Sharing Oral History With Arctic Indigenous Communities: Ethical Implications of Bringing Back Research Results

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Abstract

This article discusses ethical implications when sharing results in oral history research. We look at a case study of an Arctic community in Russian Lapland dealing with boarding school experiences. Bringing back research results about this topic provoked diverse reactions. We examine how the social life of stories and the social life of research are interconnected. By questioning the strict applicability of preformulated ethical research principles, we conclude that bringing back research results poses an opportunity to negotiate an appropriate form of reciprocity in research and to gain a deeper understanding of social processes in the communities under study. We identify principles of long-term engagement, collaborative methodologies, and inclusion into the cultural intimacy of the participating community as preconditions for a robust ground for ethics in oral history research.

Keywords

sharing of research results, oral history, consent, research ethics, collaborative methodology

> The biggest bombshell often comes when outsiders return their findings to the participants.
>
> —Burawoy (1998, p. 17)

The publication and presentation of results in communities[1] they originate from creates situations that are discussed in those communities in terms of harm or benefit, be it collective or individual, short term or long term. Through our experience of doing oral history from an anthropological stance, we try to find innovative ways to conceptualize the difficulties arising in such situations of sharing. Should the priority goal always be to avoid conflicts and minimize what could be considered as potential harm? In this article, we look upon events that took place during the publication and dissemination phase of the Oral History of Empires by Elders in the Arctic (ORHELIA) project[2] in one community and consider those events an integral part of the fieldwork and production of knowledge.

The recent decades saw growing political demands by indigenous activists that research conducted with their communities should produce outcome that benefits these communities (Battiste, 2008; Denzin & Lincoln, 2008; Jonsson, 2011; Kuokkanen, 2008). The sharing of research results is discussed and exemplified as well in anthropology (Cooper, 2007, 2008; Jaarsma, 2002). Locals in remote indigenous communities often asked us to explain why scholars visiting their communities and collecting data never contributed to solving any of their own urging problems. People sometimes even told stories in which researchers were the harbingers of unwelcomed state measures aiming at assimilationist policies or the extraction of resources.

The ORHELIA project collected hundreds of hours of oral history testimonies. Since the very beginning of the project and as an integral part of reciprocity in research relations, we planned to return these testimonies and our research results in a form accessible to the informants in terms of language, style, and materiality. To bring back research results to communities taking part in the research corresponds to the principles formulated by professional associations like the American Anthropological Association (AAA, 2012), the Association of Social Anthropologists of the United Kingdom (ASA, 2011), and the principles of research of the International Arctic Social Sciences Association (IASSA, 1998). However, in our case, the main motivation to make research results available for community members lies in the

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fact that we engage in long-term relationships with field-work communities; over the course of the past 5 to 20 years, we have been regularly returning to our respective fields and people in the Finnish and Russian North.

In this article, we question the division between the social reality under study and the social life of research. In an attempt to bridge this division, the article itself combines the two by offering both an oral history inquiry and a discussion on the return of results to participants. The discussion shows the ambivalence of harm and benefit and of ethical questions connected to them. For this reason, we hold that it is not possible to give unequivocal guidelines to researchers; flexibility, and not the blind observation of fixed sets of ethical rules, is the key to a sustainable and mutually satisfactory relationship between researchers and participants. Instead of fixed rules, this article aims at giving some orientation to other researchers who plan to navigate through the ethically important and epistemologically rewarding task of bringing back results to participants in the field. We do this by combining examples from practice with theoretical considerations.

The History of a Collaborative Oral History Research Project and Our Concept of Coproduction and Sharing of Oral History

The idea for the ORHELIA project arose long ago, when its coordinator, the anthropologist Florian Stammler, met Pupta Pudanasevich Yamal and his wife in their nomad tent on the Yamal Peninsula in Western Siberia in 2001. They told their life stories, and their grandchildren could not believe how much they had gone through. They then asked if he could record more of those histories to bring some of this rich memory to younger people. The foreign guest with his tape recorder was obviously a catalyst to inspire the historical storytelling in the family. But the anthropologist among reindeer herders is also frequently asked about the life conditions and experiences of other groups with similar livelihoods in other parts of the Arctic, and this is how the idea of a project to collect and compare oral history of neighboring groups in the Arctic was born. The project had the following field sites: in Finland the Skolt Sámi, in Russia the Sámi on the Kola Peninsula, the European Nenets, the Nenets on the Yamal Peninsula, as well as a highly multiethnic community living at the Lena river delta (see Figure 1).

We would like to employ the abovementioned "myth of origin" of the project to develop a model for our understanding of oral history in the context of the communities we are working with. Evidently, the situation in the nomads' tent, with the anthropologist asking, the elders talking, and the younger generation listening is a quite idealized one. In many cases, we find ourselves alone with some elder at the

kitchen table in a private flat. However, whatever the setting of the transmission of oral history might be, it is always an act of communication between two or more participants, all influencing each other. Different actors are involved in the occurrence of the story. We see oral history as a coproduction of stories with different people involved: the ones who ask, the ones who tell, and the ones who listen. This involvement of different actors reminds of the way how Roland Barthes sees the operator (photographer), the spectator of the picture, and the spectrum (the depicted person or thing) as coproducers of the photography (Barthes, 1981). Their communication through the lens of the camera and the paper print produce a meaningful picture. The good photography needs obviously more than just the skills of the photographer, and a picture tells more than the message he or she had in mind when releasing the shutter. The same is true for a told story: the text knows more than the author (Müller, 1994, p. 257). We can also refer here to Mikhail Bakhtin's understanding of polyphony, initially developed on the example of Dostoevsky's novels (Bakhtin, 1984). What is true for written stories is even more true for the spoken performance.

Starting from this performative and collaborative understanding of oral history, it was logical to make collaboration with storytellers and their communities the backbone of the project's methodology. Collaborative research methodologies are widely discussed in anthropological literature. Across disciplines, they are widely considered part of best practices in research involving indigenous people (Cruikshank, 1990; Fluehr-Lobban, 2008; Holmes & Marcus, 2008; Lake & Zitcer, 2012; Morrow, 1995). For instance, community-based participatory action research is also required for health research with indigenous communities by federal grant funders in Canada, Australia, and New Zealand (Minkler & Wallerstein, 2003). For us, collaborative research meant that people from the participating communities took part in all the phases of the project—the planning phase, the fieldwork, the analysis of the stories, and the dissemination and publication phase. We asked our interview partners and local cultural activists to actively collaborate in the research outcome production by taking part in the selection, annotation, and translation of the interviews, discussing appropriate forms of publication, and organizing events to present the research results. One could even say they were involved already before the project officially started and will be involved in activities after the research project has finished— through long-term social relations of the researchers in the field. Without such long-time relations with not only families of reindeer herders, hunters, and fishermen, but also indigenous scholars, political activists, teachers, and state administrators, we would not gain the needed trust to do research in remote Arctic places in Russia and Finland.

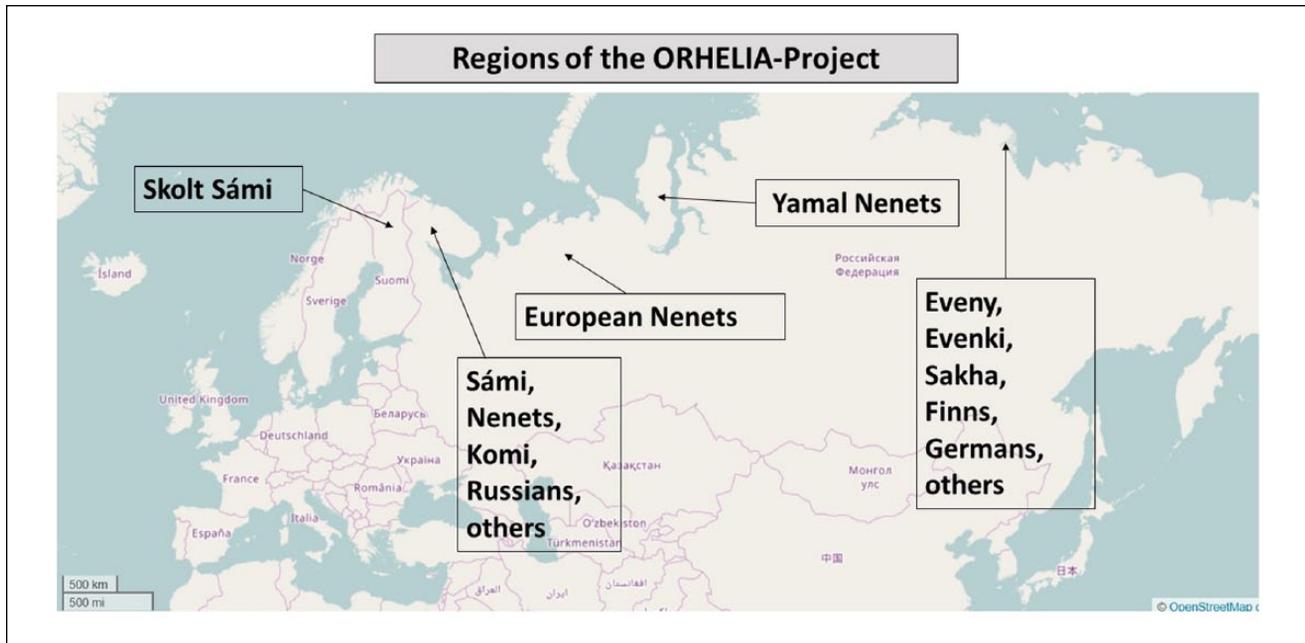As a part of our collaborative approach, we were looking for an appropriate way to disseminate our research in a

Figure 1. Regions of the ORHELIA project.
Note. ORHELIA = Oral History of Empires by Elders in the Arctic.

nonacademic form among our project contributors, and we opted for creating a memory stick containing interview extracts, selected with consent by the authors of the stories. There are several reasons why we chose this format. We did not create an Internet page as a first popular outcome because it is a highly public format, not limited in the number of potential recipients, whereas the Universal Serial Bus (USB) stick is a hardware with a limited number of units, to be given only to the contributors to our project. For both reasons—that it has less publicity and that it is a piece of nicely packed, tangible hardware—our field partners perceived the USB stick as a personal gift and as an acknowledgment of their contributions. An Internet page for the broader public as well as a popular book, both of them in three languages (Russian, Finnish, English) are currently under development. Another, more practical reason why we opted for a memory stick was that in remote regions such as, for example, the Yamal tundra, people do not have any Internet connection most of the time, but they usually have laptops, which they use for watching movies in the evenings. Hence, the idea behind a USB stick with multimedia content about biographies of elders from their and neighboring Arctic regions was that it could arouse interest about the past among family members from different generations. The stick contains a selection of interview extracts from the different field sites of our project. The extracts are either audio or audiovisual recordings, with subtitles added for a better understanding. In addition, it provides pictures and short biographical information. The concept is to present firsthand materials, not analysis. The contributors are

central, not the researchers. We presented the ORHELIA memory stick in several of the communities where we had conducted our research and tried to give a personal copy to each research participant.

The Remedial School of Lovozero as a Case in Point

Lovozero, in the Murmansk Region in Russia, is one of the villages where we collected and later shared stories. There we presented the ORHELIA memory stick and showed excerpts from our interview recordings from different regions and about different topics. The presentation was advertised beforehand in the local newspaper and took place in a public location. About 40 members of the community were present: people who had participated in the project and others who had not, from all generations and with different ethnic backgrounds. This presentation, discussed in the current article, was an important part of our endeavor to share research results.

In this section, we outline the historical dimension of the topic that prompted most of the reactions after the presentation. In the following section, we focus on the reactions triggered by the presentation and reflect on some more generally valid implications for us researchers.

The by far most controversial topic of the presentation turned out to be the local, so-called remedial boarding school (*vspomogatel'naia shkola-internat*). In the 1970s, this school had been officially set up for mentally retarded children—speaking in terms of that epoch—but there is

evidence that in this multiethnic village it was filled with many Sámi children from relocated families, many of whom were mentally healthy and whose only "retardation" consisted in underaverage knowledge of Russian language and other features of the majority culture.

Why did we choose, out of a vast amount of stories, the topic of the remedial school for our public presentation? To answer this question, we would like to go one step back and ask how the remedial school became a topic of research and how we as researchers became interested in it. First of all, our approach of nonstructured, narrative biographical interviewing—or rather conversation—left it to the interlocutor what to speak about. During the more than 90 hr of conversations recorded among Russian Sámi people, the remedial school was a recurring theme, albeit not an immediately accessible one. Although by far not all of the interviewees attended this school, many of them, while attending the regular boarding school of Lovozero, were affected by fears of ending up there, or at least bewilderment on why some classmates used to suddenly disappear and later turn out to have been transferred to the remedial school.

What follows is an excerpt from an interview with Ol'ga Fedotovna Aleksandrova, an inhabitant of Lovozero, Murmansk Region, Russia.[3] We presented this part of the interview in consent with the interview partner on the USB stick as an audio recording in her home village.

Three people are sitting at the [medical-psychological] commission.

They are looking at you.
Yes, they are giving you cards, checking if you did it correctly. And everything that they tested was to check our level of mental disability. Well, was it not like that? Although they too, probably, sat and looked at us. They would say, "Well, go work with her." You know, I'm just looking at this from a distance now, and it's not the first time—the picture has been imprinted in my head since childhood. If I were in their place I would have looked on the same way, and having looked, would have said, "What is going on with her?" Children are different. She should do things that will relax her—there is a reticence. She cannot speak, she knows but keeps silent. It's clear.
Yes, but they were not concerned with this.
Because it seems they got instructions from above. It [the remedial school] was opened, so it had to be done. There, the money is parceled out, they hired educators. And so we sat. Necessary means necessary. Well, on this they made money. And I was thinking the whole time what would happen if I did not go. I would go to mum and say . . . I do not know if mum knew what was going on—I always imagine myself in her place. I think, what would she have done, when that is how it is, without your knowledge, without the parents, they bring [pupils]

to the commission. I do not know if she did not care about us . . . [too emotionally moved to continue]. She had some concerns, like feeding us. But if school then school, if another school then let it be another school. The most important thing is that we are dressed, have shoes on, that is it, as quickly as possible. Well, that is how it was. And this, it will likely follow me my whole life. It is not understandable, for what, why. And why me? [. . .] And most of all, it is so hurtful. And I, if I may say so honestly, I am hurt because my mother was not able to protect me from such arbitrariness—to just go and stand her ground and say, "Why does she have to study there?" Well, all the same, I do not know. It is very hurtful. And I—of course already now, maybe I already forgive her. Of course I forgive, but it is still there, and that is why I try for the children, to fight somehow for the grandchildren. Just as I can. Because as much as I remember myself from childhood, I was very withdrawn. I do not know why. Oh, I was so quiet. As they say, I know but do not speak. Maybe this also came into play when they brought me to the commission. The commission was in this little room of the director's. Oh boy, the room is high, almost 2 m. Closets, shelves, of course. There is this table at which an elderly man with glasses and two women are sitting, and there is so much pressure. The table is for testing me for debility [*debil'nost'*]—am I mentally retarded or not [chuckling]. So they tell me to add this and that. You add everything after all. I am thinking, why are they asking me, if everything here is clear. Well, this is a child's perception. Of course I immediately, it was obvious . . . maybe the voice was something . . . they said something, I got upset. But when one gets upset the tears come right away, it is all so close to the surface. So they say alright and give me a bandage and gauze, there go on now.
[. . .] I was in third grade, already before the holidays—well as usual we are cleaning the property, probably because it is spring, the month of May. We were cleaning the property, that is all, cleaning and waiting, because we know that they are going to give report cards. And there she [the teacher] comes out, we are all standing like this. And she opens them up [the report cards]: this one and that one. And what about us? What about the rest? "There is none for the rest." That was after the commission. And that's it, we leave. We come back and [at home] they ask me, "Where is your report card?" I say I did not get one. And for some reason nobody went to figure out why the child did not receive a report card. Maybe they were too busy. They didn't care about it. And then, the first of September, I went and they told me that I do not study here anymore.
So until the holidays nobody told you that you would be transferred?
No. And also there was this thing, that to this kind of commissions it turns out you must go with your parents.

I remember, they told me, where is your mum? I say, "She did not come." Why did I even do it at all then, I would not have come [if I had known]. But for some reason I decided and thought, well, if it is necessary, then it is necessary. Because . . . in my memory it is imprinted—me sitting there . . . oh. But then it turns out that comrade Olga Fedotovna Kirillova [the interviewee's maiden name] is not studying here. Oh how I wanted, I remember, at that time, to go to school. There was the regular [elementary and secondary] school nearby. I am thinking, maybe I should go there. I had no desire [to go to the remedial school], everybody could see we were all fine [meaning mentally well].

Figures from the State Archive of the Murmansk Region confirm[4] the information given by interviewees, that a higher-than-average number of Sámi children attended the regular boarding and the remedial boarding schools in Lovozero, as compared with the general multiethnic composition of the settlement. Through the analysis of this interview and similar stories, we started to see the reasons for this tendency within the general social structure in Lovozero, which was a fairly new order. The emergence of this new social order coincides with the end of the relocations of Sámi people from all over the Kola Peninsula to urbanized settlements—mostly to Lovozero—by the early 1970s. There is no space here to concentrate on the circumstances of these relocations; it may suffice to say that this process had lasted from the 1930s onward: first in the context of collectivization (1930s-1940s), then, after the war, due to Khrushchev's agricultural amalgamation policy (*ukrupnenie*) as well as in connection with the militarization of the coastal regions during the Cold War (1950s-1960s), and, throughout the whole mentioned period, due to industry and infrastructure development. As a result, almost the whole eastern part of the Kola Peninsula was emptied from civilian settlements, most of which had been Sámi villages (on Kola Sámi relocations see Afanasyeva, 2013; Allemann, 2013; Anderson, 1996; Gutsol, Vinogradova, & Samorukova, 2007). There are estimates that 70% to 80% of the Eastern Sámi population in the 20th century had to resettle due to state measures at least once, and many people more than once. In most cases, the destination was Lovozero (Bogdanov, 2000).

Nowadays, Lovozero is widely known as the "capital" of the Russian Sámi because most of them live there. Yet to call it a Sámi capital is misleading because only about 20% of the population of Lovozero are Sámi (Rantala, 1995). In earlier times, Lovozero had been a tiny Sámi settlement, which grew bigger in the late 19th century as a consequence of the immigration of a significant group of Komi together with Nenets reindeer herders, and in the 20th century as a result of an influx of population from the rest of the Soviet Union ("Lovozero, Lujavv'rsyiit," 2013; Ushakov & Dashchinskii, 1988). Since the late 1960s, the highly multiethnic Sámi/Komi/Nenets/Soviet-incomers mix of the vast lands of the

Kola Peninsula has been concentrated on the tiny village of Lovozero. We have an apparently paradox situation that although the Sámi never had lived in such a big group together, they are a minority in their new "capital," contrary to their old settlements where they made up the majority.

The relocated groups of Sámi met serious housing and job difficulties after their relocation (Allemann, 2013; Gutsol et al., 2007). Our interviews show that a struggle for local resources began between the locals and the resettled, while in terms of power relations those who had been in Lovozero before the relocations, mostly Russians and Komi, were at advantage compared with the resettled people, mostly Sámi. The district and village administration as well as the reindeer herding state farm (*sovkhoz*) always remained dominated by Russians and Komi.

In 1970, the remedial school was opened,[5] in addition to the two already existing schools (a regular daytime school and a so-called national[6] boarding school). Although official documents do not suggest any causal connection, notably, the opening of this school coincides precisely with the last major relocation.

As already mentioned, many children from relocated Sámi families were transferred to the remedial school, usually after completion of the first, second, or third grade at a regular school. Remedial schools were a countrywide, standardized educational institution for children with forms of mental disability (Bogatyreva, 1971). As interviewing both among former pupils and teachers from Lovozero has shown, there was a specific local pattern on how "retardation" was defined (it is possible that similar patterns were existing elsewhere, but we do not have research data about it). According to the Soviet rules, a yearly assessment was carried out by a group of experts called medical-psychological commission (further in this text: the Commission) arriving at the regular boarding school from the regional center—as described in the quoted interview—and asking questions using country-wide questionnaires (Zabramnaia, Shostak, Pikulin, & Bezrukova, 1971). These questionnaires were clearly not adapted to local peculiarities and contained questions that assumed an urbanized, Russianized upbringing all over the Soviet Union. The assessment was also taking into account the general Russian language proficiency of a child.

In Lovozero, many children who had spent their preschool childhood with their parents and not in day care failed in front of the Commission, and such children came mainly from relocated Sámi families who had not spent all their lives in Lovozero. As we can see from the quoted interview, failure was due not only to the questions asked but also to the psychological pressure in this unusual situation of standing alone as a small child in front of a commission of unknown people coming from a large city. According to several informants, having children undergoing the Commission testing without their parents was a common practice, although the norms prescribed the presence of a parent

(Ministerstvo prosveshcheniia SSSR, 1974). Our interviews have shown that parents of the children concerned were often working in the tundra at the moment of the Commission's appearance and were thus not available, or they were not aware of the seriousness of the situation because of their own low level of formal education and/or their own issues connected to the relocations, such as job, housing, or alcohol abuse problems.

The remedial school in Lovozero thus became the perpetuator of a local social order in which the relocated families were at the bottom. The school was more effective in reproducing the social hierarchy (cf. Bourdieu & Passeron, 1990) than having any assumed therapeutic effect for the purportedly "retarded children." In a general school system designed to provide similar opportunities to everybody and with a low level of separation, the remedial school was one of the few, but very strong means to separate allegedly weak children from their peers who had a firmer social background, or in Bourdieu and Champagne's (1992, English translation see 1999) words, to make "outcasts on the inside" (the authors used the term in a study of French high schools). The situation described above of parents not being able to accompany their children to the Commission or to guide and advise their children about the right choices and to oppose a higher authority's decision, is also mentioned by Bourdieu and Champagne. Their study concludes that children from the most deprived families, especially those with a migration background, can rely less on their parents' advice and support when it comes to decisions about their future, and they remain dependent on the enactments by the schools or simply chance and mischance (Bourdieu & Champagne, 1992, p. 73). The relocated Sámi in Lovozero were the most recently immigrated group of people in Lovozero, and the most disadvantaged ones in the access to resources.

The Soviet schooling system was nominally highly inclusive and, within the frame of a determinate cultural domination, has indeed given educational opportunities to millions of people, including Sámi children (an element also present in many of our interviews and resulting in many positive assessments of the educational opportunities offered). However, the relocated Sámi children who were transferred to the remedial school without being "really" disabled are an example of strong exclusion within an otherwise inclusive schooling system and of a mechanism of disguised reproduction of social classes and racial discrimination in a nominally egalitarian system.

Harm or Benefit for Communities

In this part of the article, we discuss the ambivalence of harm and benefit when sharing contested versions of oral history with the community they stem from.

A general principle of research with humans in social sciences is that it should not cause harm to people involved (see AAA, 2012, Statement 1; ASA, 2011, p. 4). The collaborative and participatory research methodologies deployed in our project require a potential benefit for all involved parties. Avoiding harm and maximizing benefits for the research partners and their communities appeared to be basic ethical principles of our work. The professional ethical principles as well as literature on research ethics use the term of harm as something unquestionably negative (cf. Iphofen, 2015, p. 11 on nonmaleficence).

In practice it is almost inevitable that the sharing of research results in public settings in the communities they stem from causes not only positive reactions. However, it is also difficult to give a clear definition of harm (Haggerty, 2004). Some members of the community may even challenge the legitimacy of the presented research. A risk appears to be refused future access to the community. We may even hypothesize that conciliating meaningful collaborative research with a full guarantee that nobody feels hurt is not possible.

The Lovozero Case in Terms of Harm or Benefit to the Community

With the outline about the relevance of the remedial school as a topic both on the level of the local community and on a higher systemic level in mind, let us now get back to the reactions during the presentation of the memory stick in Lovozero. There were both elders and youngsters of mixed ethnicities at the presentation, and during the playback of that recording everybody was silent and listening very carefully. It was very tangible how such a topic has much more actuality in Lovozero than, say, a story about the war or about the Stalin purges. Two older adults stood up during playback and left. It later turned out that they left for very different reasons: one woman was the interviewee herself, and she left because it was emotionally too difficult for her to listen to herself. She later called back, felt guilty, and even wanted to apologize for having left. The other woman left because, as she later said, she did not want to hear how foreigners again came to Russia to look only for the bad things.

After the presentation, there was a vivid discussion and yet another woman, a former teacher with 50 years of practice, even stated that such things as described in the recording never happened at the boarding school. Other people expressed their approval and support for our work. They emphasized the importance of uncovering the sad story of this school. From an epistemological point of view, this was satisfying because once more we got a strong proof of what always accompanied us while doing oral history: there is no one and only truth, but so much depends on different social contexts in which meaning is created (cf. Portelli, 1991).

The reactions and disputes after the presentation raise a whole bunch of ethical questions. Did we harm or benefit the interviewee who had first given consent to staging her

testimonies, but left during the actual staging and then apologized for leaving? Is there a benefit to the whole village by addressing the hidden pain from that school, and can this benefit outweigh the harm we possibly did to some individuals? Anxiety or distress are also a form of harm, and in this sense we exposed the contributor featured in the presentation about the remedial school to this sort of harm. On the contrary, she repeated many times—before and after that presentation—what feelings of liberation the conversations about her remedial school experience gave her; she had never talked about it even to her husband. It was a gradual process of opening up, and she gradually came to the conclusion that her village should know about her experiences, which were by far not unique. In general, an important point to assess is whether, from the perspective of the person or collective involved, potential harm is only temporary or lasts for a longer period. Long-term benefit might outweigh short-term harm. In the case of the interviewed individual, the long-term benefit might be, at least partially, to cope with a trauma. A long-term harm might be her possible stigmatization in the village. In this case, however, she had already decided that she would not feel threatened by such a potential harm. Her conclusion was probably founded on her current stable social standing as a pensioner, that is, as a person who is not anymore involved in any professional context, and as a beloved mother and grandmother. A long-term benefit to the village could also be increased awareness of what had happened, and this may increase social cohesion.

As to possible harm to the larger community, one might ask the following questions: What are the potential sources of conflict in the village and how can they be avoided? Should they be avoided at all? Indeed, the boarding school is a highly sensitive topic because former pupils and teachers still live together in one village. If we admit that there is a potential for conflict, should we refrain from raising this topic on a local level? Does a firm ethical ground exist in oral history research, on whose basis we can decide to share only certain research results with the communities of the contributors and not others? Or is the epistemic "surplus" in understanding local discourses a more valid motivation for a collaborative production and sharing of research results than ethical deliberations against the sharing and in favor of a certain censorship?

We would like here to cite an exchange of correspondence that we had with Yulian Konstantinov, a social anthropologist who has been studying the Kola Peninsula for more than 20 years. In reaction to our story about the presentation in Lovozero, he wrote,

> There is a Western ethical requirement that work is to be presented and, in a way, "shared." But it cannot be shared in this way, I am inclined to think. I have met people who are still offended at what Charnolusskiy had written in the late 1920s about tundra people, let alone what Ivanov-Dyatlov wrote at the same time about Sámi hygiene [Charnolusskiy and Ivanov-Dyatlov were Soviet ethnographers, comment by L.A. and S.D.]. The theme of this getting offended requires a thought, as it belongs to a discursive genre. The idea there is that foreigners or outsiders intrinsically cannot know the "truth." This is a form of romantic metaphysic. There cannot be any "sharing" in this context by definition. So Western ethical sentiments are misplaced. (Y. Konstantinov, personal communication, November 8, 2015).

We argue that for those locals who were supporting the statements of our presentation about the remedial school there was another intruder besides the researcher: the state. People depicting the Soviet state as an intruder are often close to activist stances. It is easier for them to support the sad stories about negative aspects of the Soviet educational system because they do not very strongly identify themselves with state structures. However, there are many people who are satisfied with the career and other opportunities they were given by the Soviet state (cf. Konstantinov, 2015). For those people, it was much more difficult to acknowledge our accounts—at least in public. In this sense, we have not been creating a new rift, but making visible existing fault lines by relating to a rupture that already exists widely throughout the post-Soviet countries: the question of seeing oneself rather as a victim or a beneficiary of the state, or even as oppressor or oppressed—two views which can easily coexist within one person, depending on the topic and the setting (cf. Humphrey, 1994).

During our fieldwork and presentation of research results, we noticed that people who have been at the regular boarding or remedial boarding school as pupils are more inclined to agree with the negative evaluation of the remedial school. Some remembrance can be painful but they were usually favorable to our research and to making it public. Criticism has come mainly from former teachers, but not from all. The key to understanding the reasons for these heterogeneous reactions lies in the assumption that the quest for meaningfulness of one's own actions is a basic trait of humans (Goffman, 1955; Haumann, 2006). What is intriguing in oral history interviewing is that over time, in the course of a life span, positionality and perspectivity may change according to changing discourses and ideologies. Hence, when we talk or hear about the state as an intruder, we should not see this as an absolute perspective. Positions can also change depending on the audience, which means there is a continuous instrumental use of positions. Positions can change very radically, for example, when political upheavals provoke the falling of ideological barriers (followed often by a new ideology or a struggle between ideologies). We must consider and reconstruct positionalities (Davies & Harré, 1990). When somebody talks about events in the past, we should reconstruct the current position

(which we can see in the narrated life history) and the past one (the experienced life history; see Allemann, 2013, pp. 17-21; Fischer-Rosenthal & Rosenthal, 1997). The interviewee reinterprets her past actions in the light of her current position, which might differ from her past position. Some motivations that counted as meaningful and "good" in the past may count as "bad," obsolete, or useless today (or in a given context, for example, in front of a certain audience) and are therefore rejected by the interviewee.

Two teachers, both already retired, whom we interviewed in Lovozero, can exemplify this. Both teachers, A and B, worked all their lives in boarding schools and ended their careers with director posts. However, Teacher A, who had been director of the regular boarding school, has been energetically denying that any forms of infringement and abuse reported by some former pupils are true. Teacher B, instead, told that in her opinion, retrospectively, the remedial school in Lovozero, of which she had become director, was a mistake. The difference in the biographies of the two teachers, which we consider to be crucial for their different evaluations, is that, whereas all of Teacher A's professional merits rest on her career as a teacher in the Soviet educational system, Teacher B, starting from the late 1970s, began developing Sámi language teaching and in post-Soviet times became known as one of the grand old ladies of Sámi ethno-political, linguistic, and cultural activism. Teacher B had a second anchor and hence did less feel the need to deny the dark sides of the Soviet educational system. She did not perceive their acknowledgment as a threat to a positive meaning of what she devoted her life to.

The example of the remedial school shows that public presentations of problematic and painful parts of local history might be considered necessary to make up for injustice and harm caused by events in the past. In this case, certain members of the community considered the public representation a threat to the meaningfulness of their individual biographies and to the image and prestige of the educational system they were part of, and even to the national image of Russia and its Soviet past. The researcher took the risk to make this conflict visible and allied with the storyteller. To take sides with the storyteller and to identify with the moral evaluations of the social group close to the storyteller does not guarantee the avoidance of a public scandal if stories are made public in the community that were not told publicly before.

However, in this concrete case, by the opinion of our research partners, although some emotional harm for certain individuals was created, bringing back those sensitive topics to the communities did not create a greater conflict within the village population but was considered a stimulus to tackle a problematic issue of the past.

Written Consent as a Universal Remedy?

Free prior and informed consent (FPIC) is today a basic requirement in most social research involving human participants (Fluehr-Lobban, 1994; Piquemal, 2001). Also in State-Indigenous relations, FPIC is widely recognized as best practice (Heinämäki, 2016). The role of FPIC in anthropological fieldwork is theoretically contested, and the possibility to reach consent in a final, written, and fully informed form is questioned (Bell, 2014; Thorne, 1980; Wax, 1980). We would like to take a look at FPIC by asking what is a meaningful and trust-building consent and whether a written form is always the better option.

Consent is often the precondition for a research project being financed and hence being possible. The strictness of the requirements about FPIC varies heavily depending on national legislation and the rules set by research funders and academic institutions. In general, ethical review boards, codes of conduct, and guidelines require to obtain informed consent for the participation in and storage and publication of research (e.g., Finnish National Advisory Board on Research Ethics, 2009; National Committees for Research Ethics in Norway, 2006). Concerning indigenous peoples, there are several international instruments stipulating FPIC requirements: they do often primarily aim at consent in industrial and other economic development, but in many cases have validity also for research (an overview is given by Wilson, 2016, p. 3). In the case of oral history recordings, not only questions of the rights to personal information but also authorship rights and copyrights are affected. In many cases, written consent is seen as the preferable option (e.g., East Midlands Oral History Archive, n.d.; Heritage Lottery Fund, 2014; "Written or Oral?" n.d.).

The idea behind signing a written consent form in oral history research is to inform the participant exhaustively about the possible consequences of the research and the publication of the research results. The declaration of consent to particular ways of research and publications is supposed to provide a forum to discuss conditions and responsibilities on both sides. It should follow professional ethical guidelines and ideally also allows the participants to influence according to their own moral standards particular codes of behavior for the researchers and the institutions involved.

Having put an FPIC form signed by participants *ad acta*, researchers could, quite comfortably, see themselves legally and ethically on the safe side and not feel the need for further reflection on the topic. Ultimately, funding agencies, researchers and their institutions, and publishers seek to avoid through FPIC procedures the risk of being sued by research participants for the violation of their individual rights. A signed FPIC form also implies the assumption that the informant feels more secure after the procedure of negotiating this form of written consent as a form of risk management; consent forms or similar written and signed documents are widely seen not only as a legal instrument but also as a trust-building tool (Johnston, 2010). However, what we experienced time and again with informants throughout our fieldwork regions in the Russian Arctic is that our contributors are in general reluctant to sign consent

forms. Given that FPIC is considered first of all good research practice to respect the needs and rights of the local research participants, we consider it necessary to reflect on the reasons for this reluctance.

FPIC is often expected to produce a signed agreement. Despite being called a process, and despite the option of review and revision (Wilson, 2016), a signed document is nevertheless perceived by both sides as something more or less fixed and legally binding. However, we think it is unwise to assume a priori that such a document necessarily enhances trust and confidence on the side of the research partners. People in communities we work with rely not so much on written documents than on oral agreements to secure responsibility. In remote communities, deepening informal social relations comes closer to what we may call compliance with the local rules of behavior. The written form is rather associated with state institutions, toward which people in general do not have much trust. They trust personal relationships and informal networks more than the official legal system and legally binding documents as a form of risk and harm management (for the role of the informal in Russia see Ledeneva, 1998, 2006; Zdravomyslova & Voronkov, 2002). With the written document signed before the research starts, trustful informal relations are, at least partially, substituted by a trust-hindering legalist framework. People mistrust their own abilities in handling the pitfalls of legal formalities: to understand the legal language and procedures and to use them in an appropriate way in case of violation. The move toward the legal regulation of potential conflict hence results rather in a feeling of disempowerment than the empowerment envisaged by FPIC procedures. One could say that this result, which is exactly opposite to the result presupposed by FPIC, is due to a dialogic discrepancy in which there is no shared code between the researcher (or his/her funders/employers) and the research contributor: the discourse of empowerment through formalization on the one side and the discourse of disempowerment through formalization on the other side do not match (for the concept of "dialogic discrepancy" between concerned outsiders and locals in a post-Soviet setting see Konstantinov, 2015, pp. 25-83, 216-220, 293). Moreover, consent does not account for future political and technological developments and the growth of knowledge. The idea that the present time is unstable and the future is uncontrollable is certainly stronger in the post-Soviet world than in Western Europe, and hence concerns about what will happen in the future with that consent paper are greater. An additional factor is possible fears about the practical feasibility of changes to agreements once they are signed, the fieldwork done, and the researchers gone. These are reasons why we think that oral agreement should be seen as a valid option for FPIC procedures.

If the researcher starts a discussion about the necessity to sign a written consent form, the social relation changes immediately from informal toward more official and institutionalized. People in remote communities fear that with a written consent the kind of relationship in which the researcher is personally responsible for the future of the recorded materials is terminated, as if the researcher canceled her or his personal and long-lasting responsibility for the good use of the materials entrusted. There is a perceived incompatibility between the two forms of responsibility: the informal and personal as well as the formal and legally binding. In settings where the informal and personal relationships are a primary source of responsibility and liability, it is not uncommon that people are ready to quickly sign formalities refusing to read the fine print or be otherwise additionally informed. They refuse this information because they see it as diminishing the personal relationship of trust of the researcher and informal responsibility to prevent harm to them.

A too legalistic stance on consent procedures, which does not take into account local historical and sociocultural contexts, is thus not favorable for a trustful relationship. For oral history work in small and often remote groups of indigenous people, in which knowledge is transmitted largely orally, we consider FPIC—even if based on the notion of a collaborative and open-ended interaction—not a sufficient solution for the ethical dilemmas connected to questions of what research results to share and potential harm. We should not relegate the question of ethics solely to formal and written documents. They may not produce trust but exactly the opposite: a lack of trust.

Relativizing Harm

In practice, harm is a contested category because it is individually very differently experienced. In our context, harm can be associated with negative emotional effects as well as damage for the social prestige and the public face of people and collectives. Harm to the public image of a collective is often connected to contested understandings of different historical events and the effects of different ways to represent and commemorate them.

Some forms of harm might seem obvious. Especially within communities that are subject to external negative stereotypes and different forms of marginalization, some representations may be seen as reinforcing these stereotypes, and hence as harmful. Depicting Arctic people as backward, as notorious alcoholics, or as deficient in their intellectual or physical capabilities, increases the harm that colonial policies and settler communities did to them. These clear cases in which nobody would question the potential harm should be avoided by any means.

Oral history is not just the history of individual lives, but of collective experiences embedded in complex social relations. This is also the case in the work we have done in Arctic communities. Especially when it comes to moral

evaluations of events, peoples' actions, or common practices, a positive or negative evaluation depends also very much on the context of the representation of the event. Today, almost all indigenous groups live in social circumstances where different moral systems are competing or coexisting. They move constantly between social contexts where different moral evaluations dominate. This is true for any person belonging to a culturally distinctive group or subculture, for example, also in culturally highly diverse urban environments.

The rules of recognition and respect are often set by concepts and values of the larger society and the mainstream culture. How a group wants to be seen by others depends to a large extent on the way the outer world defines reputation. In cases when the cultural values of the group differ greatly from the ones of the surrounding society, the public image may have almost nothing to do with the culture represented. Groups can have an interest to fight stereotypical representations of otherness and the exotic and to replace them by more appropriate ones. Conversely, they could as well try to gain profit from the popularity of such stereotypes, where a distorted image can prove more useful than being simply unnoticed; it can serve as a safe façade to hide spaces of hidden cultural autonomy. Collecting and publicizing oral history means to be involved in identity politics of a social group aimed at changing, modifying, and manipulating its public face, as well as external moral and social evaluations of the group (cf. Cruikshank, 1998, pp. xi-xvii, 45-70).

If we talk about stigma, to get involved in some action that enforces stigmatization seems unquestionably to be a no-go in fieldwork ethics. It seems that, as an external label, "stigma" is an appropriate description of the harm that is symbolically done to the status of a person. However, the representation of stigma and of facts of stigmatization can but does not incontestably reinforce stigma. Stigma can be redefined individually or collectively into a reason of pride. Practices associated with and knowledge about stigma, which would reinforce stigmatization if known publicly, are often carefully contained inside groups, and their knowledge marks the borders of what Michael Herzfeld (1997) called cultural intimacy. Practices that cause embarrassment if information about them crosses certain social borders and becomes known beyond them, serve to construct and maintain these particular borders. These features and practices may be revealed not only in rituals of transgression like carnival, but also in political activism aiming at social change.

Identity politics aiming at rehabilitation and compensation for former injustice and at recognition and respect for a marginalized group can favor the dissemination of information associated with shame and stigma. The history of suffering and victimhood can become an important asset in identity constructions and politics (Gallinat, 2009; cf. Nyyssönen, 2007). In these cases, stories that confirm the disadvantaged position and tell about misery, suffering, marginalization, and stigmatization might fit the agenda of storytellers very well. But identity politics may also engage in narratives that stress the achievements of a group, its level of sovereignty and autonomy, and the agency of people in the face of more powerful actors.

These situations are very much controlled by the people involved. Oral stories are verbal representations of events and practices removed from their original context, but they are embedded in a controllable face-to-face interaction: the storytellers know usually who is present during the performance and can decide spontaneously what to silence and what not, and how to tell the story. Often skilled narrators who have taken over the role of mouthpiece of a community make sure that the intended purpose is achieved.

These are all relativist standpoints. If in oral history it has become commonplace that there is no single truth, we would also suggest to turn away from the quest for a single moral concept of good and evil, of harm and benefit. The same person can consider something as harmful in one situation and not harmful in another. Harm can turn out to be beneficial in the long run, and a research partner can even consciously opt for some form of self-inflicted harm for intended beneficial ends. Besides that, moral evaluations differ according to social groups, political fractions, religious denominations, and other factors, and feelings associated with them are not static either.

The boarding school topic, highly present throughout the circumpolar Arctic (for an overview see Krömer & Allemann, 2016), serves as an example for very different evaluations: In North America, it is told mainly in the context of collective trauma and the overcoming of trauma (Bombay, Matheson, & Anisman, 2014; Regan, 2010), whereas in Northern Russia and Siberia, the dominating narrative is different. Boarding schools are mainly blamed for language shift and assimilation to Russian mainstream culture, but often created also plenty of positive associations by their former pupils, such as opportunities for social upward mobility (Bloch, 2004, 2005; Dudeck, 2013; Dudeck & Ventsel, 1998; Liarskaya, 2004, 2013). The indigenous intelligentsia, longing for roots or regretting language shift, often blames the school for having lost traditions and language. Conversely, reindeer herders and other tundra-connected people, who live and work in collective enterprises and either speak their native language or shifted to Russian, often do not blame the school for the language shift, and they have many positive recollections while not denying also traumatic experiences. However, even traumatic experiences are not necessarily seen exclusively as harm. Quite some people frame painful memories of school attendance, similarly to recollections from the army service, as needed transition rituals that enabled them to stand hardship and let their people learn to deal with the outside (Russian) world. If in the remembrance of peoples' past

harm is not an objectively given category, it also cannot be objectified in representations of such remembrance. Accordingly, also benefit is relative. Being aware of the relativity of harm and benefit is part of the deeply relational understanding of truth in oral history. For researchers, "the ethical demand is to 'get it right,' not in any ontological sense, but in being true to the world under study and to the epistemological premises of anthropology" (Hastrup, 2004, p. 470). This is a prerequisite for the development of our further thoughts on how to deal with ethical dilemmas around harm and benefit.

Negotiating and Assessing the Risk

With individuals or groups we are collaborating with, the question of harm should be directly discussed. We consider this moment of discussion and the ethics of these negotiations as crucial for successful oral history research. In some sense, such a negotiation is part of any individual interview. Often the negotiated character of the situation remains hidden in the text of the interview as the questions focus on the content of the story, and not on the question what would be appropriate, legitimate, and respectful to ask and to tell, what is acceptable to know for the concrete listener(s), and a wider public.

We consider the concept of cultural intimacy mentioned above (Herzfeld, 1997) as helpful to understand how the proliferation and restriction of knowledge and information that would be embarrassing if shared with a wider public constructs social borders and creates collectivities. It reminds us of the social productivity of the secret as described already more than a century ago by the German sociologist Georg Simmel (1908). Both Simmel and Herzfeld underline that not only sharing knowledge but also restricting knowledge transmission creates social closeness. To cross illegitimately the informational border and pass on intimate knowledge outside the circle of knowing people can cause harm for the social prestige and the public face of the group. If this kind of harm is threatening the border created by a secret, feelings of shame and embarrassment become the guardians of social status and belonging.

The above described borders of intimacy, created by restricted knowledge, are not always constant, but often context dependent, fluid, and negotiated. How much they are fixed, formalized, and detachable and how much they are situational and fluid, depends on the context and the way they are created and negotiated. In our field site, we encountered contested and changing borders of privacy. We understand the public telling of the story about the remedial school experience as part of this negotiation process, which took place between different members and factions of the community and involved the researcher as well. Several participants believe that the public representation harms the reputation of a community, be it a supposed community of

Russian Sámi, or all the villagers seen as one group, or the collective of teachers these persons identify with. Others see in it a step toward restoring reputation, both of people who suffered the stigma of remedial school attendance and, more widely, of the Sámi part of the community as the most prone to be sent to the school. We risked a public outbreak of controversy and even a refusal by a critical mass of people or by especially influential persons, which could have made further fieldwork impossible. These are situations when there is no clear moral ground for assessing what is good or bad for people.

Two principles may be of guidance here. The first one is to secure conditions in which the researcher acquires deep knowledge and sensitivity for the local value systems and their dynamics and possibly competition inside the local context. The researcher has to acquire firm competence about the concepts of respect and social recognition and the different layers of what we called with Herzfeld cultural intimacy, as well as the different forms of the social face of individuals and groups that could be at risk by interference through common ways of spreading information and sharing knowledge.

The other principle is to engage in intensive and sensitive negotiations. The preconditions for that should be the awareness for the negotiated character of dissemination of oral history stories. On one side of these negotiations, there is a potentially global public with a supposed wish to acquire knowledge. On the other side, there is the teller and groups she or he belongs to, with a desire not only to share knowledge about the past but also to keep certain elements of it inside the realm of cultural intimacy. The researcher is in between, included partly in the lifeworld of the storyteller and at the same time representing the curiosity of the outer world. The storyteller and researcher explore together which information can legitimately cross the border and how. Inclusion into cultural intimacy and responsibility for respectful information management go hand in hand.

We consider inclusion into cultural intimacy as the main resource for successful oral history fieldwork. This is true for two reasons. First, becoming part-time socialized in an intimate social context is the precondition for participant observation, the main research method of social and cultural anthropology, and we think that participant observation, to a greater or lesser extent, should be part of any oral history inquiry. Second, social borders and ruptures as well as dynamic processes of change linked to them are one of the main targets of oral historians' scholarly interest. Compared with other historiographical methods, oral history's strong intersubjective character and anthropological stance is especially apt to discuss such questions.

Situations in which social and moral borders become openly addressed and discussed are at the core of the method of participant observation. Doing anthropological fieldwork, the researcher often goes through events that are

dramatically and densely challenging the position of the researcher, when her or his status in the group changes abruptly, often causing severe emotional reactions not only to the researcher, but sometimes also to people of the researched community. Peter Berger (2009) termed these crucial moments in fieldwork as Key Emotional Episodes. The cockfight experienced by Clifford Geertz in Bali (Geertz, 1972; discussed in Marcus, 1997) could serve as a classical example. A dramatic conflict, which demanded to take sides, marked the inclusion of the researcher into the complicity of the local community. To foresee this kind of conflicts is as difficult as to predict their positive or negative outcome for fieldwork. Researchers would be ethically on the safe side if they would completely avoid such situations. However, in that case, they would also be doomed to stay outside of the realm of cultural intimacy. If access to the emic views, the internal cultural values, and the backstage regions of public images (Goffman, 1959) is the precondition for an appropriate understanding of the cultural context oral history emerges from, we cannot refrain from such contentious situations that can put at risk the future of our research.

To take publicly sides for one fraction can be counterproductive for the researcher's and for the partner's position in a given community. Depending on the situation, for a research partner in the field to be backed by an outsider can be seen by other community members as the proof for the legitimacy or illegitimacy of the claim somebody makes. However, to strive as a researcher toward a neutral position can be also fatal as usually neutral standpoints are interpreted as support of a dominant position. Hence, if avoiding risk is impossible, we can try to minimize it. In our opinion, the only way to minimize the risk for destructive conflict lies in developing a sense for the potential outcome of unexpected situations in the field, beyond the fixed frames given by guidelines. A prerequisite for this is to explore with lengthy fieldwork ongoing processes of social change, identity politics, and struggles about the legitimacy of symbolic resources. By this, we mean cultural capital in the sense of Bourdieu (2002) and the struggles between dominant cultural capital and subaltern or unconventional alternative versions of cultural capital. In this way, we can acquire a firm knowledge about local concepts of the social face, reputation, or prestige. This will help us to appropriately assess and act in unexpected situations in the field requiring the researcher to take a position.

Pragmatism as a Ground for Ethics

Instead of trying to boil down ethical principles to a minimum of universally applicable rules or values to guide our behavior, we suggest looking for pragmatic reasons to unite our own interests with the interests of other agents in the field. As a robust motivation to engage in the occasionally

risky practices of negotiated ethics as discussed above, we would propose the *epistemic surplus* we gain from such practices. To involve the research partners in all phases of the research process from planning to dissemination, to discuss with them the borders of public representation and cultural intimacy, to broach contested themes, to take sides for some agenda of your research partners, all that we do in the first line is due to our scholarly interest and not because doing good is a goal per se. Our suggestion to make the dissemination process itself an object of research enables us to understand the concepts of cultural intimacy and of the collective social face as well as the interplay of moral orders and politics of representation and identity valid for the specific context.

The unavoidable risk we have to take and the shared responsibility we have to carry are only justified on the background of sound knowledge and long-term social relations we establish in the community. Things that seem appropriate for everybody at the present day can become problematic in the future. In times when funding agencies increasingly demand applicability of research, for example, in form of policy advice, but provide only limited support not allowing for long-term fieldwork, we see an increasing tendency toward engaged fieldwork involving advocacy for minority groups side-by-side with ethno-political activists. This often involves moral preconceptions developed "from the desk" by information available online and already knowing with what fraction to side, what agenda to back, and what issue to pursue before entering the field. Researchers and other engaged visitors move in quickly, collect information based on those preconceptions, and alarm the public about some urgent issues and problems (Allemann, 2017; for more on this phenomenon in the context of the Russian Sámi, see Berg-Nordlie, 2011; Konstantinov, 2015).

Long-lasting social relations between the partners that produce oral history together mean to establish relations of responsibility and reciprocity, where both sides are interested in maintaining the relationship and reaching a common understanding of justified sharing, respect, and acknowledgment of each other's values. Researchers can obviously not decide alone what is good and what is bad. But in their engagement with local communities, they encounter situations in which they have to take sides and certain risks to cause tensions or conflicts. As we exemplified with the presentation of the remedial school story and its aftermath, this sharing process is not only an ethical obligation but also an integral part of the research process, which provides us with an epistemic surplus.

Conclusion

Based on our experience of oral history research in Arctic indigenous communities, we tried to deliver some arguments for the necessity to share the results of oral history

research in communities they stem from and for the ways we consider it appropriate. We regard the act of sharing as an integral part of the research itself as it can give us crucial additional insights for understanding oral history. We raised questions, such as "What and how should we share?" "What forms of representation should we choose?" "In which way should we make decisions about the forms of sharing with the community?" and "How can we define harm?" However, in the same vein as the mentioned AAA and ASA guidelines, we deliberately did not try to give clear-cut answers, but rather raise oral history researchers' awareness of these issues. On the basis of a concrete example of bringing back and presenting materials in a community where we did oral history research, we developed thoughts that might be of help to researchers while preparing for similar situations.

Harm, and accordingly benefit, have been central keywords throughout the whole article. We showed that the goal to strictly avoid harm and conflicts is hardly possible to fulfill. As we have extensively discussed, this is the case not because harm is an unavoidable by-product of research, but due to the utter relativity of harm. Just to recall a few examples of this relativity, long-term benefit might outweigh short-term harm from the perspective of the research participants; depending on the setting, virtually every person can see herself or himself rather as a victim or as a beneficiary of the state; the representation of stigma can be seen as reinforcing stigma and negative stereotypes, or it can be used as a political instrument with the goal of overcoming marginalization; what tends to be called a "community" nevertheless consists of individuals with distinctive and often competing interests and worldviews, and assumptions about benefit or harm for a whole community can therefore easily turn out to be untenable.

We see three driving forces behind the necessity and the ways to bring back research results to the places they stem from. The first lies in the interests of the individual teller of a concrete story, the second in the—admittedly often heterogeneous and even conflicting—interests of the community or communities a teller identifies with, and the third is rooted in the interests of the research. The choice of what and how to bring back and present depends on properly understanding and carefully balancing those interests.

To have this done in an ethically sensitive way, we suggest to focus on two main principles: collaboration and long-time engagement. We clearly vote against sticking to preformulated rules drafted in advance from the desk of the researcher without deep knowledge of the cultural context of the field she or he is working in. The existence of such rules is justified, but only as a larger frame and an initial entrance into the sensitive topic of bringing back research results. Also, we are skeptical about signing prior consent as we consider oral history research a collaborative and open-ended endeavor: for both research partner and researcher, it is impossible to know beforehand where the narration, and hence the research, will lead to, because a good biographical interview will possibly unleash unexpected flows of remembrance and narration. Therefore, we suggest that researchers should acquire social competence in the given cultural context and engage in ongoing negotiations. Social competence is, we believe, only possible to be gained through long-term engagement with a particular social context and if the researcher manages to become included in the realm of cultural intimacy of the social group under study.

In our pragmatic attitude, we opt for facing the obvious truth that we deal with ethical questions not because doing good is a goal per se, but because it is part of our wish to gain scholarly insight. We think that it is impossible not to take sides as a researcher in oral history, and therefore utopic to believe that harm can be completely avoided. We rather think of an ethically correct way of bringing back research results in a wider sense: as part of a commitment toward long-lasting reciprocal relations, as described above, and as a potential epistemic surplus.

Notes

1. We are aware that it is wrong to make generalizing claims about "the" Russian Sámi as it is wrong in the case of many other groups consisting of individuals with strongly differing interests but seen by outsiders as unitary communities (for a deconstruction of the Russian Sámi "community" see Allemann, 2017; for similar reservations, see Konstantinov, 2015, p. 18). We use *community* as a term rather neutrally designating people grouping or grouped

together according to their place of dwelling, common past, ethnicity, or other criteria but avoiding generalizing and monistic assumptions about common unitary interests and perspectives.

2. The Oral History of Empires by Elders in the Arctic (ORHELIA) project was run between 2011 and 2015 by the Arctic Anthropology Research Team at the Arctic Centre, University of Lapland, Rovaniemi, Finland. More information on the project is available at www.arcticcentre.org/ORHELIA

3. Interview with Ol'ga Fedotovna Aleksandrova, born in Lovozero, Russia, in 1960, retired; recorded by Lukas Allemann, October 30 and November 3, 2013; the interviewee wished and consented to having her name printed.

4. See, for example, archival documents: Commission on Public Education and Culture (1970, 1973, 1974).

5. See archival document Executive Committee of the Lovozero District Soviet of Workers' Deputies (1970).

6. "National" in its Soviet and post-Soviet use is synonymic with "indigenous" or, more widely, "ethnic," as it covers also those ethnic groups that do not have indigenous status such as the Komi. The term *national* thus refers to a specific focus on certain ethnic groups.

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