Combining historical and ethnographic methods:

Theoretical, methodological, and ethical implications

Edited by Sofie Steinberger,
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Cover: The Matenadaran, the world's largest repository of Armenian manuscripts, with statues of Armenian historians, Yerevan, Armenia. (Photo: Susanne Fehlings 2008)

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1. Introduction

Gerda Kuiper, Lena Rüßing, Sofie Steinberger

The interdisciplinary working group *Entanglements of Historical and Ethnographic Research (Approaches)* was founded in 2021, in the midst of the COVID-19 pandemic. The participating researchers work on diverse topics but share an interest in combining historical and ethnographic research approaches. While some group members are trained historians, others have a background in social and cultural anthropology. They all have received much of their training in German institutions, and many of them have been affiliated to the University of Cologne (some still are). The aim of the group is to facilitate interdisciplinary discussion and to share experiences on combining approaches from the two disciplines.

At a time when personal exchange at the researchers' home institutions was difficult, the group fruitfully examined theoretical, methodical, and ethical questions in monthly online meetings between 2021 and early 2023. In this Cologne Working Paper, we bring together some of the topics discussed in these meetings. The contributions are all based on earlier discussions within the group and also all have been peer-reviewed within the group. The result is a diverse collection of papers, which hopefully will be a source of inspiration for other researchers working on the interface of history and anthropology.

Linking history and anthropology: A long tradition

We readily acknowledge we are not the first historians applying ethnographic methods nor the first anthropologists visiting archives or conducting oral history interviews. There is, however, no clearly defined field combining the two disciplines. At first sight, historians and anthropologists have therefore primarily merged their approaches by borrowing from each other's methodological toolkits, in an approach labelled „ethnohistory".

Robert Carmack reviewed the developing field of „ethnohistory" in the first volume of the *Annual Review of Anthropology,* in 1972. He stated there was no overarching theoretical approach and that the definition should be methodological. „Ethnohistory is a special set of techniques and methods for studying culture through the use of written and oral traditions" (Carmack 1972: 232). Strong (2015) pin-pointed the early development of the field in the United States in the late 1940s. The attention to historical dynamics was not wholly new in anthropology: as Strong mentioned, for instance Julian Steward, Robert Redfield and Edward Evans-Pritchard already studied cultural change. What set ethnohistory apart as a developing field of study was its clear, empirical purpose: methods from history, (cultural) anthropology and archaeology were combined to reconstruct the history of colonized people, primarily in the USA. Ethnohistorians were for instance called up as expert witnesses in court cases about indigenous land claims. Although in later years the concept of ethnohistory has been applied in different contexts and has been defined in various ways, the methodological „marriage" seems to have remained the key characteristic (see also e.g. Pannabecker 1990).

Despite the lack of a shared topic, „the following subjects are the ones most often studied by ethnohistorians: specific history, historical ethnography, and folk history" (Carmack 1972: 235). The studies into „specific culture history" were studies into the origins of specific cultural traits in societies often neglected by historians, such as studies among indigenous groups in North America, as for instance was done by Franz Boas, and studies into oral traditions in African societies. Historical ethnographies had the broader aim of reconstructing past cultures or societies (Carmack 1972: 238). Finally, folk historians studied and document how societies view their own past (in a similar way as ethnobotanists study people's understanding of plants in their environment). In short, the prefix „ethno" in ethnohistory could refer to either a specific ethnic group, to an ethnic

group's understanding of its own past (similar as in ethnobiology), or also to the application of the ethnographic method in historical research (Strong 2015: 7).

Historical anthropology as a field seems to have developed in the same time period as ethnohistory, after World War II. The focus in this field is not methodological but rather about a new topic of inquiry or perhaps one could say about a new unit of analysis. Anthropologists realized they could no longer study societies as timeless and unaffected by global processes. The focus in this field is not on re-con-struction, as in ethnohistorical inquiries into specific histories or historical ethnographies (cf. Carmack 1972). Instead, it takes this process of writing and making histories as an object of study. Historical anthropologists thus take a critical distance and study history „from the margins" (Axel 2002), therewith creating space for marginalized and silenced voices in historiography.

In the field of historical research, oral history has to be mentioned when it comes to the introduction of ethnographical methods in the discipline. Starting off as a simple way of documenting contemporary opinions by ordinary people in the first half of the 20th century, after the Second World War oral history was used as a methodological approach to capture the perspectives of the „political, economic, and cultural elites" (Ritchie 2011: 4). In the 1960s and 1970s, the view emerged in both Europe and North America that the methodological approach of oral history should primarily represent the voices of those who were absent from the historical record. In particular, the work of social and cultural historians such as Paul Thompson (2000), Lutz Niethammer (1983-1985; 1985), Michael Frisch (1990), and others aimed to give a voice to those who had previously had no voice due to political, legal, and social marginalization and oppression, thus calling for a more democratic and inclusive historiography. Thereby, the direct and personal interaction with the interviewees fueled continuous debates regarding ethical aspects of the methodology, debates that are also common in anthropology (Sheftel/Zembrzycki 2016).

Historical methods, but also a deeper understanding of the workings of historiography, have thus enriched anthropologists' work. Vice versa, anthropology's attention for cross-cultural comparison and for making the „familiar strange" has broadened the scope of historians' work and deepened their understanding of certain past phenomena, such as kinship relations (Macfarlane 1988).

Strong (2015: 9-10) and Pannabecker (1990: 16) identified several common methodological challenges in conducting ethnohistory, such as the „mixing up" of different epistemologies when combining approaches that examine different time-scales (synchronic versus diachronic studies) and that use different modes of data validation. They also pointed at the tensions between historians' tendency to produce empirical, particular case studies and (some) anthropologists' wish for theoretical framing, cross-cultural comparison and synthesis. Pannebecker (1990: 16) moreover highlighted that the approach of ethnohistory requires a researchers' competencies in both historical and anthropological methods.

Such methodological challenges with combining different epistemological approaches and with feelings of insecurity when applying a method from another discipline also surfaced in the discussions in our working group. At the same time, we experienced that our projects and research processes connect to each other in various ways – also beyond the methodological. To illustrate these discussions, let us now outline our individual contributions to this publication.

The contributions

In Chapter 2, Susanne Fehlings discusses the transfer of an approach and method that was developed by historians – the concept of *histoire croisée*, introduced by Michael Werner and Bénédicte Zimmermann (2002) – to the discipline of social and cultural anthropology. Fehlings argues that the method's focus on interweaving and „intercrossing" perspectives makes it particularly relevant for anthropologists – who traditionally are experts on one specific culture, which is increasingly limiting in today's world full of encounters. She observes that Werner and Zimmermann formulated the approach of *histoire croisée* in responses to challenges that have been discussed among anthropologists as well, such as how to avoid the terminology one wishes to overcome. Fehlings (this volume) writes: „In short, and translated into the language of anthropologists, Zimmermann and Werner suggest increasing the

importance of different '(native) points of view', whereby the researcher must reflect his own positionality and impact on the object of research, and, at the same time, combine the study of structure *(longue durée*) and agency (individual scope for action)." To illustrate the theoretical discussion, Fehlings introduces examples from the Caucasus, where she has been conducting fieldwork since 2008. These examples make clear that history itself (especially national histories) can become a point of contestation. Following Werner and Zimmermann (2002), Fehlings considers *histoire croisée* – preferably used in teams of researchers – as a tool for overcoming such nation-centred histories.

In Chapter 3, Oliver Tappe, likewise aims to include multiple perspectives, but rather by drawing attention to marginalized voices *within* the society under study. Tappe emanates from the premise that it is one of the aims of anthropological research to give voice to the marginalized, the „wretched of the earth" as Frantz Fanon (1963) famously put it. Tappe investigates how we can pursue the ambitious task of reconstructing the past from a grassroots perspective beyond the reach of oral history, where we must make do with archival sources written by colonizers, missionaries, and other hegemonic actors who dominated certain discourses or silenced others. Drawing on his various research projects in and on Laos, Tappe discusses the challenges and opportunities of using the ethnographic method in the archives and of conducting „fieldwork between folders" (Ladwig et al. 2012). Tappe draws parallels to the ethnographic method of multi-sited fieldwork (Marcus 1995), where the researcher looks for overarching (but contingent) worldviews across different sites/archives, and points at the role of serendipity and chance, which are important factor in ethnographic fieldwork and in archival research alike. Is an ethnography of and/or in the past possible at all? How can one create a dialogue between the archives and the field? Tappe's reflection on fieldwork in the archives discusses the opportunities and limits of doing historical anthropology.

In Chapter 4, Lena Rüßing reflects on questions of methodology, positionality, and accountability which arose from her historical research with and about Indigenous peoples in Canada. Based on her doctoral thesis, which was affiliated to a German historical institute, she explores the issue of how a respectful engagement with Indigenous knowledges, histories, and cultures is possible in an institutional context in German-speaking countries, where there is no departmental structure that recognizes Indigenous Studies as a discipline in its own right. Rüßing analyzes the various ascriptions of positionality that she encountered in the field as a non-Indigenous, Western European historian and which affected her ability to build oral-history relationships with Indigenous people. She argues that the fact that she was partly seen as a representative of an exploitative Western scholarship intertwined with a colonial history of oppression and marginalization should not lead her to turn her focus away from the histories and cultures of Indigenous peoples (as some non-Indigenous historians have done), but rather to find methodological approaches for a respectful approach to Indigenous histories and cultures. The method she pursued was an oral history methodology that drew on ethnographic and decolonizing research methods on the one hand, and scholarly work on oral history and Indigenous research approaches on the other. Particularly, for shaping her oral history methodology, Rüßing took inspiration from the framework of „Indigenous storytelling" and its seven ethical principles, which was developed by Stó:lō scholar Jo-Ann Archibald Q'um Q'um Xiiem (2008).

In Chapter 5, Ole Münch elaborates on what we can learn from the oral history debate when working with interviews which someone else has conducted instead of ourselves. Just recently, a debate has started within German and British academia about „secondary analysis" and the „reuse" of so called „social data" (see e.g. Savage 2013: ix, 16-18; Lawrence 2019; Brückweh et al. 2022). Historians have begun to systematically approach the technical, ethical, juridical, and theoretical problems arising when they deal with such material. Münch highlights difficulties by using his own research as an example, where he heavily relies on interviews with former members of Mass Observation – a research project launched by a group of UK amateur sociologists in the late 1930s, who aimed to study their own society as „native anthropologists". The organization for some time was forgotten but sparked great interest again after the „cultural turn" in the 1980s. The oral interviews conducted then with former members of the organization have often been referred to – but have not been contextualized and re-analysed in a similar fashion as for instance social

surveys are sometimes re-analysed. In his contribution, Münch particularly focuses on an interview by Nick Stanley from 1981 with Celia Fremlin, one of the members of Mass Observation. Münch shows how Fremlin's later work and engagement in the women's movement shaped her narration of the events in the 1940s that the interview was about – but also the way in which the interviewer formulated questions. He therewith argues that when it comes to historical analysis, „reusers" and „secondary analysts" have an advantage over the original interviewers.

In Chapter 6, Sofie Steinberger discusses yet another aspect of oral history interviews, namely the choice of language. Her contribution shows convincingly that this at first sight methodological question also can hold important empirical lessons. In preparation of her fieldwork, Steinberger had learned Darija (Moroccan Arabic) in order to conduct oral history interviews with residents of the Moroccan town of Nador on the border with Spain. She had not been able to learn the local language Tarifit, but expected she would get a long way with Darija. To her surprise, her interlocutors preferred to speak Spanish with her, the language of the former colonizer. Through a closer examination of the historical context, Steinberger highlights that this preference stemmed from issues of belonging. She points out Spanish does not have a negative connotation in Nador but rather is a form of cultural capital (in the past as well as in the present, for instance with regard to negotiating border control to Spain). It also is the language associated with the memories that were discussed during the oral history interviews, which helps in recalling these memories (cf. Marian/Kausganskaya 2007). Moreover, against the background of language politics in Morocco and a history of marginalization of the region in which Nador is located, the choice for Spanish can be considered as a political act. Thus, Steinberger on the one hand discusses power hierarchies that are involved in the use of different languages in the interviews and the following transcription, translation, and publication processes; on the other hand, she sheds light on how local memory narratives and politics of belonging can influence methodological and ethical aspects of research.

The final contribution focuses on methodological questions. Gerda Kuiper reflects on her experience of having to cut off fieldwork for her research project due to the start of the COVID-19 pandemic in the spring of 2020. Kuiper had set out to study the trade in second-hand clothes on Tanzania's southern coast. She had decided to combine ethnographic and historical methods in an attempt to understand the development of the second-hand clothing trade and the intertwinement with the historical marginalization of the southern coastal region. However, Kuiper had spent only about two months in the field instead of the five months she had planned for when she had to leave due to the pandemic. After her return to Germany, she attempted to supplement the bits and pieces of data that she had been able to collect. She for instance enlisted the help of an assistant in Tanzania, conducted interviews via Skype, and carried out online research in archival databases. In her contribution, Kuiper describes this process of remote data collection, reflects on its practical, methodological, and ethical limits, and analyses what kind of potential the use of remote ethnographic and historical fieldwork methods could have in a post-pandemic, yet increasingly insecure world.

Multiplying perspectives – pushing disciplinary boundaries

The discussions in our working group showed us there were many similarities between our historical and anthropological projects, some of which we would like to highlight in this brief discussion.

Giving voice

As Rüßing makes clear in her contribution, the opportunities that can arise for historical research through the inclusion of ethnographic research methods lie in the fact that the voices of those who can only be found to a limited extent in the archival sources can be included (at least for historical research projects that deal with a time period in which people can still be interviewed because they are still alive). Both Rüßing and Tappe point out in their articles, following Ann Laura Stoler (2002, 2008, 2010) and others, that the archives in/of their research contexts are sites of colonial knowledge production. The archives are thus to be understood as sites whose sources express the colonial oppression and marginalization of indigenous voices.

In his contribution, Tappe emphasizes that although

the sources in Western European archives might follow colonial systems of suppressing indigenous voices, this process of knowledge production was not unified. Sources in these archives also represent „fragmentary histories of entanglement", which were „produced by both the colonized and the colonizers in concrete moments of encounter". In these fragmented histories of entanglement, colonial anxieties and failures come to light just as much as indigenous perspectives. In this respect, archival work also offers an epistemological value for anthropological research, provided one is prepared to accept „contingency and fragmentation" and is not afraid to make the effort to „carve out [the] indigenous perspectives from the plethora of files and documents stored in the European archives" (see Tappe in this volume).

In addition to the opportunities that unexpected, serendipitous findings in archives offer for highlighting historical indigenous voices, Tappe argues that ethnographic field research enables a better understanding of the background of the sources in the archive. By relating the historical sources to the realities of life and „lived" stories in the field, processes of silencing and marginalization can be better understood. Rüßing likewise underlines in her contribution that using ethnographic methods as a historian, going into the field so to speak, and collecting oral histories of indigenous peoples, appears to be another sensible way to counter the marginalization of indigenous voices in Western archives. Democratizing the historical record and making it more inclusive is one of the central approaches of oral history, which has a long tradition in historical science and also frequently been adopted by anthropologists. At the same time, using ethnographic methods to collect oral histories in the field can contribute to a balanced historiography that is based both on the voices that are alive in the archival sources and on voices that continue to be alive in the field.

Remoteness

The contributions by Münch and Kuiper highlight the methodological and epistemological uses of distance, in time and space. Münch's contribution shows that for analysis, distance in time might be an advantage. He argues that a researcher who re-analyses an oral history interview conducted by another researcher at an earlier point in time, might be better able than the interviewer her- or himself to put an interview into a wider historical context instead of taking the interview's content at face value.

Kuiper's contribution shows that remoteness in space must also not necessarily be an impediment to carrying out historical and anthropological research, especially not in this digital age. The inclusion of remote methods has several advantages, among others methodological (more opportunities for data triangulation and a more prominent and recognized role for local assistants) and ethical (lower carbon footprint and research methods that are less invasive for interlocutors). Her contribution underscores – as for instance Rüßing's does – that anthropological and historical methods that comprise remote elements should still base in long-term and meaningful relations between the researcher and the researched, and should retain a close eye for local context (cf. Günel et al. 2020). Nevertheless, under these conditions, remote methodological elements can be important tools in carrying out anthropological and historical research. At the same time, it must be acknowledged that important context gets lost when searching archives digitally or when conducting interviews online.

The importance of local cultural context

Although contextualization is central to historians' work and to the analysis of their research topics, the contributions by Rüßing and Steinberger highlight insights historians can gain from the anthropological attention for specific cultural context, both when it comes to methodology as well as to interpretation. First of all, Rüßing's oral history interviews provided space to oral traditions, pushing the discipline of history to acknowledge the validity of such sources, which are still often seen as only a supplement in history - in contrast to in anthropology or ethnohistory (cf. Carmack 1972). Secondly, historians can profit from using ethnographic methods when they aim to conduct research that does not appear alienated from the country and its people. Such research requires the historian to leave the site of the archive and relate the sources studied there to the country and its people and their current history, at least in contemporary historical projects in which the past projects into the present. Doing this in an ethically balanced way that opens up opportunities for building sustainable research relationships with people in the field and at the same time leaves room for continued independent interpretations and reflections,

is certainly a challenge that should not be underestimated. In addition, the strict institutional funding frameworks of national and international education policies often offer little scope for establishing long-term research relationships with the people being researched, which can have negative consequences in terms of the way research projects are methodologically implemented (see Rüßing's paper in this volume).

Steinberger's contribution shows that not only methodological but also cultural context is just as important in historical research as it is in anthropological research. In her case, not only the content of the oral history interviews she conducted, but also the choice of language by her interviewees had empirical value. Her experience of preparing for fieldwork by learning a language that was not actually used or preferred in the field resonates with experiences of numerous young and enthusiastic anthropologists, who found out their field site looked very different from what they had imagined beforehand. But Steinberger's productive use of this twist also resonates with anthropological approaches: she fruitfully uses the unexpected preference for Spanish as starting point for examining wider historical and political relations in her field site, and argues „that the use of cultural and regional symbols, or preferring Spanish over Darija, are to be understood as reactive responses towards the suppression of diversity by the Moroccan central government" (Steinberger, this volume).

Historiography and political processes of nation-building

Apart from the uses of ethnographic methods for understanding the past, Tappe argues that the other way around, histories that archival sources reveal can help to better understand present-day social dynamics, cultural contexts, processes of marginalization and power asymmetries in the society under study. Steinberger's contribution underscores this point by highlighting political projects of nation-building and belonging. With reference to for instance Blommaert (2005) and by drawing on archival sources and oral history interviews, Steinberger examines the role of language in the creation of the Moroccan nation-state and in processes of marginalization within this state. The contribution by Fehlings likewise draws attention to processes of nation-building and to the question what anthropologists can take from historians for a better understanding of present-day political relations. Her examples from the Caucasus hold important lessons for understanding the role of (re-)writing national histories in the creation of nation-states and the political structuring of feelings of belonging. The question how scholars can avoid falling into the trap of reproducing such national(ist) histories is an intriguing one. One avenue would be to observe implicit political acts of resistance, as Steinberger did. Another is, as Fehlings suggests, the approach of *histoire croissée*, which purposefully includes several perspectives and looks at crossroads rather than at nation-states' (imagined) singular paths.

Conclusion

Anthropology and history have sometimes been described as antithetical in their aims. Yet, as anthropologists and historians in conversation have found out before us, the disciplines have a lot in common. Through the discussions in our group, we found that we shared similar methodological challenges and opportunities, such as issues of language, the role of serendipity, and (temporary) lack of access to archives or field sites. But our different projects also echoed each other beyond the application of combined methods. As the papers in this volume show, we share similar concerns and interests, such as how to give voice to marginalized people, the role of historiography in contested and exclusionary processes of nation-building (and how to overcome this), and opportunities and pitfalls of a researcher's remoteness in time and space.

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2. About the *Histoire Croisée* Approach in Social and Cultural Anthropology

Susanne Fehlings

„The only thing that we learn from history is that we learn nothing from history." Hegel

Introduction

Starting in 2002, Michael Werner and Bénédicte Zimmermann published several articles about the concept of *histoire croisée*. The first of these articles (on which I will focus in this article), titled *Vergleich, Transfer, Verflechtung: Der Ansatz der Histoire croisée und die Herausforderung des Transnationalen (Comparison, transfer, entanglement: the histoire croisée approach and the challenge of the transnational)*, was published in German in the journal „Geschichte und Gesellschaft". It was followed by a French version in 2004 and an English version in 2006 (Werner/Zimmermann 2004, 2006). Since then, the euphonious term *histoire croisée* has been taken up by anthropologists, who have appropriated and adapted it, for example for the study of the history of their own discipline.[1]

With their *histoire croisée* approach Werner and Zimmermann responded to a problem in the science of history writing, which was triggered by the founding of the European Union: the consequent need for a „European history". Werner and Zimmermann concluded that in order to write this history national-historical perspectives must be overcome. They noted that globalization and the structural change of European unification should be taken into account and suggested *histoire croisée* as a scientific project through which the problem of the transnational can be considered (Werner/Zimmermann 2002: 602). At the same time this project was clearly going along with a political agenda, which was inspired by the idea of a peaceful coexistence, cooperation and exchange in Europe and across the globe.

I start this essay with a short introduction to *histoire croisée* as a concept and a methodology by comparing it to similar approaches in history and anthropological research. In the subsequent sections I examine what it means to transfer concepts, theories and methods from one discipline to the other, talking about the relationships between history and anthropology. Finally, I discuss what is new about *histoire croisée* and what its benefits are as a novel theoretical approach and methodology applied in anthropology.

The *Histoire Croisée* Approach

To outline the novelty and advantages of *histoire croisée*, Werner and Zimmermann begin their article by pointing out the shortcomings of previous approaches that similarly attempted to break down national limitations in the writing of history. Two such attempts were „historical comparison", and „transfer" or „relational history".

Werner's and Zimmermann's objections to *historical comparison* (as advocated by Lucien Febvre and Marc Bloch, for example) are like the criticism that has been brought forward in anthropology against cultural comparison. Werner and Zimmermann argue that „while the comparative method tends to focus on synchrony, inquiry into transfers is clearly situated in a diachronic perspective" (Werner/Zimmermann 2006: 35, compare Werner/Zimmermann 2002: 609). Therefore, one would have to ask why one element of society or culture is used for comparison and another is not (2002: 610). The fact that the observers' perspectives (bias), the comparison levels and the categories and entities to be compared differ would make the comparison an arbitrary undertaking (2002: 610). And indeed, as has been widely discussed in anthropology, for example in the

[1]For example, my colleagues Richard Kuba, Jean-Louis Georget and Hélène Ivanoff use the *histoire croisée* to write the sometimes contradicting and sometimes interwoven traditions of early 20th century' German and French Anthropology.

context of the ontological turn, it is even problematic to transfer certain terms and concepts from one cultural or historical context (e.g., Western) to another (e.g., non-Western) (Descola 2013; Viveiros De Castro 2012).

Werner and Zimmermann also identify some shortcomings of *transfer history, connected and shared history (Beziehungsgeschichte)* or so-called *entangled history* (as introduced by Michel Espagne and Jürgen Osterhammel). These approaches emphasize diachronic processes and change through contact and interaction – through acculturation, socialization and appropriation. They are concerned with commonalities and connections between groups, societies, countries and nations, and study historical overlaps and contact history (2002: 612-615). Such histories have been described by anthropologists since the early days of the discipline. Anthropologists experience contact with other cultures during fieldwork, every day. But they also walk in the footsteps of early explorers like Captain James Cook, whose „first contact" with the Indigenous people of the South Sea ended with him being killed under obscure circumstances (Brumann 2015; Obeyesekere 1997 [1992]; Sahlins 1995). Such stories, and the history of colonialism, have triggered debates about the role of anthropologists in contact situations, in which Western explorers represented the interests of Western powers and colonizers. Contact history is often a history of the relationship between oppressors and oppressed, as Eric Wolf (1997 [1982]) shows by pointing to the dependencies of Indigenous people within the world system. Other anthropologists like Marshall Sahlins (1999) or Scott (1977) however have shown that these oppressed and allegedly powerless Indigenous people do have their own forms of agency and rebellion.

Anthropologist Sidney Mintz (1986, 1997) showed that contact always shapes both sides involved in the encounter. He described how the discovery and consumption of sugar in the colonies changed the lives in the colonies as well as in Western (British) society. Thus, his work on sweetness and power is often mentioned as a classic of transfer history.[2] Werner and Zimmermann point to the problems inherent in this kind of transfer history: firstly, it is difficult to define the limits of processes of transfer. Where does transfer start and where does it end? (2002: 615) Consequently, it is difficult to define a research unit. Secondly, again, there is the question of the validity of the frame of reference. According to Werner and Zimmermann, the history of transfer is in a quandary, specifically in that

> „[it] takes the historical dimensions of the study of international relations seriously, but at the same time cannot escape the problem of not being able to reflect upon its own frame of reference" (Werner/Zimmermann 2002: 615; translation by the author).[3]

Their critique points out that cultural explanatory models and categories are thought to be timeless, although they are also subject to dynamics and adaptations and are reinterpreted by actors. Transfer history thereby fails to overcome national fixations from which it takes its vocabulary to describe transnational processes (2002: 616). It is marked by „a reflexivity deficit due to a lack of control over important self-referential loops" (Werner/Zimmermann 2006: 36).

Histoire croisée, by contrast, according to its advocates, attempts to escape this dilemma by turning „intercrossing" into its underlying principle and methodological approach (2002: 608, 2006: 37). According to Werner and Zimmermann it does not only examine historical processes of entanglements; it is not only about a shared or entangled history, not about, for example, mutual exchange and influence of colonizing and colonized states. It is about the intertwining of the dimensions of analysis: of

[2] But connected, shared and entangled history has implicitly been addressed in various – even early – anthropological theories: for example, within the „cultural-historical method". The cultural-historical method started from the assumption that there were a few „original cultures" whose elements had spread across the globe. Contemporary local cultures were thus understood as the product of diffusion processes of various cultural elements that had to be identified, attributed, and traced back to their origin. Representatives of this approach were Fritz Graebner (1905), who speaks of „cultural complexes" and Hermann Baumann (1934) and Leo Frobenius (1897-1898), who coined the term „Kulturkreise" (see Hahn 2019). Diffusionism, modernization theories, Marshall Sahlins' concept of the „indigenization of modernity" (1999) and Appadurai's of so-called „global flows" and „scapes" (1996), as well as attempts to explain cargo cults provided other attempts to describe and explain imbroglios, dependencies and mutual adaptations across cultures and geography.

[3] „Die Transferforschung nimmt zwar die historische Dimension der Erforschung transnationaler Beziehungen ernst, aber sie entrinnt dabei nicht immer der Gefahr, ihr eigenes Bezugssystem nicht mehr zu hinterfragen und ihre Fragestellungen somit von dem intendierten Historisierungsprozess auszunehmen".

synchrony and diachrony, symmetries and asymmetries, and of different units of analysis from different perspectives, which must be considered (2002: 618). The different perspectives on the object, as they say, in a certain sense multiply the object of study (and its/their interpretations) (2002: 618, 636). It is then how these different objects relate to each other, and how the different perspectives are co-conceived and thus create meaning, that becomes the centre of interest (2002: 619). Deductive and inductive methods are employed in a mixed manner, with the significance of the deductive part being increased (2002: 620). In short, and translated into the language of anthropologists, Zimmermann and Werner suggest increasing the importance of different „(native) points of view", whereby the researcher must reflect his own positionality and impact on the object of research, and, at the same time, combine the study of structure *(longue durée)* and agency (individual scope for action).

Zimmermann and Werner explicitly link this approach to the study of the transnational (2002: 628). The problem with the transnational, as they see it, would be that it is difficult to think it beyond the nation. Although the transnational creates new processes, spaces, and fields of action, it is ultimately nation-state determinants that define it (2002: 630). This observation is supported by my research on shuttle traders, who are involved in global trade but have to cope with state borders and national legal law regulations all the time (Fehlings 2022). Just consider what happened during the recent pandemic: national borders played a greater role in defining the respective rules about how to deal with Covid-19 than the actual transnational phenomenon – the virus itself and its spread (Billé 2020). Even *histoire croisée* cannot solve this problem. But what it can do, if we trust Werner and Zimmermann, is to direct the focus toward entanglements/intercrossings and changing conditionality (Werner/Zimmermann 2002: 630).

A History of History in Anthropology

The question I will discuss is: what has anthropology learned from history so far, and is there anything it can learn from the novel approach of *histoire croisée*?

In his Marett Lecture titled Social Anthropology: Past and Present, one of the fathers of modern anthropology, Edward Evan Evans-Pritchard, in 1950 advocated treating the subject of anthropology as one of the humanities, not as a natural science. For Evans-Pritchard the work of an anthropologist consisted of three phases, which it has in common with the work of historians. First, according to Evans-Pritchard, the anthropologist „seeks to understand the significant overt features of a culture and to translate them into terms of his own culture", then „in the second phase of his work the social anthropologist goes a step further and seeks by analysis to disclose the latent underlying form (patterns) of a society or culture"; finally, in the third phase „the anthropologist compares the social structures his analysis has revealed in a wide range of societies" (Evans-Pritchard 1950: 122). Because Evans-Pritchard agreed with Kroeber „that the fundamental characteristic of the historical method is not chronological relation of events but descriptive integration of them", he concluded „that while there are, of course, many differences between social anthropology and historiography they are differences of technique, of emphasis and of perspective, and not differences of method and aim" (1950: 122).

Indeed, anthropology and history (especially pre- and early history) share common or at least entangled origins and (hi)stories. In Germany as elsewhere this fact resonates in mixed collections including both ethnographic and archaeological objects (Hahn 2019), the naming of scientific associations (Geisenhainer 2023; Lentz/Thomas 2015), and the research history of institutions like my workplace, the Frobenius Institute (Kuba 2006; Kuba/Thubauville 2011).

The archive of the Frobenius Insitute contains around 8,600 rock-painting copies, which were replicated by professional artists (see Figure 2.1), who between 1912 and 1964 took part in the institute's archeo-ethnographic expeditions in Africa, Oceania, Australia and Europe (Frobenius Institute 1998).

Since the 19th century the purpose of such expeditions and collections has been „to show the origin and development of social institutions" (Evans-Pritchard 1950: 119) and to identify evolutionary stages. This resulted in a hunt for examples to illustrate such stages, and „vast repositories of ethnological detail were stocked and systematically ar-

Figure 2.1: Artists Katharina Marr and Elisabeth Pauli copying rock art in Wadi Nocham near El Richa in Algeria, Frobenius expedition 1934-1935. (Copyright: Frobenius Institute, photo archive, Register-Nr. FoA 12-1999)

ranged, as, to mention the largest of these storehouses, in The Golden Bough"[4] (1950: 119). The theoretical and methodological foundations of this activity have been criticized for many reasons. Within the current postcolonial debate, criticism focuses on moral issues and on ethics. Earlier, Evans-Pritchard rejected evolutionism for the same reason he rejected French structuralism and British functionalism: for its quest for „natural laws", which, he was convinced, cannot be applied to the study of societies and cultures. Still, the idea that so-called primitive societies can tell us something about our own past has not vanished. It can be detected in Marxist theory as well as in current approaches of archaeologists or behavioral scientists, especially in projects that advertise themselves as interdisciplinary.

I believe, with Evans-Pritchard, that the work of anthropologists and historians has much in common. Anthropologists, although they record the so-called present, produce – in a way – chronicles. They record oral histories, and like microhistory (microstoria), in Carlo Ginzburg's sense, talk about „the cheese and the worms" (Ginzburg 2007 [1976]). Ironically and unfortunately, by the time an ethnography gets published, ethnographic data often reflects the past, not the present. The Armenian anthropologist Levon Abrahamian once jokingly told me that it was his and his colleagues' goal to work to such an extent as „not to leave any material for historians". If historians in the future should want to write about the past, they would simply have to read ethnography. Although I doubt this is going to happen, I somehow like this idea.

But usually, historians are not interested in anthropology for data, but for theory, which they feel to be lacking in their own discipline. This tendency is reflected, for example, in the project of so-called ethnohistory. Ethnohistory was first used to identify Indigenous claims to land in the USA after World War II. It was then institutionalized as a method that combined the work of archaeologists, historians and anthropologists. This collaboration did not always go smoothly. Very similarly to what I experienced myself in on-going collaborative projects (Iwe et al.

[4] Evans-Pritchard refers here to the work by James G. Frazer (1906-1915).

2017), within the project of ethnohistory, historians criticized anthropologists for disregarding change and material culture, while anthropologists saw deficits in applying their theory „correctly" among historians. Thus, a balance had to be struck between diachrony and synchrony, „downstreaming" (from the past to the future) and „upstreaming" (from the present to the past), and between narrative writing and theoretical jargon (Chaves 2008).

Lessons from History and the Problem of Historical Truth

Many anthropologists are not satisfied with the way in which historians apply anthropological theory to historical or archaeological data. For anthropologists, linking social theory with the evidence of some artefacts in the absence of a broader context seems ridiculous. The anthropologist Sydel Silverman (1979), however, conversely complains about the practice of anthropologists, using „real history" solely as a kind of background for a synchronously approached ethnography. In that way, according to his critique, pieces of history are torn out of context and presented as examples of general structures (patterns) or as traditions or customs.

Indeed, as observed by Devons and Gluckman, there seems to prevail among anthropologists „a rule of disciplined refusal to trespass on the field of others" (1964: 162-169), and a conviction that historical facts in the sense of chronological events should be left to the experts, the historians. This conviction might derive from the heritage of the 20th century's French and British schools of Anthropology but may also have a purely pragmatic basis. History, which is not the focus of most anthropological studies, is just too complex. It is difficult to be properly grasped with anthropological methods, which (usually) do not include extensive and systematic archive work and source criticism.

Thus, as I observe, anthropologists (including myself) adopt general concepts rather than research methods from historians: Fernand Braudel's *„longue durée"* (1977, 2001), because it can be used to explain continuities (see, e.g., Moore 2018), without having to dig into details;[5] Benedict Anderson's „invented history" (1995, 1997), because it connects the past with the present (see, e.g., anthropological research on nation building) and saves us the trouble of meticulous source checking;[6] Pierre Nora's *„lieu de memoire"* (2005), because it establishes a relationship with space;[7] Maurice Halbwachs's (1985a, 1985b), Jan and Aleida Assmann's (1993, 1988, 1999) and Erich Hobsbawm's and Ranger's (Hobsbawm 1998; Hobsbawm/Ranger 1983) concepts of memory, because they can bridge the past, the present and the future by focusing on the question of identity formation, which again is linked to social, political, religious and economic practice;[8] and Lisa Malkki's (1995) notion of „mythico-history" because it removes the contradiction between in-

[5] Braudel distinguishes between different time levels: *longue, moyenne* and *courte durée*. The *longue durée* describes the slowest form of development of historical processes. It is therefore about „structures" that are preserved over a longer period of time.

[6] In *The Invention of the Nation*, Anderson (1997) describes how history and individuals are reinterpreted as national events and heroes for present purposes and visions of the future. Like Hobsbawm (1991), Anderson (1997) describes the manipulation of history and traditions as a constructed and artificially manufactured continuity in which certain events – as the basis of current nation-state legitimacy – are quite deliberately invented, emphasized, omitted or forgotten.

[7] A *„lieu de memoire"* combines different archaeological layers. Nora writes with regard to French history: „History, more precisely French national history, has always been written from the point of view of the future. Depending on the implicit, sometimes even explicit, idea of what ought to be or what the future would look like, a general memory of all that the community had to preserve of itself in order to be able to tackle what awaited it and what it had to prepare, was formed in the indeterminacy containing all that was past" (2005: 18).

[8] Halbwachs was rediscovered by the German Egyptologist Jan Assmann, who took up his ideas of „collective memory" and developed them further. For Assmann, „collective memory" is a collective term „for all knowledge that governs action and experience in the specific interactional framework of a society and is in line for repeated practice and instruction from generation to generation" (1988: 9). Assmann also distinguishes between „communicative" and „cultural" memory. What he calls „communicative memory" is „all those varieties of collective memory (...) that are based exclusively on everyday communication" (1988: 10). For him, „cultural memory", on the other hand, is characterized by „remoteness from everyday life" and is handed down through institutionalized communication (recitation, inspection, contemplation) and cultural formation (texts, rites, monuments) (1988: 12). Similar distinctions are made by Aleida Assmann between the terms „memory" and „recollection" (1993: 13-35, Assmann/Friese 1998). In a highly simplified form, the thesis here is that „memory" is rigid, stored knowledge, whereas „recollection" is living knowledge that is constantly visualized and relived.

Figure 2.2: The National Parliamentary Library of Georgia. (Photo: Susanne Fehlings 2022)

vention and truth, and enables the anthropologist to talk about values and morality.[9]

Be that as it may, modernization and globalization theories have brought temporality and change – and consequently chronology – back into focus. Anthropologists cannot ignore history (and historians), because – last but not least – history impacts the assessment of the present and the imagination of the future (Bryant/Knight 2019). Furthermore, anthropologists' interlocutors are often obsessed with and love to talk about history, which, as I witnessed in Armenia, can be given existential importance (Fehlings 2017).

The statement of the anthropologist Eric Wolf (1997 [1982]) that non-European peoples are not frozen in history and that history was not first imported by colonial powers is common sense, today (Ghosh 2021). In places with long traditions of local history writing, like in the Caucasus (where I conducted my own research), chronologies, lists of rulers, and accounts of events, developments and changes can easily be traced back to ancient times. Following the advice of Silverman, anthropologists could easily gain access to thousands of years of history, which are stored in the local archives and libraries (see Figure 2.2).The status of these archives and libraries within their own societies is easily recognizable, as it is reflected in ostentatious architecture (see Figure 2.3).

Still, having worked in the archives of Yerevan to learn more about just a very short period of city construction (1920s-1950s), I very well understand the limits of anthropological investigation of historical documents and accounts. It is so much easier to read the summaries and analyses of historians.

The situation might differ in societies with pronounced oral traditions, which – although historians are interested in „oral history" – are sometimes not

[9] The task of *mythico-history*, according to Malkki, was „to teach, to explain, to prescribe and to forbid" (1995: 54). Moral instruction also included a clear distinction between „good" and „evil": „It was concerned with the ordering and reordering of social and political categories, with defining of self in destiny to others, with good and evil. It was most centrally concerned with the reconstitution of a 'moral order' of the world. It is seized historical events, processes, and relationships, and reinterpreted them within a deeply moral scheme of good and evil" (1995: 55-56). In the Hutu imagination, according to Malkki, it is the Tutsi who embodied evil. The Hutu, on the other hand, were described as „harmless" and „good".

regarded as „real history" by local or international historians. Oral accounts often transmit genealogies. But dating and precise argumentation is usually less important. Oral accounts, like, for example the Bosnian songs first described by Mathias Murko (1929) at the beginning of the 20th century or the Manas epos in Kyrgyzstan (Chadwick/Zhirmunsky 1969), depend on interpretations of skilled performers, and usually stress moral lessons and applicability of knowledge. Anthropologists have recorded such oral performances and have sometimes tried to reconstruct written chronologies based on these records – sometimes with a practical aim, as American ethnohistory shows. Quite often, in such endeavors, anthropologists found themselves caught up in complexity because there are many contradicting versions and interpretations of the history of the same region. Such diverging versions are usually linked to claims of different interest groups that cannot all be satisfied at the same time. So ethnohistory, although it endeavored to understand local history as an interaction with colonial history and explicitly tried to include the local perspectives, finally came under criticism as a „colonial practice" (Dirks 2001; Chaves 2008).

The problem of diverging narratives is not a feature of oral traditions alone, as is shown in the example of history writing in the Caucasus. The Caucasus has a very eventful and bloody history. In addition to conflicts between local ethnic groups and tribes, it saw fights involving Romans, Persians, Arabs, Turks, Byzantines, Ottomans and Russians (Hovannisian 1997). In the period between the two world wars, short-lived nation-states were founded in the Caucasus on the Western model. The territories of these nation states, after a chaotic period, were integrated into the Transcaucasian Socialist Federative Soviet Republic (TSFSR), which was founded in December 1922 as part of the Soviet Union. In 1936 the TSFSR was dissolved again and was split up into three so-called Soviet republics: the Georgian, Armenian and Azerbaijani SSR, which partly corresponded to the independent republics of the interwar period (Suny 1997a, 1997b). As part of Soviet policies each of

Figure 2.3: The Matenadaran, the world's largest repository of Armenian manuscripts, with statues of Armenian historians, Yerevan, Armenia. (Photo: Susanne Fehlings 2008)

these so-called titular nations was given a history.[10] This was a very complicated process, which was adapted to changing ideologies and politics, in which certain parts of local history were concealed, emphasized or, in the sense of Anderson (1997) and Hobsbawm and Ranger (1983), invented (Shnirelman 2001). The effects were long-lasting. To this day, parts of the local history written in Soviet times are used, for example, to justify territorial claims – as, for example, in the context of the Karabakh conflict. History itself becomes a point of contention and the reason for war and conflict.

Historians such as Jörg Baberowski (2003) have highlighted the destructive power of Soviet nationalities policy. A key role thereby was played by Stalin, who established a reign of terror. This terror sometimes makes one forget that Soviet ideology at the same time really was an ideology in the sense of an idealistic project. It was (theoretically) based on the idea of building a new and better world – a peaceful one, in which all peoples would join to achieve a new and superior stage of living (see Figure 2.4).

And indeed, steps were taken toward this end, one of which was an attempt to rewrite parts of regional history. In October 1976, members of the Soviet Academy of Sciences gathered in the Georgian city of Sukhumi to work on a so-called „universal Transcaucasian history". This unifying project was discussed for the last time in 1988 and finally failed, because the different parties and scholars could not agree on a common version: there were too many and incompatible versions of the local past (and too many different interests) (Shnirelmann 2001: 12), and unification turned out to be a purely colonial project, which gained no acceptance among locals.

With *histoire croisée* Werner and Zimmermann propose a method to deal with exactly this kind of problem: writing a history that can bring together otherwise incompatible versions of the past. They say that

> „histoire croisée offers new answers to the question of how we, although primarily caught up in national-centred perspectives, terminologies and categories, can nevertheless meaningfully tread paths that help to overcome the limitations and circular reasoning of a nation-centred social history"[11] (Werner/Zimmermann 2002: 607).

In the English version the authors are more modest. Here they say:

> „Histoire croisée cannot escape the weight of such pre-established national formatting, but its inductive orientation aims to limit these effects through an investigative mechanism in which the objects, categories, and analytical schemes are adjusted in the course of research" (Werner/Zimmermann 2006: 46).

The Appliction of *Histoire Croisée* in Social and Cultural Anthropology

So what is new about *histoire croisé*e, and what can anthropologists learn from it? According to Zimmermann and Werner *histoire croisée* is innovative because it a) focuses on objects and processes rather than models (2002: 621), which is b) how categories and their formation are to be questioned (2002: 623, 626), and in so doing c) the researcher must be aware of his role and reflect on his own point of view (2002: 623). *Histoire croisée* looks at d) different temporal and spatial scales (*Maßstab, échelle*), which relate to each other, and it looks at the connections between these levels, which are being thematized (2002: 621-622, 624). Let me discuss these points one by one.

To begin with: many of these points have been discussed in anthropology for many years.

a) The focus on objects such as social practice, ritual, political activity etcetera as well as on process-

[10] This might sound surprising at first. After all, the Soviets were concerned with nothing less than an international revolution and the establishment of the world communist community. Nevertheless, it was believed that every society, on its way to progress, must necessarily pass through the evolutionary stage of the nation (Kappeler 2005). Thus, to build an internationalist community, it was first necessary to liberate the peoples oppressed during the tsarist period and, as a preliminary measure, to promote their nation-building in the Soviet multiethnic empire. The Soviet republics were thus to form the medium through which socialist content could be conveyed and a fusion (*slijanije*; Russian: слияние) of the peoples prepared (Halbach 2003: 661): they were „national in form, socialist in content" (compare, Kappeler 2005: 239, Baberowski 2003: 208ff, Marx/Engels 2003 [1848]: 31, 45-46). Ethnic majorities were usually declared titular nations. Clear borders were drawn and troublesome elements (minorities) were deported, like for example the Crimean Tatars, ethnic Germans, Chechens or Kalmyks.

[11] In der Tat bietet die *Histoire croisée* neue Antworten auf die Frage, wie wir, obschon primär in nationalzentrierten Sichtweisen, Terminologien und Kategorien befangen, dennoch sinnvoll Wege beschreiten können, welche die Begrenzungen und Zirkelschlüsse einer nationallastigen Sozialgeschichte überwinden helfen

es has been at the centre of classical anthropological field research since the method of long-term fieldwork and of participant observation was coined by Bronislaw Malinowski and described in his monograph „Argonauts of the Western Pacific" (1922). Since then, it has been reflected in anthropological writing, which is sometimes used as a hermeneutic approach to data interpretation, for example in so-called „thick description" as exemplified by Clifford Geertz in his famous essay on the Balinese cockfight (1972, 2003).

b) Equally, the study of local categories – or, in a more contemporary jargon, of local ontologies (see, e.g., Viveiros De Castro 2012) – is at the very centre of the discipline and has been tackled in many ways (Heywood 2017). Anthropologists differentiate between emic and etic perspectives and terms, which are embedded in their own systems of meanings (see, e.g., Harris 1976). Durkheim and Mauss (1963 [1903]), Leach (1981), Dumont (1980), Needham (1963) and many others were concerned with the understanding of local classification models – with the meaning of concepts in their specific contexts. Thus, Needham compared the work of the anthropologist to the situation of a person born blind, who suddenly gains eyesight, and who, overwhelmed by the variety and chaos of unfamiliar forms and colours, must first learn to distinguish and classify them to be able to understand what she/he sees, and so to really see (1963: vii-viii).

c) Self-reflection, then, has been practiced – almost to excess – beginning at the latest with the Writing Culture Debate (Clifford/Marcus 1986). During this debate, which paved the way for the so- called „crisis of representation", the anthropological method of knowledge production was severely attacked and questioned. The colonial context and power constellations as well as the role of anthropologists within these constellations became a topic, and anthropologists asked themselves who had the right to speak about what, and who was allowed to represent whom and how? (e.g., Asad 2007; Fabian 1983; Guha 1982) These questions led to new research ethics, and to new relationships with the research subjects, who were now perceived as interlocutors. It also resulted in the questioning of major categories (Said 2009 [1978]), and in new forms of anthropological writing, which gave a voice to the people who were being researched. But along with new innovative approaches, these reforms also triggered uncertainty among postmodern anthropologists, who occasionally felt paralyzed by the new and often emotional debates about morals. Sometimes this led to a kind of navel-gazing self-reflection driven to absurdity (Kohl 2000).

Figure 2.4: Soviet poster „Peace! Friendship!" (Source: Russia Beyond 2019)

Finally, like *histoire croisée*, anthropology d) looks at different scales (*Maßstab, echelle*) – both temporal and spatial – which relate to and constitute each other, and thematizes their connections: between structure and agency, macro and micro levels, between models, discourses, case studies and individual perspectives (Werner/Zimmermann 2002: 626).

It seems to me that regarding the above-mentioned points (a–d) anthropologists can learn little from *histoire croisée*. In my opinion, what is interesting about *histoire croisée* for anthropologists is, first, the methodological tool of interweaving/intercrossing perspectives, and, second, the attempt to reflect on and relate these perspectives to each other. How do objects and perceptions depend on each other, how do they co-constitute and shape each other, and where can we find symmetries and asymmetries?

Although anthropologists deal a lot with „transnational issues", e.g. migration, interethnic conflicts, exchange and trade, cultural contact and so on, they are usually specialists in specific social, societal, religious, ethnic or otherwise defined groups. Almost always, interaction in transnational or other contexts of encounters is described and assessed through the lenses of the specific local expertise of the anthropologist, who then adds his own interpretation, which (as was widely discussed during the Writing Culture Debate) is biased by his own background. Thus, for example, I look at encounters between Chinese and post-Soviet/Georgian traders through the lenses of Georgians and Russians (Fehlings 2020, 2021), while the Chinese perspective – because I do not speak Chinese – remains mostly closed to me. But of course, this Chinese perspective is part of the complex whole that makes and shapes the nature of the transnational encounter. To fully grasp this encounter, one would indeed have to study the intertwining/intercrossing of different dimensions and their relations to each other. For example, one would have to consider macro-politics such as regional geopolitics in the Caucasus and China's Silk Road diplomacy, but also mutual perceptions of Caucasian and Chinese actors such as state representatives, elites, and citizens. More important, one would have to research how these perceptions constitute each other, for example through understanding and misunderstanding. One would have to look at macro- and micro-economics, Chinese and Georgian culture, respective practices, customs, ideologies, and values, and different pasts and presents, including points of mutual contact and exchange, which might even leave traces in myths, literature and art. One would have to learn Georgian, Russian, Mandarin and Uygur, and become a Sinologist and Caucasiologist at the same time.

An attempt to approach one topic through different perspectives was made in so-called polyphonic or multivocal anthropology, which uses the so-called Rashomon effect for data presentation. But such attempts have usually been applied to capture different voices and perspectives within one coherent context, or a „social situation" within one nation state (see, e.g., Gluckman 1940). If attempts are being made to describe the perception of one national group by another, rarely are both positions, perspectives and contexts treated with equal care. This is not a question of goodwill but, as shows my own research on traders, often a question of available expertise. This deficit surfaces gravely and sometimes with very bad and immediate consequences in the study of conflicts – such as in one-sided studies on, for example, the Karabakh war or the war in Ukraine. Such topics understandably evoke emotions, which are difficult to overcome. Still, *histoire croisée* could give us a tool to understand different positions and to look at them – at least for a moment – from some neutral distance, as a juxtaposition of data on diverging experiences, assessments, and perspectives (Werner/Zimmermann 2002: 632). Do Ukrainians, Western- and Eastern Europeans, and Russians refer to the same thing when they use the term Fascist or Nazi? What does the notion of homeland mean for Armenians and Azeris? What kinds of worldviews, ontologies, values, practices, and interpretations coexist, interact or collide, in what ways and in what contexts? What is the basis of understanding and misunderstanding? Of trust and mistrust? Which perspectives lead to which assessments and actions?

But although I believe that one can benefit from looking at encounters, interactions, problems and conflicts from different perspectives, I do not believe in *histoire croisée* as a peace-building project. The intention to make politics and to build peace through writing history is not a new phenomenon, nor is the failure of this attempt. This is well reflected in the above-mentioned attempt to write a „uni-

versal Transcaucasian history" between 1976 and 1988. It would be equally difficult to reconcile Putin's reading of Ukrainian history with a Ukrainian perspective. But if we, as scientists (anthropologists and historians), can manage to treat *histoire croisée* not as a peace-making project, but as a purely scientific and analytical tool, it might help us to take a step back and to better understand and to reconsider all, or at least more, ingredients and flavors of very complex encounters.

Conclusion

As explained at the beginning, the approach of *histoire croisée* can be interpreted as a reaction to political developments. The question is: can *histoire croisée* unite? Apart from being a hugely ambitious project, *histoire croisée* is also a very intellectual undertaking. Perhaps this is how European history can be written today, and, as new French-German and Polish-German collaborations show, systematically taught with jointly written schoolbooks in schools. The political and diplomatic course has been set for this. But would *histoire croisée* have made a universal Transcaucasian history possible? Could it today? Probably not. The political climate does not allow the peaceful coexistence of different versions of the truth – at least not in the political arena.

As an anthropological method, however, the approach promises interesting insights, a new approach to intercultural/transnational encounters, and an opportunity to make new connections between ethnographic data collected from different perspectives – from different sides of an encounter. Given the limited local expertise of anthropologists, I doubt an *histoire croisée* can be a one-wo/man project. Probably, an *histoire croisée* or – if you will – an *ethnographie croisée* can only be managed in the form of a collaboration that unites different skills and expertise: the knowledge of different languages and cultures, and a vast knowledge of historical as well as of ethnographic data.

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3. Doing Historical Anthropology in the Archives

Oliver Tappe

Introduction

It is certainly one of the aims of anthropological research to give voice to the marginalized, to overcome „epistemic injustice" (Fricker 2007), to let the subaltern speak (Spivak 2008), as it were. Oral history is a useful and frequently used methodological approach when trying to understand past lifeworlds of indigenous and local communities (Petit 2020), albeit not without limits. How can we pursue the ambitious task of reconstructing the past from a grassroots perspective beyond the reach of oral history, where we have to make do with archival sources written by colonizers, missionaries, and other hegemonic actors who dominated certain discourses or silenced others (Trouillot 1995; Stoler 2008; Zeitlyn 2012)? What are the methodological and epistemological implications of linking „classic" ethnographic work in the field with archival „fieldwork between folders" (Ladwig et al. 2012)?

This reflexion on fieldwork in the archives addresses such questions and discusses the opportunities and limits of doing historical anthropology (which of course includes oral history, despite the focus on archival research in this paper). The archive as site for practicing ethnographic work on past lifeworlds needs particular scrutiny here. What are the benefits of a methodological turn that moves „away from treating the archives as an extractive exercise to an ethnographic one" (Stoler 2008: 47; Farge 1989)? Taking a few case studies from Laos as a vantage point, I suggest a historical ethnography of the remote in time and space (see Kuiper's contribution to this issue), inspired by approaches of (global) micro-history and multi-sited anthropology (arguably somewhat between armchair anthropology and time-travel, yet informed by ethnographic experiences in the present).

Research on Laos and Relevant Archives

In my various research projects so far, I have worked with French (colonial) archives in order to explore sociocultural, political and economic dynamics in past and present Laos. These have included projects on intercultural interactions and sociopolitical dynamics in upland Laos (Houaphan Province), Vietnamese labour migration to Laos, and formal and informal mining. In all research contexts, one of my main ambitions and challenges was to assess local perspectives and agency under colonialism (Laos became independent in 1953, thus oral history only helps to a limited extent, not least as it is usually dominated by memories of the civil war – a sideshow of the „American War" – that culminated in the communist revolution of 1975; see Evans 2012). To achieve this, I had to rely on the very archives that more often than not represent the suppression and silencing of indigenous voices.

French archives certainly provide valuable insights into questions of economy, governance, and cultures of colonial Indochina (present-day Laos, Vietnam, and Cambodia). Most importantly, the *Archives nationales d'outre-mer* (ANOM) in Aix-en-Provence hold the (largely administrative) files of the *Gouvernement général de l'Indochine* and the subordinate *Résidence supérieur* du Laos. Both administrative bodies of the colonial government left detailed information about the local economic and political structures. Moreover, the archives hold colonial periodicals and the files of relevant institutions such as the *Agence économique francaise d'outre-mer* (AGEFOM). The archives of the *Missions Étrangères du Paris* (MEP) include detailed accounts of missionaries who produced invaluable ethnographic information in their missions. It is for this reason that Jean Michaud (2007) called these French missionaries „incidental ethnographers". The archives of the *École Française d'Ex-*

trême-Orient (EFEO) and the Musée du Quay Branly hold important documentation on scientific missions and early anthropological studies. Other useful archives are the *Service historique de la defense* (the archive of the French ministry of defense that holds material on colonial wars), the *Centre des archives diplomatiques* (Nantes) – including French embassy files from pre-1975 Laos – and, last but not least, the *Archive nationale du monde du travail* (Roubaix), offering information on colonial economy and labour.

ANOM, MEP and EFEO are perhaps the most relevant archives for my historical anthropology of Laos (see Hildesheimer 1997; Clémentin-Ojha/Manguin 2006; Dion 2017). As for the ANOM, French colonial interests and bureaucratic zeal manifest themselves in the structure of this archive and its various files (including, since 1966, the documents from the ministries in charge of the colonies and the – incomplete – archives transferred from the colonies at the time of their independence). In contrast, the EFEO archives document the cultural dimension of French colonialism in Laos that was marked by archaeological and ethnographic missions.

The MEP archive is particularly interesting as source of ethnographic knowledge. Most prominently, the missionary Antoine Bourlet published fine-grained ethnography of the Lao and Tai people in the Lao-Vietnamese borderlands in the founding issues of the journal *Anthropos*. He wrote about land-tenure systems, local livelihoods, cosmology, and ritual practice, among others (Bourlet 1906; 1907; 1913). His interest in local culture and society is also reflected in his various reports to the bishop and publications in missionary journals, and in his private correspondence. Yet other missionaries also offered detailed accounts that constitute veritable goldmines for historical ethnographers. Jean-Marie Martin (1899), Jean Mironneau (1935; 1972), and others wrote about local sociopolitical dynamics and cultural aspects, that, for example, helped me to reconstruct the assassination of the missionary Verbier in 1895 (Tappe 2022a; see below).

These sources have to be taken with a pinch of salt, though. As best reflected by Jean-Baptiste Degeorge's (1924) history of the mission in „Chau-Laos" (the Tai-Lao speaking hinterland of Thanh Hoa and Nghe An, including Houaphan on the Lao side of the border), the missionaries considered themselves conquerors, entering the „savage" uplands to save the native souls. Part and parcel of this conquest was to learn about local *moeurs et coutumes* (mores and customs) in order to develop strategies for conversion (for example linking the idea of the Holy Spirit to the local animist spirit pantheon). Some, like Bourlet, became meticulous ethnographers and developed a genuine interest in local culture. Arguably, this ethnographer was not „incidental" at all (cf. Michaud 2007).

Opportunities and Limits for an Archive-based Historical Anthropology

The different French archives constitute a variegated landscape of colonial knowledge production. Perhaps most crucial for assessing the possibilities and limits of historical anthropology here is an understanding of the power asymmetries represented by the (colonial) archive. In his critique of colonial knowledge production, Dirks (2002) considered the archive as a powerful tool of colonial domination. However, we should not overestimate the unity of archival bodies. For example, letters written by French administrators from Laos and Annam (Vietnam) in the 1890s reveal heated debates about border demarcation in the Lao-Vietnamese uplands, clearly informed by the respective interests of Lao, upland Tai, and Vietnamese notables (Tappe 2015). Therefore, we can identify more complex power dimensions and contestations beyond the (certainly existing) power asymmetries between colonizers and the colonized.

According to Ann Stoler, colonial archives are not merely sites of knowledge retrieval but also of knowledge production, and historical anthropologists must pay attention to „(…) processes of production, relations of power in which archives are created, sequestered, and rearranged" (Stoler 2008: 32). More than being a manifestation of colonial domination, the archive also reveals colonial anxieties and failures, hinting at the „fragmented, ineffectual, and tensional aspects of colonialism and its forms of knowledge" (Roque/Traube 2019: 13). We thus need to consider what the fragments and tensions of the archives produce and make visible as well as what they hide and conceal.

Given the specific colonial context of archival production, it is very likely that the archives hide more than they make visible with regard to local indige-

nous perspectives. Yet given the fact that colonial and missionary archives have been produced through the complicity of local counterparts – often indigenous elites (not really „subalterns speaking", so that we have to consider yet another dimension of power asymmetries here) – focusing on colonial encounters and interactions opens a venue for understanding. Such „entangled intercultural processes" (Roque/Traube 2019: 14) include both European and indigenous (contradicting) conceptions, agents, and social worlds. In consequence, archives hold epistemological value for investigating indigenous lifeworlds if we accept – or even embrace – contingency and fragmentation.

Given the specific (bureaucratic) structure, materiality, accessibility, and lacunae of different archives, doing archival fieldwork requires a „sense of archival texture and its granularity" (Stoler 2010: 272) as a precondition for the critical judgment of and negotiation with the sources. Understood as multi-sited archival fieldwork, it calls for an „ethnography of the system" (Marcus 1995: 99) – that is, an analysis of the overarching worldview that pervades and saturates specific archives, especially concerning colonial administrative, military, or missionary agendas and their respective plans and practices. As stated above, such a „system" must be perceived as a tensional and fractured world, shaped by contingencies, contradictions, and anxieties.

As in all anthropological work, archival research depends to a great deal on serendipity and surprise (Guyer 2013), all systematic approaches notwithstanding. It is perhaps the greatest fascination of archival work when, for instance, one finds a letter including a small envelope which more than a century ago included a sample of raw opium (see Figure 3.1). Such random findings offer a glimpse of the everyday life of colonial administrators in their remote outposts. Sometimes the occasional side note on an official document is more reminiscent of Kurtzesque „kill all the brutes!" outbreaks that betray colonial anxieties and misinterpretations (see Figure 3.2). It is easy to get excited and distracted by such telling fragments. I leave the question open as to whether those bits and pieces, especially when appearing without much context, are distractive and misleading or, rather, the truly interesting stuff.

Searching for indigenous voices in between such articulations of European subjectivities can be frustrating, though. Even if there is some verbatim doc-

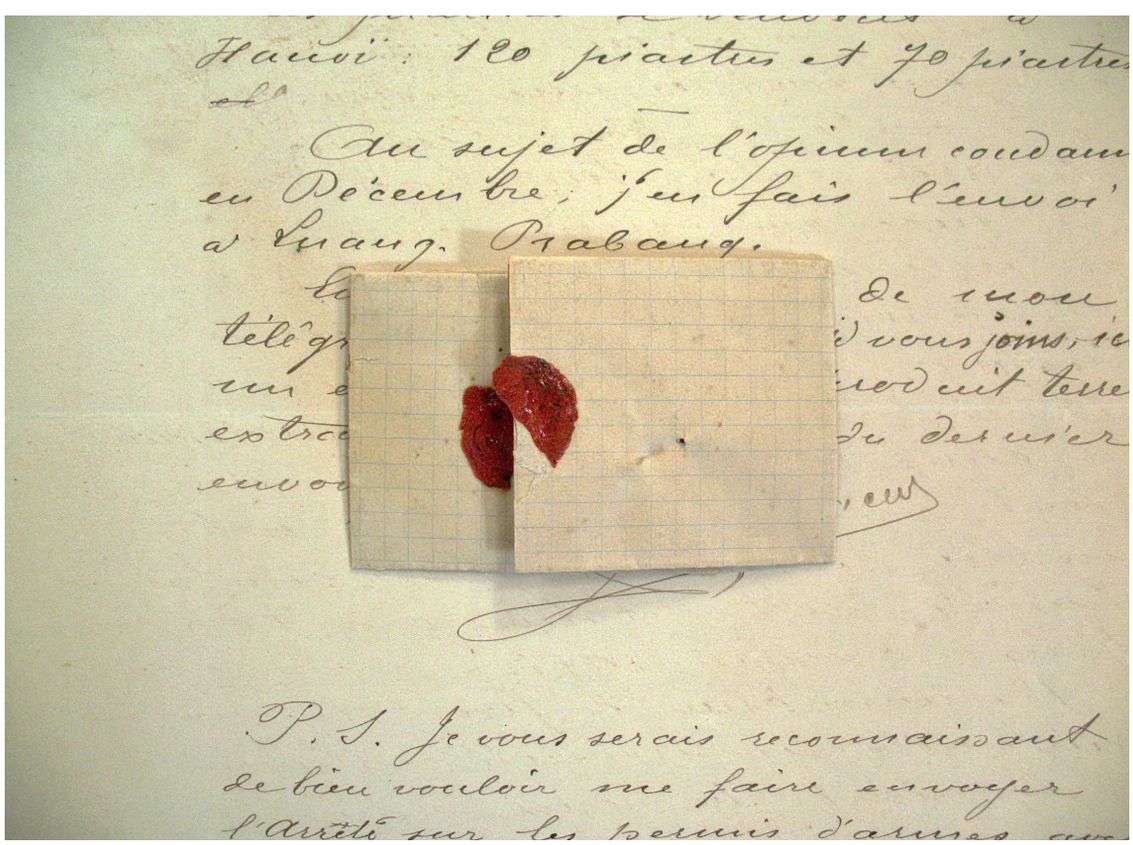


Figure 3.1: Empty envelope, previously holding a sample of opium. (Source: 1910, Résidence supérieur du Laos, E4, ANOM/Aix-en-Provence)

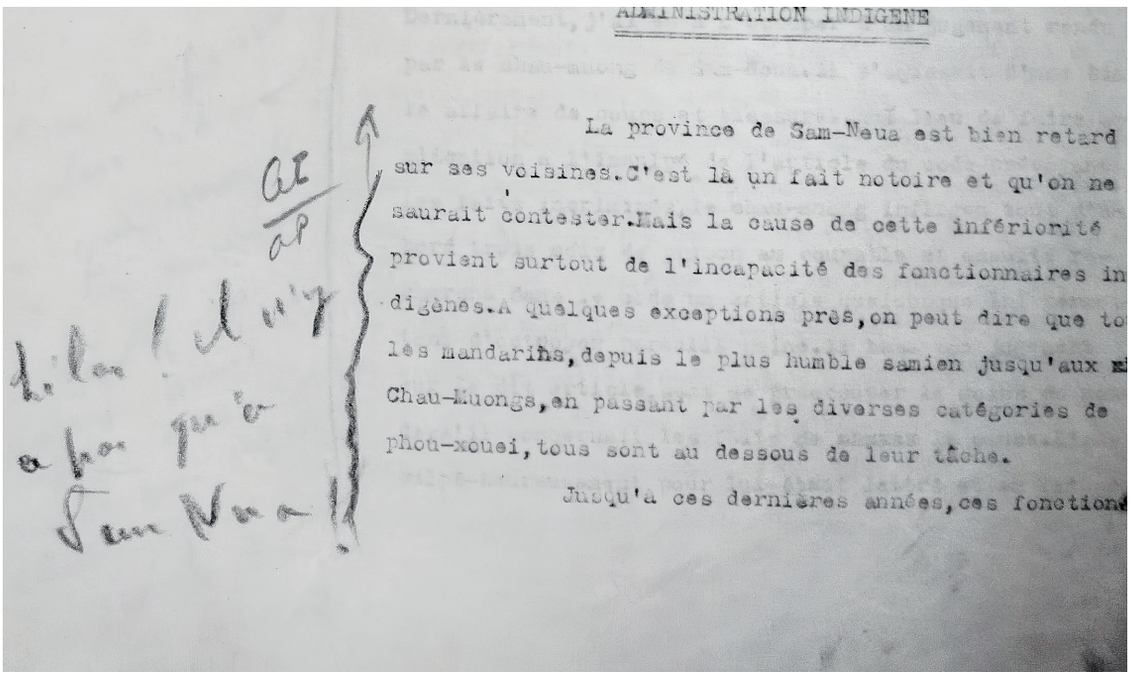


Figure 3.2: „Hélas!": Notes on an administrative report complaining about „l'incapacité des fonctionnaires indigènes". (Source: 1925, Résidence supérieur du Laos, E4, ANOM/Aix-en-Provence)

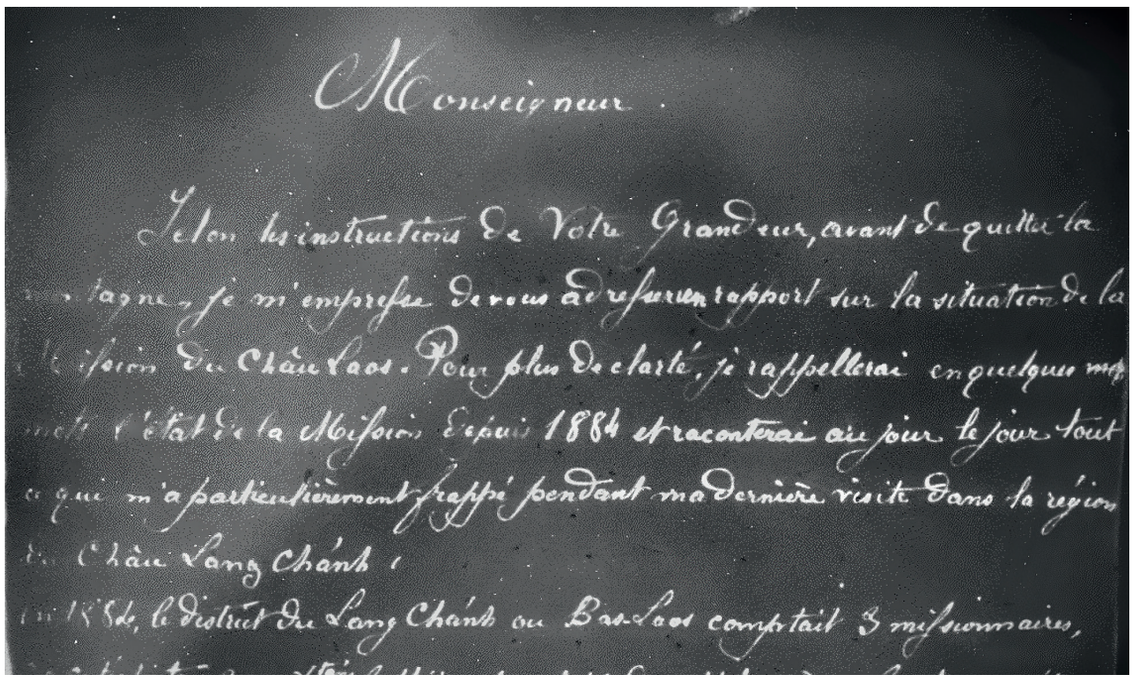
umentation in official reports or letters by missionaries, sometimes the handwriting constitutes a terrible obstacle… not least if only available as faded microfiche (see Figure 3.3). These are the practical challenges of archival work, besides all the structural limits involved in exploring past indigenous life-worlds.

Informative articulations of indigenous views are petitions directed to the colonial authorities – yet usually only when there is trouble, so that we learn less about inconspicuous everyday life. In remote areas such as Houaphan (north-eastern Laos) they were often written by Buddhist (ex-)monks, as even the local nobility was not necessarily literate – and signed these petitions with thumb prints (see Figure 3.4). As these thumb prints are a colonial technology of power, even such rare findings of indigenous agency are linked to the colonial apparatus. Therefore, historical anthropology in upland Laos calls for an approach that considers both entanglements and fragmentation.

Fragments and Traces: Methodological Challenges

Lacking the ability to time-travel and to talk to people in the past, historical ethnographers certainly need to borrow from the historians' methodological toolkit. Exploring the history of upland societies in Southeast Asia requires a broad method mix, from research in colonial and missionary archives, to oral history interviews in upland villages today (see, e.g., Salemink 2003; Scott 2009; Padwe 2020; Petit 2020). My research is also informed global micro-history that resonates well with approaches of translocal, multi-sited ethnography (Marcus 1995; Falzon 2009; Coleman/von Hellermann 2011; Greiner 2010). Global micro-history approaches emphasize the interlinkages and circulations between different micro-cases, actors and things, thus transcending the global-local binary (see De Vito/Gerritsen 2018; Epple 2012; Bertrand/Calafat 2018). In Southeast Asian contexts, this approach helps to connect European and Chinese imperialism in the region with sociopolitical dynamics on the ground – allowing for historical, multi-sited ethnographies of the remote in both space and time.

In my various attempts of doing historical anthropology in and of upland Laos, I have benefited from a wide range of potential sources retrieved from the aforementioned archives, from French colonial administrative reports and correspondence to missionary accounts, but have also made use of Lao, Siamese and Vietnamese written sources (like petitions and chronicles). Arguably, all these different sources


Monseigneur.

Selon les instructions de Votre Grandeur, avant de quitter la
[illegible] je m'empresse de vous adresser un rapport sur la situation de la
Mission du Chân Laos. Pour plus de clarté, je rappellerai en quelques mo
mots l'état de la Mission depuis 1884 et raconterai au jour le jour tout
ce qui m'a particulièrement frappé pendant ma dernière visite dans la région
du Chân Lang Chánh.

En 1884, le district de Lang Chánh ou Bas-Laos comptait 3 missionnaires,

Figure 3.3: Père Verbier's last letter. (Source: microfiche, MEP Paris)

provide selective glimpses into the past, revealing only fragments and traces of past indigenous lifeworlds. Instead of trying to reconstruct an imaginary historical whole, we may embrace the fragmentary nature of history and employ a Benjaminian approach to history – one that considers historical knowledge as fragmentary and contingent (Ladwig et al. 2012).

My idea of historical anthropology is inspired by Walter Benjamin's notion of the allegorist working with fragments and thereby creating sense through a technique of montage, a method he adopted in his experimental Arkaden project (Benjamin 1999). An ethnographer in the archive might feel like a „rag-picker" (*Lumpensammler* in Benjamin's words) handling disparate sources in order to produce something new and more or less coherent, a fragmentary image of past lifeworlds – perhaps also a work of *bricolage* in the sense of Lévi-Strauss (1966). Creativity and imagination are certainly required when doing remote ethnography of the past.

Piecing together traces from different archives is particularly indispensable when oral-history accounts are missing, not least when superimposed by traumatic decades of war and revolution as in the case of Laos. However, semi-structured oral-history interviews on specific locales – even if not yielding direct „evidence" and historical verification to the given research questions – offer valuable inspiration to get a sense of sociopolitical dynamics and cultural contexts in the past and present Lao-Vietnamese borderlands. (Ethno-)historical imagination needs to be nourished by field experiences, exposures to the real lives of people that provide „interpretive energy" (Roque/Traube 2019: 7) for understanding past indigenous lifeworlds through archival work.

A telegram from a remote colonial post in the highlands, a letter to the bishop from a missionary living in an upland village, a local myth reflecting traditional sociopolitical hierarchies, or a petition by a local leader: such sources provide disparate yet connected fragments and traces that inform our understanding of past lifeworlds in upland Southeast Asia. Following Paul Ricoeur (1990: 116), I hold that the trace is the epistemological presupposition of the archive, and it acts as a link between different spatio-temporal sites. Traces can function as devices for reassembling fragments of local pasts that are dispersed within distinct field sites.

In my study of the assassination of the missionary Verbier and the encompassing sociopolitical dynamics, I relied on „para-ethnographers" (Marcus 1995) such as missionaries and administrators as well as local notables (at least „elite" indigenous voices). When enriching this combination of historical anthropology and microhistory with approaches like the Extended Case Method (which indeed requires a profound knowledge of the respective cultural con-

Figure 3.4: Thumbprints on a petition in colonial Laos. (Source: 1908, Résidence supérieur du Laos, E4, ANOM/Aix-en-Provence)

text as precondition for the analysis of a specific case; see Burawoy 1998; Rössler 2008) we might be able to reassemble the disparate fragments and traces from the archives.

Moving back and forth between the French archives and the villages in Laos, the epistemological value of ethnographic field research for making sense of historical events cannot be overestimated. Discussions with village elders about indigenous historicities, experiences of ritual practices and the persistent fear of spirits, and investigations into present-day sociopolitical dynamics in upland Tai and Lao communities have offered me a set of lenses to explore past events that escape local oral history but that have left unevenly distributed traces in the archives.

Selected Case Studies

In the following, I will introduce three case studies illustrating my idea of practicing historical anthropology: the aforementioned assassination of the missionary Verbier, Vietnamese labor migration in colonial Indochina, and Lao tin-mining communities. These examples suggest a fruitful dialogue between archival research and present-day ethnographic fieldwork.

The Assassination of Père Verbier

In 1895, the French missionary Père Verbier was assassinated by some notables from the upland Tai communities who – curiously enough – had earlier invited him to establish a mission in the Lao-Vietnamese borderlands (Tappe 2022a). In his last letter to his bishop, preserved on a hardly decipherable microfiche (see Figure 3.3), he even indicated a cordial atmosphere. To understand this tragic incident at the colonial periphery, I focused on the local sociopolitical dynamics that were affected by intra-ethnic rivalries, previous transregional warfare and migrations, and colonial interventions. It became clear that this attack went beyond a mere colonizer vs. colonized antagonism but that it rather happened within a complex „frontier assemblage" (Cons/Eilenberg 2018), with different local actors competing or collaborating, carefully experimenting with precarious relationships with external powers (such as French missionaries) to their own advantage, as well as renegotiating or revoking those relations.

My historical-anthropological interpretation was informed by previous studies on historical encounters and interactions across cultural difference in this ethnically heterogeneous region. When trying to understand processes of mutual mimetic appropriation within the colonial encounter (Tappe 2018; Ladwig/Roque 2020), I struggled with the challenge of balancing the Western accounts of encounters with the

„Other" with the perspectives of the different ethnic groups in upland Laos. In fact, this was almost impossible to achieve as even voices of local notables were lacking, and many of my conclusions included a great deal of historical imagination – albeit informed by useful experiences with cross-cultural encounters in present-day Laos.

That said, the interplay of archival and field research opens up new horizons for historical investigation. An understanding of indigenous ideas of sociality and cosmology help to analyse socio-political dynamics in which non-human agency also plays a crucial role. For example, it is enticing to speculate whether the killer of Verbier, a powerful upland Tai notable, was counselled by a mo phi (shaman) corresponding with the capricious spirits of the land. As other ethnographies in Laos show (Stolz 2021; Petit 2020), social trouble is often explained with reference to the agency of angry spirits, demanding diverse ritual options of appeasement. Perhaps killing Verbier was the ultimate sacrifice, or at least a radical demonstration of power (local ancestor spirits vs. the Holy Spirit)?

Vietnamese Labor Migration

In my studies of Vietnamese labor migration to Laos (Tappe 2019) such sociocosmological aspects were clearly less relevant. Rather, the main challenge was to assess patterns of unfree labor and precarity through colonial reports. I found only rare instances where „the coolie speaks" (Yun 2008) and which offered unfiltered insights into the „coolie" workers' daily plight. At least some ILO reports (Goulin 1937) and the results of colonial enquêtes responding to labor unrest and public critique, provided hints at labor precarity under colonialism (as did controversial pamphlets and novels by colonial critics; see Monet 1930 and Pourtier 1931).

In the 1920s, the Guernut Commission was established to document the socioeconomic conditions in colonial Indochina, not least as poverty and social unrest concerned the colonial administration (Brocheux/Hémery 2009; see the prolific documentation in the Guernut files, ANOM, FM Guernut//24). Another example was the Inspection général du travail that was set up in 1927 to check the working and living conditions at plantations and mines. Some inspectors noted the widespread practice of corporal punishments for the slightest offence, and the scandalous sanitary and medical conditions on the workplace, resulting in gradual reforms of the colonial indentured-labor regime.

In a famous anticolonial pamphlet written by the Vietnamese worker-turned-revolutionary Tran Tui Binh, we find the story of how Inspector Delamarre visited a clinic on a rubber plantation in southern Indochina where half of the patients had been injured by beatings, and where he noted numerous dirty sheds containing „coolies" shackled and close to starvation (Binh 1985: 37). Delamarre's (1931) own published report is heavily downplayed in this respect, though (cf. Aso 2018). Thus, we have to search the diverse unpublished reports and testimonies in the archives for the occasional eyewitness account of precarity and exploitation, or sometimes outraged letters written by colonial administrators (see examples in ANOM, FM/AFFECO 26; Bunout 1936; Monet 1930).

The Guernut files hold numerous testimonies (voeux) by local functionaries and even workers complaining about harsh labor conditions, abusive foremen, or deceitful recruiters (see ANOM, FM Guernut//24; Aso 2018; Tappe 2019). Even if it is difficult to assess the context of these testimonies and inherent power differentials, this is as close to individual voices as one can get. The presence of such voices certainly reflects the anxieties, vulnerabilities, and errors of the colonial regime, which does not appear here as a mere power machine producing monolithic archives (Roque/Traube 2019: 13; cf. Dirks 2002).

Lao Tin Mining

For my current project on tin mining in central Laos those sources haven proven very useful so far – mainly to understand the life of the Vietnamese „coolie" workers, but less so the local Lao communities who have practiced artisanal and small-scale mining since precolonial times (Tappe 2022b). Sources that inform about labour precarity, early labour struggles, and Vietnamese communist agitation (for instance the *sûreté* files used by Geoffrey Gunn (1988)) reveal close to nothing about the social and cultural life of the Lao peasant-miners. We have a few travelogues (e.g. Raquez 1902) that offer some rare insights into local livelihoods and religious practice but nothing in comparison with the detailed ethnographies produced by the MEP missionaries in the northern Lao-Vietnamese borderlands, as mentioned above.

So what about the MEP archives? I went there with exaggerated hopes that were fueled by „para-ethnographers" such as the missionaries Bourlet and Mironneau, but was disappointed. Missionaries like Victor Barbier (who oversaw the construction of the church in the tin-mining area of Khammouane Province) worked mainly with the Vietnamese mining communities and largely ignored the local Lao Buddhist communities. Those were not targeted anyway as, the missionaries focused rather on animist uplanders. Even for the Vietnamese communities they gave only scarce information about everyday life as such details were well-known already and the missionaries apparently did not bother to write anything ethnographically relevant.

Instead, I found detailed information about missionary administration and the challenges of translating catechisms or training Vietnamese priests, which was interesting, but not exactly my concern, as I am not writing a history of the mission (see Keith 2012 for a concise history of the Catholic mission in Vietnam). Thus, my hopes that I can easily explore the lifeworlds of the Lao miner-peasant communities through the eyes of the missionaries (as it is possible for the Lao and Tai in NE Laos) were not fulfilled.

Perhaps the private correspondence of French managers and engineers might help, but this is hard to find. For example, the private archive of Léon Belugou (director of the *Compagnie Fermière des Etains d'Extrême-Orient*) stored in the *Archive nationale du monde du travail* holds accounts of administrative and fiscal issues, but only a little information about local mining traditions. One document describes local mining, including an illustrative sketch of the *„puits indigènes"* („pits of the indigenous people"; see Fonds Belugou 176 AQ 23) on the banks of the Nam Phathaen (precising Raquez's descriptions of thirty years earlier; see Raquez 1902: 499-503). These are the rare archival finds I have to rely upon. How my fragmented image of tin mining in Laos will look after assembling such bits and pieces remains to be seen.

Conclusion

Reassembling the fragments and traces from the archive methodologically recalls Benjamin's *montage* or Lévi-Strauss's *bricolage*. This is arguably the main task and challenge for historical anthropologists. More often than not, indigenous voices are silenced, filtered or distorted in the archives, especially due to the colonial context of most of the archives at stake. Thus, an understanding of the underlying power asymmetries and conditions of colonial knowledge production as reflected in the various archives constitutes one key aspect of critical inquiry. Who collected the „data" and for what purpose (and what was ignored)? What are the implications of the historical ethnographer's appropriation of those data, perhaps even running the risk of colonial complicity? What can we distil from it when balancing critical distance and intimacy with the sources?

The specific limits of the archives notwithstanding, anthropologists interested in past indigenous lifeworlds might gain a lot from fieldwork in the archives (ideally complementing oral history research), as even the occasional serendipitous finding may yield epistemological value. Moreover, the archives represent fragmentary histories of entanglement (Thomas 1991; see Lindner 2011, and Fehlings, this issue, on *Verflechtungsgeschichte* and *histoire croisée*), produced by both the colonized and the colonizers in concrete moments of encounter. Such histories reflect (colonial) knowledge transfers as well as, eventually, „indigenous conceptions, agents, and social worlds" (Roque/Traube 2019: 14). The challenge remains to carve out those indigenous perspectives from the plethora of files and documents stored in the European archives.

Most importantly, it can be intellectually refreshing to move back and forth between „real-life" fieldsite and archive when researching postcolonial societies. Knowledge of present-day sociocultural configurations certainly helps with interpreting the archival fragments and, arguably, puts anthropologists in a better position to detect marginalized voices. Vice versa, historical insight gained from archival research – sometimes thanks to incidental para-ethnographers from the past – may assist to assess contemporary social dynamics and power asymmetries. Or, to put it briefly, historical anthropology can be fun!

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4. Studying Canadian Indigenous Histories from within a German Historical Institute: Some Reflections on Methodology, Positionality, and Accountability

Lena Rüßing

Introduction

When I arrived in Canada in the spring of 2016 for my first research visit, the country was in the midst of political, social, and academic debates on coming to terms with its settler colonial history and the associated atrocities committed against Indigenous peoples. The Truth and Reconciliation Commission (TRC) had just published its final report in 2015, calling all Canadians to take action on the country's settler colonial past and its continuing legacies in the present (see TRC 2015). In particular, the commission had investigated the settler colonial history of so-called Indian residential schools, which had systematically been implemented in Canada from the second half of the 19th century onwards to disconnect Indigenous children from their families and home communities with the goal of assimilating them into the growing Euro-Canadian settler society (see Miller 1996; Milloy 2003). The residential school system, which was funded by the Canadian settler colonialist government and run by Christian mission churches, became a crucial assimilative instrument in an overarching settler colonial policy of oppression towards Indigenous peoples in order to acquire Indigenous lands. Through practices of compulsive assimilation, tribal and kinship systems were destroyed in order to eliminate Indigenous peoples from the land to which white settlers laid claim. In the schools, where more than 150,000 Indigenous children had been sent until the last school closed in the late 1990s, many children were sexually and physically abused. They also sustained widespread cultural abuses as many were banned from speaking their native languages and practicing their cultural traditions (see, e.g., Knockwood 1992; Haig-Brown 1988; Deiter 1999; Grant 2005). The tragic outcomes of both the residential school policy and other settler colonial practices reverberate in Indigenous communities to this present day. Indigenous people suffer from the loss of their language, culture, and family and community life – losses which often manifest themselves in substance abuse, poverty, suicidal tendencies, poor parenting skills, domestic violence, and loss of pride and self-esteem (see, e.g., Deiter 1999; Ing 1991; Bombay et al. 2014; Bombay et al. 2011; Miller/Danziger 2000). At the same time, however, the Indigenous experience of settler colonialism in general, and the Indigenous experience of residential schools in particular, have given rise to a movement of Indigenous resistance to the continuing patterns and practices of settler colonial domination and assimilation on the one hand, and Indigenous efforts to revitalize Indigenous languages, knowledge systems, cultural practices, and relationships to land on the other (see, e.g., Corntassel/Bryce 2012; Corntassel et al. 2009; Coulthard 2014; Alfred/Corntassel 2005; Alfred 2009; Alfred 2017; Smith 2021; Battiste 2000a; Tuck/Yang 2012; Miller 2017; see also Figure 4.1).

Thus, I began my doctoral research on the contemporary history of reconciliation between Indigenous peoples and the Government of Canada, Christian churches, and the Canadian society more broadly in a publicly controversial political climate. My work started in the public climate of Indigenous resurgence, emerging public awareness of Canada's settler colonial past and its impact in the present, and the Truth and Reconciliation Commission's call on all sectors of Canadian society to look in the mirror and

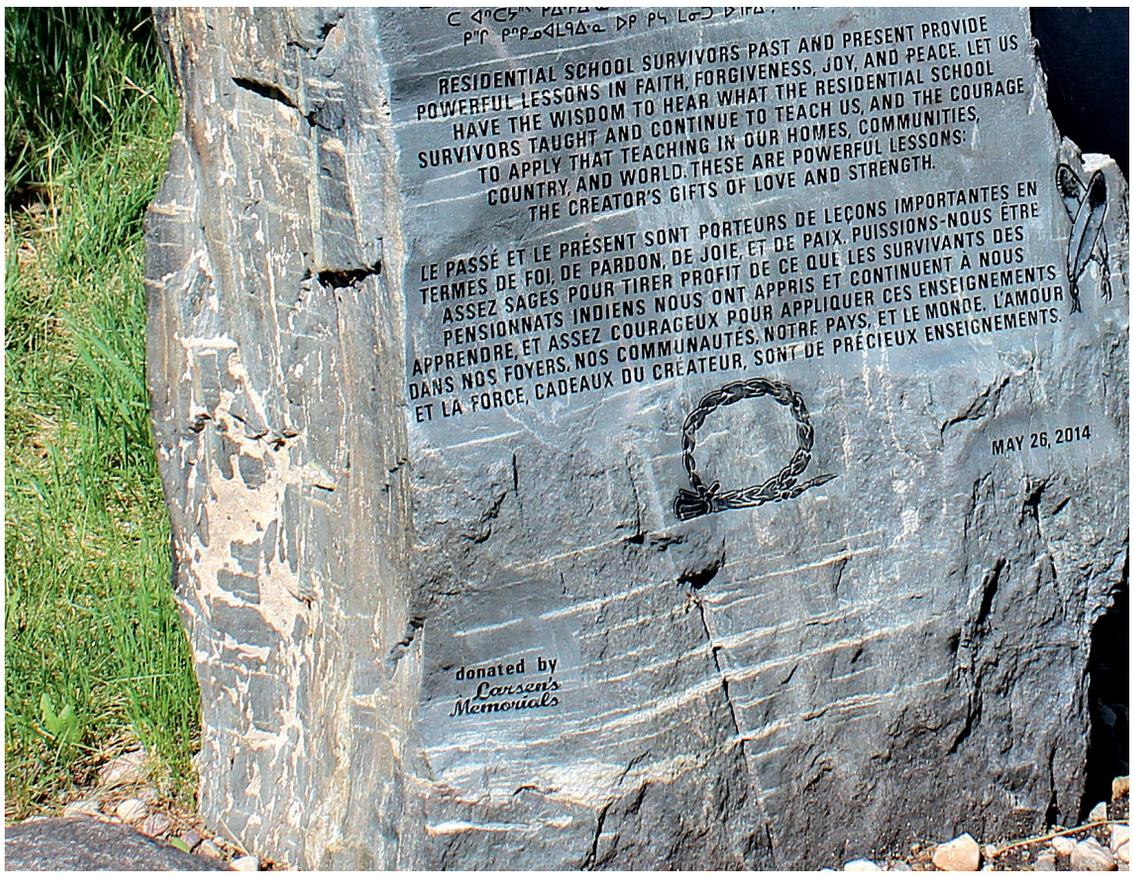

Figure 4.1: A stone monument unveiled in 2014 to honor former residential school students near the Canadian Museum for Human Rights in Winnipeg, Manitoba. (Photo: Lena Rüßing, 2016)

make sweeping changes in Indigenous-Settler[12] relationships to address the apparent and alarming political, legal, economic, and social inequalities that underlie these relationships.

It quickly became clear that the methodological orientation of my work would become the greatest challenge, for three reasons: firstly, unlike academic-institutional contexts in North America and other settler colonialist nation-states such as Australia and New Zealand, an institutional context is missing in Europe, or at least in the German-speaking countries, that recognizes the position of Indigenous Studies as an independent discipline. Secondly, my research took place against the backdrop of a long history of colonialist-scientific research on Indigenous peoples, their histories, and their cultures, which has exploited, marginalized, and racialized Indigenous peoples and their knowledge systems, cultural practices, and spiritualities (see Smith 2021; Kovach 2021). The colonizing impact of research advanced and propagated by Western European empires and their academic institutions is rightly not forgotten in Indigenous communities in North America and elsewhere and becomes a sensitive issue in research on and with Indigenous peoples in Canada, such as that conducted by me as a white European woman in the Department of Modern History at a classical historical institute in Germany, the Institute of History at the University of Cologne. This addresses the third point, the issue of positionality. Since I was born as a white, German woman in reunified Germany, and continue to live there, I am not a settler in the land that is now called Canada, nor do I have any connection to the Indigenous peoples there. As such, the ethical question arose as to how I can establish meaningful research relation-

[12] I use here the term „settlers" when referring to people without Indigenous ancestry. It includes both settler populations with European descendance and diverse non-European immigration groups who make a home in Canada, but where descendants of European settlers certainly form the largest population group. The term, as such, covers a broad variety of people who have different ties to and standings within Canada's settler colonial history, and hence hold different perspectives on this history, but who nevertheless benefitted from the displacement of Indigenous peoples from the land.

ships with Indigenous peoples in Canada, especially with Indigenous individuals who have experienced the residential school system, when my position as a researcher is alienated from Indigenous communities and their ways of being in and knowing the world. How can I develop meaningful research relationships with Indigenous peoples when my research is situated in the Western European academy which Indigenous peoples associate with the long history of colonial domination and assimilation, or as Māori scholar Linda Tuhiwai Smith has put it, where „colonial education" became „the major agency for imposing […] positional superiority over [Indigenous] knowledge, language and culture" (Smith 2021: 73)?

I will elaborate on these methodological, institutional, and ethical challenges involved in my work in more detail below. In response to these complexities, I decided on a methodological approach that combined historical-research approaches in the form of archival work with ethnographic-research methods. I will draw here specifically on the oral-history methodology I have chosen, which was inspired by ethnographic and decolonizing research considerations, as well as scholarly works on oral history and Indigenous works on how research should be conducted. My oral-history research implementation was particularly inspired by the works of oral history scholars such as Linda Shopes (2006), Michael Frisch (1990), Valerie Yow (1995; 2016), Paul Thompson (2000), William Schneider (2011), and Brian Calliou (2015), and by Stó:lā scholar Jo-ann Archibald Q'um Q'um Xiiem's framework of „Indigenous storywork" amplified on the following pages (see Archibald 2008). The article is written in the form of a personal reflection, dedicated to illustrating the ethical and methodological difficulties in relation to my work and positionality. It should be understood as a proposal that offers methodological ideas for non-Indigenous students and scientists who want to study Indigenous histories in North America and in other settler colonialist nation-states from Europe. I also focus here only on the process of how I put my oral-history research into practice during my research stays in Canada. For reasons of space, I will not refer here to how I then prepared and made accessible the oral sources in my writing process, although this is also an important question methodologically. I also refer my methodological and ethical considerations primarily to the historical discipline in which my doctoral research is situated, but I claim that the considerations I have made may also be relevant to other disciplines.

Methodological-Institutional Complexities

Indigenous Studies as an independent academic discipline in the form of research centres, departments, faculties, and programs developed from the late 1960s onwards at universities in North America and other settler colonialist nation-states such as Australia and New Zealand (see, e.g., Moreton-Robinson 2016; Andersen 2021). Originating from the scientific discipline of anthropology, the development of Indigenous Studies was significantly influenced by the global upheavals and political-social movements that emerged after the Second World War. The proclamation of the Universal Declaration of Human Rights in 1948, the civil rights movement in the United States with its historical high phase in the late 1950s and 1960s, the decolonization and independence movements in the European empires' (former) colonies, and the Indigenous sovereignty movement in the 1960s and 1970s all created a public social platform for Indigenous peoples, African Americans, women, gays and lesbians to bring their experiences of marginalization and demands for the recognition of their rights into public and political discourse (see Moreton-Robinson 2016: 6). The Indigenous sovereignty movement was particularly concerned with the defense of Indigenous lands, the assertion of Indigenous peoples' sovereignty on their territories due to their primordial attachment to the land, and the recognition of Indigenous epistemologies flowing from their place-based existences and relational connectedness to the land and its living context (see Moreton-Robinson 2016; Alfred/Corntassel 2005; Corntassel/Bryce 2012). Settler colonialist universities became a central reference point in this discourse as Indigenous intellectuals and scholars „entered university in unprecedented numbers" (Moreton-Robinson 2016: 7) to emphasize Indigenous worldviews, knowledge systems, and pedagogies grounded in past and enduring relationships with the land, thus bringing Indigenous epistemologies back to centre stage (see ibid.;

Moses 2010; Moses 2011). The Indigenous intellectual sovereignty movement was hence always activist, guided by the impulse to challenge Western scholarship and knowledge production, which were understood to be an instrument of colonial domination – or, as Mi'kmaq professor Marie Battiste has put it, it was an intellectual fight against the „cognitive imperialism" of Western academia, which had silenced, marginalized, and excluded Indigenous voices and had followed „the imposition of one worldview on a people who have an alternative worldview, with the implication that the imposed worldview is superior to the alternative worldview" (Battiste 2000b: 192f.).

Not only Marie Battiste but also other Indigenous intellectuals such as Māori scholar Linda Tuhiwai Smith (2021), Plains Cree/Saulteaux Pasqua First Nation scholar Margaret Kovach (2021), and postcolonial theorists such as Edward W. Said (2003) have underlined in their standard works how Western European knowledge production and retrieval were embedded in the imperialistic and colonial project of racial superiority, exploitation, and Indigenous marginalization. In this historical colonial project, universities „were established as an essential part of the colonizing process, a bastion of civilization and a sign that a colony and its settlers had 'grown up'" (Smith 2021: 74), and the „negation of Indigenous views of history was a critical part of asserting colonial ideology, partly because such views were regarded as clearly 'primitive' and 'incorrect' and mostly because they challenged and resisted the mission of colonization" (ibid.: 33). Hence, Indigenous Studies take an „endogenous approach" (Moreton-Robinson 2016: 8) by aiming to revitalize Indigenous peoples' own stories, versions of history, and ways of being in and knowing the world, which are rooted in relational connectedness to the land and the universe, and thus, as Moreton-Robinson has pointed out in relation to Indigenous Studies in North America, differ „from the traditional disciplines that pursued exogenous studies of Native American communities" (ibid.: 7).

Today, Indigenous Studies captures a multitude of related disciplines under its umbrella, such as Native Studies and First Nations Studies in Canada, Native American Studies/American Indian Studies in the United States, Native Hawaiian Studies in Hawaii, Māori Studies in New Zealand, Sámi Studies in Scandinavia, Aboriginal Studies in Australia and so forth (see ibid.: 8; Andersen 2021: 19 (footnote 2)). All these disciplines are context-specific, geographically bounded, and dependent on their institutional context, informed by the distinct Indigenous cultures, languages, and knowledge systems that they address. Yet, all of these disciplines are not only associated with the umbrella of „Indigenous Studies", but they also represent disciplines in their own right. Therefore, *Indigenous* in Indigenous Studies is merely a generic term that refers to an Indigenous place-based existence that preceded the implementation of colonial projects and refers to a common experience with colonialism shared by Indigenous peoples around the world, but it does not reflect the *different* relationships to place, cultural traditions, and histories of these communities across the globe. In other words, the term is a connector that holds the related fields together, but it is not per se a term that Indigenous peoples use for self-identification to describe themselves and their relationships to place. Indigenous Studies is thus deeply „multidisciplinary, multinational, and multicultural" (Moreton-Robinson 2016: 8), and continues to be an emerging, dynamic field of inquiry, with diverse debates about *what* Indigenous Studies is and *why* it is the way it is. It „is served by several journals […] that were established and are controlled by Indigenous people" in several countries and work together with „several publishing houses" for the publication of monographs and collected editions (ibid.: 9). Furthermore, Indigenous Studies is guided and influenced by „Indigenous professional associations and research centres, which organize research-related activities as well as conferences to enable intellectual engagement and the formation of national and international networks" (ibid.). The most important conference in the field is certainly the conference of the Native American and Indigenous Studies Association (NAISA) which annually attracts hundreds of Indigenous scholars and „a small number of non-Indigenous scholars" from across the globe (ibid.).

While Indigenous Studies has been established in its own expanding research centres and departments across North America and other settler colonialist nation-states, such an institutional context is largely missing in Europe, particularly in the German-speaking countries. The absence of Indigenous Studies in the German-speaking academic landscape can cer-

tainly be traced back to the different histories of the German-speaking countries and the settler colonialist nation-states in North America, Australia, and New Zealand. While Indigenous Studies developed out of the experience of settler colonialism, whose central characteristic trait was the frontier between newcomers (settlers) and Indigenous peoples, such a frontier and direct territorial encounter with Indigenous peoples did not exist in the German-speaking countries. Consequently, a development of the discipline of „Indigenous Studies" was not obvious there. In the German-speaking countries, most programs dealing with the histories, cultures, and epistemologies of Indigenous peoples in settler colonial contexts are located in the departments of literary studies, such as departments of English and American Studies, or in departments of ethnological institutes, or are anchored in area studies, such as American Studies or Canadian Studies, which cover a broad field of research related to the foci of their geographic areas, including research topics related to Indigenous peoples. There are undoubtedly professional networking opportunities for scientists in these disciplines and fields, for instance, through the various area studies associations, such as the Association for Canadian Studies in German-speaking Countries, or through the „American Indian Workshop" as the main research platform and annual conference for scholars in Europe whose research is concerned with the histories and cultures of Indigenous peoples of North America.

At the historical institutes in the German-speaking countries where my doctoral research was institutionally located, however, the study of the history of Indigenous peoples of North America and of their cultures, epistemologies, and research methods is rather a marginal issue in the departments of North American History and Modern History, apart from a few exceptions. This institutional reality has methodical implications for historians such as myself whose research is concerned with this specific aspect of North American history, i.e., the histories and cultures of Indigenous peoples in North America, here particularly in Canada. In other words, the challenge that automatically arose in my research methodology was to methodically address the two complexities outlined here: *firstly*, the complexity of an institutional context in German-speaking countries that lacks a departmental structure that recognizes Indigenous Studies as a discipline in its own right, and in which the study of the history of Indigenous peoples in North America and other settler colonialist nation-states tends to be a marginal topic at the historical institutes and in their departments of North American History and Modern History. Certainly, I could have countered this issue in that I could have included an Indigenous scholar from the field of Indigenous Studies or an Indigenous scholar working in the field of history on my PhD supervisory team. However, the guidelines of my Graduate School, through which my doctoral studies were financed, stipulated that within the first six months of my doctorate a supervisory team of three professors had to be appointed, of which at least one should belong to the University of Cologne, and it was difficult to establish a supervisory collaboration with a scientist from the field of Indigenous Studies within such a short time frame. At this early stage of my PhD, I had not yet established any research collaborations in Canada, and contacts had not yet been sufficiently made, let alone funds raised to establish research contacts on the ground in Canada. Of course, I could have changed my supervisory team at a later point in time. However, this would have caused delays in the course of my doctorate, since new agreements would have had to be made with new supervisors with regard to the direction of my work – delays that I could not afford with a scholarship term of three years to finance my doctorate. Due to these structural formalities and time limitations, I ultimately refrained from including an Indigenous scholar in my supervisory team – unfortunately, I learnt that it is often the supervision and examination structures at the academy and the associated financial limitations on the duration of research projects that stand in the way of decolonization efforts.

Secondly, there is the long history of colonial knowledge production in Western academia in which non-Indigenous, white scholars wrote about Indigenous peoples without hearing their stories and versions of history, but by defining their existences and histories through the preconceived white gaze of racial superiority. This is not to say that Indigenous intellectuals have not had an impact on academic historiography from the late 1960s and have not contributed to changes in historiography with their interpretations of settler colonial history, which can be seen as resistant responses to the colonial policies

of Indigenous oppression and assimilation and as expressions of Indigenous survival and revitalization (see, e.g., Brownlie 2009). In fact, beginning in the late 1960s, academic researchers began to pay more attention to Indigenous understandings of history by incorporating Indigenous perspectives on colonial history into their works (see ibid.; Miller 2017: 62-65). As the Canadian historian James R. Miller has stated, „[t]he irrelevant or passive Indian was replaced by a more assertive and effective Indigenous agent of historical change" (Miller 2017: 63). However, the shift in historiography toward a more inclusive history that takes Indigenous historical perspectives into account has been a lengthy process and has not been able to hide the fact that the structures in academic historiography have continued to be characterized by structures of power inequality and the marginalization of Indigenous methodologies and pedagogies, not least financially. Thus, despite decolonizing trends and efforts in Western scholarship since the late 20th century to acknowledge Indigenous voices, histories, and epistemologies from around the world, I continued to be viewed by some Indigenous interviewees in the field as representing the Western scientific landscape with its history of exploitative, colonizing knowledge production that has had profoundly destructive consequences for Indigenous peoples around the world. How, then, will the methodological-institutional complexities described above affect my ethical engagement?

Reflections on Positionality and *Ascriptions* of Positionality

In order to address the methodological-institutional complexities outlined here, I saw scholarly engagement with Indigenous research paradigms as a necessary prerequisite for beginning the methodological orientation of my work. In doing so, I drew strong inspiration from the idea of relationality, which is central to Indigenous research pedagogies and holds that all living beings and things are interrelated and interdependent. Retired professor Stan Wilson, who is an Elder and member of the Opaskwayak Cree Nation, explained this idea by means of the Cree word *wahkotowin*, meaning that „we are all related":

> „'Wahkotowin', we as human beings are in a relationship. But it extends beyond the idea of relationships between and amongst humans, human beings. It includes all other living things, all other living beings. And in that relationship we are accountable for our words and our actions in that relationship to keep a balance" (Wilson in Schellhammer 2022).

Similarly, Opaskwayak Cree scholar Shawn Wilson has emphasized:

> „Identity for Indigenous peoples is grounded in their relationships with the land, with their ancestors who have returned to the land and with future generations who will come into being on the land. Rather than viewing ourselves as being in relationship with other people or things, we are the relationships we hold and are part of" (Wilson 2008: 80).

Figure 4.2: The land and its entwined relationships between human and non-human beings are central to Indigenous approaches to relationality. Manitoba, 2016. (Photo: Lena Rüßing)

In other words, it is not only the embeddedness into a „web of relationships" (ibid.: 81; see also Figure 4.2) that is important in Indigenous research paradigms, but also the accountability that is held in and for these relationships. As Stan Wilson writes, „our relational responsibilities require that we incorporate them into our research methodologies" (Wilson/ Schellhammer 2021: 87).

In following the path of *wahkotowin*, the question of my own positionality comes to the fore: specifically, what is my position in relation to the place (land) and people I am researching, and how is that going to impact my research methods and what are the accountabilities that arise from these relationships? As a white, Western European German woman with no original connection to Indigenous peoples in Canada and the land – in other words, with no original connection to the living context of place and people – one could argue that it is better not to engage with Indigenous peoples and their histories so as not to speak for Indigenous peoples and run the risk of instrumentalizing their histories from an alien perspective that does not have the experience and background of the community. This, however, would be a methodical call for avoidance stating that non-Indigenous white scholars do not need to pay attention to Indigenous peoples and their histories as it is not their „business" to write about them. This, I argue, would be a continuation of a Western academic discourse that follows patterns of colonial sidelining, rejection, and marginalization of Indigenous voices and histories. I agree with literary studies scholar Sam McKegney (2007: 42-45) here that avoidance based on an interrogation of our own positionality and the resulting recognition, in my case, that I am „white" cannot be the path forward if we are to honestly engage with colonial history and its legacies, and if we are to understand them as a shared responsibility to be addressed, although the responsibilities in relation to that history are certainly different for the „colonizers" than for the „colonized". Thus, I saw my position in relation to my research and the accountability that arose from this position as a commitment to the avoidance of neglect by drawing on and emphasizing Indigenous residential school histories, voices, and knowledges in my work, but in a way that dealt respectfully with these histories and perspectives and acknowledged clear limitations.

I understood *wahkotowin* in my research as the acceptance and acknowledgment of limitations, both in my spoken and written words: limitations in the way I access, convey, and analyse Indigenous ontologies, epistemologies, and histories because of my German-European background alienated from an everyday Indigenous community context; limitations in the way I write about Indigenous histories and cultures, specifically in English as the language of research representation that has had a deeply destructive influence on the suppression and elimination of Indigenous languages and knowledge systems that are intrinsically tied to oral transmission; limitations in the way that my work remains anchored in Western scholarship, with all its still-dominant institutional power structures from which my doctoral work has benefitted, not least financially.

Nevertheless, despite these limitations, I think there are good reasons why it is important to take a look from the outside, as I am advocating here with my commitment to avoid negligence. Firstly, a „view from the outside" helps to counteract the essentialization of race or ethnicity. To be clear, this is not an argument to undermine Indigenous scholarly discourse, which has underlined that the assertion of an Indigenous „essentialism" is crucial for Indigenous peoples who, unlike peoples in other locales, still live under ongoing conditions of settler colonial domination, dispossession, and marginalization since, as the Australian historian Patrick Wolfe has put it, „settler-colonizers come to stay" (Wolfe 2008: 103). Against the background of the structural specificity of settler colonialism in the form of settler permanence, Indigenous scholars, such as Crow Creek Sioux Nation professor Elizabeth Cook-Lynn, have emphasized that Indigenous essentializations are „in fact, a defensible notion, that Indians must fight off domination by outsiders in order to make themselves heard within their own experiences" (Cook-Lynn 1997: 20; further, see, e.g., Coulthard 2014; Simpson 2014). My argument is not to deny the need, stressed by Indigenous peoples, to draw on strategies of essentialism as an element of resistance to ongoing patterns of settler colonial domination. Rather, my argument is along the lines that an „external" perspective, in this case a non-Indigenous one, can sharpen or reveal things that have become so self-evident to Indigenous communities because of their destructive experiences with settler

colonialism that they no longer need explaining. Here, the „view from the outside" can help to revisit certain key topics and make them accessible to a wider audience.

A second argument for the importance of a „view from the outside" is that such a perspective opens up the possibility of sharpening the eye for the ambiguities and complexities of positionality. It opens up an empirical perspective on how attributions of positionality are made and negotiated in the field. In my case, my positionality as a non-Indigenous, German woman was negotiated differently in the field. I received no response to many e-mails I wrote to Indigenous people and Indigenous residential school activists during the course of my research requesting an interview. I learnt over time that many Indigenous people cannot and do not want to talk about the traumatic experiences they have had in the residential schools, especially not with people they do not know and who are not part of their community. Who wants to share with a stranger about their devastating experiences of sexual, physical, cultural, and emotional abuse? Here, the situation is no different from other contexts where people and groups have been subjected to massive violence and where it is difficult to report what has been experienced.

In addition, Indigenous people with whom I conducted interviews about their Indian residential school experiences and their role in the process of coming to terms with the history and legacy of the schools offered to use their contacts in the Indigenous „world" to put me in touch with other Indigenous individuals. Often, however, this offer would end with them telling me that the people they had approached for me were not willing to talk to me. I was told that they were tired of being constantly „researched" by non-Indigenous people who write „about" them and that there is a distrust of researchers representing Western science institutions since Indigenous people often have no influence on what will happen to their oral histories in such contexts. By these individuals, I was thus seen as a representative of exploitative Western science, which had been involved in colonial regimes and whose legacies are still reflected in scientific structures and related inequalities today.

However, this is only one side of how my positionality was negotiated in the field. The other side is that it was precisely due to my European, German origin that interviews with Indigenous people came about. I was perceived as „interesting" and „different", as an outsider who was neither categorized as a „settler" in the land called Canada today nor as „indigenous". I was perceived as „sitting outside", so to speak, as a neutral person not being involved in Canada's settler colonial history and its legacy. Some former residential school students I interviewed saw the interview as an opportunity to make their residential school experiences accessible to European and German audiences by bringing their stories – via the interview – across the Atlantic. Others were interested in why I, as a German, wanted to learn about the experiences of Indigenous peoples in Canada; after all, my country had no „frontier history" with Indigenous populations. It was interest in the motivation of my research, I learnt, that made them participate in my oral history research.

In summary, it was the „view from the outside" that presented me with the ambiguities of positionality. The different attributions of positionality that I encountered in the field influenced whether or not I was able to conduct interviews, and whether or not I could establish oral history relationships. For me, the accountability associated with *wahkotowin* in this context meant acknowledging these complex attributions of positionality and dealing respectfully with the research relationships they created. Reflecting on my position in relation to my research led to a commitment not to evade and divert focus from Indigenous peoples' histories and cultures, but to engage respectfully by listening to Indigenous people as a non-Indigenous, white researcher, interacting with them, and learning about them and from them in direct encounters. Yet *wahkotowin* for me also meant accepting the limits of relations in cases where oral history accounts did not materialize due to a certain ascription of positionality, in my case, that I was seen as a representative of an exploitative Western science.

Reflections on my Methodology of Oral History

To follow the relational accountability involved in *wahkotowin*, specifically the responsibility for the „web of relationships" (Wilson 2008: 81) that my work finally established – not only relationships to

place and people, but also the relationship I held with my ideas, research questions, and methods –, it became clear to me that I did not want to make what Jana Sequoya called „the alienated forms of archive material" (cited in McKegney 2007: 45) the exclusive source of my historical work. The archives that were to be consulted for my research on Indian residential schools and the associated reconciliation discourse were mostly government and church archives whose sources represent the colonial patterns of knowledge production and appropriation. Certainly, the written sources in these archives can be read, in the words of Ann Laura Stoler, „against their grain" (2002: 99) to identify lines of silences, exclusions, and expressions of Indigenous resistance to and refusal of the settler colonial endeavour of residential schools and other settler colonial politics. However, the undertaking of reading sources „against their grain" cannot put aside the fact that Western-colonial archives complied with the powerful regularities, technologies, and conventions of the colonial regimes which considered Indigenous voices as an excludable, inferior, and an omissible source of knowledge (see Stoler 2002; see also Tappe in this volume).

Thus, besides my archival labour, which eventually also included two Indigenous-controlled archives, I particularly pursued an oral history methodology inspired by the scholarly discourse on oral history to democratize and decolonize the historical record by „making the telling and writing of history more inclusive" (Freund et al. 2015: 3). While the early oral history projects in the United States in the period immediately after the Second World War concentrated on the perspectives of „political, economic, and cultural elites" (Ritchie 2011: 4), a „‚bottom up' approach" (ibid.: 5) increasingly took hold in the 1960s and 1970s in both Europe and North America, influenced primarily by the works of social and cultural historians and leftist political and social movements that opposed the prevailing bourgeois-elitist historiography (see Ritchie 2011; Obertreis 2012; Freund et al. 2015). This „‚bottom up' approach" (Ritchie 2011: 5) was intended to include and centre the voices of those previously absent from the historical record. The focus thus fell on the voices of women, African Americans, lesbians and gays, workers, migrants, Native Americans, and other groups that „had been ‚overlooked' or ‚oppressed'" (ibid.: 4; further, see Obertreis 2012: 8-9). The aim was to give these social groups and minorities far away from power a voice they had not had before, and in this way to work towards a more democratic and inclusive historiography. As Paul Thompson, the best-known representative of British Oral History, has stated in the preface to the first edition of his seminal book *The Voice of the Past*: „the richest possibilities for oral history lie within the development of a more socially conscious and democratic history" (Thompson 2000: vi).

My oral history research followed these considerations of a more inclusive and democratic historical record. In an ethnographic sense, my oral history research opened up the space to live *wahkotowin*; that is, to build meaningful research relationships, not only with the individuals I interviewed, Indigenous and non-Indigenous alike, but also with the land and the living context in which the oral stories were embedded and by which they were influenced. It was an approach to oral history as *mobility* in the form of traveling through the historic field. In 2016 and 2017, I spent many months traveling across Canada (a year in total), each time starting in the province of Ontario in the east of the country and ending on the west coast in British Columbia, to build research relationships and get a sense of the reconciliation discourse and its trajectories in the country. During these journeys, I not only spoke with a variety of stakeholders who were and are involved in addressing the history of Indian residential schools in different regions of the country, but also conducted 24 semi-structured interviews with Indigenous leaders, former residential school students, government representatives, church officials, lawyers, and others who have been involved in the establishment and work of various instruments to address the settler colonial history of residential schools and their legacies, such as the Truth and Reconciliation Commission. Through the principle of mobility inscribed in my oral history research in that my research was not limited to a specific region or area, I had the opportunity to connect with people in different regions and communities.

In a decolonizing sense, my oral history methodology provided the space for the acknowledgment of Indigenous oral traditions. It is through oral histories that Indigenous understandings about the past and present are transmitted, preserved, and disseminat-

ed.[13] Indigenous oral histories can be expressions of resistance to colonialism and its continuing legacies, as well as a counter-narrative to and reinterpretation of publicly dominant historical accounts, and they can also be expressions of the revitalization of Indigenous cultures, spiritualities, sovereignty, and treaty rights (see Bauer 2017; Archibald/Lee-Morgan/De Santolo 2019; Archibald 2008). As Stó:lā scholar Jo-ann Archibald Q'um Q'um Xiiem et al. have stressed, „like all peoples, our stories were part of articulating our world, understanding our knowledge systems, naming our experiences, guiding our relationships, and most importantly, identifying ourselves" (Archibald/Lee-Morgan/De Santolo 2019: 5). Thus, by adhering to oral history's maxim of democratizing the historical record by centring voices that have been marginalized, oral history can provide an opportunity to bring Indigenous stories to the centre as voices that have been ignored in the classical historical record for far too long. However, Indigenous oral histories are still considered in Western historiography as a supplement: nice to have, but complementary to the „real" research reflected in the use of written sources. Western historiography sometimes finds it difficult to accept the validity of these oral sources, seeing them more as hearsay than a source that provides „true" facts about the past (see Johnson 2005; Calliou 2015). In the context of global decolonization efforts, the historical science will have to continue to grapple with what it wants to admit as a valid source, and above all, with the question of *who* decides what is considered worth knowing and a valid source?

Reflections on Ethical Principles

Indigenous research pedagogies point out that research methods must be based on ethical principles that must be related to place and community and clarify the researcher's responsibility to place and people – a responsibility that also applies to researchers like me who are „sitting outside" because they are not a settler of the land, nor do they have an Indigenous background. My research drew much inspiration from Stó:lā scholar Jo-Ann Archibald Q'um Q'um Xiiem's framework of „Indigenous storywork" – a theoretical, methodological, and pedagogical framework to create an ethical research methodology to establish ways of working with both Indigenous peoples and Indigenous knowledges, histories, and stories (Archibald/Lee-Morgan/De Santolo 2019; Archibald 2008). To be clear, I did not intend to make Jo-Ann Archibald's framework my own, but rather to see it as an inspiration to relate it to my own oral history methodology and positionality by reflecting on how the ethical principles contained in this framework come to bear in my own work. The framework of „Indigenous storywork" is based on seven principles: respect, responsibility, reciprocity, reverence, holism, interrelatedness, and synergy (Archibald 2008: 1; Archibald/Lee-Morgan/De Santolo 2019: 1). I have incorporated these principles into my oral history methodology, not only for interviews with Indigenous people, but also with non-Indigenous individuals, because in my view these principles should apply to all living beings and things that are involved in research. At the same time, I have linked Jo-Ann Archibald's framework of „Indigenous storywork" to ethical and methodological considerations discussed in scholarly work on oral history and conducting interviews (see Yow 2016; Yow 1995; Shopes 2006; Frisch 1990; Schneider 2011; Calliou 2015).

Respect in my oral history work meant showing equal respect to each story heard and to the person who shared the story. Each oral history account was important and had equal value in my work. Respect implied creating interpersonal relationships in which oral histories could be shared and conveyed, and learning not only about the cultural and community context in which the oral history account was embedded, but also about the interviewee's belonging to place, land, institutional context, and culture. My interview partners had diverse belongings: belongings to church, government, and First Nations communities, mixed belongings to Indigenous and non-Indigenous „worlds", and belongings to specific institutions, organizations, and commissions that were involved in the addressing of settler colonial

[13] Although Indigenous peoples today also rely on written language, using Western technologies among other means to communicate their knowledges and teachings, the oral tradition and its revitalization after the destructive effects of colonization is a central concern of Indigenous peoples in their efforts to restore their communities (see, e.g., Corntassel et al. 2009).

history in general and residential schools specifically. In each interview setting, the individual had to be perceived in his or her social and socio-political location by expressing respect in a context-specific manner. While my interviews with First Nations Elders often involved small gifts of money and the sharing of tobacco[14] as a sign of respect (see here also Calliou 2015: 34), interviews with government and church representatives required me to show the necessary respect to individuals according to their respective official functions. Thus, for example, I had to comply with visitor and security protocols for interviews I conducted with government officials in the buildings of the Canadian Parliament in Ottawa.

Responsibility meant the preparation of ethical approvals and their submission to university research ethics boards, in my case to the ethical board of the Global South Studies Center at the University of Cologne, to comply with the legal standards provided in Canada for research involving human subjects and to inform my interviewees about the purpose, objectives, and methodology of my work, including explaining my own background and position.[15] Although ethical approvals are required by university ethics committees, it is pivotal to consider their limitations, which are manifested in the fact that communities involved in the research often do not have an institutional voice in the development of these ethical-approval documents, nor do these documents provide any information about enduring and sustainable research relationships, as their focus is only on monitoring research responsibilities in advance, not after research has begun.[16] In addition, I provided each interviewee with an interview-consent form. It explained the interviewee's rights as to confidentiality, anonymity, and voluntariness and gained the interviewee's permission, recorded by signature, that his/her interview or portions thereof could be used by me in my dissertation and further publications.[17]

With regard to the interview-consent form, two aspects were interesting: firstly, each interviewee wanted to be referred to by their full name; no one wanted to remain anonymous. My interpretation is that by mentioning their name in my research, the interviewees were linking recognition of their identity and their knowledge of the history of Indian residential schools and the process of addressing this history. Furthermore, some former residential school students I spoke with who shared their destructive experiences in the schools did not want these stories to remain „nameless" but to be linked to a concrete human fate. In turn, one Elder I interviewed in The Pas, northern Manitoba, linked the mention of her name to the recognition and visibility of the research relationship between me and her. As she stated in conversation with me: „Please use my name in your research – as a sign of a friendly partnership." I later learned through the work of Shawn Wilson (2008) that it is often common in Indigenous research methods to use names, especially when doing research with Elders. As Wilson explains: „Because you are talking about a special relationship that you have. Especially with Elders, that's a special relationship. And then you are also saying that they are passing on knowledge that they are entitled to pass along. And in a sense they are giving you permission to put that in your thesis, so if you are leaving out their name, how can people know that you have the authority to present this information? You are not respecting the relationship you built with that Elder or with the knowledge that they've shared with you" (Wilson 2008: 115).

The second aspect to consider in relation to the interview-consent form is that while all Indigenous interviewees signed the form, this does not mean that they told me a „complete" story in the interview. Certain oral histories that relate to the cultural context of the community are controlled by tribes who „actually own the stories, tell them on special occasions, and consider them property" (Schneider 2011: 58). William Schneider has noted in his work

[14] In many Indigenous teachings, the smoke from burned tobacco has a purpose of carrying thoughts and prayers to the spirit world and also has a healing effect by ridding a place of negative spirits.

[15] My ethical approval was issued in 2016 at the Global South Studies Center at the University of Cologne as part of the DAAD thematic research network „Remapping the Global South".

[16] Madeline Whetung (Nishnaabeg) and Sarah Wakefield have written an interesting article here about the challenges they experienced in addressing institutionalized research-ethics policies, protocols, and processes at the University of Toronto (see Whetung/Wakefield 2019).

[17] For the importance of consent forms/release forms, see also Yow 2016: 156 and 174 („Checklist for critiquing interviewing skills"); Shopes 2016: 138 and 141-145.

on interviews in cross-cultural settings that „in the Western tradition, we ask the individual who is recorded to sign a release form to make the recording available to the public, but we also recognize that the individual may not be able to speak about certain topics that are controlled by their cultural group" (ibid.: 57). Although tribal ownership did not play a large role in my work, as I did not conduct oral history research in and with a specific community, it is nevertheless important to note that some of my Indigenous interviewees may not have been able to talk about all facets of the residential school issue, such as how the legacy of the schools has impacted their community or how it is discussed within their community, because these accounts may be reserved for their tribe.

Working in accordance with the principle of *responsibility* also required having preliminary conversations with my interview partners prior to the interview (see also Yow 2016: 153f.), either in person or via e-mail and phone, to ensure their informed consent to the interview and to establish what Shawn Wilson (2022) has called a „safe space" from which research relationships can be developed and put into practice.[18] In the light of the painful residential school memories former residential school students shared with me in their interviews, a „safe space" in a physical sense was important. Places chosen by the interviewees included, for instance, living rooms, yards, tribal offices, library rooms, hotel meeting rooms, small cafés, and cabins in the bush. It was in this ethnographic sense that my research came alive on the land, specifically in these „safe spaces" chosen by the interviewees, where stories and knowledge were shared in relation to place, community, and past and present history.

Finally, the principle of responsibility required attention to cultural protocols in conducting interviews (see Calliou 2015). The consideration of cultural protocols was particularly important for the research visit I undertook in July 2017 to northern Manitoba, where I was invited by members of the Nisichawayasihk Cree Nation. The visit also provided me with the opportunity to connect with members from the neighbouring communities, the Opaskwayak Cree Nation located near The Pas and the Pukatawagan First Nation (also known as Mathias Colomb (Cree) First Nation). Cheryl Linklater and Colleen Hunter†, both members of the Nisichawayasihk Cree Nation, served as intermediaries between me and the interview partners. They also provided me with the assistance necessary to adequately follow the principles of *responsibility, respect,* and *reverence* in interviewing. *Reverence* here meant treating interview relationships and the knowledge shared in the oral narratives with respect and learning to engage with oral histories and life experiences without violating cultural protocols and community ethics. A gift of tobacco and small monetary gifts were the protocols for me to follow – they served as a sign of respect and reverence for the knowledge, history, and experience shared in an interview, and demonstrated gratitude for the knowledge imparted in the oral account, and they are, as mentioned above, especially offered when seeking the knowledge of and corresponding with an Elder (see here Calliou 2015: 34).

The principle of *synergy* inherent in my oral history methodology was expressed not only through the creation of relationships to place and people, but also through the building of connections between these relationships. Some interview partners reacquainted me with other interview partners, and thus synergy meant thriving on this web of relations and holding these relationships with respect and responsibility. The synergy effects unfolded through what can be described as „being there": being at community cultural events, taking part in cultural activities out in the woods, such as sweat lodges[19], visiting Indigenous community and health centres, being at conferences, in archives and museums. Through this „being in the historic field, being there", oral history relations could be created, which unfolded synergies in such a way that new relations to new individuals arose from them. It was, so to speak, a snowballing method that came to fruition in a concept of „shared

[18] According to Shawn Wilson, building a „safe space" in Indigenous research methods encompasses all dimensions of research, including the writing process, the way research data are collected and generated, the relationship between the researcher and his or her research participants, and the responsibility for the knowledge shared by those involved in the research (see Wilson 2022).

[19] The sweat lodge ceremony is practiced by many Indigenous peoples across North America. It is an Indigenous religious ceremony for prayer, healing, and purification. Its concrete structure and process differ from nation to nation, depending on their respective cultural protocols and the specific purpose to which the ceremony is dedicated.

authority". The ideas about the concept of „shared authority" were developed by historian Michael Frisch (1990), who discusses oral history as a „shared" endeavour between interviewer and interviewee, where issues of „authority" must be negotiated between interviewer and interviewee with regard to the conduct, interpretation, dissemination, and responsibility of oral history. In conducting my interviews, I entered this space of „shared authority" by working with my interviewees as they made contacts for me with other individuals they knew. Interviews often came about only because I was introduced by an interviewee to another person whose insights were assumed by the interviewee to be important to my work. In Winnipeg, for instance, I conducted an interview with a former residential school student who is a grassroots activist working for reconciliation between Indigenous and Settler peoples in the province of Manitoba, and across Canada. She, in turn, put me in touch with another Indigenous grassroots activist in Winnipeg with whom she was friends. In Toronto, I interviewed a former representative of the Assembly of First Nations (AFN), the national Indigenous organization that represents the interests of First Nations individuals and communities in Canada. He, in turn, put me in touch with other individuals who had been instrumental in setting up the Truth and Reconciliation Commission.

In addition, I also cooperated with people who were not part of the immediate interview setting, but who had an influence on the setting insofar as they played a part in the interviews coming about at all. For example, the interview with the grassroots Indigenous activist in Winnipeg came about thanks to an archivist in the Canadian Museum for Human Rights in Winnipeg who introduced me to her. In Orleans, a residential district on the east side of Ottawa, I interviewed a former residential school student who had worked for the Truth and Reconciliation Commission as a member of its survivors committee. Because he suffered from health problems, the interview with him was possible only thanks to the support of his wife and his son, who helped to organize the interview, and thanks to the presence of his wife during the interview. Thus, the space of „shared authority" in my oral history research was expanded to include individuals who did not play a *direct* role in the interview setting by being interviewed, but who nonetheless had an influence or „authority" on the interview situation because they were instrumental in making interviews happen and possible in the first place. As such, synergies unfolded via different people and places.

The principles of *reciprocity* and *interrelatedness* are important in an oral history methodology because the narrator and the listener are connected in the created space of the interview through the act of sharing, listening, learning, and understanding. From the reciprocal nature of the interview, responsibilities arose for me as a listener in relation to the use, interpretation, conveyance, and dissemination of the oral history account. The interviews I conducted were tape-recorded to ensure a verbatim reproduction, unless the interviewee indicated that recording was not desired (something that no one indicated in my research). All audiotapes remained with me as the researcher to ensure confidentiality and protection. The principle of reciprocity also requires that interview relationships be maintained and that the research be made available to the participants, not only at the time of the interview, but also beyond it (see here also Shopes 2006: 153). Specifically, for my research, this means informing my research participants about what exactly will be published from their interviews in my work. However, this is not an easy task as Western research paradigms and methods are based on the principle of independent, neutral research, which should maintain a distance from those with whom and about whom it conducts research. Indigenous research paradigms, on the other hand, start from the premise that „all knowledge is positioned" (Reder 2016: 7) in that it comes from the living context of land and community, which is dynamic and fluid, and they emphasize that „all knowledge is generated from particular positions, that there is no unbiased, neutral position possible" (ibid.). Knowledge rather flows from the building and maintaining of relationships with place and people, and from communicating, sharing, and interpreting knowledge in these relationships in a reciprocal way. Bringing both perspectives together in research is a balancing act, but I kept in mind William Schneider's insight: „The interview is a shared product of both the interviewer and the interviewee, and requires mutual support or it will not work" (Schneider 2011: 59). What one understands by „shared" is certainly a matter of in-

terpretation in any oral history work, and working according to a „shared authority" also presents various challenges (see, e.g., Shopes 2003). However, I share the view of Shopes (2003 and 2006), Yow (1995 and 2016), and others that the interview does not finish with the end of the conversation. An ethically responsible approach to oral histories requires that I give my interviewees insight into my interpretations before publishing my doctoral thesis as a book. In the interpretive phase of my research, i.e., the phase after the interviews in the field were completed, I left the space of „shared authority" for a time. I am aware that here, too, there are different approaches to developing interpretations in relation to what is said, for instance interviewer and interviewee working together with the oral narratives and developing interpretations together. However, in a qualifying work such as the dissertation, I considered it my responsibility to develop independent interpretations in light of what was said. I viewed the oral histories I collected as sources in which the interviewees had already availed themselves of their opportunity to interpret. Now it was my task to reflect on their interpretations and make sense of them, outside the space of „shared authority", so to speak, for a time. For the publication process of my dissertation, I now intend to re-enter this space of „shared authority" in order to engage in a mutual exchange with my interview partners about my interpretation of their oral histories. For, as Shopes has aptly put it: „the interviewee is deemed to have a certain authority over what he or she has said in an interview" (Shopes 2006: 144).

The principles of reciprocity and interrelatedness are, however, important not only from a methodological point of view, but also on the level of content. In my doctoral thesis, both principles intermingled with the analytical study of the history of residential schools and their legacies, provoking the question of how to write about residential schools. The principle of interrelatedness connected here with the principle of *holism*, as I analytically situated the schools within a broader history of settler colonialism, in which the schools became a significant element of Indigenous assimilation, and thus interpreted Indigenous residential school experiences in relation to this broader settler colonial endeavour, analysing the school history in a more holistic way. In particular, the study of Indigenous oral histories meant studying Indigenous residential school experiences not only as individual experiences, but as an intergenerational experience that impacted families and communities across generations, affecting them holistically in terms of harmful cultural, emotional, mental, and physical effects. In short, ethical principles must be reflected not only in the method, but also at the content level.

Conclusion

My response to the methodological and institutional difficulties my doctoral research faced – the extensive invisibility of Indigenous Studies at historical institutes in Germany, the long history of colonial knowledge production about Indigenous peoples, and my positionality as a white, European, German scholar with no Indigenous background – was an oral history methodology that was inspired by ethnographic and decolonizing research methods, as well as scholarly discourse on oral history that aims to make the historical record more inclusive. The oral history approach seemed to me – from the position I was researching and writing – to be the best way to engage with *wahkotowin* as it allowed me to build relationships with place and people, including Indigenous leaders, Elders, and former residential school students, and to avoid writing a dissertation alienated from the land, people, and the history of the land I was researching. In other words, it did not mean sitting exclusively in the archives and writing „my history" from there, but rather building oral history relationships with diverse people. The oral history approach also provided space for the recognition of Indigenous oral traditions and histories – voices that had been marginalized for too long in the past.

However, the oral history approach was again associated with new challenges. It required taking the focus away from archival labour and the written source and opening it up instead to ethnographic research methods. Although, as explained earlier, historical scholarship has a long oral history tradition that requires ethnographic work, historians are not familiar with ethnographic methods in the way that anthropologists are, or do not learn directly in their studies how to conduct interviews in a culturally

competent manner in the field – skills that are, however, necessary if interviews are to be conducted in cross-cultural contexts. Turning here to Indigenous research methods and ethical pedagogies is a valuable way to learn how to do oral history research in a culturally respectful and responsible manner with Indigenous individuals and communities.

Engaging with Indigenous oral history also raises the well-established question of what we recognize as a valid source, what systems of knowledge we accept and, related to this, what research methods we use, and above all, who should decide. In the face of the global climate crisis and the serious environmental, social, and political upheavals that accompany it, the Covid pandemic, a new war of aggression in Europe with Russia invading Ukraine, and the consequences of colonialism that continue to be felt worldwide, I believe the answer can only lie in a plurality of knowledge systems within which Indigenous research methodologies and epistemologies are not an adjunct but an integral part of global responses to world affairs.

An oral history method that follows decolonizing methodologies is also about reflecting on one's own place and position and how these two things influence ethical behaviour and ethical relationships. This also requires being open to and accepting different attributions of positionality that result from the lived, empirical field and that enable or even complicate and prevent research relationships. However, questioning one's own positionality – in my case that I am „white" and „non-Indigenous" – should not lead to avoidance strategies in a postcolonial context, where the prefix „post" is an indicator of the persistence of settler colonial patterns of Indigenous marginalization and white supremacy, but rather to a conscious, responsible investigation of the injustices perpetrated upon Indigenous populations. To refrain from investigating simply because one does not belong to the community in which the crimes happened or because one does not share the cultural background of the community is to perpetuate a colonial discourse of ignoring, disregarding, and excluding.

Finally, oral history research – and not only with Indigenous peoples – relies on building research relationships between the interviewee and the researcher, but especially with Indigenous peoples whose research paradigms are rooted in a deep relational approach, oral history research relies on a sustained approach that often cannot be implemented through the research-funding opportunities of Western research institutions with clearly defined funding frameworks and terms. In other words, the time required to build trust in research relationships and to make those relationships sustainable is often not provided for in Western institutional funding systems and paradigms, which can have drastic consequences for the way research is conducted methodologically. In particular for research with Indigenous peoples, for whom the relational approach is central, narrow funding frameworks can have deleterious effects. Thus, in a very realpolitik sense, and against a backdrop of ethical purpose, conducting research with and about Indigenous peoples and their histories, not only in Canada but globally, will require the staying power to move Western institutional funding systems in the direction of recognizing and addressing the need to encourage the building of sustainable and long-term research relationships.

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5. Amateur Sociology as an Escape from Domestic Gloom: Re-Analysing an Oral History Interview with Former Mass Observation Member Celia Fremlin[19]

Ole Münch

Introduction

"You were putting yourself in the way of death", Nick Stanley animatedly declared (Fremlin, 18.9.1981: 7). It was an autumn day in 1981 and his interviewee, Celia Fremlin, had just told him about her experiences during the Blitz. She had spent the early 1940s in London working as a social researcher for an organization named Mass Observation, observing and chronicling phenomena such as how people behaved in air raid shelters, or what social scenes played out on the streets in the wake of a bombing. The recording of the interview reveals that Stanley was briefly lost for words after Fremlin ended her account.[21] A cascade of follow-up questions ensued: did you really experience the Blitz at first hand? Weren't you afraid? By probing in this way with a mixture of surprise and curiosity, he prompted Fremlin to embellish her story with increasingly dramatic details, which only made her seem all the more daring: „Oh yes", she stated, „where it looked like being most dangerous, we'd head for there. When the fires started in the city and all the sky was aglow, we all leapt onto our bicycles and cycled towards the fires until we got to where the action was."

Nick Stanley was interested in stories of day-to-day research like this one – and he helped shape these accounts, in part through his manner of asking questions (cf. Maubach 2013: 31). At the time, he was writing a Ph.D. thesis on Mass Observation, an organization that enjoys great popularity today among British social scientists and historians (Savage 2010: 57). It was made up of a group of journalists, writers, and other amateurs who in the 1930s and 1940s set about revolutionizing sociology with the help of qualitative methods (Calder 1985; Hinton 2013).[22] The Mass Observers had in mind an „anthropology of ourselves" (Madge/Harrison 1937: 10). They observed and interviewed their contemporaries, and they maintained a panel of diarists who recorded their everyday lives in the service of science. The anthropologist Bronisław Malinowski was, with a few reservations,[23] full of praise for the initiative, and became one of the organization's most important champions (Malinowski 1938). The work of the Mass Observers produced an impressive arsenal of cultural historical sources, which are housed today in a dedicated archive.[24]

Another part of Mass Observation's appeal as an object of study is that its members worked outside academia. Tom Harrison, one of the project's masterminds and leaders, scorned university research as „timid, bookish and unproductive" (quoted in Pol-

[20] This article was translated into English by Jozef van der Voort (www.jvdv.net). Any mistakes, however, are my own and may have been "smuggled" into the text after his work was finished.

[21] I am grateful to Jessica Scantlebury of the Mass Observation Archive (MOA) for providing me with access to a digital version of the recording. When I quote from the interview in the following, however, I do so from a transcript read and approved by Celia Fremlin herself, which is also stored in the MOA (SxMOA32-32).

[22] See the Mass Observation agenda, as outlined in the organization's founding document (Harrison/Madge 1937).

[23] Malinowski conceded that the young organization needed a little more time to hone its theoretical approach, arguing that this was needed partly in order to distinguish relevant observations from irrelevant ones (Malinowski 1938: 84–5).

[24] See the website of the archives: Mass Observation (2015): *About Mass Observation*. Online: http://www.massobs.org.uk/about/about-mo. (Last visited: 4/2/2024).

len 2013: 213; see also Malinowski 1938: 84). However, this attitude is also one of the reasons why the project faded into obscurity after the war. Another is that statistical methods and grand theories enjoyed great favour for a time in sociology (see Hinton 2013: 372), and both were very far removed from the agenda of Mass Observation.

By 1980, however, when Nick Stanley was working on his Ph.D., times had changed. A shift was in progress in various academic disciplines that has since been dubbed the „cultural turn". People began to take an interest in culture – in how people lived their everyday lives and understood themselves and their life-worlds (Sheridan et al. 2000: 38-39, 95-106; see e.g. Nash 2001: 78–82; Reckwitz 2003: 287-288). The pioneers of this movement included Howard Garfinkel's circle of ethnomethodologists (Reckwitz 2003: 283-284; Weingarten et al. 1976), whom Nick Stanley explicitly compared with the Mass Observers (Stanley 1981: 258-263): „What the anthropologist, ethnomethodologist or reader of M-O accounts obtains in good accounts is the way the natives conceive of their own world" (ibid.: 262). In his Ph.D., Stanley sought to demonstrate that the new (cultural) sociological currents had a British forerunner that offered some methodological lessons (ibid.: ii). Fortunately, many of those who had worked for Mass Observation were still alive and willing to be interviewed at this time. They told Stanley about the inner workings of their organization and produced narratives that historians and social scientists have drawn on heavily ever since.[25] But what exactly do these texts reveal?

Anyone using Stanley's interviews as a source, no matter for what purpose, should in any case reconstruct their unique narrative logic and historical context, as I will do in the following with the help of a case study. As it happens, no research has yet been published that seeks to historicize this frequently used set of interviews, even though British (see e.g. Savage 2013: ix, 16-18; Lawrence 2019) and German (Brückweh et al. 2022) sociologists and social historians are embroiled in a lively discussion over how materials generated by social science surveys in the past can be re-evaluated. It may make sense to consider such materials from perspectives that the original researchers never intended.

Stanley's interviews are especially fascinating for researchers today because he spoke to people who themselves had worked as social scientists (or as social science „amateurs"), creating empirical materials. For modern social scientists and historians, certain passages thus make for curious reading, as the objects of study ask themselves the very same questions as their modern-day researchers. For instance, Celia Fremlin complained in her interview about „oral historians" (Fremlin, 18.9.1981, 11) who had raised objections to a research project in which she had been involved (Harrison 1976). The project was on the Second World War, and was based partly on eyewitness interviews. Doubts were raised as to whether the interviewees might have misremembered their experiences. Around thirty years later, we could ask the same question of Fremlin's own interview. Is her account of her own bravery during the Blitz truth or fiction? Fortunately, oral history is a well-developed field of research (Obertreis 2012) that permits other, perhaps more interesting, questions.

These days it is generally acknowledged that people always recall their past selectively – and „for a purpose"[26] (Welzer 2012 [2000]: 248; also see Obertreis 2012: 28). This does not (necessarily) mean that all of their memories are inaccurate; rather, while recounting their recollected experiences, they merge them into a narrative that is tailored to another party. In the process, various projections come into play that might influence the course of the story, depending, for example, on the age and gender of the interviewer. The interviewee may also have a tried and tested „standard narrative" in mind. Finally, we know that people who tell stories from their own lives often build elements into their narratives that are drawn from media discourses (see Jureit 2012 [1999]).

Most historians and social scientists studying these and other influences have focused on interviews they conducted themselves (see Apel 2015: 243); yet there is no reason not to expand the discussion to older oral history accounts. Indeed, contextualiz-

[25] The Mass Observation Archive holds a number of source files under the call number SxMOA32 – one for each person who worked on the project. Among other things, they contain transcripts of the interviews that Nick Stanley conducted with some of them.

[26] The original German phrase is *„anwendungsbezogen"*.

ing interview situations may be easier in retrospect than in situations where the researcher was personally involved. Oral historians are suspected of being susceptible to bias in their research because, unlike other historians, they may „smuggle" their own views into the source text, for example by asking leading questions (ibid.: 242). This risk at least does not apply to secondary evaluation. And if the original interviewer did steer the conversation in a particular direction, for the purposes of secondary analysis this is merely one aspect of the historical context, to be reflected on alongside others.

With these considerations in mind, in the following I will consider Nick Stanley's interview with Celia Fremlin on its own terms and undertake a close reading of it. In the process, I will focus on those episodes of the interview that are biographical in the narrow sense of the word in order to tease out some of the central narrative motifs. I will then situate those motifs socio-historically. One particularly salient context is that of British gender history. My core argument is that the contemporary women's movement influenced how Celia Fremlin recalled her life during her interview with Nick Stanley. The case study of Fremlin's interview illustrates some of the idiosyncrasies of oral history, as well as some of the problems historians encounter when reusing old oral history accounts.

Celia Fremlin Talks about Her Life: Episodes in Amateur Sociology

The interview was already underway when Nick Stanley started recording. The first thing we hear is Fremlin's voice: „I very much felt like I was carrying the thing on" (Fremlin, 18.9.1981: 1). „The thing" referred to Mass Observation. Stanley had asked her about her most recent research project, the „mugging survey". Night after night Fremlin had roamed the streets of London conducting interviews with the aim, as she explained, of showing that the city was far less dangerous than was generally assumed – especially for women. „I was appalled that half the women in this city can't go out at night unless somebody is with them…I thought I want to try and fight this."

During the course of the interview, it becomes clear that by beginning in this way, Stanley pre-structured Fremlin's biographical narrative. The mugging survey provided a „meaningful end point" (see Welzer 2012 [2000]: 251) that the narrative would lead towards. In its main passages, that narrative seems like an answer to the question of how Celia Fremlin became the fearless woman who got involved in such a project in the first place. Mass Observation plays an important role in this story. In fact, Stanley suggested as much to her by asking: „What do you think you relied on from your experience of M-O for the mugging survey?" (Fremlin, 18.9.1981: 2).

In her answer, Fremlin told him about an inner development she had gone through, explaining that Mass Observation taught her how to approach people of varying dispositions and persuade them to be interviewed. This understanding of human nature was not something she had grown up with. Born in 1914, she was from a well-to-do background and had been brought up as a „lady" (ibid.: 3) for whom it was not at all the done thing to strike up conversations with strangers. It was Tom Harrison, the head of the Mass Observation project, who taught her to overcome her reserve simply by sending her out into the streets with a questionnaire: „he threw us in at the deep end" (ibid.: 4). This was a biographical turning point. But why had she joined Mass Observation in the first place?

Originally, Fremlin had studied classics, a degree that, in her words, qualified her for nothing beyond teaching – a job that went very much against her own inclinations: „the one thing I was determined not to do was teach" (ibid.: 4). Here, as elsewhere in the interview, it is striking that the narrative centres on the intrinsic motivations of its protagonist. Her likes and dislikes are what drives the plot forwards – above all her curiosity: „Having been brought up in this very comfortable upper-class sort of way I terribly wanted to know what it was like to be the other half and the only way seemed to be to get there and work there and live there and see" (ibid.: 4). How exactly had she achieved this breakthrough, Stanley enquired? „I thought 'right, the thing I am qualified in [classics] isn't a qualification for anything I want to do, therefore I must treat myself as unqualified, like any fourteen-year-old'" (ibid.: 4-5). In this part of the interview, Fremlin gently mocks the academic values of the bourgeoisie to which she belonged. „So what can you do if you are unqualified? – You can do low-grade catering jobs." She there-

fore started out working in food services, before taking on a role as a maidservant.

Crucially, however, Fremlin set down her experiences as a maidservant in writing (remaining true to her roots in at least this sense) in the form of a semi-fictional social commentary titled The Seven Chars of Chelsea (1940). This became one of the anchor points in the interview, with both participants coming back to it repeatedly. The book was published at the beginning of the Second World War, Fremlin recalled (Fremlin, 18.9.1981: 3, 6), and was received positively in the newspapers. One review was written by Tom Harrison, meaning that Fremlin had come to the attention of Mass Observation.

At this time Harrison was reorganizing and reorienting the organization in an attempt to make it a „useful" part of the war effort, so to speak. And he wanted Celia Fremlin to be involved, so he wrote her a letter: „You seem to be just the kind of person that I want for studying the attitudes to ARP [Air Raid Precautions]", Fremlin summarized its contents. She accepted the invitation, and what followed was a time of great enthusiasm. „There was no question of paying. It was a hobby…it seemed jolly interesting anyway" (ibid.: 3). In the beginning she was employment as a waitress and sending Harrison regular reports on her working life (ibid.: 6). Yet she quickly found herself in a task force made up of four women: „He used to call us his 'spearhead'. We were available to be sent anywhere where the bombing had been severe or there was a riot going on or some fuss, you know" (ibid.: 3).

Fremlin's days as a mobile observer were numbered, however, as she was soon called up for civilian service. In 1942 she began working in a radar equipment factory. Yet this did not prevent her from continuing to collect social scientific data for Mass Observation: „we…gossiped absolutely non-stop all day. There was an embarrassment of material pouring in all the time" (ibid.: 9-10). In fact, during this time she worked on a new book called War Factory (1943), which was eventually published under Tom Harrison's name (Fremlin, 18.9.1981: 12). One reason for this were the security regulations in force during the war, though another may have been Tom Harrison's sense of self-importance. „He had a big outsized ego, I'll grant you that", Fremlin explained, before immediately adding: „We rely on people with egos to get a lot of things going" (ibid.: 12). She thus protected Harrison – and it will soon become clear why. For now, it should be noted that her passion for amateur sociology was dampened when her first child was born in the spring 1943. „That did end any full-time work for me", she stated. „I used to send in reports about what happened round about here, but it wasn't the same" (ibid.: 8).

After this interview episode, Stanley and Fremlin talked for a while about the inner working of Mass Observation, as well as the research methods that underpinned *War Factory*. Then Stanley moved the conversation forward three decades into the 1970s: „The third publication you were involved in was *Living Through?*" (ibid.: 10). This refers to *Living through the Blitz* (1976), a retrospective project about wartime bombing raids. Why had Harrison specifically chosen Fremlin for the project, Stanley asked? „I was terribly enthusiastic", came the answer, „rather like Tom himself I rather liked living in danger" (Fremlin, 18.9.1981: 10). In fact, Fremlin repeatedly compared herself to Harrison throughout the interview – to a man known to his contemporaries as a daredevil and an adventurous amateur academic, but also as a temperamental narcissist and enfant terrible. „I'm a bit like Tom, I suppose – everything's an ego-trip for me", Fremlin declared (ibid.: 14; see also Heiman, 1997).

Living through the Blitz proved to be Fremlin and Harrison's last joint project, as shortly after its conclusion he died in a traffic accident. With this episode, however, the interview had almost reached the present day, the immediate lead-up to the mugging survey. At least, that is how Fremlin presented it:

> „I'm sure that when I embarked on [*Living Through the Blitz*] that it was with a feeling of being excited in being drawn back into a thing I hadn't thought of for decades. It was being involved in that that made me think of this mugging survey thing. I suggested it to Tom and he said 'When we've got this thing off our minds this shall be our next project'" (Fremlin, 18.9.1981: 13).

The biographical narrative then ends where it began, with Fremlin wandering London's streets at night. She and Stanley talked for a while longer, but there are hardly any biographical passages in the rest of the interview (ibid.: 14-19). Instead, the conversation increasingly took the form of a project discussion, with two researchers debating about the advantages and disadvantages of amateur sociology, interview methods, and publication strategies –

the last of these because Fremlin was having trouble finding a publisher for her project. Stanley suggested that she could incorporate the Brixton Riots into her study (ibid.: 16). These were a series of ethnic conflicts between young people and the police that had broken out six months before the interview.

Fremlin returned to this point shortly before the end of the interview: „you might have asked me what impression I got about the colour problem in Brixton, about the Muggers being mostly black" (ibid.: 17-18). In fact, she told Stanley, she had stopped short of mentioning black people as robbers and troublemakers in her survey, as this would only worsen the situation for their parents and sisters – not to mention the adverse effects on peaceable „black boys". However, she also reported some „interesting things" she allegedly „found out" in this area – it is easy to see that a racial or culturalist bias coloured her perspective. According to Fremlin, the practice of mugging could be explained with reference to the muggers' roots in Caribbean village life. Many of them saw themselves primarily as „hunters", she declared, in accordance with their roots, and added that in their view work was something for women. Once again, a fascinated Stanley dug deeper: „Did you in fact interview them?" „Oh yes", Fremlin replied, „mostly they're quite nice about it and say things like 'It's daft, isn't it, to carry their purse open in their pocket like that, isn't it?'... They were not hostile at all, just giving me a few tips" (ibid.: 18).

A Tale of Personal Courage

It would have been possible to recount Celia Fremlin's life story from a different perspective. A literature search quickly reveals that she was known in the 1980s primarily as an author of crime fiction and thrillers (Simmons 2017 [1958]). She was also politically active, including in the Communist Party during her student days in Oxford („that was where all the fun was"; Fremlin, 17.3.1980: 1). In later years she became known as a pugnacious advocate for assisted suicide and euthanasia. Yet Stanley was above all interested in Fremlin's role in the Mass Observation project, as well as in Mass Observation's importance for her career as an amateur sociologist. As such, his interview does not necessarily meet the criteria placed upon oral history interviews, especially in Germany, where there is a tradition of beginning the conversation with as open a question as possible (Maubach 2013: 35-40) in order to allow the interviewee to recount their biography „with the emphases (and omissions) to which they are accustomed, or which they deem appropriate for an interview with an agent of academic research or the public sphere" (Niethammer 1985: 402).

One could argue that Stanley would have been more likely to draw on British influences such as Paul Thompson and the History Workshop Movement, which was already prominent in the early 1980s (Obertreis 2012: 8; Thompson 2017 [1978]). In fact, however, he did not lean on any explicit theory or methodology of interviewing, and it is doubtful whether it would have served his purpose.[27] Instead, he followed his interests by asking leading questions which tightly corralled the conversation, with the result that it jumped from one amateur sociological study to the next. In this way, Stanley predetermined the structure of the biographical narrative.

Yet Fremlin filled his frame creatively, placing her own accents. For instance, in almost every sociological episode they discussed, she stressed the curiosity and enthusiasm with which she went about her work – how her exuberant zeal drove her to defy social boundaries and all manner of dangers. Linguistically, Fremlin's laconic, nonchalant tone throughout large parts of the interview is also striking, given that she was speaking of things that would have been sources of anxiety for many contemporaries – from incidents in the Blitz and interviews with punks and muggers to the conditions in the hostel where she lodged alongside other factory workers during the Second World War. She had described this accommodation vividly in *War Factory* – "it sounded very grim in part", Stanley noted. Yet his interviewee brushed off his concern: „I don't know. I certainly wouldn't have missed it. I loved the war actually. I know it's an awful thing to say but I did" (Fremlin, 18.9.1981: 10).

In a way, the markedly blithe way in which Fremlin told her story suited its content. She was talking

[27] I am grateful to Nick Stanley for his willingness to answer some questions about his interview style via email.

about her own personal courage, and she did so with courageous words. More specifically, one could say that her narrative hinges on an unspoken theme: her own daring attitude in the face of all danger. This attitude also comes out in the way she repeatedly compares herself to Tom Harrison, as well as how she theorizes about herself and her story: „I suppose my driving force in my life has been to have as many experiences as possible in my span on earth. There are all the things there to do and see – I think I was already motivated that way" (ibid.: 5). Mass Observation assumes the role of a catalyst in her narrative. While working for the organization, Fremlin learned to translate her bold curiosity into a transgressive act – that of establishing contact with strangers.

Stanley's astonished questions certainly influenced the story too; without his involvement, Fremlin would not have distilled her core theme so precisely. Yet we should not assume that she completely „improvised" her narrative during the course of their conversation (Schütze 2012 [1983]). As a professional author and former Mass Observer, she was very used to giving interviews (see e.g. Fremlin, 17.3.1980; Fremlin, 24.2.1980; Evening Standard 25 Sept. 1979). When one considers other sources from the 1960s and 1970s, it also becomes clear that her narrative of courage is situated in a specific historical context, in which it had a particular meaning.

The Age of Domestic Noir

Celia Fremlin's mugging survey touched a nerve in the late 1970s; it was provocative and polarizing. In the end it was never published in book form, but Fremlin made herself widely known as an amateur sociologist by giving interviews and revealing some of her insights in popular newspapers and magazines (see Fremlin 18.9.1981: 1). In October 1979, for instance, she published an article in the *Cosmopolitan* that presented anecdotes from her research and expressed indignation at her peers: „Here in the midst of our glittering permissive society, with women's lib going from strength to strength, millions of women still sit imprisoned in their own four walls" (Fremlin 1979: 140).

Fremlin was referring to the contemporary women's movement, which she evidently welcomed and wanted to support, and which has often been referred to as second-wave feminism in the past (see e.g. Binard 2017). This phrase served to distinguish the women's liberation movement of the 1960s and 1970s from its predecessors: those women in the nineteenth and early twentieth centuries who fought for legal equality, improved working conditions, and the right to vote – among them Emmeline Pankhurst and the suffragettes. The image of successive 'waves' of feminism, however, has rightly been called into question. It makes sense only if one focuses on activists from the white „Western" middle class and their intellectual tradition (see Freeland 2022: 78; Delap 2020: 20-21; Stevenson 2020: 8-13). However, they were the ones prominent in British media in the late 1970s, and Celia Fremlin referred to them when she wrote about „women's lib". As such, they are of interest here.

The members of the women's liberation movement made extensive reference to the above-named earlier activists as role models, but their criticism of the patriarchy had different emphases. An initial sense of their agenda can be gleaned from the 1963 book *The Feminine Mystique* by Betty Friedan. She co-founded the American National Organization for Women in 1966 and became one of the most influential feminist thinkers (Thompson 2002: 338; Binard 2017: 1–2). The term 'feminine mystique' refers to the notion that women could only find personal „fulfilment" as wives and mothers – an idea that was propagated by contemporary women's magazines, Friedan argued (1963: 21-23; see also Levine 2015). Yet she felt it was strange that successful and financially secure women and mothers were increasingly suffering from depression and anxiety disorders (Friedan 1963: 14-15) or embarking on extramarital affairs (ibid.: 210-212). Friedan blamed this on the social confinement, boredom, and monotony of domestic life, which resulted in a form of alienation for many women, and she proposed an escape route in the shape of education and serious, responsible work (ibid.: ch. 14; Shriver 2010: vi).

The Feminine Mystique inspired bourgeois feminist activists in many Western countries, including Britain (Binard 2017: 1-2). In the spring of 1970 between 500 and 600 women – Marxists, socialists, and representatives from across the political spec-

trum of the New Left – gathered at Ruskin College in Oxford for a conference. Many of them had been disappointed by their male comrades and complained about sexism among left-wing political activists. At the end of the conference, the attendees agreed on four demands: equal pay; equal education and employment opportunities; free access to contraception and abortion; and free twenty-four-hour nursery provision (Nava 2020: 62).

These four demands show how influential Marxist ideas were in the feminist movement at the time (Rees 2010: 339).[28] Yet for female students in the 1960s and 1970s, it was by no means necessary to read Karl Marx, Friedrich Engels, or Betty Friedan in order to fear the domestic life-world. It sufficed to listen to the Rolling Stones („Mother's Little Helper", 1966) or Marianne Faithfull's 1979 hit „The Ballad of Lucy Jordan". Or they could read one of Celia Fremlin's novels. Fremlin certainly was not part of the avant-garde of the women's liberation movement, if nothing else because of her age (she turned 56 in 1970). Nonetheless, I would argue that she was part of the cultural history that underpinned it.

Celia Fremlin and the Women's Movement

Anyone seeking to examine Fremlin's role in British women's history might begin with her debut book of social reportage on *The Seven Chars of Chelsea* (1940). Fremlin was certainly not the first woman from a respectable background to disguise herself in order to venture into poorer female life-worlds and study them incognito. She was working in a genre dominated by women (Seaber 2017: xx). It is easy to see that the class system coloured these reporters' perceptions. Nonetheless, as women, they drew attention to female life-worlds at a time when academic social sciences and opinion polling were dominated by male perspectives (Faragher 2021: 136).

Fremlin's later novels are interesting in this respect as well, as they also describe female life-worlds, albeit with the difference that they foreground the specific unease of domesticity, falling into the genre of „domestic noir" (Simmons 2017 [1958]). A good example is her debut novel *The Hours before Dawn* (2017 [1958]), which tells the story of Louise Henderson, a young mother deprived of sleep after the birth of her third child. In fact, her baby's crying disturbs not only her own sleep, but that of her husband and her nosy neighbours, who complain. The situation comes to a head when a mysterious lodger named Miss Brandon moves in with the Hendersons. A suspicious Louise fears that this stranger poses a threat to her family, and her concerns prove justified. Unfortunately, however, nobody believes her warnings, calling her mental health into question instead.

This nightmarish tale won Fremlin the 1960 Edgar Allen Poe Prize and paved the way for her career as a writer of crime fiction. Yet Fremlin had one thing in common with feminist authors such as Betty Friedan: she owed her success to a time when it was becoming increasingly common to imagine the privacy of one's own four walls (along with the close family relations they contained) as an oppressive place. By 1994 Fremlin had written sixteen novels and three short story collections, most of which were about the terror of intimate relationships (Simmons 2017 [1958]: 243).

The women's movement became more radical over the course of the 1970s. Its activists met for another seven national conferences, passing resolutions and making further demands – for instance, for legal and financial independence and control over their sexuality. At one of these meetings, in 1977, there was a momentous schism: a „revolutionary" wing formed that questioned the socialist approach adopted to date and, with it, the entire movement (Rees 2010: 341–344). The revolutionaries argued that the socialist feminists had failed to understand the root cause of women's oppression, which lay not in capitalism or the division of labour within the family, but in heterosexual sex (Jeffreys 1977b) and male violence (Parr/Hanmer/Packwood/McNeill 1977) – and that it was high time to recognize men as the enemy (Jeffreys 1977a). The trench warfare that subsequently broke out was fought so vehemently that the previously unified women's movement broke apart. The national WLM conference held in Birmingham in 1978 was the last of its kind.

In order to situate this conflict historically, we must recall that at the time a serial killer known as the Yorkshire Ripper was making headlines by targeting

[28] According to this approach, the root of the problem was not men per se, but the role of the family and its gender-based division of labour under capitalism (see Mitchell 1966).

women. He was the reason why the police advised women to stay off the streets after dark. To the revolutionary feminists, this was tantamount to admitting defeat in the face of male violence, and they responded by organizing night-time demonstrations in several British cities, thereby initiating the Reclaim the Night marches,[29] a form of feminist protest that was part of British women's history until the 1990s (and was revived in 2004).

1977 was not only the year of the first Reclaim the Night march; it was also the year in which Celia Fremlin began her nocturnal tours of London. Violence against women, fear of domestic confinement, and the ensuing battle for the streets were the topics on everyone's lips, and Fremlin was inspired by them. What exactly did she see as her contribution to women's liberation?

With her mugging survey, Fremlin wanted to encourage her peers – to show them empirically that the streets were a low-risk environment and their fears were unfounded. On closer inspection, it is also clear that this reassurance was underpinned by a social psychological line of reasoning: „'Muggers can only operate in an atmosphere of everyone else being frightened,' declared one of the girls being interviewed. I am certain that she is right. Fear does breed danger, inevitably" (Fremlin 1979) A BBC interviewer pressed Fremlin on this point, asking if she really meant that women generate the danger themselves simply by being afraid? „I think it is very much engendered by that", (Fremlin, 24.2.1980, 7.08-7:11) Fremlin replied. Anyone who found themselves in danger at night had above all to be bold, to interact confidently with potential malefactors. When Fremlin herself was dragged into a side street by a „young chap", she said she simply talked to him: „You work on the number 14 bus, don't you?" (see ibid., 6:28-6:58) In Fremlins view, dangerous situations could generally be defused with paradoxical, transgressive interventions such as these. In this way, she argued, courage could make the streets safer, liberate women from domestic confinement, and help achieve emancipation.

It is no surprise that Fremlin and her agenda sometimes encountered hostility among feminists.[30] Her position could be understood as an attempt to downplay violence against women or, more controversially, to blame its victims. It would be interesting to trace the reception of the mugging survey in more detail – in feminist magazines, for instance.

Here, however, I am interested in a somewhat different point – namely, how Celia Fremlin's social psychological agenda shaped the biographical narrative she presented in her interview with Nick Stanley. When that narrative and the mugging survey are considered in parallel, a consistent picture emerges. In her conversation with Stanley, Fremlin described how she had boldly faced down various dangers, and how Mass Observation had taught her to go on the offensive socially in order to strike up conversations with strangers. This transgressive skill was what she sought to communicate to other women through her mugging survey, thereby contributing to their liberation. The core themes of Fremlin's life story and her mugging survey overlap in a curious way.

Conclusion

According to her daughter, Celia Fremlin had a reputation in her family for embellishing the truth – for taking creative, authorial liberties with it (Simmons 2017 [1958]: 243). With this in mind, one might suspect her of having in a sense embellished *herself* during her interview with Stanley, styling herself as what she saw as the model of a liberated woman. Fortunately, there is no need to assume that Fremlin was following a deliberate plan in her self-presentation. Nor is there any reason to doubt either the key facts of her narrative or those parts of it in which she engaged in self-interpretation. Yes, it is possible that she was fully aware of her own motives (and vanities). Nonetheless, a biographical narrative is not an objective record. To recount our lives, we invariably have to omit certain episodes while emphasizing others and „tailoring them to the plot" (Welzer 2012 [2000]: 251).

[29] This mode of demonstration was partly based on the German example of Walpurgisnacht (Mackay 2015: 1–2).

[30] Nick Stanley has informed me by email that Fremlin's mugging survey was heavily criticized by staff at the Mass Observation Archive. The head archivist at that time was an active feminist. The British Library holds a set of biographical interviews with her (Sheridan, 1.5.2017, 17.5.2017, 5.9.2018, 20.9.2018, 17.10.2018, 7.11.2018, 5.12.2018, 9.1.2019, 3.4.2019).

As we have seen, the key points of such a narrative develop in a reciprocal interplay of questions and answers. To understand this dynamic, however, we also have to take into account whether and how the interviewer and interviewee referred to historical context. When Stanley suggested to his interviewee that she should talk about her time at Mass Observation as a *precursor* to her politically charged mugging survey, he perhaps unintentionally introduced contemporary ideas and debates about the role of women as a point of reference. This is the context in which Celia Fremlin's amateur sociological project proved so controversial: she decried the fact that women no longer left the house at night – in a sense remaining trapped in their 'domestic gloom' – for fear of encountering danger on the streets after dark. Through her research, Fremlin sought to demonstrate that these anxieties were unfounded – and that especially when they came face to face with muggers, it was important for women not to give in to fear, but to find the courage to make paradoxical interventions.

This social psychological agenda in turn shaped the way Fremlin presented her own career to Stanley. Oral historians stress that people remember their lives in the form of meaningful chains of association. In this case, the chain started and ended with a research project intended to embolden women. It therefore makes sense that Fremlin would present her biographical narrative as a story of her own courage – especially when we consider that a fascinated Stanley was always ready to ask questions about the more daring episodes in her account.

Anyone using old interviews as sources should thus be aware that interviewees are locked into a unique communicative dynamic, on the basis of which they look back on themselves in light of considerations drawn from the present. These are just two reasons why oral historians claim that the biographical narratives they create and work with are „social constructions" (Obertreis 2012: 27–28). Secondary analysis of biographical interviews is not least about understanding the dynamics behind these stories, regardless of whether we are primarily interested in these dynamics themselves or in the speaker's recollections.

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6. Struggles for Belonging: Reflections on Oral History Interviews, Language Use and Memory in the Context of the Border City Nador (Morocco)

Sofie Steinberger

Introduction

Regarding interviews in a multilingual setting, Elke Scherstjanoi writes that „only the unalienated communication in the language of the contemporary witness is able to create the intimacy and certainty that characterizes a productive interview situation" (Translation by the author; Scherstjanoi 2008: 248). Despite the fact that people obviously can express themselves more precisely in their mother tongue, quite often it is not possible to cover all the languages spoken in certain areas when doing interviews. This was the case in my research for my dissertation project in the border region of Melilla (Spain) and Nador (Morocco). The project focuses on the changing spatial imaginaries of the border region between Melilla and Nador and the development of feelings of belonging through daily cross-border practices and discourses of memory between the 1940s and 1990s. The time frame includes the colonial episode – the region formed part of the Spanish protectorate in northern Morocco that existed from 1912 to 1956 – as well as the post-colonial developments until Melilla was sealed off by a European border fence in 1998.[31] I was planning on doing oral history interviews with Spaniards and Riffians, as the local Moroccans call themselves, referring to the Rif mountains. As Moroccan Arabic, Darija,[32] is the most-spoken language in Morocco (Sadiqi 2003:48) and I also wanted to spend some time in Moroccan archives, I studied Moroccan Arabic to a level that allowed me conduct interviews with the Moroccan population. For practical reasons I did not learn the local dialect, Tarifit. My knowledge of Spanish was already sufficient. However, when I arrived in Nador, several of my interviewees explicitly did not want to talk to me in Darija, either because they did not speak it or because they did not want to speak it. How could this be?

Not being able to conduct the interviews in Moroccan Arabic came as a surprise and put me in a dilemma: the only other language in which most people could communicate with me was Spanish, the language of the former colonial power. However, it seemed that people did not bother to talk to me in Spanish. Why?

In the following, I will elaborate on Morocco's language politics, and the use of language in the region of Nador and put them in a historical context. Concentrating on the interviews with my Riffian interview partners in Nador, I will then discuss the relation of the official image of the Rif region in Morocco and the Riffian collective memory, and why giving interviews in Spanish can be understood as a political act within the Nadori Riffian interviewees' struggle for belonging to Morocco.

Language Politics in Morocco

In Nador, Riffians speak Tarifit, while non-Riffian inhabitants speak mostly Darija, the Moroccan Arabic dialect. Tarifit, Tamazight, and Tashlehyt are the

[31] Here, the term „post-colonial" is used in the sense of a temporal aftermath after the end of colonial rule. Nevertheless, many post-colonial developments in the region of Nador are rooted in the pre-colonial and colonial period.

[32] *Darija* is the most spoken language in Morocco, which is an Arab dialect mixed with French and Spanish vocabulary; the official written language is, however, Moroccan Standard Arabic.

Figure 6.1: The Spanish Protectorate zone 1927 – 1956, since the so-called „pacification". (Source: Biblioteca AECID (Madrid) - Signature 4C-257/10)

three main variations of the Amazigh language, the native language of today's Morocco. Depending on the source, they are still spoken by about either 25% or 50% of all Moroccans (Graybill 2019: 8). With the Arab invasion in the 7th and 8th century and the introduction of Islam, Arabic became an additional language that developed to become the official written language in Morocco, while the Amazigh varieties were mainly used orally (Graybill 2019: 9). From 1912 to 1956, during the period of Spanish and French colonialism in Morocco, Spanish and French became administrative languages alongside Arabic. The local elites who wanted to obtain higher ranks in the colonial administration, and also others who had close relations with the French and the Spaniards due to work and trade, needed to learn those languages. After independence, the Moroccan government declared itself part of the Arabic world, and Modern Standard Arabic and French became the official languages of the country's administrative and educational system. By the 1980s, Darija had become the educational language. However, French remained the language mostly used in the scientific context. Although with about 15 million speakers Morocco has the largest Amazigh-speaking population in the world, only in 1994 Amazigh was recognized as part of the Moroccan national identity by King Hassan II. In 2001, his successor Mohamed VI established the Royal Institute of Amazigh Culture (IRCAM); in 2003, it was introduced as an educational language in some schools, and in 2011, the Amazigh language and its varieties were recognized by the Moroccan government as a second official language in a new constitution (Marley 2003; Sadiqi 2003; Ennaji 2005; El Aissati/Karsmakers/Kurvers et al. 2011; Graybill 2019). After the end of the Spanish protectorate, in Nador the locally spoken native language remained Tarifit. Nevertheless, many of the inhabitants of the region of Nador also spoke Spanish. The reason for their knowledge of Spanish is rooted in their close relationship with the Spanish territory of Melilla, which – together with the city of Ceuta – remained in Spanish possession after 1956.

Historical Context of Spanish-Riffian Relations

Melilla was seized by Spanish troops in 1497 within the context of the expulsion of the Muslim and Jewish populations from the Iberian Peninsula, and with the objective of controlling the Mediterranean commerce (Driessen 1992: 16-18). It then developed

Figure 6.2: People waiting at the Spanish border crossing point in order to enter Melilla. (Photo: Sofie Steinberger)

from a military post into the administrative centre of the eastern part of the Spanish Protectorate in northern Morocco (1912-1956) (see Figure 6.1.) and finally became an autonomous city within the Spanish parliamentary monarchy today. Commercial and belligerent interactions with the surrounding *kabilas* shaped the contact between the Spanish soldiers and the autochthonous population from the beginning of the Spanish presence in the region (Steinberger/Aziza 2022: 224ff).[33]

With the suppression of resistance in 1927, the cross-border activities diversified from trade to other interactions in the field of labor, as well as administration and education. Spaniards and Riffians started to cohabitate – mainly in urban centres like Nador – and continued to do so after the Moroccan independence in 1956. The end of Spanish presence in Morocco was heralded in 1973, when Moroccan legislation prohibited the possession of land and companies by foreigners without a Moroccan business partner and introduced the obligation of a work contract for a residence permit in Morocco. So, most Spaniards left the country (López García 2008: 44).

Economic interdependencies due to trade and job opportunities form the backbone of cross-border relations (Berriane/Hopfinger 1999: 93-117). Melilla became the educational, leisure, and medical centre for the neighbouring *kabilas* fostered by growing social entanglements through family ties. At the same time, Nador and its province became the weekend escape, food market, and playground for commercial endeavors (mainly) for the European population (Bossard 1978: 121f; Steinberger/Aziza 2022: 235f) (see Figure 6.2). In Melilla, intended changes of the right of residence led to a successful civil rights movement by the Riffian residents in 1985, leading to their recognition as Spanish citizens. Many Riffians in Nador thus have Spanish relatives and/or other close relations to Spain. However, since the 1980s, Spanish border policies have undergone a transformation that has gradually made cross-border contact more difficult.

Historical Context of Moroccan-Riffian Relations

At about the same time as the civil rights movement in Melilla, in 1984, the Moroccan military brutally repressed demonstrations in Nador by students who took to the streets against the rise in prices of basic goods in particular and against socio-political and economic marginalization of the Rif in general (Suárez Collado 2019; Anonymous 1984; Almagro 1984).[34] These demonstrations were an expression of a ripened discontent within the Riffian population with the post-colonial politics of the Moroccan government, mingled with a growing political con-

[33] *Kabila* is often translated with the term „tribe". They were hierarchically structured territorial and social organizational units, often but not necessarily based on family lineage. *Kabilas* could contain several clans that built territorial subdivisions, also called fractions. Several *kabilas* could unite in a confederation. Within the *kabilas*, councils, also called yemaas, formed by the most prestigious men of the *kabila* were responsible for jurisdiction and other decision making. During the protectorate the *kabilas'* territories were adopted as administrative units, and many territories still carry the former *kabilas'* names (Munson 1989). The city of Nador, for example, belonged to the *kabila* Mazuza. The homonymous province of Nador that surrounds Melilla consisted of about 20 *kabilas*. In the region of Nador several *kabilas* form the confederation Guelaya, which covers the whole peninsula of Three Forks. Nowadays, people living in the region still refer to their *kabila* when they refer to their origins.

[34] Melilla served as a sanctuary for some of the wounded protestors.

Figure 6.3: Market in Nador with goods from Melilla. (Photo: Sofie Steinberger)

sciousness of being part of the neglected autochthonous ethnic Imazighen population in Morocco. As a response, in a speech to the Moroccan public, King Hassan II referred to the Riffians as contrabandists and „scum" (Beauchesne/Vairel 2021: 10;[35] Truth and Reconciliation Commission of Canada 2015).

Still today, Riffians are broadly perceived as dissidents in the Moroccan public, media, and political discourse (Nahhass 2019: 1). This stereotype originated in the relationship between the Makhzen – the Moroccan central government and its entourage – and the Rif, and its interpretation by European colonizers as well as scientific scholars since the 19th century.[36] Before European colonialism, the territory controlled and taxed by the Moroccan sultan (*bled Makhzen*), and the Rif, an autonomous territory controlled by local tribes (*bled siba*), were organized in an arrangement of mutual support in case of foreign interference (Mouna 2018: 31-35; Wyrtzen 2015: 16-19). Due to the Riffians' rejection of taxation and control which also resulted in continuous renegotiations of alliances within the region, in the 19th century, French and Spanish explorers described Riffians as anarchic, violent and rebellious. In the beginning of the 20th century, European forces used the lack of control over the *bled siba* regions as a justification for their military interventions and consolidated the image of an anarchic, separatist, and dissident Rif. Later, these assumptions were reproduced unquestioned by international scholars (Nahhass 2019: 4f).

The images of a supposedly anarchic *bled siba* and an orderly *bled Makhzen* were reinforced by the differing development of the two protectorate zones: the French administration had more funds to invest in infrastructure and industrial projects, whereas the Spanish zone was characterized by subsistance farming due to its mountainous and less accessible territory. This also showed in the interaction of Riffians with Moroccans from the French zone. The contact of Nadori Riffians to the Moroccan interior was limited throughout the 20th century. Moroccan goods were more expensive than the Spanish ones,

[35] See also parts of Hassan II's speech on youtube: https://www.youtube.com/watch?v=oqlnGZFyRsM (03/10/2022).

[36] First, French and Spanish explorers described Riffians as anarchic, violent and rebellious. Later, European forces used the lack of control over the *bled siba* regions as a justification for their military interventions and consolidated the image of an anarchic, separatist, and dissident Rif. Later, these assumptions were reproduced unquestioned by international scholars. (Nahhass 2019: 4f).

and Moroccan media such as TV channels were more difficult to receive (interview with Amina M., Nador 2019, Hamid R., Melilla 2019).[37]

However, there was a great affection amongst Riffians towards the Moroccan state. This can be seen in Nador's role in the armed fight against colonization and for Moroccan independence. Between 1920 and 1926, the Riffian leader Abdelkrim El-Khattabi gathered great parts of the kabilas in the Spanish protectorate zone and founded the so-called Rif Republic to confront French and Spanish forces (Madarriaga 2010). During the fight for independence, the region of Nador functioned as training centre for the Moroccan and Algerian liberation armies (Zade 2001: 175).

The unequal economic and social situation in the two former protectorate zones (French in the south, Spanish in the north) led to the formulation of demands by Riffian leaders regarding an adaption of the new economic and administrative system to the differing needs in northern Morocco. The imposition of the economic system of the former French zone on all Moroccan territory after the independence in 1956 led to rising prices and poverty in the north. All public positions were now conditional on knowledge of Arabic and French. The result was that the representatives of the state were now predominantly Moroccans from the former French zone, who spoke neither Tarifit nor Spanish. Some Riffians experienced this as new colonization (e.g. interview with Ahmed Y., Melilla 2019). The government, however, rejected these demands and brutally suppressed the subsequent Rif rebellion in 1958/59 (Aziza 2019). According to Badiha Nahhass (2019: 9), the continuing marginalization of the Rif after Moroccan independence and the consequent concentration on the cultivation of cannabis, commerce, and contrabandism between the Rif and the Spanish exclaves Melilla and Ceuta, as well as labor migration to central Europe, additionally led to a „space of economic dissidence."

Since the 1960s, a cultural movement amongst Imazighen started to grow, which also gained ground in the Rif. It gathered pace and importance in the 1990s due to a series of international conferences (*Congrès Mondial Amazigh*) on questions related to Amazigh issues across North Africa and in the diaspora, and culminated in the *Déclaration de l'autonomie du Grand Rif* (Declaration of the autonomy of the Grand Rif) in 2007 and the foundation of the *Assemblée Mondiale Amazighe* (World Assembly of the Amazigh People) in 2011 (Suárez Collado 2012, 2013; Raha 2007).[38] Riffians from Nador belonged to the initiators of these institutions. When a street vendor was killed in 2016 in the Riffian city of Al Hoceima, thousands of Riffians took to the streets, and the numerous demonstrations turned into the *Hirak* movement. The demonstrators expressed deeply rooted feelings of *Al-hogra* by referring to the region's violent and marginalized history (Rhani et al. 2022: 328).[39] The protesters carried flags of the Rif Republic, or the Tamazgha tricolor in blue, green, yellow and the red letter „z" in Tifinagh representing the Imazighen.[40] Afterwards, the movement's leaders were sentenced to long terms in prison.

Methodological Aspects and Observed Use of Spanish in Nador during the Research

As mentioned before, to my surprise, most of my interviewees preferred to speak Spanish during the interviews instead of Darija. After sketching the political background to this language preferences, I will now describe what this meant for my research and my observations.

The Riffians' choice of Spanish as the interview language posed methodological challenges. In several respects, the concerns of Scherstjanoi (2008) outlined in the introduction apply to my project: Span-

[37] Even today, Nadori Riffians tend to say „I am going to Morocco" when they travel to the interior or the Atlantic coast in Morocco; see interview.

[38] The main aim of the international non-governmental organization *Assemblée Mondiale Amazighe* is to connect Imazighen associations all over the world and to defend the rights of Imazighen.

[39] According to Rhani et al. (2022: 328), the term „refers to the sentiments of resentment, humiliation, injustice, and power abuses; in short, it designates a shared feeling that the Rifian (sic) is less than a full-fledged citizen, the pariah of a state of lawlessness."

[40] The Tamazgha tricolor is a flag that represents all regions in which the Amazigh varieties are spoken. This region is also called Tamazgha (El Guabli, 2022: 1093).

Figure 6.4: Spanish and Arabic inscription in a building in Nador, which historically was a sales office for flour of a big mill in the region. (Photo: Sofie Steinberger)

ish interviewees have an advantage over most Riffian interviewees when it comes to formulating complex thoughts and feelings, as Spanish is their mother tongue, while for most Riffians Spanish is their second language. The latter have acquired Spanish in everyday life, through their work or at school. This manifested itself in different language levels, from fluent to rusty. The level of language skills, however, can easily lead to misunderstandings and misconceptions regarding the interviewees' social positions. Therefore, I avoided drawing conclusions about, for example, knowledge, education, and social status based on language, but concentrated on what I was told or observed. In addition, to prevent false representation and the fostering of stereotypes I edited all transcripts of the interviews. The editing entailed adding in written form information the interviewees sometimes expressed with the help of gestures, or information that was translated from Tarifit into Spanish by a family member. Sentences were cleared of grammatical errors, stuttering, word-finding problems or incorrectly used words. Mistakes were replaced with the correct grammar and vocabulary. These are only some of the aspects I had to consider when transcribing and analysing the interviews.

While speaking Spanish during my encounters with Nadori Riffians I made several observations that surprised me. Here are a few examples: one day, I was walking through Nador with an interviewee who I had asked to show me the city. We went from one public place to another, through crowded streets and markets. While we were walking, he told me loudly and with great gestures about his ideas of what was going wrong in Nador's society, about poverty, prostitution, and especially, the – in his eyes – hypocrisy regarding sexual relations and religion. All these are sensitive topics in Morocco that one normally would not talk about openly in the public. I met another interviewee in a café in Nador. He was accompanied by a friend, who I had interviewed before. During the interview, the interviewee started to heavily criticize the former king, Hassan II. This is another taboo in Morocco. His friend preferred to leave the table, apparently not wanting to be part of the conversation. Other Riffian interviewees preferred to meet in Melilla because they felt less observed there.

In all these cases, the Spanish language and the Spanish territory of Melilla were used to behave and express opinions in a way in which people would not behave publicly when speaking Darija, as many more people, such as officials, would understand.

Of course, the interview situations outlined above could be influenced by different aspects: first, there is my position as a female German historian travelling alone, a strange outsider. Just like Lena Rüßing during her research in Canada (see Chapter 3, this

volume), I did not have any specific function in the region; I represented neither Spain nor Morocco, and I was not a part of the local system of very dominant social control. Many people in Melilla and Nador were interested in my origins and why I was there. This outsider aspect had the effect that some interviewees expressed themselves more openly, as they would mention after interviews.

Second, the function of memory itself might be a reason for the use of Spanish: as Viorica Marian and Margarita Kausganskaya (2007: 925) found out, „the accessibility of autobiographical memories [is] improved when the language used at the time of remembering corresponded to the language in which memories were initially formed." Obviously, interviews in Tarifit would have been the best option. However, since all my interviewees spent most of their lives in a much closer relationship with the Spanish exclave than with the Moroccan State, using Tarifit and Spanish in their daily lives, choosing Spanish over Darija when talking about their lives in/between Nador and Melilla appears reasonable. Also, the different way of expressing themselves might be explained with Marian's and Kausganskayas' investigation about cultural frame-switching. Not only do „bicultural bilinguals possess a highly fluid self and can shift cultural values and attribution patterns, depending on immediate cultural environment"; additionally, „language creates a strong context that can draw different aspects of ourselves forward" (Marian/Kauganskaya 2007: 930). This means that interviewees might change their communicative behavior when speaking Spanish with me as they reproduced internalized cultural aspects they learned with the language.

Third, Spanish is not connotated negatively in Nador. Rather, speaking Spanish serves as cultural capital in the Bourdieuan sense. Speaking Spanish fluently has turned out to be useful in many ways. During the time of the protectorate, it was mainly the elite who had the money to afford good education for their children, who usually attended Spanish institutions in Nador, Melilla or even the Peninsula. Nevertheless, work relations in the mines, military, service sector, or commerce, as well as cross-border family bonds, led to good Spanish skills amongst laborers, merchants, and peons. Several remnants, such as shops as shown in Figure 6.4, testify of the historical economic and social interaction of Spaniards and Riffians during the Spanish Protectorate. Consequently, after independence, Nadoris of all social positions could use their knowledge of Tarifit, Spanish, and sometimes Arabic for economic purposes. In combination with their family networks in Europe, Algeria, or other cities in Morocco, and Melilla as a free-trade port, the knowledge of language and cultural codes allowed many to build up successful import-export companies.

This again had great impact on the education of the following generations, especially on girls. Female interviewees raved about the Spanish schools and their education in Nador and Melilla, as well as about Melilla as a space where the sometimes very strict norms of male and female social roles valid in Nador could be evaded and women could enjoy public life more easily (interview with Fatima A. and Amina M., Nador 2019). Furthermore, Spanish skills and the knowledge of Spanish cultural codes made it easier to get a job in Melilla; it could help Riffians to evade control at the border, or even allowed them to function as mediators between Spanish- and French- or Arabic-speaking border officials after 1956 (interviews with Hamsa Y. 2018 and Musa A. 2019, Nador). Nowadays, Spanish border control often ignores the Schengen regulations – which allows residents of Melilla and the province of Nador to commute only with their ID, without the need of an international passport or visa – and denies access to Melilla arbitrarily. In this case, Spanish skills can help Nadoris to defend their rights (interviews with Amina M., Nador 2019 and Rachid B., Melilla, 2018). Spanish skills were and are thus a means to open possibilities on various levels, especially to economic success. As Spanish is part of daily communication in the region, my interviewees might have liked to use a language with which they could identify and at the same time confront me „at eye level" without any political intention. In this case, the choice of language would simply be the result of a regional history.

But several of the interviewees asked me why I had studied Darija, and did not accept my reasons; this also occurred in cases when Spanish was not explicitly chosen over Darija as the interview language, as Spanish was the language we communicated with from the start. In their eyes, my knowing Darija made me rather suspicious as they explained that Darija was connected with representatives of the

state and the central government. These reactions openly showed a great distrust in the latter. Reasons named for the distrust included both current politics and Riffian history. Several of them – quite rightly so – told me that I should have learned Tarifit instead. So, even if the use of Spanish was not explicitly expressed as a political act, the choice of Spanish shows a deep-rooted local memory that included a historical grown distinction between the Rif and the central state.

This local memory also shows in another observation I made. Using Spanish in the region was also a sign of cultural differentiation from the rest of Morocco. Riffian Nadoris see themselves as the autochthonous population (Hamid R., Melilla 2019). The non-Riffians are linked to the central government's policy concerning the Rif as well as to negatively perceived general developments in the region since the late 1980s. At that time, migrants from the Moroccan interior started to settle in Nador to take advantage of the Schengen regulation. Many moved to Melilla later. In the eyes of long-established Nadoris, these migrants are held responsible for the deterioration of relations between Riffians and Spaniards:

> „We understood the language, the customs, we knew what we had to do and what we didn't have to do. And they came from the villages, and they didn't know that there are litter bins, there are public toilets. […] The Spaniards didn't complain about us and they weren't afraid of us. Now they are. It's weird! We are weird. When you speak in Amazight or Arabic in Melilla they say 'uh, a Moroccan!'." (interview with Faris M., Melilla 2019)

Additionally, in Melilla, those Moroccans who do not speak Tarifit or Spanish are recognized as non-Riffians by Hispano-Riffians.[41] They often have difficulties in finding houses or apartments and are charged more money for rent.

Apart from my personal influence on the interview situation, in the region of Nador, Spanish is part of the local culture, history, and memory, and thus part of a political struggle. Many Nadoris switch easily between the cultural codes connected with Spanish and Tarifit and feel themselves to be a part of the Spanish-speaking community:

> „I had the fortune to follow bilingual Arabic-French education up to the baccalaureate in Nador. And in the last three years of secondary school, as a second language, I chose Spanish. Spanish because it suited me, I knew it much better. Maybe because I learnt Spanish very well when I was a child, because of the cartoons on TV, because at home, even in Beni Enzar,[42] we used to watch the Spanish channel. We knew all the Spanish ministers but hardly anyone from Morocco. And we still have this Spanish-speaking culture." (interview with Hamid R., Melilla 2019)

Language, Memory, and Belonging

Like in the case of pre-colonial Morocco, governments do not always control the entire state territory. Therefore, Jan Blommaert asks us to differentiate between nation and state, as the idea of a nation as a cohesive group and the state as a political territory are often not congruent (Blommaert 2005: 238f). As Benedict Anderson has demonstrated, nations are „imagined communities" (Anderson 2006). In order to create national cohesion, collective memory discourses, official languages, and traditions are created, which themselves represent invented geographies. These unifying measures are also called „politics of belonging." Politics of belonging structure societies by creating groupthink and by „sorting" people according to categories such as race, class, gender, sexuality, and citizenship (Halse 2018: 4; Yuval-Davis 2006: 204ff). Language, and especially written language, plays an important role in the politics of belonging and the creation of a „nation", as it is directly linked to education and administration (Blommaert 2005: 244; Mense 2016: 50-52). This leads to a connection of social position and language use:

> „Language users have conceptions of language and language use: conceptions of 'quality', value, status, norms, functions, ownership, and so forth. The conceptions guide the communicative behaviour of language users; they use language on the basis of the conceptions they have and so reproduce these conceptions. These are ideological constructs, and they are sites of power and authority. Language use is ideologically stratified and regimented, and the 'best' language/language variety is distinguished from „less adequate" varieties in every instance of use. […] Thus, written language would be valued more highly than spoken language; standard

[41] Hispano-Riffians are Spaniards with Riffian origins.
[42] Beni Enzar is a town located between Melilla and Nador.

> more highly than dialect, specific expert registers more highly than general lay registers, and so forth" (Blommaert 2005: 241f).

Consequently, people who do not master the language norms are not perceived as belonging or wanting to belong to the national construct.

For centuries, the Riffian society passed on its knowledge and memories orally. There did not exist a firmly established written language for all Amazigh-speaking regions. Apart from the Tifinagh alphabet, people would use the Arabic and Latin alphabets (Ennaji 2005: 73f). According to Ennaji (2005: 73), in Morocco „[an Amazigh] native speaker is considered literate if s/he can write Arabic or French. This is why literacy is associated with the latter languages rather than with [Amazigh] or Moroccan Arabic." Though, finally, Tifinagh was declared the official alphabet for all Amazigh languages in Morocco in 2011, it is also discussed critically amongst the Amazigh-speaking communities as an invented artificial script (Aixelà Cabré 2022: 195f).

As mentioned before, after the Moroccan independence, Tarifit and Spanish were the known languages in the region of Nador. With the changing language politics after 1956 (see above) Riffians have suffered disadvantages and discrimination, as my interviewees recalled:

> „My father spoke Riffian – Tarifit – and Spanish because he studied Spanish when he was a soldier under the Spanish flag in Nador. When he emigrated to Germany, he learned German. But he did not speak Arabic or the Moroccan dialect Darija. One day, when he went to the Moroccan administration in Nador, they detained him for three days and beat him because he did not speak the language requested." (interview with Houssein M., Melilla 2018)

In the region of Nador, Spanish is still a local marker, which differentiates long-established Nadoris from Arabic- and French-speaking officials or Moroccans from southern regions, the former French protectorate zone. The use of the Spanish language has different connotations, which are rooted in the region's history.

The interviews took place in 2018 and 2019, when the Hirak movement and the violent reaction by the state were still on everyone's lips. In Nador too, the local memory is marked by the violent reactions of the Moroccan central government to Riffian claims for recognition and attention to local needs. The feeling of *Al-hogra*, or nostalgic references to the Spanish Protectorate, were present in many interviews with Nadori-Riffians, sometimes expressed very emotionally, sometimes stated in calm personal analyses.

However, the aims of the Hirak movement were not separationist. On the contrary, it demanded a more adequate response of the Moroccan government towards the regional specifics within the Moroccan state regarding the Riffian culture and identity including investment in education, healthcare, infrastructure, and industry in consultation with the local population, and the demilitarization of the Rif region (Wolf 2019: 4f). Even in the *Déclaration de l'autonomie du Grand Rif* – apart from elaborating the geographical, cultural, and historical peculiarities of the Rif region as unifying elements – the idea of autonomy was not only considered for the Rif region but thought of as a new political structure for all Moroccan regions (Raha 2007).

In her article „Colonial Memories and Contemporary Narratives from the Rif. Spanishness, Amazighness, and Moroccaness Seen from Al-Hoceima and Spain", Aixela Cabré (2019: 858) writes that „Moroccaness in Al-Hoceima is an identification that usually fails. This is because people belonging to or identifying with this national identity strongly feel Riffian or Imazighen, not Moroccan." In my interviews too, I found a stronger connection to the Rif and the Riffian cultural heritage than to Morocco amongst my Riffian interviewees. It is important to note that the Riffian society is in itself quite heterogeneous. There might exist Riffians who would prefer an independent state. Nevertheless, amongst my interviewees there was a clear awareness of being Moroccan citizens.

Therefore, I argue that the use of cultural and regional symbols, or preferring Spanish over Darija, are to be understood as reactive responses towards the suppression of diversity by the Moroccan central government. Spreading these symbols and expressing the regional peculiarities can be conceived as elements of „strategic essentialism", in Gayatri Spivak's understanding. Different groups deliberately chose these symbols to make the Riffians' particular interests heard within Morocco. This argument is supported by the Riffian sociologist Nahhass, who not only criticizes the historical construction of the image of a „dissident Rif" but also sees the Riffian revolts and demonstrations as an attempt to reorga-

nize the relationship of the region with the central government: „In other words, the construction of national belonging, but a belonging in tension and in dissent" (Nahhass 2019: 18). This aligns with Yuval Davis' (2006: 206) ideas of politics of belonging:

> „[t]he politics of belonging includes also struggles around the determination of what is involved in belonging, in being a member of a community, and of what roles specific social locations and specific narratives of identity play in this."

The preference for Spanish over Moroccan Darija for political as well as skill-related reasons can be seen as part of the struggle about the region's relationship with the central government and, in the end, with its struggle for belonging within Morocco. The use of Spanish can thus be seen as an expression for the demand of diversity within the Moroccan context. Rejecting the use of or simply not learning the official spoken language, Darija, is an oppositional reaction to the perceived rejection by the central government. Conversely, it can be seen as an expression of the desire for recognition of belonging including the local needs and cultural characteristics by the central state.

Conclusion

My lack of language skills in Tarifit and the consequent preference of Spanish over Darija as the interview language by my interviewees not only put me in a personal and methodological dilemma but also directly threw me into an ongoing heated debate about the Riffians' place within the Moroccan society. The use of Spanish in the interviews, however, gave me an insight into the very local specificities regarding struggles for belonging in the Nador region.

As already mentioned, conducting the interviews in Tarifit would have allowed some of my interviewees, who did not grow up bilingually with Spanish and Tarifit, to express certain ideas and memories in a more detailed manner. Also, my role as a researcher might have influenced the decision to talk in Spanish rather than in Darija. However, the preference of Spanish over Darija together with the local memory discourse and observations of the use of Spanish in daily life showed that the choice of language is the result of a complex relationship of the Nadori Riffian population to the Moroccan central government, and part of an ongoing struggle for recognition and belonging. Imposed during the colonial period, Spanish later turned into an economic and social asset and still forms part of the local Nadori Riffian culture.

In Morocco, the imposition of Arabic and French as official languages together with economic and administrative regulations after Moroccan independence in 1956 was a means to create national cohesion with a certain idea of Moroccanness. Criticism and demonstrations towards this Moroccanness were and are suppressed with great force. A process of coming to terms with the past and a certain recognition of the crimes committed against the Rif population during Hassan II's rule only began in the 2000s, after 40 years of silencing (Aziza 2019a: 104-117).

The refusal to speak Arabic and the decision to speak Spanish instead are part of a rejection of these politics of belonging. By choosing to participate in interviews in Spanish and not in Darija, the Riffian Nadoris expressed a criticism of the central government regarding its politics concerning the region. At the same time, they demanded the government's recognition of their belonging to the Moroccan state and its equal preoccupation with the region of Nador as much as with the rest of Morocco. It shows the region's peculiarity as well as Morocco's historically grown cultural complexities, and the need for recognition of these peculiarities by the Moroccan central government based on a different, more open concept of Moroccanness, and thus of belonging in Morocco.

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Interviews

All interviews were conducted by Sofie Steinberger during her research in Melilla and Nador in 2018 and 2019 and are archived privately.

7. The Inclusion of Remote Methods in Anthropological and Historical Research: Opportunities and Limitations

Gerda Kuiper

Introduction

My research project on the trade in second-hand clothes in Tanzania took an unexpected methodological turn when the COVID-19 pandemic started. I had intended the research to be based on a combination of methods from cultural anthropology and history, for reasons I outline below. But differing from what I had planned, I also ended up combining on-site and remote data-collection methods. In this contribution, I describe this data-collection process and reflect on lessons learnt for the integration of remote data-collection methods in (combined) anthropological and historical research.

The inclusion of remote methods is not new. Already before the pandemic, historians incorporated remote elements into their methodology, for instance by making use of electronically available sources and combining them with print sources (Chassanoff 2013). And even though physical presence in the field seems to be essential for anthropologists, the traditional practice of long-term ethnographic fieldwork in one place is problematized within the discipline. Reasons for this problematization are that long-term fieldwork is often incompatible with a researcher's family life; that interlocutors might be mobile and not located in one „field site"; and that the assumed clean break between „the field" and „home" increasingly proves to be fuzzy or even non-existent. Therefore, as stated by Günel et al. (2020: n.p.), „anthropologists have been innovating methods and epistemologies to contend with intimate, personal, political, or material concerns". These methods, which include remote elements, are emphatically not about reducing research to brief field visits, but remain intimate practices based on long-term commitments and an eye for context (ibid.).

I argue that the recent experiences with pandemic restrictions can help researchers to innovate further and to think about sustainable forms of continuous engagement with interlocutors and with archives while reducing physical presence. The pandemic has reminded anyone who used to travel before that mobility is a privilege (Surie von Czechowski 2020). Also for a variety of other reasons, such as the unfolding climate crisis, the possibility that researchers can travel to field sites or archives should not be taken for granted. Magnani and Magnani (2020) argued that instead of letting crises frustrate methods, anthropologists must be ready to respond rapidly by adapting research practices, making use of new possibilities for interaction, and adopting mixed methods. This call remains opportune in a world in crisis, for anthropologists and historians alike.

The Research Project and its Intended Methodology

With my research project, I aimed to investigate the history of the trade in second-hand clothes (SHC) in Tanzania. Most research on the SHC trade in the Global South focuses on the current global value chain of the clothes, which has developed since a wave of trade liberalizations in the 1980s (see e.g. Brooks 2013). An important exception is the book by anthropologist Karen Tranberg Hansen (2000), which describes the history of the SHC trade in Zambia since the colonial period. However, also her study mainly focused on global connections and on

Figure 7.1: The *mnada wa mitumba* (second-hand clothing auction site) in Mtwara Town. (Photo: Gerda Kuiper).

the trade in large cities. In contrast, I aimed to study the SHC trade in smaller towns and rural areas, with a focus on local perceptions of this globalized trade. I chose the towns of Mtwara and Lindi, located on the Indian Ocean coast and close to the Mozambican border, as my main study sites. These secondary towns – about 100 kilometres apart from each other – were once port cities of great importance in East Africa, but the region has developed – or rather underdeveloped – into a periphery since the British colonial period. In the present day, the local ports are hardly in use and the region mainly receives its supply of clothes (and other goods) via road from the current main port of Tanzania in Dar es Salaam, some 500 kilometres to the north. Some traders based in the south themselves travel regularly to Dar es Salaam to get new stock. Others order their goods through *mitumba* middlepersons based in Dar es Salaam. It is noteworthy that many of the middlepersons and traders in Dar es Salaam are labour migrants originating from Lindi and Mtwara (Liviga/Mekacha 1998). The historically developed networks that facilitate the trade within Tanzania are therefore often tied to the southern region – but in complex ways. By scrutinizing these ties, I aimed to study the incorporation of seemingly peripheral regions into global economic flows through *translocal* trade and migration networks. I expected that the concept of translocality, defined as „the emergence of multidirectional and overlapping networks that facilitate the circulation of people, resources, practices and ideas" (Greiner/Sakdapolrak 2013: 375), could help in breaking away from a static understanding of the SHC trade as a case where global dependency relations erase local economic agency.

I planned to make use of a mixed-methods approach, combining historical and anthropological research methods. I understand these methods to be complementary because of the shared interest in continuity and change between the two disciplines (Macfarlane 1988; Pannabecker 1990). As Pannabecker (1990) argued for textiles research, the „ethnohistorical" method enables the researcher to extend analysis beyond the „ethnographic present" when looking at the dynamic cultural context of clothing. Moreover, I expected that the heterogeneity of sources typical for this kind of combined methodology (cf. Pannabecker 1990: 15) would result in a more thorough understanding of the shape and meaning of the SHC trade on Tanzania's southern coast.

The main anthropological methods I proposed to use were (a) semi-structured interviews with traders, consumers, government officials and tailors, and (b) (participant) observation, for instance as a consumer by purchasing second-hand clothes, or as an observer by traveling along with the traders and their goods. The main historical methods I planned to use

were (a) oral history interviews with long-term traders and (b) research in on-site and online archives. Whereas I planned to conduct the search of online archives behind my desk in Germany, all other research activities were planned to take place elsewhere. The pandemic that made travel impossible therefore thoroughly „frustrated" my methods (cf. Magnani/Magnani 2020).

Data Collection up Until the Pandemic Started

I started data collection with the historical component of my research – not in Tanzania, but in the UK. In August 2019, I carried out research in the UK National Archives, in the library of the School of Oriental and African Studies (SOAS), and in the Bodleian Library in Oxford. The various archival sources provided insights into how my study regions, Lindi and Mtwara, became a periphery within the colony of Tanganyika and remained so after Tanzania gained independence (e.g. as an aid-receiving region). The archives also contained some evidence of the long history of the imports of clothes and second-hand clothes into East Africa as a whole, and of the role of British NGOs in the distribution of and trade in SHC in Tanzania in the decades after independence.[43]

At the end of January 2020, I travelled to Tanzania. After organizing my research clearance, I started out with a few days of library research at the University of Dar es Salaam (UDSM). I then travelled to the southern coast, where I carried out interviews with traders, consumers, and tailors around sites where *mitumba*[44] are auctioned and at unlicensed market stalls (see Figure 7.1). These activities provided insights into the local workings of and perceptions of the trade.

The research project also unexpectedly turned into a family enterprise when my (Tanzanian) husband decided to try out the trade himself. He ordered a few bales of clothing in Dar es Salaam through a befriended middleman and had them transported to the south on a bus. He then had the clothes auctioned with the help of an experienced auctioneer. Whereas a few bales contained good pieces of clothing and gave some profit, most bales yielded less than they had cost. I observed how my husband struggled to make some profit with this business and discussed his difficulties with established traders, which showed me the importance of experience and connections in this trade. It also taught me about the seasonality of the trade: my husband conducted this business in the rainy season, when people in rural areas have less to spend than in harvesting seasons. Observing this venture of my husband was the closest I got to participant observation during this brief period in Tanzania, next to purchasing a few pieces of clothing myself.

When the pandemic started, I was in doubt whether I could continue my research. While most parts of the world quickly moved into lockdown, everyday life in southern Tanzania largely continued as usual. In neighbouring Kenya, imports of SHC were temporarily banned (Oudia 2020), and Melkumyan et al. (2021) noted for bazaars in the South Caucasus that informality intersected with new regulations, changing the nature of informal trade in the bazaars. But on Tanzania's southern coast, the *mitumba* trade continued as usual, albeit at a slower pace. Nevertheless, when the coronavirus reached Tanzania, public spaces closed down and it was clear I could not finish my library research or start my intended research in the Tanzania National Archives (TNA) in Dar es Salaam. Moreover, I decided myself to reduce my mobility and to avoid crowded places, as I assessed the risks of the virus differently than other people around me.[45] I eventually left Tanzania towards the end of April 2020. By that time, I had spent about two months in the field instead of the five months I had planned for. I had not yet conducted oral history interviews nor had I visited several of the types of places where *mitumba* are sold, such as itinerant trader markets or licensed shops. I had not spoken to government officials about their perspectives on the trade and I had not „travelled along"

[43] For more on these findings, see Kuiper (2020b).

[44] Since the 1980s, imported SHC are locally known as *mitumba* (Swahili for „bales", referring to the way the clothes are packed for transport), which is the term I will use here whenever referring specifically to the trade in SHC in Tanzania in the past few decades.

[45] I have analysed these differences in risk perceptions and the consequences for the position of a researcher elsewhere (Kuiper 2021).

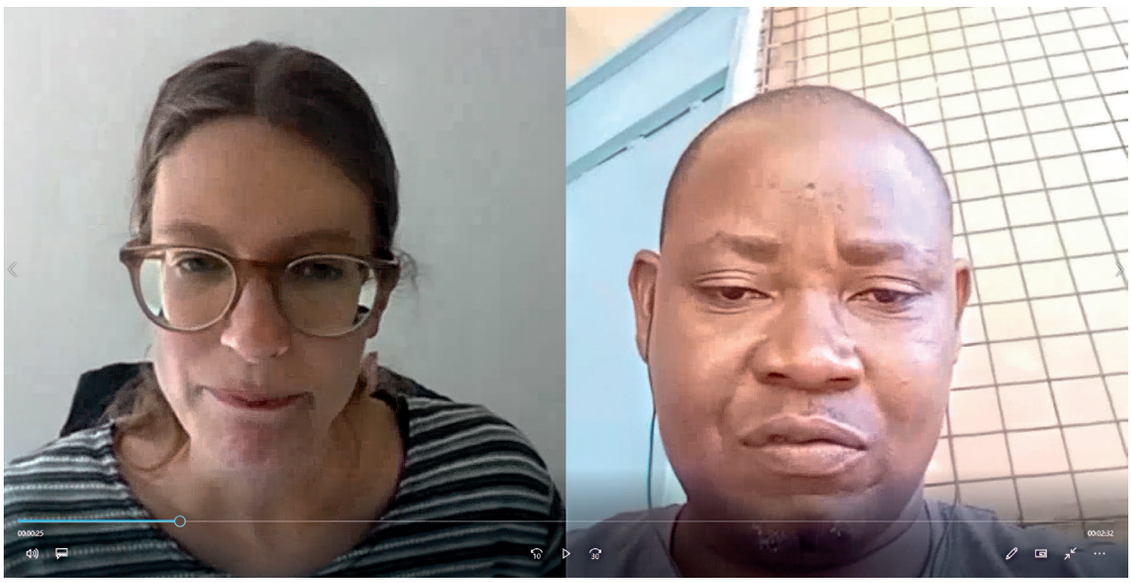

Figure 7.2: Still from a remote oral history interview over Skype.

with the goods, for instance to village markets, as I had planned to. My fieldwork was „quarantined" (Kuiper 2020a).

(Modestly) Successful Remote Methods

When it became clear in the course of 2020 that I could not return to Tanzania within the framework of my project, I looked for ways to convert the remaining methods to a remote format. The project funder agreed that I would use remaining fieldwork funds to pay for remote assistance and a smartphone for the assistants. I thus shifted to remote methods, with varying success.

I first of all could of course continue with online research, such as a keyword search in the online archive of the Swahili newspaper *Mwananchi* and its English counterpart *The Citizen*. The articles I found provided many insights into perceptions on the *mitumba* trade in the past ten years or so, but these archives have little historical depth since only the articles from the most recent years are available. In addition, it was not as easy as I had hoped to access governmental documents online. I had some experience with the method of searching online archives from previous research in Kenya, where court cases, parliamentary records, and the government gazette (since the colonial period!) are not only put online but can even be easily searched through a keyword search. I did not have this luxury in the Tanzanian context, where much fewer government documents are published online. This lack of online archives made it even harder to access governmental opinions on *mitumba*.

Francis, a student at UDSM at the time, assisted me in accessing some documents I could not access online. He visited the Tanzania Bureau of Standards to obtain a copy of the official quality standard on second-hand clothing. He also visited the UDSM library to finish the review of MA and BA theses and other literature that I had started myself. The library has a good online catalogue, which made it possible to give Francis clear instructions. I was thus able to get copies of relevant parts of theses on, for instance, marketing strategies of *mitumba* traders in Dar es Salaam and on the question of why Tanzanian consumers prefer non-Tanzanian products (including clothes).

Another method I had originally planned to use was oral history interviews with long-term migrant traders on the development of the trade, their life histories, and their (translocal) social networks and trading practices. I decided to conduct a handful of these (semi-structured) interviews via Skype, with traders based in Dar es Salaam who had been in the trade for a decade or more (see Figure 7.2). I managed to do four interviews but I encountered major obstacles compared to face-to-face interviews. First of all, I had to depend on snowball sampling: I asked one trader I had already known for many years – Mudi – to ask a few of his fellow traders whether I could talk to them via his phone. Moreover, the in-

Figure 7.3: A temporary stall at the itinerant traders' market. (Photo: Omari Chinowa)

terviews were rather short for oral history interviews and lasted only around 20 minutes, except for the one with Mudi, with whom I spoke for 50 minutes. Due to fluctuating connection and the lack of rapport, it was not possible to go into depth in these interviews. Nevertheless, the interviews did bring out the perspectives of these traders on their own socio-economic position and on the changing government policies since they had entered the trade. Moreover, an advantage of interviewing via Skype was that it was easy for me to ask if I could record the interview while the camera was part of the interview setting anyway and was therefore not experienced as invasive.

Finally, I asked Omari, the assistant who had accompanied me when visiting trading places in Mtwara while I was in Tanzania, to carry out short structured interviews with itinerant traders. These traders move around the Lindi and Mtwara regions as a group.[46] They have a fixed monthly schedule and although some sell other goods, such as kitchen utensils, many of them specialize in *mitumba*. I prepared a questionnaire in Swahili, and instructed Omari on how to install an audio-recording application on the research smartphone. He started out the interviews by approaching the leaders (*viongozi*) of the group, for whom I had prepared a separate set of questions. He asked them about the origin of the group and how it functions. He afterwards interviewed 16 *mitumba* traders about their business. An important insight of these interviews was that many of these traders were based in Dar es Salaam, even though they would travel around Lindi and Mtwara for about three weeks per month. The interviews moreover provided insights into motivations to trade in second-hand clothes (and not in other goods) and to be an itinerant trader (as opposed to opening a fixed market stall). Finally, the interviews gave some information on the trade in *mitumba* in villages, which was important, as I had not learned much about this aspect through other methods.

A disadvantage of the method was that the interviews were conducted in the busy market and it was sometimes difficult to hear what was recorded. Another disadvantage was that certain pertinent follow-up questions were not asked. These interviews were more structured than the semi-structured interviews I usually conduct when I am in the field myself. An advantage of these structured interviews was that the comparative analysis of the socio-economic situation and perceptions of the different traders was more straightforward.

Omari also took some photos of the itinerant traders' market with the smartphone, which comple-

[46] In Lindi, the group is known as *watu wa tarehe moja*, the people of the first of the month, because they always set up camp in Lindi at the start of the month before moving to larger villages around Lindi, and south to Mtwara. However, I learned from the interviews that the group was registered as *Chama cha Wafanyabiashara Kusini* (Association of Business People in the South), or CHAWAKU.

mented the pictures I took of other trading places when I was in Tanzania myself (see Figure 7.3).

Methods that Proved to be Difficult without Physical Presence

My plans for participant observation (beyond purchasing some clothes and observing my husband's attempts in the business) fell through. I could not find a remote alternative to, for instance, traveling along with the itinerant traders. I considered trying out a tool called *Ethnoally*, developed as a way of including digital tools into ethnographic and participatory research (Favero/Theunissen 2018). I installed the application on my phone, but I found it to be quite complex to use – probably too complex for the context I was working in, where most people have had no or only limited exposure to smartphone applications. Moreover, I realized assistants and interlocutors would need to invest much more time in this kind of participatory research than they would in the case of simple interviews or in the case of participant observation, where a researcher tags along and does all the observing, note-taking and archiving herself. Even though the tool is free, it would require more resources than I had available, and therefore I did not follow up on it. I concluded that the tool might be useful when planned into one's methodology carefully and for certain research topics, for instance when studying people's perceptions of their environment. But I could not use it as a substitute for participant observation.

Research in the TNA also was not possible remotely. I considered asking an assistant to go there on my behalf once public buildings opened again, but I lacked an overview of the collection since there is no online catalog and could not give clear directions. I thus lacked access to the most important source for getting an overview over policies between independence and roughly the year 2010. Whereas the (under)development in Lindi and Mtwara has been researched by others, there is no comprehensive literature on SHC imports into Tanzania during this period. The lack of direct sources on policies on the SHC trade remains a challenge in writing up my research.

A final method I had planned for but which did not work out remotely was conducting interviews with licensed shopkeepers who (also) sell *mitumba*. I wanted to include them to observe similarities and differences in trading practices and perceptions between different types of traders, such as licensed versus unlicensed, and itinerant traders versus traders with a fixed trading spot. However, I had not become acquainted with such shopkeepers during my stay in Tanzania, and Omari, my assistant in the south, was also not familiar with one. We thus lacked an entrance, which shows how crucial a researcher's existing networks are when conducting remote methods.

Results of the Research Project

Despite the remaining gaps, the on-site and remote methods together gave me sufficient insights to write up some results – albeit partly different results than I had foreseen.

Firstly, a proposed Special Issue on African moral economies inspired me to look at my data in the light of moral economic contestations of distribution and redistribution as mediated through the *mitumba* trade. The article (Kuiper 2023) is based on sources that were available online, grey literature, and a few documents I accessed in the UK archives, supplemented with consumers' and traders' perspectives that I recorded during on-site and online interviews. The article focuses on national debates in Tanzania, even though I had set out to study translocal dynamics in the trade. Through the (on-site and remote) interviews conducted in Lindi and Mtwara, I did find that there were certain „multidimensional and overlapping networks" that enabled the trade (cf. Greiner/Sakdapolrak 2013: 375). But I could not fully comprehend or document such complex networks and their role in the trade since I missed the opportunity to „travel along" with the clothes or with the itinerant traders.

Secondly, although I have been able to trace the colonial history of the SHC trade in East Africa, information on the period after independence is rather patchy, since I could not access the national archives in Tanzania and mainly based my investigation on grey literature. I was also not able to fill this gap with the remote interviews I conducted with long-term traders since their experiences only dated back to the 1990s. The finding that the trade dates back to

the colonial period is nevertheless meaningful, since the long history I documented came as a surprise to both *mitumba* traders I spoke to and to scholars to whom I presented my findings. I therefore still intend to write an article on the longer history of the trade in East Africa, as it can shed new light on current debates on the trade. However, this article, too, will by necessity focus more on East Africa as a whole rather than on particularities of the trade in Lindi and Mtwara since I have too few sources on the history of textile trade in this particular region.

Enabling Factors and Limitations for (partly) Remote Fieldwork

My experiences with changing regular fieldwork to remote methods highlight some enabling factors and some challenges and limitations for remote anthropological and historical research, whether conceptualized as such or initiated later, when field visits have become impossible for one reason or the other.

The increasing digitization of archives is an important enabling factor for remote historical research. At the same time, relevant context gets lost when one cannot physically browse a collection (Chassanoff 2013). In addition, relevant sources might be missed when one relies on keyword searches and when lateral searching is not possible. Moreover, digitization is a highly uneven process. Whereas remote archival research worked well in my previous research in Kenya, I hardly found any digitized archives for Tanzania. Such uneven processes of digitization of archives increase inequalities between regions and countries. Areas and topics that are represented in digitized archives are more likely to be researched because of the ease of access. Already-marginalized viewpoints are even more likely to be left out. The same counts for digital tools that aim to make anthropological research more participatory and inclusive: the viewpoint of people without access to smartphones and the internet or without the necessary skills to use them are by default excluded (Kuiper 2020a). Gellner (2020: 271) stated during the pandemic: „Activities that previously felt like guilty procrastination can now be relabelled netnography, but this leaves the digital divide, between those online and those barely online or not online at all, more gaping than ever." This digital divide cannot be wholly overcome, but can at least become less marked when applying a comprehensive mixed-methods approach, ideally combining remote access to archives and digital tools with visits to paper archives and face-to-face interaction.

My experiences also showed that initial fieldwork or previous contacts are indispensable for finding respondents for remote-methods research. Unless online interaction is an integral or major part of how the members of the community under study communicate with each other, one needs to have an initial network to get remote fieldwork started.

It is common for anthropologists to work with assistants during fieldwork, but they become indispensable for remote ethnographic research.[47] The researcher depends much more strongly on assistants' resources, knowledge, experience, and networks. At the same time, one also needs to take the position of assistants into account and make sure their work does not expose them to risk. An ethical issue I faced was the question of when it would be safe again for the assistants to move around on my behalf during the pandemic. Even though Tanzanians did not commonly practice social distancing themselves (Kuiper 2021), it is still a different matter to ask someone to go to public places or interact with people in busy places on one's own behalf and I did not do so until 2021. Even remote methods can therefore under certain circumstances become unfeasible.

Working with assistants and conducting interviews via video calls or by phone also comes with specific methodological and ethical challenges. Similar to what has been observed about archival research above, it becomes a challenge to grasp the broader context and to find out about phenomena the researcher is unfamiliar with, as an assistant might only report on what the researcher asks for. This lack of immediacy also plays up when addressing the question of positionality: anthropologists commonly reflect on their intersubjective position in the field when analysing their data and assessing empirical

[47] Compared to anthropologists, historians reflect little on the role of archivists and other „gatekeepers" in shaping their access to historical sources. This is the more remarkable since enlisting the help of an archivist or using finding aids compiled by archivists are common methods for historians to locate relevant sources in a particular archive (Chassanoff 2013).

value (Reyes 2020). This process is even more complex or even impossible when one's relation to interlocutors is not immediate but is mediated through another person and/or through a technical device (which for instance does not involve all senses). On the other hand, when conducting research with people in disadvantaged positions, it might be an advantage to have the interview mediated through an assistant or through technology. I argued for during the pandemic that there is a need to think of non-invasive methods, commensurable with social distancing (Kuiper 2020a). The need for less invasive methods also remains during non-pandemic circumstances, for instance when being seen with a foreign researcher can be dangerous or socially unacceptable for interlocutors. Similarly, Peirson-Webber (2021) noted that her interviewees were more comfortable during oral history interviews over video conferencing than in in-person interviews and were more inclined to discuss sensitive issues, perhaps because both the interviewer and interviewee were talking from their respective living rooms and were not in a traditional interview setting, which can make interviewees feel uncomfortable. There are therefore certain methodological and ethical advantages to interviews conducted by assistants or over the internet. Nevertheless, the lack of context remains a major limitation in such remote data-collection methods.

Conclusion

We must prepare for a future in which the possibility of travel is not a given (to the extent that it ever was), even after the COVID-19 pandemic has receded. My experiences with shifting to remote data collection halfway through my project show that remote methods open up certain opportunities – but also imply many constraints. For instance, whereas digitization provides new opportunities for accessing archives and for conducting interviews remotely, it also comes with new methodological and ethical challenges, such as how to overcome the „digital divide" to reach people and histories that are not connected to or represented on the internet. In addition, creating networks or „building rapport", already common challenges in on-site fieldwork, become even more challenging in remote data collection. The result might be that access is so patchy that a researcher is left with more questions than answers. However, there are a number of advantages when remote data collection can be combined with or preceded by (shorter-than-usual) on-site fieldwork. Günel et al. (2020) call this approach „patchwork ethnography", but in my opinion it can also apply to historical methodologies or to a combination of ethnographic and historical methodologies. Methodologically, a combination of remote and on-site data collection provides more possibilities for triangulation, more input from local assistants (who in some cases have a better rapport than foreign researchers) and more time efficiency. Ethically, such an approach – for instance when based on one rather than several field visits – could help in reducing the carbon footprint of research, might be more accessible to poorly-funded researchers, could be experienced as less invasive by interlocutors, and helps researchers in achieving a comfortable balance between work and family life. I therefore consider such an approach, one that combines remote and on-site methods, to be more than something to resort to during pandemic times: it constitutes a valid and promising methodological approach for ethnographic and historic research, both now and in the future.

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