Malinowski: Pioneers of Anthropological Fieldwork, 1870-1922*. Foreword by Thomas Hylland Eriksen. New York/Oxford: Berghahn Books (EASA Series 44).
- Rosa, Frederico Delgado and Han F. Vermeulen. [Forthcoming]. "Online Interactive Archive: Ethnographic Monographs Before *Argonauts of the Western Pacific* (1870-1922)", *HAR History of Anthropology Review*.
- Satia, Priya. 2020. *Time's Monster: How History Makes History*. Cambridge, MA: Harvard University Press.
- Schapera, Isaac. 1930. *The Khoisan Peoples of South Africa: Bushmen and Hottentots*. London: Routledge.
- Shankland, David (ed.) 2014. *Westermarck*. Herefordshire: Sean Kingston Publishing. Royal Anthropological Institute Occasional Paper 44, in association with the Anglo-Finnish Society.
- Shankland, David. 2019. "Social Anthropology and its History". *Zeitschrift für Ethnologie* 144: 51-76.
- Shankland, David. 2022. "Edward Westermarck, a Master Ethnographer, and his Monograph *Ritual and Belief in Morocco* (1926)". In: Frederico Delgado Rosa and Han F. Vermeulen (eds.) *Ethnographers Before Malinowski: Pioneers of Anthropological Fieldwork, 1870-1922*. New York/Oxford: Berghahn Books, pp. 117-149.
- Simpson, Audra. 2014. *Mohawk Interruptus: Political Life across the Borders of Settler States*. Durham, NC: Duke University Press.
- Skalník, Peter. 1995. "Bronislaw Kasper Malinowski and Stanislaw Ignacy Witkiewicz: Science versus Art in the Conceptualization of Culture". In Han F. Vermeulen and Arturo Alvarez Roldán (eds.) *Fieldwork and Footnotes: Studies in the History of European Anthropology*. London/New York: Routledge (EASA Series), pp. 129-142.
- Slobodin, Richard. 1997. *W. H. R. Rivers: Pioneer Anthropologist, Psychiatrist of "The Ghost Road"*. 2nd revised ed. Stroud: Sutton Publishing.
- Slotkin, James Sydney (ed.). 1965. *Readings in Early Anthropology*. Chicago, IL: Aldine Publishing/New York: Wenner-Gren Foundation for Anthropological Research.

- Smith, Linda Tuhiwai. 2012 [1999]. *Decolonizing Methodologies. Research and Indigenous Peoples*, London, New York, Zed Books.
- Spencer, Baldwin, and Francis James Gillen. 1899. *The Native Tribes of Central Australia*. London: Macmillan and Co.
- Spencer, Jonathan. 2000. "British Social Anthropology: A Retrospective". *Annual Review of Anthropology* 29(1): 1–24.
- Steinen, Karl von den. 1897. *Unter den Naturvölkern Zentral-Brasiliens: Reiseschilderung und Ergebnisse der Zweiten Schingú-Expedition 1887–1888; Zweite Auflage als Volksausgabe*. Berlin: Dietrich Reimer.
- Steinmetz, George. 2013. "A Child of the Empire: British Sociology and Colonialism, 1940s-1960s". *Journal of the History of the Behavioral Sciences* 49(4): 353–378.
- Stocking, George W., Jr. 1966. "Franz Boas and the Culture Concept in Historical Perspective". *American Anthropologist* 68(4): 867–82.
- Stocking, George W., Jr. 1995. *After Tylor: British Social Anthropology, 1888–1951*. Madison, WI: University of Wisconsin Press.
- Streck, Bernhard. 1997. *Fröhliche Wissenschaft Ethnologie: Eine Führung*. Wuppertal: Edition Trickster/Peter Hammer Verlag.
- Strehlow, Carl Friedrich Theodor. 1907–1920. *Die Aranda- und Loritja-Stämme in Zentral-Australien* [The Arunta and Luritja tribes of Central Australia]. 5 vols. in 7 parts. Frankfurt am Main: Joseph Baer & Co.
- Turner, Victor (trans.), and Henrique de Carvalho. 1955. "A Lunda Love Story and Its Consequences: Selected Texts from Traditions Collected by Henrique Dias de Carvalho at the Court of the Mwantianvwa in 1887". *Rhodes-Livingstone Journal* 19: 1–26.
- Urry, James. 1973. "'Notes and Queries on Anthropology' and the Development of Field Methods in British Anthropology, 1870–1920". *Proceedings of the Royal Anthropological Institute of Great Britain and Ireland for 1972*: 45–57.
- Urry, James. 1984. "A History of Field Methods". In: Roy F. Ellen (ed.) *Ethnographic Research: A Guide to General Conduct* (with a foreword by Sir Raymond Firth). London: Academic Press, pp. 35–61.
- Urry, James. 1985. "Ethnographic Fieldwork: A Brief History of the Central Anthropological Initiation Rite". *Sites: A Journal for Radical Perspectives in Culture* 11: 6–12.
- Urry, James. 1993. "Radcliffe-Brown's 'Pronunciamentos' on Anthropology and his Invention of British 'Social' Anthropology, 1913–1944". In: James Urry, *Before Social Anthropology: Essays on the History of British Anthropology*. Chur/Reading: Harwood Academic Publishers (Studies

in *Anthropology and History* 6), pp. 120–138.

Urry, James. 2006. “The Ethnographicisation of Anglo-American Anthropology: Causes and Consequences”. *Sites: A Journal of Social Anthropology & Cultural Studies* (n.s.) 3(2): 3–39. Available online at <https://sites.otago.ac.nz/Sites/article/view/14>

Van Gennep, Arnold. 1995 (1922). *Traité comparatif des nationalités*. Paris: Éditions du CTHS.

Van Gennep, Arnold. 1998-1999. *Le folklore français*, édité par Francis Lacassin. 4 vols. Paris: Robert Laffont (coll. “Bouquins”). vol. 1, p. 55.

Vermeulen, Han F. 2015. *Before Boas: The Genesis of Ethnography and Ethnology in the German Enlightenment*. Lincoln and London, NE: University of Nebraska Press (Critical Studies in the History of Anthropology). Paperback edition 2018.

Vermeulen, Han F. 2016. “The Origins of Ethnography.” Weblog, posted by the University of Nebraska Press in Guest blogs:

<https://unpblog.com/2016/10/18/from-the-desk-of-han-vermeulen-the-origins-of-ethnography/>

Vermeulen, Han F. 2019a. “Gerhard Friedrich Müller et la genèse de l’ethnographie en Sibérie”. In *BEROSE International Encyclopaedia of the Histories of Anthropology*, Paris. URL: [article1686.html](https://berose.org/article1686.html)

Vermeulen, Han F. 2019b. “Ethnographie, Ethnologie und Anthropologie im 18. und 19. Jahrhundert: Einheit, Vielfalt und Zusammenhang”. *Mitteilungen der Berliner Gesellschaft für Anthropologie, Ethnologie und Urgeschichte* 40: 91–117.

Warren, Louis S. 2017. *God’s Red Son: The Ghost Dance Religion and the Making of Modern America*. New York: Basic Books.

Westermarck, Edward. 1891. *The History of Human Marriage*. London: Macmillan and Co.

Westermarck, Edward. 1906–1908. *The Origin and Development of the Moral Ideas*. 2 vols. London: Macmillan.

Westermarck, Edward. 1926. *Ritual and Belief in Morocco*. 2 vols. London: Macmillan.

Westermarck, Edward. 1932. *Ethical Relativity*. London: Kegan Paul, Trench, Trubner.

Westermarck, Edward. 1939. *Christianity and Morals*. London: Kegan Paul, Trench, Trubner & Co.

Young, Michael W. (ed.). 1979. *The Ethnography of Malinowski: The Trobriand Islands 1915-18*. London/New York: Routledge & Kegan Paul.

Young, Michael W. 2004. *Malinowski: Odyssey of an Anthropologist, 1884–1920*. New Haven, CT: Yale University Press.

Young, Michael W. 2013. "Malinowski, Bronislaw (1884-1942)". In: R. Jon McGee and Richard L. Warms (eds.) *Theory in Social and Cultural Anthropology: An Encyclopedia*. Los Angeles, CA: Sage Publications, vol. 2, pp. 506–511.

Young, Michael W. 2018. "Le Jason de l'anthropologie: vie, oeuvre et legs de Bronislaw Malinowski". In *BEROSE International Encyclopaedia of the Histories of Anthropology*, Paris. URL: [article1228.html](https://berose.org/article1228.html)

Zumwalt, Rosemary Lévy. 2019. *Franz Boas: The Emergence of the Anthropologist*. Lincoln, NE: University of Nebraska Press.

Zumwalt, Rosemary Lévy. 2022. *Franz Boas: Shaping Anthropology and Working for Social Justice*. Lincoln, NE: University of Nebraska Press.

Exercise in Setting the Canon: Twelve Classic Ethnographic Accounts Between 1870 and 1922

(Combined proposal by the editors of the present special issue)

1. Boas, Franz. 1888. "The Central Eskimo". In *Sixth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution 1884–1885*, 399–669. Washington, DC: Government Printing Office.
2. Carvalho, Henrique de. 1890. *Etnografia e história tradicional dos povos da Lunda* [Ethnography and traditional history of the Lunda peoples]. Lisbon: Imprensa Nacional.
3. Snouck Hurgronje, Christiaan. 1893–1894. *De Atjèhers* [The Achenese]. 2 vols. Batavia: Landsdrukkerij/Leiden: E. J. Brill. English translation of vol. 1, Leiden 1906. Indonesian translation, 1985.
4. Mooney, James. 1896. "The Ghost-Dance Religion and the Sioux Outbreak of 1890". In *Fourteenth Annual Report of the Bureau of American Ethnology 1892–1893: Part 2*, 641–1110. Washington, DC: Government Printing Office.
5. Best, Elsdon. 1897. *Waikare-moana: The Sea of the Rippling Waters*. Wellington: Government Printer.
6. Langloh Parker, Katie. 1905. *The Euahlayi Tribe: A Study of Aboriginal Life in Australia*. Introduction by Andrew Lang. London: Archibald Constable.
7. Rivers, William Halse Rivers. 1906. *The Todas*. London: Macmillan and Co.
8. Fletcher, Alice Cunningham, and Francis La Flesche. 1911. "The Omaha Tribe". In *Twenty-Seventh Annual Report of the Bureau of American Ethnology 1905–1906*, 17–672. Washington, DC: Smithsonian Institution; U.S. Government Printing Office.
9. Piłsudski, Bronisław. 1912. *Materials for the Study of the Ainu Language and Folklore*. Edited by

Jan Michał Rozwadowski. Cracow: Imperial Academy of Sciences.

10. Walker, James R. 1917. *The Sun Dance and Other Ceremonies of the Oglala Division of the Teton Dakota*. New York: American Museum of Natural History.

11. Rattray, Robert Sutherland. 1923. *Ashanti*. Oxford: Clarendon Press.

12. Westermarck, Edward. 1926. *Ritual and Belief in Morocco*. 2 vols. London: Macmillan.

[The last two books resulted from ethnographic fieldwork carried out before 1922]

Here is where the reader takes over!

[1] <https://www.therai.org.uk/events-calendar/eventdetail/792/-/before-and-after-malinowski-alternative-views-on-the-history-of-anthropology>

[2] Frederico Delgado Rosa and Han F. Vermeulen (eds.) *Ethnographers Before Malinowski: Pioneers of Anthropological Fieldwork, 1870-1922*. Foreword by Thomas Hylland Eriksen. New York/Oxford: Berghahn Books (EASA Series 44), June 2022. xviii + 522 pp. 29 ill. <https://www.berghahnbooks.com/title/RosaOther>

[3] According to the RAI, the countries in order of the representing visitors were: India, United Kingdom, USA, Greece, Germany, Poland, Romania, Bangladesh, Italy, Hungary, Mexico, Portugal, France, Philippines, Brazil, Canada, Japan, Russia, Serbia, Austria, Norway, Belgium, Bulgaria, Colombia, Denmark, Netherlands, Ukraine, Vietnam, Australia, Czech Republic, South Africa, Taiwan, Türkiye, Argentina, Armenia, Finland, Georgia, Indonesia, Ireland, Peru, China, Malaysia, Nepal, Pakistan, Singapore, Slovenia, Sweden, Thailand, Albania, Belarus, Bolivia, Bosnia and Herzegovina, Chile, Croatia, Cyprus, Egypt, El Salvador, Estonia, Ethiopia, Republic of Korea, Lebanon, Lithuania, Luxembourg, Malta, Moldova, Mongolia, Slovakia, Spain, and Sri Lanka.

[4] On the empathic politics and ethics of Mooney's ethnography, see Warren (2017: 297–363).

[5] This paper is a summary of the chapter from the edited volume *Ethnographers Before Malinowski* (2022).

[6] This line is reminiscent of Malinowski's outrageous but ironic title, *Sexual Life of Savages*, the point of which was to show how strict and punctilious the rules for sex were in the Trobriand Islands.

[7] Introductory note: Aboriginal Australians are advised that this article contains the names of deceased people. As a white Anglo-Australian woman, I underline my respect for the Yuwaalaraay people, past and present. This paper is a summary of the chapter from the edited volume *Ethnographers Before Malinowski* (2022).

[8] Papers of Marcie Muir relating to Katie Langloh Parker, approximately 1838–2000. Canberra: National Library of Australia, MS 10182. 'Life and Times of an Australian Collector: Catherine Langloh Parker', URL:

<https://bonniewilliam.com>

[9] This paper is a summary of the chapter from the edited volume *Ethnographers Before Malinowski* (2022).

[10] This paper is a summary of the chapter from the edited volume *Ethnographers Before Malinowski* (2022).

[11] Henrique de Carvalho and Manuel Sertório de Aguiar (phot.), 1890. “Álbum da Expedição ao Muatiãnvua”, Sociedade de Geografia de Lisboa; and Biblioteca Nacional de Portugal, E.A. 95 P.

[12] This paper is a summary of the chapter from the edited volume *Ethnographers Before Malinowski* (2022).

[13] Malinowski was quoted by Brenda Z. Seligmann as having said: “Rivers is the Rider Haggard of anthropology; I shall be the Conrad” (quoted in Raymond Firth 1957: 6).

[14] See ‘Van Gennep, Arnold (1873-1957),’ 2022, Christine Laurière, (coordination), in *BEROSE International Encyclopaedia of the Histories of Anthropology*, Paris. URL: <https://www.berose.fr/rubrique811.html>

[15] For a history of collective surveys in France, see “Collective Surveys”, *Ethnographiques.org*, no. 32, 2016 [online].

[16] These collective field trips are sometimes accompanied by audio-visual training as in my own teaching, see: ‘Baie de Somme. The different uses of a territory’. This web-documentary won a prize for educational excellence from the Ministry of Higher Education, Research and Innovation in 2020. See: https://webtv.u-picardie.fr/search_result.php?query=webdoc&type=videos

[17] In general, ethnography is the subject of numerous publications in French, both methodological and reflective.

[18] M. Gienke and A. Macfarlane (2004). Interview with Audrey Richards: On Fieldwork [Video file]. <http://www.dspace.cam.ac.uk/handle/1810/713> ; Richards’ interview was recently published in Macfarlane, Goody, La Fontaine, and Kermodé (2021).

[19] David Mills has claimed that it is a “myth” that Malinowski did not teach field methods (2010: 34) although Malinowski’s assistance appears limited to attendance at seminars and help in writing research proposals for funding.

[20] Michael Young, personal communication for Evans-Pritchard’s recollections.

[21] Outline in David Shankland (2019: 68).

[22] The editor disagreed and the Americans remained.

[23] Phyllis Kaberry noted that Boas’ “work made little impact upon his British contemporaries in

anthropology because he did not actually produce detailed functional analysis". (1957: 73n) Another reason was that the Boasians were concerned with historical issues in which Malinowski and his students, with the exception of Isaac Schapera, showed little interest.

[24] A list of their theses can be found at

<https://www.lse.ac.uk/anthropology/research/mphil-and-phd-theses>

[25] In Jean Comaroff and John L. Comaroff (1988).

[26] London: George Routledge, 1929; Firth later claimed that his *Primitive Polynesian Economy*. London: Routledge, 1939, largely based on his Tikopia material, was in part "a supplement" to his *Primitive Economics of the New Zealand Maori*, based on "brief" and "fragmentary ... field trips among Maori" (p. 15). Firth's MA thesis on *The Kauri Gum Industry; Some Economic Aspect* (Auckland University, 1922) was not based on "fieldwork" in an ethnographic sense although Maori were involved in the industry; I am grateful to Katherine Pawley, archivist in Auckland University's Special Collections for information on Firth's MA thesis.

[27] See also H. I. Hogbin's *Law and Order in Polynesia* (1934). Richards' *Hunger and Work in a Savage Tribe* (1932) included material from her later fieldwork; only Powdermaker apparently did not produce a monograph from her thesis.

[28] Malinowski preferred to supervise those working on Oceania, Seligman on those working on Africa.

[29] Letter quoted in full by Adam Kuper (2014 [1973]: 18).

[30] Michael Young, personal communication; a possible indication of the structure of the volume might be seen from a lecture Malinowski gave in Melbourne, "Two Years Among Papuans ...": *The Herald*, 2 November 1918, 5.

[31] Firth discussed some of these early problems in his "Encounters with Tikopia over Sixty Years" (1990).

[32] In 1916 Malinowski considered writing a book titled "KIRIWINA: A Monograph on the Natives of the Trobriand Islands" (Young 2004: 488); his later idea was closer to *Argonauts: "Kula: A Tale of Native Enterprise and Adventure in the South Seas"*, Michael Young, personal communication.

[33] See Urry (1993).

[34] See also Mancuso (2021).

[35] Reprinted as Chapter X in his *Structure and Function in Primitive Society* (1952).

[36] On the differences between Evans-Pritchard's first field research among the Azande and his later work among the Nuer, see John W. Burton (1983).

[37] Columbia University required publication of the dissertation before awarding the PhD. When the

date of completion differs from the date of publication, the former date is given in brackets.

[38] Franz Boas to Kroeber, 19 January 1917, Boas Papers, American Philosophical Society. See Zumwalt (2022: xxx–xxxi) for a discussion of Boas’s “icy enthusiasm”.

[39] Malinowski to Benedict, 4 May 1933, Benedict Papers, Vassar College.

[40] Franz Boas to Malinowski, 8 November 1933, Boas Papers, American Philosophical Society.

[41] Malinowski to Franz Boas, 22 November 1933, Boas Papers, American Philosophical Society.

[42] Malinowski to Franz Boas, 22 November 1933; Malinowski to Franz Boas, 31 January 1933; Franz Boas to Malinowski, 21 December 1932, Boas Papers, American Philosophical Society.

[43] Klineberg, “The Reminiscences of Otto Klineberg”, Oral History Research Office, Columbia University, 1984, p. 35.

[44] Citing Brenner’s journal entry for 17 December 1925.

[45] Franz Boas to Herzog, 20 June 1930, Boas Papers, American Philosophical Society.

[46] Franz Boas to Cattell, 27 October 1932, Boas Papers, American Philosophical Society.