

Lukas Allemann

The Experience of Displacement and Social Engineering in Kola Saami Oral Histories



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in Kola Saami Oral Histories**

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Abstract in English

The thesis examines people's experiences of Soviet-time, state-initiated displacement and (re)emplacement on the Kola Peninsula as well as the consequences of these developments. Sources show that Saami communities bore the brunt of these processes. The work seeks to draw for the first time a holistic picture of the social transformation among the Kola Saami, while nevertheless respecting the reality of mixed and multiple ethnic belongings as well as other categories of identity in the region. Tapping extensive fieldwork by the author, the research systematically identifies, analyses and contextualises the processes and consequences of displacement as one of the most profound social transformations of the twentieth century in the Arctic. The consequences discussed include a chronic housing shortage, changed gender relations, skewed dynamics in boarding schools, self-harming behaviour, and social rifts that persist to this day. Perspectives characteristic of the state are juxtaposed with grassroots experiences.

This work is in many ways a historical anthropology of suffering, one laying bare mechanisms of scapegoating and social exclusion. Yet traumatic events are dealt with in ways acknowledging that victims can be simultaneously agents who accommodate, subvert and resist. The stages and consequences of displacement are contextualised within the larger frame of social engineering undertaken by modern nation-states across the circumpolar world, thus relativising Soviet–Western dichotomies.

Conceived as a historical-anthropological inquiry, the study draws on empirical materials produced and gathered using a combined approach of open-ended biographical interviewing, participant observation and archival research. Ethical questions prompted by this co-productive approach with a long-term commitment to field partners are taken up as an additional strand of the research.

The main methodological principle of this thesis is that the production and the analysis of materials should be phenomenologically driven and rooted in a radically interpretive, non-positivist approach. Embracing this commitment, the work tries to show that the common — but mostly unspoken — link between oral history and anthropology lies in phenomenological philosophy as the study of experience. Making this link more explicit is an important and long overdue task, because experience is the pivot between the universal and the singular.

Tiivistelmä suomeksi

Väitöskirja keskittyy ihmisten kokemuksiin neuvostoajasta ja valtion toimeenpanemasta väestön siirrosta ja takaisin asuttamisesta Kuolan niemimaalla sekä siihen, mitä tästä kaikesta on seurannut. Lähteiden perusteella saamelaiset ovat kärsineet prosessista eniten. Väitöskirjassa pyritään ensimmäistä kertaa luomaan holistinen kuva Kuolan saamelaisten kokemasta sosiaalisesta muutoksesta unohtamatta alueen etnisen diversiteetin ja muiden identiteettikategorioiden realiteetteja. Laajan kenttätöön pohjalta tutkija identifioi, analysoi ja kontekstualisoi siirtoprosesseja ja niiden seurauksia yhtenä arktisen alueen vaikuttavimmista sosiaalisista transformaatioista 1900-luvulla. Tekijän pohtimia seurauksia ovat muun muassa jatkuva asuntopula, sukupuolten välisten suhteiden muutos, sisäoppilaitosten vääristyneet käytännöt, itsetuhoinen käytös ja vielä tänä päivänä vallitseva sosiaalinen epätasa-arvo. Tutkimuksessa vertaillaan valtiollisia lähtökohtia ja ruohonjuuritason kokemuksia.

Se on eräänlainen kärsimyksen antropologinen kuvaus, jossa syitä vieritetään muiden niskaan ja ihmiset päätyvät yhteiskunnan ulkopuolelle. Traumaattisia tapahtumia silti käsitellään pitäen mielessä, että uhrin voivat samalla olla mukautuvia, mitätöiviä ja vastustavia toimijoita. Siirtojen vaiheita ja seurauksia verrataan nykyajan kansallisvaltioiden sosiaaliseen suunnitteluun kaikkialla sirkumpolaarisessa maailmassa, mikä antaa mittasuhteet neuvostojärjestelmän ja länsimaisen järjestelmän väliselle dikotomialle.

Tutkimuksen lähestymistapa pohjautuu historialliseen antropologiaan ja sen empiirinen materiaali on tuotettu ja kerätty avointen biografisten haastattelujen, osallistujahavaintojen ja arkistotutkimuksen keinoin. Tämän pitkään sitoutumiseen perustuvan, yhteistuotannollisen metodin esiin nostamat eettiset kysymykset poikivat ylimääräisen tutkimushaaran.

Väitöskirjan metodologinen pääperiaate on, että materiaalia tulee tuottaa ja analysoida fenomenologisista lähtökohdista ja lähestymistavan tulee olla vahvasti tulkinnallinen ja ei-positivistinen. Tätä silmällä pitäen tutkimus pyrkii osoittamaan, että suullisen historian ja antropologian yhdistävä – joskin vaiettu – yhteys juontuu kokemuksen tutkimukseen osana fenomenologista filosofiaa. Tämä yhteys tulee viimeinkin saada selkeämmin esille, koska juuri kokemus on universaalinen ja erityisyyden keskipisteessä.

Аннотация на русском языке

В советское время переселялись миллионы людей – в разное время и по самым разным причинам. Но всегда переселения влекли за собой существенные изменения в жизни затронутых ими людей. Последствия переселений значительно повлияли на жизненные пути переселенных и их последующих поколений. Данная работа является попыткой систематизировать переселения и их последствия в восточной, сельской части Кольского полуострова и дать им интерпретацию, с опорой на жизненный опыт жителей. Более конкретно, данная диссертация освещает аспекты истории Кольских саами, в связи с тем, что важной областью государственной «социальной инженерии» стали поселения коренных народов. Такая политика повлекла за собой, в том числе, хронический дефицит жилья, изменение гендерных ролей и отношений, скошенность функций школ-интернатов, повышенные болезненность и смертность населения, социальные конфликты.

Работа является попыткой посмотреть на события и последствия переселений в широком, пан-арктическом контексте переустройства целых обществ. Обсуждаются сходства такой политики арктических государств в XX веке по обе стороны железного занавеса (проявляющиеся, например, в переводе на оседлый образ жизни, переселениях, интернатах). Позиция государства сопоставляется с оценками ситуации переселения обычными людьми.

В рамках методологии устной истории и социальной антропологии исследование строится на эмпирических материалах, собранных с помощью комплекса методов: открытого интервью, включенного наблюдения и архивной работы. Данная работа во многом является антропологией и историей страданий. Раскрываются механизмы публичного осуждения, социального исключения и циничных знаний на местном уровне. Тем не менее, анализ материалов показывает, что быть жертвой не противоречит проявлению агентности в виде тонких форм приспособления или сопротивления к новым условиям. Отдельно рассматриваются этические вопросы, возникшие при долгосрочных отношениях с информантами и совместном создании эмпирических материалов.

Постоянным фоном при сборе и анализе материалов являлась феноменологическая философская установка. В результате данная диссертация делает теоретико-методологический вклад тем, что представляет и систематизирует феноменологию как общую философскую основу устной истории и социальной антропологии – основу, часто неосознанную, но при этом присутствующую как глубинная общность этих двух дисциплин, изучающих жизненный опыт людей. Таким образом, данное исследование показывает решающую важность изучения жизненного опыта людей для комплексного понимания как целевых установок государственной социальной инженерии, так и индивидуальных судеб людей тех сообществ, на которых она направлена.

To Cristina

Preface

This doctoral thesis reflects my path as a scholar, and has played a significant part in who I have become as a person. This was only possible thanks to exchanges with, the influence of and support by many individuals and several institutions. The thesis emerged initially from the research project ORHELIA (Oral History of Empires by Elders in the Arctic), which was at the crossroads of anthropology and history. The present work tries to include insights by anthropologists as well as historians on an equal basis and thus to bridge the gap which, in my opinion, exists between their distinct yet intersecting corpora of oral history literature. This awareness of the existence of both sides of oral history is an outcome of my own path as a researcher. My life in Rovaniemi, where I arrived in 2013 to work in this oral history project, started with a surprise: I turned out to be the only historian by training in this project, which had the word ‘history’ in its name; all the other researchers were anthropologists. During the following years, I went through an incredibly enriching learning process in which I realised that oral history is the prime venue where anthropologists and historians meet.

In the ORHELIA project, it quickly became clear that “what unites, first of all, [...] almost all people we have insofar recorded are the experiences of dislocation and resettlement” (Dudeck 2013b, 72). This thesis project was born based on this insight and on my previous oral history research about the life of the Eastern Saami during Soviet times (Allemann 2010; 2013, the latter publication is a translation of the former). Building on my master’s thesis, this was my first application of the oral history toolset I had acquired during my studies, and it was my first acquaintance with the Kola Peninsula as a field site for research. Free from narrow research questions, I wanted to know which issues and events would figure most prominently in the narratives of field partners. It turned out that these were the numerous forms of displacement that occurred between the 1930s and 1970s and continue to shape people’s lives to this day. While my early research covered a wide array of topics and served to identify this as the most salient one, it did not probe deeply into the matter. I left this task for my doctoral research.

I am afraid it is impossible to list here every single person to whom I owe a debt of gratitude. First and foremost, I am thankful to all my interview partners, and in many cases also to members of their families. I will not list names, as I would not know where to draw the line on the list and how to justify whom I included and whom I did not. The bonds created, especially with people in Lovozero, are very dear to me, and I hope to keep them for a lifetime. In the field, I would also like to

thank the staff of the Murmansk Region State Archive, of the Lovozero library and of the Regional Studies Department of the Murmansk Regional Academic Library. Among my academic contacts, special thanks go to my supervisor Florian Stammler and co-supervisor Julia Obertreis, to Yulian Konstantinov, who always generously shared his rich experience and knowledge both in the field and in correspondence, and to Stephan Dudeck, with whom I had the honour of co-authoring one of the articles for this thesis. I am also thankful to the other members of the Arctic Centre's anthropology team who were involved in oral history research, namely Anna Stammler-Gossmann, Nuccio Mazzullo, Roza Laptander and Nina Messhtyb, for numerous lively discussions, and for sharing their accumulated wisdom and experience. I am thankful to the University of Lapland for offering me stable employment during four long years, in which I had the privilege to dedicate myself solely to my PhD research, and to the Academy of Finland, which funded the projects ORHELIA (Oral History of Empires by Elders in the Arctic, 2011-2015, decision no. 251111) and WOLLIE (Live, Work or Leave? Youth – wellbeing and the viability of (post)-extractive Arctic industrial cities in Finland and Russia, 2018-2020, decision no. 314471), during which I was able to collect materials for my PhD research. I am also grateful to the rectorate of the University of Lapland for a thesis finalisation grant. I would like to thank Leif Rantala posthumously, appreciating him as the University's scholar who worked most intensively with the Eastern Saami during several decades. I had the pleasure of meeting him and discussing our research several times when I was new in Rovaniemi, before his untimely death. I would further like to express my gratitude to Ol'ga Pozhidaeva and Elena Aleshkevich, who did the hard work of transcribing all my interviews. Last but not least, a very special thanks goes to my mother Cristina Allemann-Ghionda, who supported me morally during all these years and always reacted to and commented on all my drafts with amazing speed and the fresh gaze of a scholar from a different field.

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List of thesis articles

Article 1: Allemann, Lukas. 2019. "I Should Never Tell Anybody That My Mother Was Shot': Understanding Personal Testimony and Family Memories within Soviet Lapland." *Oral History* 47 (2): 65–73.

Article 2: Allemann, Lukas. 2018. "I Do Not Know If Mum Knew What Was Going on': Social Reproduction in Boarding Schools in Soviet Lapland." *Acta Borealia* 35 (2): 1–28. <https://doi.org/10.1080/08003831.2018.1536115>.

Article 3: Allemann, Lukas, and Stephan Dudeck. 2019. "Sharing Oral History With Arctic Indigenous Communities: Ethical Implications of Bringing Back Research Results." *Qualitative Inquiry* 25 (9–10): 890–906. <https://doi.org/10.1177/1077800417738800>.

Article 4: Allemann, Lukas. 2017. "Yesterday's Memories, Today's Discourses: The Struggle of the Russian Sámi to Construct a Meaningful Past." *Arctic Anthropology* 54 (1): 1–21. <https://doi.org/10.3368/aa.54.1.1>.

Throughout this thesis introduction, these articles will be referred to as "Article 1", "Article 2", and so on, and "the thesis articles".

There is only a perspectival seeing, only a perspectival 'knowing.'
Friedrich Nietzsche¹

1. Introduction

Based on extensive fieldwork in the rural parts of the Kola Peninsula in Northwest Russia, this thesis project set out with the question of how the people experienced and still experience Soviet-time displacement and (re)emplacement and the consequences of these processes. State-induced displacement in this area of the Russian Arctic predominantly impacted Saami communities. The thesis seeks to draw for the first time a holistic picture of Soviet social transformation among the Kola Saami, while nevertheless respecting the reality of mixed and multiple ethnic belongings in this part of Russia as well as other categories of identity. Processes and consequences of displacement are systematically listed, interpreted and contextualised as one of the deepest social transformations of the twentieth century. The consequences discussed include (but are not limited to) a housing shortage, changed gender relations, skewed dynamics in boarding schools, self-harming behaviour and societal rifts. I have tried to contextualise all these events and situations within the bigger frame of pan-Arctic Soviet social engineering, and to juxtapose grassroots experience with perspectives of “seeing like a state” (Scott 1998). Conceived as an oral history inquiry with an anthropological background, the empirical materials were produced and gathered in a combined approach of open-ended biographical interviewing, participant observation and archival research. The main theoretical-methodological principle of this thesis is that the production and the analysis of the materials should be phenomenologically driven and committed to a radically interpretive, non-positivist approach. Thus, the present work tries to show that the common but mostly unspoken link between oral history and anthropology lies in phenomenological philosophy as the study of experience. Making this link more explicit is an important and long overdue task, because experience is the pivot between the universal and the singular.

I will start by introducing a few terms, underlined in what follows, that play a key role throughout the thesis: Displacement as a prime narrative site means that fundamental to this thesis are questions of displacement *and* emplacement of people in the Soviet Arctic. Alf Lüdtke, a pioneer of the German school of *Alltagsgeschichte* (everyday history) in the 1980s, raised the following important question:

¹ Nietzsche (2007, 87)

Analysis of historical everyday events and situations travels a path that penetrates into countless historical lives. It provides multifaceted insights into the modes of life of classes and groups. The unavoidably detailed steps and stages such research entails require a major expenditure of time – so what is the main motivation behind all this gargantuan effort? (Lüdtke 1995, 23)

To answer this question, I stress that the two words *displacement* and *emplacement* must necessarily come as a pair. One cannot function without the other. Where there is displacement, (re-)emplacement follows. While this sounds trivial, it is often forgotten in both scholarly and publicist accounts. Being uprooted is depicted more often than not as a permanent, passive and final condition. It does indeed take Lüdtke's "gargantuan effort" to achieve some insights about the myriad ways in which people try to stand their ground in response to and in interaction with a state's social engineering (Scott 1998). In this way of doing historical inquiry, agency thus becomes a central pillar of the very meaning of history. Ingold describes this as follows:

If production is not, as Godelier would have it, about transforming the material world, but rather about participating in the world's transformation of itself, then could we not conclude that human beings produce themselves and one another by establishing, through their actions, the conditions for their ongoing growth and development? And might it not be in precisely this mutual establishment of developmental conditions that we find the meaning of history? (Ingold 2011, 8)

A central tenet of the present research is that taking agency seriously does not mean denying the experience of being a victim. While trying to show that the common script of *passive* victims in a totalitarian state is often not tenable, I do not deny that in many instances, as presented in this thesis, people were victims. Derrida's (1981, 26–27) suggestion that individuals in history play roles that are "simultaneously active and passive" stands in a Marxian tradition and is also valid, I suggest, in societies commonly defined as having a totalitarian leadership. Konstantinov (2015) focused more on what advantages the rural side of the Kola Peninsula could take from the Soviet socio-economic transformations – by accommodating and subverting the system. By contrast, the present study focuses more on the "dark anthropology" (Ortner 2016) and the "dark heritage" (Koskinen-Koivisto and Thomas 2017) of these transformations and may well be attributed to what Robbins (2013) described as the "suffering slot" of anthropology. However, it does so by looking at traumatic events in ways that allow protagonists to be victims and agents at the same time. According to the dictum commonly attributed to Antonio Gramsci, but in fact attributed by Gramsci to Romain Rolland (Gramsci 1920, 2), in the present work the "pessimism of the intellect" goes hand in hand with the "optimism of the will"

that people tend to use to navigate through the difficulties of their lives (cf. Ortner 2016, 66).

Grassroots agency and “seeing like a state” (Scott 1998) are mutually constitutive in such a view. “In any lifeworld the individual and the system are inextricably linked. For the lifeworld orientation of historical science, it follows that the repeatedly invoked contrast between micro- and macro-history does not exist in such a perspective. In the actor’s eyes, micro- and macro-worlds are perceived simultaneously.” (Haumann 2006, 49). They go hand in hand, exactly as displacement and emplacement go hand in hand. This thesis tries to combine both perspectives. In data gathering, this was accomplished by combining oral history interviewing and long-term anthropological fieldwork on the one hand, and archive and media research on the other (see Chapter 6, *Collecting and organising materials*). The ways in which I look at these materials are inspired by phenomenology, and a significant part of this thesis is devoted to rendering phenomenology as an approach more visible and conscious.

1.1. Research questions and goals

The *initial* question of the present research was: **How did and do people experience Soviet-era displacement and (re)emplacement in Arctic indigenous regions, as exemplified by the Kola Peninsula?** With regard to ways of formulating goals and research questions by way of entry into a research venture, I agree with Willig (2014b, 145), who states: “The ethnographer does have a research question in mind; however, this question is really little more than an acknowledgement of what motivates the researcher to commence the research in the first place.” The same applies to the oral historian and, for that matter, to any genuinely qualitative inquiry.

Only *later*, in the course of the project, the broader empirical goal became to **show how far Soviet social engineering went in the North**. This does not imply depicting the Soviet Union as a unique, stand-alone project, but quite the opposite: it means embedding it in the global canon of social engineering by modern states. Based on insights from a specific case, we can ask a question with a very broad scope: **What do people’s experiences of Soviet-time displacement and emplacement tell us about social engineering as a way of organising human societies?**

By grounding this large-scale context in the case of the Kola Peninsula, this thesis seeks to make a significant **contribution to the knowledge about the modern history and contemporary situation of the Kola Peninsula**. It adopts a special but not exclusive **focus on the region’s indigenous minority, the Saami, the rural population that experienced displacement to the greatest extent**. I wanted to look into how people responded on a wide spectrum from seizing opportunities through resistance to despair and into how this social reshuffling is reflected in the

contemporary situation of the indigenous and para-indigenous (see section 1.5., *Terminological disambiguations*) populations of the Kola Peninsula. At the same time, I wanted to **look into the responses of the state to the unplanned outcomes of its own social engineering**, such as its use of boarding schools as a remedy for the housing shortage or the individualisation and medicalisation of social hardship, which allowed it to eschew responsibility.

This interplay between the state perspective and the grassroots perspective reveals that agency and meaning-giving constructions form the core of individual historical testimonies, that, because of this, such testimonies are a crucial part of historiography and crucial for understanding contemporary situations and that these fundamental insights from individual-oriented research are an indispensable component for assembling broader pictures about history and societies.

This point of departure brings us to the theoretical-methodological goals of this thesis. Very broadly, the work pursues the goal of **bringing closer to each other the rather separate traditions of oral history inquiry by anthropologists and by historians**. Anthropologists and historians have made significant contributions to oral history, often independently of one another. The goal here is to combine insights from both disciplines. This will occur, on the one hand, by using the distinct oral history theorising from both disciplines and, on the other, by combining historiographic and anthropological practice. Historiographic ‘virtues’ include source criticism and oral history interviewing techniques; examples of anthropological strengths are cultural sensitivity through participant observation and long-term relationships with field partners.

More specifically, the objective is to **formulate a common phenomenological, lifeworld-oriented philosophical basis** for a combined take on history and anthropology. The phenomenological philosophical school of thought has anticipated the practices of many oral history scholars, be they historians or anthropologists, and of many critical scholars in general. However, more often than not, phenomenology is not referred to explicitly. In this thesis, phenomenology will have a prominent position. Indeed, the initial research question formulated at the beginning of this section is a phenomenologically driven way of asking, meaning that: a) “phenomenological analysis should be guided by a phenomenological question, on the lived meaning of a human phenomenon that is experientially recognizable and experientially accessible”; b) “phenomenological research begins with a question that comprises an element of wonder: discovering [...] the strange in the taken for granted”; and c) “[phenomenological research] asks what a possible human experience is like” (Van Manen 2014, 297–98). Hence, the focus is on the ‘how’ and not on the ‘why’ (for more on this, see section 2.0, *Phenomenology as a starting point*). For a basic phenomenological question, “questions that are abstract, theoretical, conceptual, or that ask for explanations, perceptions, views, or interpretations will not lend themselves for phenomenological exploration

and reflection” (2014, 297–98). However, this thesis is not a philosophical ‘phenomenology of ...’, but an anthropological and historical inquiry that uses phenomenological awareness as a basic attitude (see Chapter 2, *Phenomenology as a starting point*). This means that phenomenological questioning has not been the only way of asking questions during my research, but has stood at its foundation in two ways: firstly, in the form of the overarching question mentioned above and, secondly, as the preferred way of asking questions to my partners in the field and to the collected materials on the desk.

Between these two levels we find the secondary questions and goals stated above, which are not only of phenomenological but also of analytical character, inspired by existing theories. Indeed, it is through analytic inspiration (see Chapter 5, *Deliberate eclecticism as a method*) that we make progress towards more concrete research goals or questions. These are to a large part retroactively formulated through the insights gained during the research. Especially in non-structured, open-ended, narrative interviews, the insights cannot and should not be anticipated by any excessively narrow and pre-formulated question, as this entails the risk of missing important points.

The anthropological and phenomenological take on oral history proposed in this thesis entail **serious ethical challenges. Bringing them to light and giving guidance on addressing them** has been an additional goal of this work. During fieldwork I felt that a phenomenological and participatory approach to field partners opened up alternative dimensions of research ethics which are usually not considered in more structured approaches to social or historical research. My goal was to understand and elucidate these dimensions, which are grounded in the prolonged and committed interaction with field partners that characterised the present project. This is a crucial consideration, because ‘our’ ethics should be first and foremost the ethics of the people we talk with, and only after that the ethics of regulatory bodies and funding agencies.

1.2. The phenomenological study of experience as a common basis of oral history and anthropology

This thesis tries to not only mind but also bridge the gap between two ‘disciplines’: oral history as conceived by anthropologists and oral history as conceived by historians. While there definitely is mutual influence, we can speak of two distinct corpora of literature. Common to both is that they clearly, though often implicitly or even unwittingly, are located in a phenomenological tradition. Phenomenology, this thesis posits, is a common denominator and philosophical foundation of all proper oral history inquiry into the life conditions of people. It will therefore occupy a prominent position throughout this thesis.

Phenomenology studies our experiences. As we use it today, phenomenology counts as one of the five core fields of philosophy, alongside ontology, epistemology, logic and ethics (Smith 2018, 11). Husserl generally counts as the founding father of modern phenomenology, and he put phenomenology first, as the most basic field underpinning all other fields, although this view is certainly not uncontested. Already at the beginning of the twentieth century, historians-as-anthropologists were an important ‘target group’ of his thinking. He suggested that we should pay attention to

the prescientifically intuited world or [...] its relative features. In a certain way, concern with this sort of thing belongs continually even to [one type of (addition by the translator)] objective investigation, namely, that of the historians, who must, after all, reconstruct the changing, surrounding life-worlds of the peoples and periods with which they deal [...], paying constant attention to the relativity of the surrounding life-worlds of particular human beings, peoples, and periods as mere matters of fact. (Husserl 1970 [1936], 147)

The central argument here is that subjectivity, relativity, perspective, taken-for-grantedness – all contained in lifeworlds (see section 2.2., *Lifeworld*) – are to be treated themselves as evidence, facts and truths (see section 2.4., *Evidence and truth*). Throughout this thesis, I will adhere to this basic premise.

There are also things that anthropologists and historians dealing with oral history can learn from each other. What anthropology can gain from oral history literature written by historians are, for instance, particularly deep insights on aspects of verbal communication with field partners, that is, the small but important details to be gleaned when doing in-depth interviewing (see Allemann 2013, 23–27 for an overview; also Rosenthal 2003).

Conversely, historians can learn a great deal from anthropologists on the non-verbal aspects of oral history, which gradually emerge during long-term field stays that include participant observation. Ortner (1995, 190) summed up what I, as a historian, learned from anthropology: “Ethnographic refusal” – this is how Ortner refers to the insufficient recognition and application of an anthropological stance² – makes many social science studies about minorities and social transformation “thin on the internal politics of dominated groups, thin on the cultural richness of those groups, thin on the subjectivity – the intentions, desires, fears, projects – of the

2 Ingold (2017) has recently argued for a more accurate disambiguation and conscious choice between the terms ‘ethnography’ (a rather descriptive discipline) and ‘anthropology’ (an open-ended process of understanding the human existence, closer to philosophy but grounded in interpersonal experience). In practice, however, we should acknowledge that even high-profile scholars, like Ortner (1995) or Denzin (2014), say “ethnographic” when they mean “anthropological” in Ingold’s sense.

actors engaged in these dramas”. Throughout my doctoral research, I tried to follow this admonition, with a special focus on the first and the third points.

In this thesis, oral history evidence made me see through and beyond dichotomic perspectives. I saw a broad variety of positions, ranging from the appreciation of upward social mobility through romanticising or instrumentalising views to remembrance of despair, despondency and destruction. Hence, I am suspicious of homogenising perspectives on ‘Soviet power’, ‘the Saami’, ‘the Russians’ and all kinds of ‘communities’ in general. At the same time, avoiding instrumentalist “grouping” (Brubaker 2004) and artificial dichotomies does not mean being blind to certain concrete inequalities, as this thesis tries to clearly show.

Getting back to Lüdtke’s (1995, 23) “gargantuan effort”, in the present case it also means that I take this research as an occasion to make both a theoretical-methodological and an empirical contribution. In practice and in accordance with my holistic approach to research, contributions of a theoretical, methodological and empirical nature go hand in hand. This is why I break down the contributions by category in the following list of research goals.

1.3. General quality criteria and pitfalls

As Berg-Nordlie puts it, “historians are spinners of narratives”. While historians and anthropologists work systematically – not simply writing down what they hear through the grapevine – their research is doubtless fraught with pitfalls. The goal of this chapter is to point out some of these and to discuss how to deal with them.

In the following, I draw up a catalogue of critical meta-questions about a researcher’s research, inspired by Mayring (2002) and further developed for the present work. The questions will not be answered directly here, but rather are meant to reflect my attempt to keep a constant level of critical self-observation in my research:

How ‘credible’ are the people I have talked to in my fieldwork?

Do I assess their ‘credibility’ by trying to understand their overt and covert motivations and interests?

Is the range of people in my fieldwork broad enough? Do I limit myself only to the ‘gatekeepers’ and other readily visible persons? Do I include the excluded?

Do I ground my theorising well enough in my field materials?

Do I systematically look for and think about instances in my field data that contradict my theorising?

Do I think about my pre-conceptions and how they influence my views?

Do I actively think about and go through alternative explanations?

Do I resist the temptation to embrace or create streamlined, teleological narratives?

Do I look for additional data from other cases for comparison?

Do I make explicit to the reader my approaches and procedures, including my own pre-understandings?

Do I make clear why I interpret something in a particular way, that is, *how* I have arrived at my conclusions? As part of the same process, do I address alternative explanations or perspectives and say why I did not pursue them?

Do I make clear what rules and principles I follow? There are some broader rules and principles I observe even if I am against the dogmatic following of procedural rules.

Am I finding the right balance between being close enough and not too close to my field partners?

Do I validate my conclusions and theorising with the people in the field? Validation here does not mean approval or disapproval, but inputs for generating further insights.

Do I triangulate (or maybe even ‘square’, ‘pentagonate’ or ‘hexagonate’) my research? Do I validate my conclusions by means of comparison with and inspiration from multiple data sources, other cases, multiple interpretations (exchanges with other people in the field and with other researchers), multiple methods and multiple theories?

These questions, should they remain unheeded, point at the multitude of potential flaws in any research endeavour. Attempts at answering to them will be found throughout this thesis, both in its methodological-theoretical and its empirical parts.

Additionally, I would like to name five widely defined potential pitfalls:

Firstly, adhering too closely to certain methods and procedures can distort results or obstruct the path to potentially better results. James Scott (1998, 7) put this succinctly: “Once you have crafted lenses that change your perspective, it is a great temptation to look at everything through the same spectacles”. There should therefore be no excessively strict specialisation in one certain method or procedure (see Chapter 5, *Deliberate eclecticism as a method*).

Secondly, all my theorising, even if taken over from previous theorising and from other cases, should be adequately related to my field. This is a core thought of phenomenological thinking (see Chapter 2, *Phenomenology as a starting point*). No matter how far I expand my thoughts, I must remain mindful of Eberle’s maxim: “the second-order constructions of the scientist have to be consistent with the first-order constructions of the actor in everyday-life” (2014, 188).

Thirdly, I should beware of what I call the *uniqueness bias*. Extreme numbers, shocking facts, surprising accounts and so on bear the risk that the researcher may feel that his or her field site is something unique without cross-checking enough with existing research on other regions, settings and societies. Comparison and transfer (Maxwell and Chmiel 2014; Erickson 2012) are thus a suitable antidote.

For example, when we look at the Soviet Union's plans for moving people and the careless implementation of these plans, we should avoid succumbing to a uniqueness bias. Scott reminds us of international organisations' complicity in relocation projects in the so-called Third World. He cites a World Bank report from the 1970s which, in keeping with the dominant discourses of the time, approved "enforcement or coercive measures" (quoted in Scott 1998, 231–32) designed to overcome habits and superstitions of a backward population and doubted the effectiveness of persuasion as the only means to effect relocation. Not only in the Soviet Union, but in the Western hemisphere and the developing world as well the discursive-ideological landscape was ill-prepared for recognising large-scale social engineering as a possible source of harm to the population. Before the post-modern era, standardisation, simplification, rationalisation, science, progress and overcoming backwardness were less-questioned and more positively connoted concepts of human development throughout the world.

Fourthly, I should beware of *home blindness*. While developing my reflections on 'the other', thinking about what is taken for granted in my own lifeworld can easily remain underdeveloped. This, for example, often results in a baffling contrast in which researchers may elaborate deconstructions of alien societies, yet simultaneously use generalising and undifferentiated terms like 'Western' or 'modern' in talking about their own lifeworld (see section 1.5., *Terminological disambiguations*; cf. Eriksen 2013, 52; Ingold 2000, 6–7).

Finally, close and prolonged collaboration with people in the field entails a great deal of mutual trust. This by default implies – to a certain degree – advocacy or partiality in the sense that the research is done not only on people, but with and for people. Precisely as 'objectivity' in historical and social research is impossible (see sections 2.2., *Lifeworld*, and 2.4. *Evidence and truth*), complete impartiality becomes impossible when the research methods include part-time socialisation in an intimate social context with field partners (see Article 3). There is a danger of either not recognising this implicit partiality or exaggerating it: excessive laboratory-like distance is as bad as excessive identification with and advocacy for the people in the field (Portelli 1998, 73; Passerini 1998, 53). Two kinds of scepticism are helpful for finding the right balance.

The first kind, following James Scott and Forrest Colburn, is "a skepticism of state-centric approaches to the study of the rural poor" (Colburn 1989, xiii; especially reflected in Articles 1 and 2 of this thesis). The second is a scepticism of elite-centred approaches to the study of minorities (reflected especially in Articles 3 and 4). In both variations, I tend to side with the views of the less visible part of a given population instead of the political leaders, activists, bureaucrats, planners and other agents, be they representatives of the majority society or of indigenous elites. In this thesis, the first type of scepticism applies mostly to the views expressed in Soviet-time archival documents and newspaper publications. It also applies to

scholarly work relying excessively on such sources. Yet, this is not to imply that such research necessarily sides with the state's perspective; it may equally well mean that the research overestimates the state's power to realise its plans and fails to take into account the ways in which such plans are subverted. The second scepticism applies mostly to views and claims by political elites, regardless of whether they come from governmental structures or from ethno-political activism. Research that one-sidedly relies on such sources entails a risk of taking their views and claims at face value and overlooking entangled relations of representation and misrepresentation.

Thus, my preference for the views of the 'silent majority' is a conscious choice, as "an enduring value of informal storytelling is its power to subvert official orthodoxies and to challenge conventional ways of thinking" (Cruikshank 1998, xiii). This challenging is possible only with a sufficiently long presence in the field, when people get used to my being there and start sharing more with me than the versions intended for visitors. Moreover, there are also *unofficial* orthodoxies to be subverted, such as the need and misery discourse (Berg-Nordlie 2011; Article 4) in Lovozero, the main field site of my research.

These general thoughts and questions about the quality of research were constant points of reference while doing my work. Accordingly, my goal was that they should be sufficiently reflected throughout this thesis.

1.4. Literature review and gap analysis

The following non-exhaustive literature overview encompasses a range of historical and anthropological research on the Kola Peninsula, on the Russian Arctic and, to a limited extent, on the circumpolar world in general. It does not include theoretical-methodological literature, which will be discussed extensively in Chapters 2 to 5. Literature related to the specific topics of the thesis articles is mentioned in the respective articles as well as in the sections where I discuss the articles.

On the 'tundra half' of the Kola Peninsula, anthropological research since the end of the Soviet Union has been conducted most prominently by Konstantinov and Vladimirova (Konstantinov 2018; Konstantinov et al. 2018; Konstantinov 2017; 2015; 2010; 2009; 2007; 2005a; 2005b; Vladimirova 2017a; 2017b; 2017c; 2017d; 2014a; 2014b; 2011; 2009; 2006; Vladimirova and Konstantinov 2002). Their work stresses that ethnic borders in the tundra half are something imported from the outside and demonstrates that in practice the importance of ethnicity is far less than is assumed by much of the rest of post-Soviet work, which has traditionally had a strong focus on the Saami (monographs: Afanasyeva 2019; 2013; Allemann 2013; Overland and Berg-Nordlie 2012; Kalstad 2009; Gutsol, Vinogradova, and Samorukova 2007; M. P. Robinson, Kassam, and Rantala 1998; and numerous articles and book chapters, which are cited throughout this thesis).

The ‘Saami bias’ of many previous studies – and to a certain extent of the present inquiry as well – has been criticised earlier, for example by Vladimirova:

Such writings, though often making a mentioning of the Komi and more rarely Nentsi ethnonym in some relation with Kola reindeer herding, implicitly but categorically put a Sami stamp on it. Such ideological strategy – keeping to “real” facts to some extent, but contextualizing them in a selective and misleading manner, creates a misrepresentative image. (Vladimirova 2006, 342)

With reference to research on Finnish Sápmi, Hokkanen (2017, 92) cautions: “Discrimination relating to disability or Saminess may become combined with other factors resulting in unfair treatment. [...] The intersectionality of different factors causing and sustaining discrimination must be taken into account.” When it comes to ethnic minorities, this intersectionality may be ignored; a whole complex of different factors risks to be prematurely boiled down by concerned researchers or activists to essentialising ethnicity factors which are then proffered as the only reason for this or that problem. I try to make the multiple factors clearer, which does not mean that a ‘Saami bias’ is a priori unjustified. The aspects of social engineering discussed in this thesis clearly impacted the Saami population of the Kola Peninsula more than others, but for a wide array of reasons, not all of which can be connected to deliberate ethnic discrimination.

There are a number of overviews and in-depth works on the pre-Soviet and early Soviet history of the indigenous population of the Kola Peninsula (Berg-Nordlie 2015; Allemann 2013, 5–9, 31–41; Kuchinskii 2008 [includes an English summary]; several entries in Kulonen, Seurujärvi-Kari, and Pulkkinen 2005; Sergejeva 2000; Volkov 1996 [1946]; Luk’ianchenko 1971). Generally speaking, the most comprehensive historiographies of the indigenous Arctic Russia are Slezkine (1994a) and Forsyth (1992). I will not mention here the rich work of early ethnographers and travellers written in the early Soviet (pre-Second World War) and pre-Soviet times. Kuropjatnik (1999) and Bodrova (2008; 2007) offer good meta-analyses of that body of literature.

The Komi minority on the Kola Peninsula has been comparatively neglected by academia, as evidenced by the far smaller number of publications on their situation. Recently, Mankova (2018a) consolidated the existing research on this minority in an article dedicated to the topic. The article is part of a recent doctoral thesis dealing with remoteness and everyday life in Krasnoshchel’e, one of the Komi-founded villages on the Kola Peninsula (Mankova 2018b). The Nenets, who migrated to the Kola Peninsula in much smaller numbers with the Komi, are usually mentioned together with the Komi; to my knowledge there is no separate research on them.

Important works on the pre-Soviet and Soviet history of the colonisation and industrialisation of the Kola Peninsula, without a special focus on indigenous

peoples, include Bruno (2016), Fedorov (2009), Orekhova (2009) and Shashkov (2004).

As far as oral history studies about the Russian Arctic are concerned, we can say that they are not abundant; they are far outnumbered by studies based on written sources such as archival data and surveys as well as by classical anthropological fieldwork. Incidentally, the few major studies about the Kola Peninsula that are based on narrative interviews appeared all in the same year (Afanasyeva 2013; Allemann 2013; Hønneland 2013). A more recent addition to the literature is Afanasyeva's (2019) oral history of boarding schools. Of the few oral history studies about other regions of the Russian Arctic and of Sápmi, quite a number have been published as part of the same research project (ORHELIA) as the present thesis (Dudeck 2018; Laptander 2014; 2017; Lukin 2017; Mazzullo 2017; Stammler, Ivanova, and Sidorova 2017).

There is a large corpus of literature that discusses relocations, urbanisation and/or various social consequences in indigenous regions across the entire Soviet North (Vakhtin 1992; Pika 1993), including Chukotka (Krupnik and Chlenov 2007; Holzlehner 2011), Kamchatka (Bogoyavlensky 1997; Rethmann 2001), Sakha (Stammler, Ivanova, and Sidorova 2017), Yamal (Laptander 2014), and Evenkia (Ssorin-Chaikov 2003; Bloch 2004). Similar developments have been discussed in the case of Arctic indigenous minorities in North America and the Nordic countries (Lantto 2014; Einarsson et al. 2004; Hamilton et al. 1996; Csonka 1995; Kohlhoff 1995; Marcus 1995; Hamilton and Seyfrit 1994; Tester and Kulchyski 1994; Hamilton and Seyfrit 1993; Fogel-Chance 1993; Wenzel 1991; Armstrong, Rogers, and Rowley 1978).

There is, of course, a body of research overlapping and communicating with this thesis. On the processes of relocation among the indigenous population of the Kola Peninsula, the foremost works are Afanasyeva (2013) and Gutsol et al. (2007). These monographs are more about the processes of relocation than its consequences; they do mention the consequences, but do not engage in a deep analysis of them. Some details about social consequences can be found in Konstantinov (2015), Overland and Berg-Nordlie (2012) and my own previous research, although in none of these publications are they the principal focus. Therefore, Afanasyeva (2013, 64) justifiably stated that “the consequences [of Kola Saami relocations] require a more comprehensive investigation.” The current thesis directly addresses this need.

In his book, Konstantinov stresses collectivisation and its consequences and shows considerable agency and negotiation power on the part of the people throughout the Soviet period; yet, in the margins of his research he also acknowledges that in the case of the resettlements people had less – but still some – control over events and that the measures had far-reaching negative social consequences. He addresses the fundamentally problematic politics of the Soviet Union in this regard:

The numerous changes of administrative, collective farm, and toponymic status that ancient Yokanga [taken here just as one example, L.A.] saw between the 1930s and 1990s is a phenomenon common in the whole region if not the whole country. This constant reshuffling of administrative, employment, and demographic organisation, and total disregard for the effects of this cavalier treatment on the inhabitants reveals important aspects of elite attitudes. These can be summarized by saying that small-scale habitations, with or without a long history behind them, can be renamed, moved, liquidated, resurrected, moved again, again renamed, etc., if they stand in the way of ideological, administrative, engineering, or military plans. (Konstantinov 2015, 155)

What Konstantinov addressed here, in the marginalia of his research, will stand at the core of this thesis. Concerning the reshuffling of dwelling and moving practices, we can refer here to Pelto's (1973, 179) ground-breaking work on the "snowmobile revolution" in Finnish Sápmi: "Neither 'techno-economic determinism' nor 'cultural causation' provides an adequate model for the complex feedback of effects in the human adaptational system." Pelto argued in the 1970s against the determinism of technocracy, so fashionable in his time, but also against cultural determinism: they both pretend to swiftly offer ready-made answers to adaptation problems. If his entire book is dedicated 'only' to the arrival of the snowmobile – with all its socio-economic consequences – then it is high time that the more comprehensive upheavals in Soviet Lapland at approximately the same time are analysed following a similarly syncretic approach.

In general, I try to avoid binary views of Soviet reality. Such views are usually based on oppositions such as totalitarian versus democratic, victim versus agent, and similar dichotomies (see Yurchak 2006 for a corresponding critique). Convictions about immutable dichotomies are often expressed through recurring epithets like Soviet "regime", "forced" relocation, and so on. Such literature often assumes minimal or no agency on the part of people, connecting such passivity with totalitarianism and brain-washing as enduring characteristics of the so-called Third- and Second-World countries. These characteristics are commonly opposed to views of the democracy, freedom (and implied superiority) of the First World. As we can see from existing scholarship, such views are usually found in pieces of research where, in Ortner's (1995) words, we can find "ethnographic refusal". Such a deficit of anthropological insights can easily lead to oversimplifying statements like "the ethnic cleansing of Finnish and Swedish rural communities proceeded without any protests on the part of the victims and their families" (Kotljarchuk 2017, 113), "the task of the schools was to turn out little Russians" (Slezkine 1994a, 237) or "the political views in the context of the Soviet Union is [*sic*] not a relevant criteria as informants grew up in the state with strong totalitarian collectivism tradition, which did not accommodate room for an individual political voice" (Afanasyeva 2019, 70). While such statements may do justice to specific situations, in the generalising way

of presenting the message they homogenise experience and overlook the diversity of adaptations. The anti-binary ‘tradition’ of research, conversely, acknowledges that forms of agency, ‘alternative’ thinking and negotiations of power in fact exist in any political system.

This is not to say that literature from the ‘binary school’ is useless. It usually focuses more on structural analysis of policies and their consequences (further examples dealing with the Soviet Arctic are Josephson 2014; Golovnev and Osherenko 1999; Pika, Dahl, and Larsen 1996; Grant 1995; Vakhtin 1992). For the most part, these sources are pioneering works of the 1990s written after the opening up of the Soviet Union. They laid the foundations for an understanding of the partially abhorrent outcomes of Soviet social engineering and remain indispensable sources of facts.

It is only on the foundation created by such studies that, from the 2000s, more time-intensive anthropological fieldwork studies could start offering finer-grained views (D. G. Anderson 2000; Rethmann 2001; Liarskaia 2003; Ssorin-Chaikov 2003; Bloch 2004; Stammler 2005; Habeck 2005; Vladimirova 2006; Dudeck 2013a; Konstantinov 2015). Such research works from a position that “dislodges an image of monolithic Soviet power” in favour of a view of Soviet power as “differentially experienced” (Bloch 2004, 15).

1.5. Terminological disambiguations

In this section I disambiguate and define my use of several highly contestable terms.

‘Western’ and ‘modern’: I will adhere in this thesis to Ingold’s (2000) sharp-minded thoughts on what it means to be ‘Western’ and ‘modern’. He writes a convincing ‘apology’ for using both terms in spite of all the generalisations and impreciseness they entail. Possible legitimate contestations of ‘Western’ and ‘modern’ are the following: firstly, in anthropology, history and other social sciences they often serve as a generalising foil against an equally essentialised ‘native point of view’. Secondly, much of the philosophical critique of so-called Western or modern thought stems itself from Western traditions of thought. In other words, Western traditions of thought are very heterogeneous and cannot be exhaustively subsumed under a generalising foil of ‘Western’: there is probably no ‘full-blooded’ Westerner on this planet, as there probably is no – I would add – full-blooded ‘Native’.

Why does Ingold still use the terms then? His good reason is that “our very [scholarly] activity, in thinking and writing, is underpinned by a belief in the absolute worth of disciplined, rational inquiry [...]. It is to this belief that the terms ‘Western’ and ‘modern’ refer” (Ingold 2000, 6). Understanding this belief has a double implication for this thesis. Firstly, it is relevant for seeing social engineering as a characteristic of “high-modernist” societies (Scott 1998). In this sense of the word, as will be shown later, the Soviet Union was a thoroughly ‘Western’ state.

Secondly, this belief is relevant in relation to how I conduct my research and to the expectations of the audience who will assess my research. I follow Ingold when he says:

However much we may object to the dichotomies to which it [the term ‘Western’] gives rise, between humanity and nature, intelligence and instinct, the mental and the material, and so on, the art of critical disputation on these matters is precisely what ‘the West’ is all about. For when all is said and done, there can be nothing more ‘Western’, or more ‘modern’, than to write an academic book such as this. (Ingold 2000, 6)

In this understanding of the term, there is no escape from being ‘Western’ when writing a doctoral thesis and thus subordinating oneself to the conventions of academic work.

‘Community’: I fully agree with Konstantinov (2015:18), who talks about an “illusion entertained by visitors, that by talking with a few local people one can hear the ‘community voice’”. In this thesis, the reader will not be able to find *the* community voice, and yet I do use the word ‘community’. This is not a paradox. ‘Community’ is a blanket term which needs thorough reconsideration, as I have argued in Article 4. Grouping people according to ethnic, national or other boundaries does not mean that there is actually such a homogeneous group with common interests as a given thing-in-the-world (Brubaker 2004). This should not be forgotten when talking about an indigenous ‘community’ in the Russian Arctic, where “many ‘communities’ [...] were created relatively recently and artificially (during the forced relocations of the 1960s or even later during the destruction of the state farms in the early 1990s)” (Gray, Vakhtin, and Schweitzer 2004, 204). What may be mistaken by some visitors as an age-old community – which per se is also no guarantee of homogeneous interests – may often in fact have been randomly put together only a few decades ago by some distant planners. This is the case for Lovozero, the principal site of my field research. Gray, Vakhtin and Schweitzer describe the resulting problem: “At issue here is the concept of collaborating with communities, which has become very popular with funding agencies. But does one really collaborate with a *community* or with *individuals* in a community? Any community will contain diverse interests” (2004, 205, original emphasis). When I use the word without further discussion, I use it to designate a group of people who live or used to live in the same settlement, and in no way wish to implicitly create homogenised “imagined communities” (B. Anderson 1983) of interests. In this sense, I try to understand and appreciate the intricate heteroglossia (Bakhtin 1981 [1934-35]) of communities.

‘Indigenous’: In this thesis, I will use the terms ‘indigenous’ and ‘native’ synonymously. In addition, I will use the term ‘para-indigenous’ and its synonymous variation ‘quasi-indigenous’. Today, only the Saami are ethno-politically organised and have a legal indigenous status in the Murmansk Region. According to the most recent

census, the other people with legal indigenous status in the Murmansk Region include 149 Nenets, but they are not indigenous to the Murmansk Region (“Chislennost” n.d.). Together with Komi, Russians and other people who do not have legal indigenous status but share similar livelihoods, the Nenets may be subsumed under the group termed ‘para-indigenous’ or ‘quasi-indigenous’ (see Konstantinov 2015, 32, 238, 313): they live on the tundra side of the town-tundra rift and have also been subject to multiple displacements by the state. However, in local and regional Soviet archival materials, the Saami, Komi and Nenets are often together referred to as “the indigenous population”. Depending on the context, throughout this thesis I will use ‘quasi-/para-indigenous’ as terms to refer to these groups of people or ‘indigenous’ in its broader Soviet meaning where archival materials are being discussed.

1.6. A note on translation, transcription and transliteration

All non-English titles in the list of references that are quoted in this thesis have been translated into English by me. The same applies to all quoted interview excerpts and archival sources.

All transcribed and quoted interviews use a set of conventions (Rosenthal, 1987) for indicating features of spoken language, adding clarifying comments, and like functions:

Q (En.) / И (Russ.)	Interventions by the interviewer (Russian: <i>interv'iuer</i>)
A (En.) / O (Russ.)	Interventions by the interviewee (Russian: <i>otvet</i>)
(4)	Speech pause in seconds
yes:	Slowing of speech
((laughter)) / ((phone rings))	Transcriber's comment about things happening during the interview
[reference is to Voron'e]	Comment added later to the transcribed text for a better understanding of the quote
<i>we had a good life</i> [italics]	Emphasis added later to the transcribed text
no [bolded]	Emphasis by the speaker
NO [bolded capitals]	Spoken loudly
'no' [in single inverted commas]	Spoken quietly
very- [word with hyphen]	Unfinished word or sentence.
yes=yes [words spaced with equals signs]	Words spoken in rapid succession
()	Unintelligible speech; the length of the empty space corresponds approximately to the length of the incomprehensible word run.
(he said) [words in brackets]	Poorly intelligible speech; the transcriber has guessed what was said.
«veža»	Non-Russian (here: mostly Saami) words. For English equivalents, see the Glossary.
“Don't go anywhere!”	Direct speech in the narrative of the responding person

For transcribing words originally written in the Cyrillic alphabet, I use the simplified ALA-LC Romanisation for Slavic alphabets. In keeping with the widespread practice, I omit some diacritics and digraphs found in the formal, unambiguous version of the system. I apply the same system of transcription for both Russian and Saami words written in the Cyrillic alphabet. Exceptions are well-known Russian proper nouns with an established English spelling (e.g. the *Bolsheviks*, instead of the *Bol'sheviki*) and authors' names and titles of publications for which the original publication has used a different spelling in Latin letters (e.g. *Argounova* instead of *Argunova*).

Russian letter	Romanisation	Examples
А (а)	A (a)	Азов = Azov
Б (б)	B (b)	Бабинский = Babinskii
В (в)	V (v)	Вороне = Voron'e
Г (г)	G (g)	Грозный = Groznyi
Д (д)	D (d)	Дзержинский = Dzerzhinskii
Е (е)	E (e)	Елизово = Elizovo
Ё (ё)	Ë (ë)	Ёлкин = Ëlkin
Ж (ж)	Zh (zh)	Жуков = Zhukov
З (з)	Z (z)	Звенигород = Zvenigorod
И (и)	I (i)	Иркутск = Irkutsk
Й (й)	I (i)	Йоканья = Iokan'ga
К (к)	K (k)	Киров = Kirov
Л (л)	L (l)	Ломоносов = Lomonosov
М (м)	M (m)	Менделеев = Mendeleev
Н (н)	N (n)	Новосибирск = Novosibirsk
О (о)	O (o)	Омск = Omsk
П (п)	P (p)	Петрозаводск = Petrozavodsk
Р (р)	R (r)	Ристикент = Ristikent
С (с)	S (s)	Сосновка = Sosnovka
Т (т)	T (t)	Тамбов = Tambov
У (у)	U (u)	Углич = Uglich
Ф (ф)	F (f)	Фурманов = Furmanov
Х (х)	Kh (kh)	Хабаровск = Khabarovsk
Ц (ц)	Ts (ts)	Цимлянск = Tsimliansk
Ч (ч)	Ch (ch)	Чальмны-Варрэ = Chal'mny-Varre
Ш (ш)	Sh (sh)	Шахтёрск = Shakhtërsk
Щ (щ)	Shch (shch)	Щёлково = Shchëlkovo
Ъ (ъ)	"	Подъездной = Pod"ezdnoi
Ы (ы)	Y (y)	Ыттык-Кёль = Yttyk-Kël'

Б (Ь)	’	Тюмень = Tiumen’
Э (э)	Е (е)	Экостровский = Ekostrovskii
Ю (ю)	Ии (ii)	Юбилейный = Iubileinyi
Я (я)	Иа (ia)	Якутск = Iakutsk

1.7. Structure of this thesis

The thesis is divided into three parts. The first part is a general theoretical-methodological proposition about phenomenologically and anthropologically inspired oral history; this analysis both preceded and was extrapolated from the empirical insights of this thesis. The second part is topically linked to the Kola Peninsula and forms the empirical core of the research. In the third part, these two are combined and discussed.

Following the present introduction, Chapter 2 is devoted to phenomenology as the basis of my research. I will explain how phenomenology shaped my ways of understanding my field partners and of doing oral history inquiry.

Chapter 3 deals with the question of how we can generalise and theorise from qualitative research based on biographies and interpretation.

In Chapter 4, I explain how oral history can make significant epistemological contributions, addressing in particular the question of credibility of oral testimonies and their interpretation.

Chapter 5 presents the array of theoretical-methodological approaches used in this thesis. On a more general level, I try to make a case for a deliberate theoretical-methodological eclecticism instead of single-method research as the preferred path of qualitative inquiry.

In Chapter 6, I outline my approach to the gathering and organisation of materials, combining methods from history and anthropology.

Chapter 7 deals with ethical questions that have not been discussed in Article 3, which is devoted to ethics.

Chapter 8 contains a history of displacement and emplacement in Soviet Sápmi. This chapter forms one limb of the empirical core of the thesis, in addition to that presented in the articles.

In Chapter 9, the published thesis articles are reproduced.

Chapter 10 discusses broader-scale insights resulting from the empirical and theoretical-methodological parts of this thesis as a whole and summarises the common threads throughout the work. The chapter also brings forward additional topical insights relating to the articles that were generated after their publication.

These discussions are followed by a concise conclusion in Chapter 11.

Part I: Towards a phenomenologically and anthropologically inspired oral history

2. Phenomenology as a starting point

Experience is the main – one may say the only – connection between the ‘reality out there’ and what we perceive. Experience *is* reality. All reality is perception, and all perception is experienced. This is why recording and interpreting individual experience plays a central role in this thesis as in any oral history inquiry. This does not mean that this study eschews the collective or the universal. Rather, it has been conducted in keeping with an attitude as described by Denzin:

No individual is ever just an individual. He or she must be studied as a single instance of more universal social experiences and social processes. [...] Every person is like every other person, but like no other person. Interpretive studies [...] attempt to uncover this complex interrelationship between the universal and the singular. (Denzin 1989, 19)

Phenomenology is the philosophical foundation for studying individual experience, and with this chapter I would like to make this foundation more explicit than is usually done in oral history. The phenomenological approach used in this thesis is a way of thinking, an attitude, a self-awareness that serves as a constant backdrop to my scholarly thinking. It is not a set of methods and procedures. I constantly try to apply my *phenomenological awareness* both to the narrated and immediately lived experiences I hear and see in speaking with my interlocutors and to my own experiences as a researcher, both ‘in the field’ and ‘at the desk’. What is, then, phenomenological awareness?

Roughly speaking, there are two ways of applying phenomenology. The first may be called *phenomenology proper*. This is seen as entailing the consistent use of *reduction*, the main thought operation of phenomenological philosophy; this means consciously looking at a phenomenon from a ‘child’s viewpoint’, that is, one as stripped of context as possible, and then reconstructing knowledge and context around the phenomenon thus bared. For this, in the language of phenomenology, the phenomenologist is required to switch from a *natural attitude* to a *transcendental attitude* (that which is ‘outside ourselves’).

The second way may be called *phenomenological awareness*: here, the researcher thinks, as it were, “I know and am inspired by the ideas of phenomenology, but I also leave the realm of strict phenomenological philosophy and do not consistently apply phenomenological thinking in its purest form. I let it become contaminated with perspectives, methods and theories from social sciences and humanities and merge them into my mixture of reflections that evolve in the course of working with my

research subject”. As is the case with Psathas (1973; 1989) in his phenomenological sociology, Garfinkel (2002; 2006) in his ethnomethodology or Ingold (2000; 2011; 2013) in his way of doing anthropology, this work is not phenomenology proper but is inspired by a phenomenological attitude: the empirical questions emerging from my original field of research at the crossroads of history and anthropology tend to be phenomenologically inspired. Thus, phenomenology becomes a base layer, a backdrop against which to ask questions, to use existing theories and to build up thoughts. This is my phenomenological awareness, which aims at developing a complex account of many different types of awareness: self-awareness, awareness of other persons (empathy, intersubjectivity, collectivity), linguistic awareness, temporal awareness and spatial awareness, to name just a few types of ‘awarenesses’ (see Smith 2018).

One of the goals of this thesis is to make phenomenological awareness as a basis of qualitative inquiry more explicit. It is therefore necessary to give a short overview of what forms the basis of phenomenology as one of the major disciplines of philosophy. Edmund Husserl is considered the founding father of phenomenology as a separate philosophical discipline. It was not quite Husserl’s strength to express himself succinctly, which might explain why his original works are, at least outside of narrow philosophy, not widely quoted in contemporary phenomenologically inspired research. In this chapter, I will try to offer a compact – compared to Husserl’s wordiness – combination of several quotations from his work that are relevant for phenomenology-driven anthropological and historiographical scholarship. Husserl reflected on the most basic trait of what I have called phenomenological awareness above:

No conceivable theory can make us err with respect to the *principle of all principles: that every originary presentive intuition is a legitimizing source of cognition, that everything originarily* (so to speak, in its “personal” actuality) *offered to us in ‘intuition’ is to be accepted simply as what it is presented as being, but also only within the limits in which it is presented there* (Husserl 1983 [1913], p. 44, original emphases).

In short, this means that lived experience is the first thing Husserl urges us to look at, before existing ‘third-party’ knowledge. Obviously connected to experience is subjectivity:

In the phenomenological approach we do not see any possibility of a world without (transcendental) subjects. Therefore, the question of the origin and the essence of the world has also a subjective side already at the very outset of our enquiry. So we ask ‘How did I get acquainted with the world?’ or ‘How did my world become my world and the world?’ *In the phenomenological order the how-question precedes the what-question* (Himanka 2001, 194, my emphasis).

These are the basic questions about the approach to the world that I am researching. Using the ways of how-questioning, I explore my research field, ask questions of my interlocutors in the field and question the materials I have gathered. The last comprise both 'hard' materials (interview recordings, archival materials, newspaper articles, field notes) and 'soft' materials (my experiences). Eberle (2014, 184) called phenomenological approaches "a proto-sociological foundation to the methodology of social sciences", and one may add that they also serve as a proto-anthropological and proto-historiographical foundation. By practising 'pre-reflective reflection' on the lifeworlds of individuals in their pre-reflective, everyday state, phenomenological approaches in the social sciences prepare the ground *before* setting out to taxonomise, classify, codify or abstract, *before* engaging in theory-laden and theory-generating reflection on human action and interaction. This initial focus on experience and subjectivity requires a priori that the individual subject be at the centre-stage of any phenomenologically oriented inquiry. With a phenomenological stance we can surmount "the aporia that empiricism and rationalism have produced by separating the cognising subject and the objective world" (Eberle 2014, 185).

2.1. Reduction

To many social scientists working with qualitative methods and a postmodern research ethos, but not trained specifically in phenomenology-driven studies, many aspects of phenomenology may sound familiar. The reason for this is that many scholars in philosophy, history, anthropology, psychology and sociology before and after the term "phenomenology" became established have found inspiration in similar approaches that circulate under other names and do not explicitly refer to phenomenology. The philosopher says: "In phenomenological reflection, we need not concern ourselves with whether the tree exists: my experience is of a tree whether or not such a tree exists. However, we do need to concern ourselves with how the object is meant or intended" (Smith 2018, 8). The social constructivist says: "Constructivist inquiry starts with the experience and asks how members construct it." (Charmaz 2006, 187). Concepts such as perspectivism (used by Nietzsche and other philosophers), positioning (B. Davies and Harré 1990) or grouping (Brubaker 2004) share with phenomenology (de)constructivist thinking and the appreciation of individual experience as a way to generate insights. Denzin (2009, 29) posits that critical qualitative inquiry "must start with the personal and the biographical and our own location within the world around us", a claim reflecting a typical instance of a thorough but non-explicit phenomenological attitude. It is thus no wonder that one can discover phenomenology in so many pieces of scholarly literature not intentionally related to phenomenology. Before Derrida made the term "de-construction" famous, Heidegger had used the terms "de-construction" and

“construction” as steps in phenomenological reduction (Van Manen 2014, 107). Thus, in one way or another, phenomenology is deeply implied in all constructivist approaches.

Phenomenological awareness may sound somewhat abstract – so far at least – because phenomenology in its original form gives no clear ‘recipes’, despite the omnipresence of the term *reduction* as a sort of mysterious formula through which ‘pure’ phenomenology seems to be achieved. According to Himanka, “after reduction we understand the standpoint we had before reduction as something Husserl calls *natural attitude*. The attitude opened in reduction Husserl calls *phenomenological* or *transcendental* attitude.” However, Husserl failed “to clarify what kind of performance this change of attitude [reduction, L.A.] actually is. [...] As a consequence, there are very few followers of Husserl who venture to state that they actually work by performing reductions.” (Himanka 2010, 620) Reduction is not a methodological step-by-step procedure. It describes an attitude, one which is taken up in some form in many constructionist approaches. One must find one’s own way of doing reduction. However, despite the lack of a rigorous procedure, no other discipline has reflected so much on disassembling and reassembling than phenomenology, and given it the name “reduction”.

Phenomenological reduction consists of two basic steps, corresponding to disassembling and assembling. The first, disassembling step Husserl called *epoché*, which means suspension or bracketing of the natural or everyday attitude and thus, at least in its purely philosophical and initially egological version, reaching the pure transcendental ego. The second, assembling step is called, somewhat misleadingly, *reduction* and re-constitutes meaning; that is, it returns to the world as it shows itself in consciousness. For disambiguation, Van Manen (2014, 215) proposes the terms *epoché-reduction* and *reduction-proper* for the two distinct steps of reduction. Drawing mainly on Van Manen (2014), in the following table I have tried to summarise these steps and display those categories of reduction that are relevant for an oral history inquiry.

Table 1: Forms of phenomenological reduction.

Reduction an attentive turning to the world when in an open state of mind, through which insights may occur. NOT a determinate set of steps, technical procedure, rule, tactic, strategy! Consists of:	
preparatory move Husserl: Epoché (gr. abstention, staying away from) Van Manen: Epoché-Reduction (ER) =bracketing or deconstruction negative move: suspends or removes what obstructs access to the phenomenon	proper reflection Husserl: Reduction (lat. <i>re-ducere</i> : to lead back) Van Manen: Reduction-Proper (RP) =reflection and reconstruction positive move: returns, leads back to the mode of appearing of the phenomenon NOT reductionism; quite the opposite!
<p><i>Selection of conceptual categories of the epoché-reduction:</i></p> <p><u>Heuristic ER=wonder</u>: bracketing the taken-for-granted-attitude.</p> <p><u>Hermeneutic ER=openness</u>: bracketing interpretation as much as possible; becoming aware of, explicitly addressing and trying to overcome pre-understandings, frameworks, theories, feelings, preferences, expectations in order to avoid premature, wishful, one-sided understandings: to practice radical openness.</p> <p><u>Experiential ER=concreteness</u>: explicating concreteness or living meaning by bracketing all theory or theoretical meaning; seeing that scientific theories are abstractive and because of that less sensitive to the concrete.</p> <p><u>Methodological ER=approach</u>: bracketing all conventional and formalised techniques and seeking/inventing an approach that might fit most appropriately the phenomenon under study.</p>	<p><i>Selection of conceptual categories of the reduction-proper:</i></p> <p><u>Eidetic RP='whatness' (Husserl)</u>: grasping some essential insights in testing the meaning of a phenomenon or event by varying its aspects through the process of variation in imagination (in a thought experiment) or through comparing empirical examples (eidetic means 'relating to mental images', from Greek <i>eidōs</i>=form).</p> <p><u>Ontological RP=mode of being (Heidegger)</u>: explicating the modes or ways (instead of the whatness) of being that belong to or are proper to something. It was Heidegger who shifted the focus from Husserl's earlier focus on ontic meaning (whatness of being) to ontological meaning (mode of being), which is more useful for social sciences.</p> <p><u>Radical RP=self-givenness</u>: focusing on the way that a phenomenon gives itself as itself. Phenomenology should not depend on any constituting or sense-making subject or agency; it should solely attend to the self-givenness of phenomena. This form is too radical for social science approaches.</p>

Phenomenological reflection is not introspective but retrospective: if one reflects on one's anger, then this anger is already being processed and therefore part of it is already past. This is why phenomenology is always recollective, always about something that has already been lived through. In this way, a phenomenological attitude is apt as an approach to history.

In practice, the suggested categories of epoché-reduction and reduction-proper are applied in concert. They are not applied as a toolset, but rather as an attitude. In the case of this thesis, this was an attitude that I tried to keep as a background

state when analysing what I heard and saw during fieldwork. The table above is only a conceptual categorisation of this attitude. It makes clear that phenomenological reduction means quite the opposite of the kind of reduction that we pursue in our everyday situations. The latter form is employed by humans on a daily basis in order to make daily life more or less fast and efficient: it aims at reducing doubt by drawing on the fount of everyday certainties that we acquire through our socialisation (Schutz and Luckmann 1979, 1:32). Conversely, reduction in phenomenology aims at reducing our everyday certainties by employing doubt and wonder and thus slowing down perception.³ A great deal of qualitative and quantitative inquiry, in sociology for instance, queries people, naively assuming that they have straightforward and introspective access to their experiences. Such inquiry largely relies on the insights offered by the people queried. In contrast, the reductive strand of phenomenology aims to guard our gaze against the taken-for-grantedness of everyday interpretation, established values, existing theories and much more, and to make us more thoughtful about the experiences we acquire through our bodies, language, habits, things, social interactions and physical environments. The aim in keeping a constant level of phenomenological awareness is thus to make those perceptions visible and then to conceptualise them. What “appears” through the phenomenological gaze “is not at all something apparent or clear-given. If it were, then phenomenology would not be necessary: we would simply see what it is that ‘appears’” (Van Manen 2014, 61).

Thus, phenomenology is the study of phenomena; the phenomena studied are somebody’s experiences, and this has been my initial interest in all of the data serving as a basis for this thesis. By way of example, if I saw a horse, I could later doubt that what I saw was a horse, but I cannot doubt that I had the experience of seeing a horse. In the same vein, if an ethno-political activist tells me that she speaks for her people, I can doubt that she speaks for all her people, but I cannot doubt that I had the experience of hearing from her that she claims to be speaking for her people. What becomes the object of my reflection is this experience. Here I should ask: How should I understand this experience and under what circumstances did this experience come into being? This in turn prompts me to take a closer look at the position of both my interlocutor and myself in the situation where the experience occurred.

We see that phenomenological reduction disassembles and re-assembles, and through this lays bare what was invisible. It has now become more evident that, while there is much ‘unspoken’ phenomenology in virtually all de-constructive qualitative inquiry, being aware of and using to some extent the phenomenological arsenal of reduction can greatly improve the results of such research.

³ Interestingly, at about the same time as Husserl, in the beginning of the twentieth century, the Russian formalists identified similar functions in art (see Shklovsky 1965).

2.2. Lifeworld

Towards the end of his life, in the *Crisis of the European Sciences* (originally published in 1936), Husserl turned from his strictly egological phenomenology to what he called the *lifeworld* (*Lebenswelt*) of individuals in their environment. This opening of phenomenology toward the ‘world’, and thus toward a wider array of scholarship, was motivated by a discontent with the status of science and with ‘modern man’ at the time in the light of the recent technical progress, whose pace was unprecedented. In many respects this has not lost any of its relevance:

The total world-view of modern man, in the second half of the nineteenth century, let itself be determined by the positive sciences and be blinded by the ‘prosperity’ they produced, meant an indifferent turning-away from the questions which are decisive for a genuine humanity. Merely fact-minded sciences make merely fact-minded people. The change in public evaluation was unavoidable, especially after the war [...]. In our vital need – so we are told – this science has nothing to say to us. It excludes in principle precisely the questions which man, given over in our unhappy times to the most portentous upheavals, finds the most burning: questions of the meaning or meaninglessness of the whole of this human existence. (Husserl 1970 [1936], 6)

In his writings, Husserl condemns the idea “[of ascribing] to the newly formed idea of ‘objective truth’ a higher dignity, that of a norm for all knowledge” (Husserl 1970 [1936], 121). Husserl developed his concept of the lifeworld precisely because of these reflections. In the course of the twentieth century, the term became a regular label of phenomenologically oriented social sciences. I will therefore try to elucidate its meaning and connotations, and its relation to phenomenologically inspired historical inquiry.

For Husserl, the “only real world, the one that is actually given through perception, that is ever experienced and experienceable [is] our everyday life-world” (Husserl 1970 [1936], 49). Innovative for his time, this is a post-positivist, essentially post-modern definition, stating that reality, for us humans, is inseparable from and grounded in the horizon delimited by our perceptions and experiences:

The life-world, for us who wakenly live in it, is always already there, existing in advance for us, the ‘ground’ of all praxis whether theoretical or extratheoretical. The world is pre-given to us, the waking, always somehow practically interested subjects, not occasionally but always and necessarily as the universal field of all actual and possible praxis, as horizon. (Husserl 1970 [1936], 142)

This implies that whatever scholarly work we do, we are supposed to start from the human being’s natural attitude in the lifeworld. We unavoidably do this because

the lifeworld is pre-given and each individual is positioned in it. Phenomenology highlights and ponders this:

An explicit elucidation of the objective validity and of the whole task of science requires that we first inquire back into the pre-given world. It is pre-given to us all quite naturally, as persons within the horizon of our fellow men, i.e., in every actual connection with others, as 'the' world common to us all. Thus it is, as we have explained in detail, the constant ground of validity, an ever available source of what is taken for granted. (Husserl 1970 [1936], 121–22)

Husserl implies several things here: Firstly, the taken-for-grantedness, or *doxa*, which consists of opinions, dogmas, illusions and unreflexive thinking in the daily acting in the lifeworld is a ground for validity, and it is a ground that is worthwhile understanding. As a term in social sciences, *doxa* was taken up most prominently by Bourdieu (1977).

Secondly, while being able to have its validity, quantifying and objectifying research, which does not look into *doxa*, is rarely enlightening as a stand-alone exercise. An example from my field of research might be the numerous reports and articles on indigenous mortality to be found in medical and sociological research (for example, Young and Bjerregaard 2008). The hard facts are indispensable, but equally indispensable are convincing explanations of causes. Unless they are grounded in an exploration of lifeworlds, explaining causes does not go beyond making shallow guesses. Exploring the 'soft facts' is worthwhile in its own right: it is not merely a matter of adding an ornament to the 'hard facts', an epistemic exercise in itself or done to uncover details of the past that are intriguing but insignificant for the 'big picture'. Rather, the 'soft facts' form an indispensable foundation for scholarly work that wishes to contribute to a transformation of the world by understanding past mistakes and current problems in order to avoid their repetition in the future.

Thirdly, *pre-givenness* (*Vorgegebenheit*) is a characteristic and inalienable trait of the lifeworld. An implication of this is that seeing the lifeworld as a pre-theoretical, naïve world would be naïve in itself. Humans, to a greater or lesser extent socialised in a tradition of theorising, start thinking theoretically when they are still within in the horizon of their lifeworlds. Their lifeworld inevitably influences their thinking. To strictly separate a 'scientific', 'theoretical' life and 'everyday life' would be as difficult as the Cartesian separation of mind and body, and of humans and nature. This is a fundamental viewpoint of phenomenology-driven scholarship (Husserl 1970 [1936], 221; Carr 1970, xl–xli; Ingold 2000, 172–73). Even an artificially created notion of reality is ultimately grounded in perception and experience. Husserl thus contradicts an objectivist notion of reality. According to Husserl's translator, Carr:

Husserl's point is that it is not merely our various theoretical approaches to the world that derive from a tradition. The very notion or motive of approaching the world 'theoretically' at all is itself a tradition [...]. In order to understand Galileo's accomplishment, for example, we must understand not only his inheritance of the 'ready-made' science of geometry but also his inheritance of the very *idea* of science, the *task* of finding nonrelative truths about the world as such, the task the Greeks defined by distinguishing between 'appearance' and 'reality,' between *doxa* and *episteme*. (Carr 1970, xxxviii)

This idea pertains to my research in two ways: even contemporary historians are too often one-sidedly interested in a history that is imagined as *episteme*, that is, 'how it really was'. Anthropology, and anthropology-influenced history and sociology have been giving more, and due, attention to *doxa*. Focusing more on *doxa* as a researcher means being aware that looking for the 'nonrelative' truths is at best one-sided and at worst simply impossible. Scholars are well-advised to be critically aware of their own cultural inheritances, such as the very idea of science with its separate 'reality' (*episteme*) being set hierarchically above 'appearance' (*doxa*). Such an awareness should also lead to an awareness of the inheritances of individuals and societies one does research on, for those are part of individuals' and societies' pre-givenness – indeed, the *doxa*. Such inheritances live in the form of discourses, for example when hardship and success are explained with reference to ethnic traits (see Chapter 8) or a strong belief in medicalising, fact-obsessed psychology leads to false diagnoses of mental deficiency among indigenous children (see Article 2). Other forms of inheritances as aspects of lifeworlds are presented in Article 4, where I identify an array of competing discourses as parts of the lifeworlds of my field partners. Looking behind the well-organised grid of *episteme* into the organic mesh of *doxa* leads to crucial insights.

Doxa often remains unsaid and implicit. However, while the implicitness of *doxa* as the basis of the common lifeworld simplifies social interaction, its unspokenness as fine attunement to the world is "richly organized for practical ends" (Carr 1970, xli) and makes it all the more complex and worth uncovering. A stance dismissing *doxa* as mere simplifications misses *doxa's* complications as the other side of the coin. Missing them would be a simplification in itself. More concretely, with reference to the example from Article 2, this means that not taking into account the *doxa* of the current and reconstructed past lifeworlds of myself and of my interlocutors would have had two consequences. On the level of my own activity as a researcher, I would have questioned less the relativity of the hegemonic *episteme* of a medicalised approach to mental disease, for a medicalising approach to psychological issues has long since become a widespread unspoken *doxa* also in contemporary, developed countries (J. Davies 2017b). On a second level, I would have been less aware of the *episteme* structures in which teachers and psychiatrists of the time (the 1970s

and 1980s) were caught and as a consequence neglected the role of *doxa* in what caused lower-than-average school performance by indigenous children. This issue is discussed in Article 2.

While Husserl's lifeworld concept paved the way for phenomenology to enter the social sciences, the first social scientist to make the term popular in his discipline was the sociologist Alfred Schütz (in American editions of his works spelled Schutz). His work *The Structures of the Life-world*, posthumously completed by Thomas Luckmann (Schutz and Luckmann 1973), has been particularly influential. In the fashion of structuralism, the book's central claim is that the principles of these universal "structures" (but not the structures themselves) are the same for all human beings in the world. Lifeworld here means that the individual and collective stock of knowledge and experience serves people as a constant and implicit frame of reference. The lifeworld is a "paramount reality" (Schutz and Luckmann 1973, 6, 21), because all other, loftier 'worlds' (like religion, art and science) are grounded in and born of it. The authors posited that the lifeworld, this paramount reality, should be the primary object of inquiry in social sciences. Indeed, Schütz's phenomenological sociology had a belated but decisive influence, creating in his discipline a new paradigm that successfully challenged Parsonian structural functionalism.

Schütz emphasises the general intransparency of the lifeworld's stock of experiences. While this might be a 'flaw' according to 'scientific' criteria, it is what makes the daily routine quick and efficient. Accumulated interpretations serve as guidelines for how to behave in daily life, being procedures that have proved themselves earlier (Schutz and Luckmann 1973, 11–15, 163–71). The phenomenological description of such universal formal structures is a subject for philosophy; the concrete content of these structures, however, differs throughout the world and is therefore an object for empirical research, which builds on these basic insights.

From time to time, an individual's smooth flow of acting based on sedimented experiences of the lifeworld may be disrupted due to novel circumstances requiring "a re-explication of the horizon of the kernel of experience which has become questionable" (Schutz and Luckmann 1973, 11). This can range from Schütz and Luckmann's encountering a mushroom "whose back does not fit into any set of typical mushroom backs" that they knew (Schutz and Luckmann 1973, 12) to biographical disruptions based on collective experiences (such as the end of the Soviet Union) or individual events (such as the birth of a child or an accident). It is worth paying special attention to such life experiences, which Norman Denzin (1989) called *epiphanies*, that is, times of change, rupture and revelation. These are "experiences [...] that radically alter and shape the meaning persons give to themselves and their life projects. [...] Having had such experiences, the individual is often never quite the same" (Denzin 2014, 578; see also Haumann 2006, 47). Each biography has such epiphanies; in the case of the biographies analysed in this thesis, the most common are events occasioned by state-induced displacement. Sensing

and reconstructing moments of disruption, as well as understanding certain views, interpretations, perceptions, values and beliefs in the light of such disruptions, pose a special challenge to oral history interpretations (see especially Article 4).

Schütz and Luckmann gave a concise definition of the lifeworld, which essentially captures Husserl's extensive writing on the subject: "The life-world, understood in its totality as natural and social world, is the arena, as well as what sets the limits, of my and our reciprocal action" (Schutz and Luckmann 1973, 6). This means that the lifeworld is the area of practice, and this is why the way in which we act and choose to act occupies a central place in lifeworld analyses. Related to this is a general interest in the analysis of agency (see section 5.4., *Agency*) and a sense of meaningfulness (see next section).

2.3. Creation of meaning

A focus on meaning-giving constructions has become well established in contemporary anthropology and oral history. In his programmatic text on what oral history is, Portelli suggests the following:

What is really important is that memory is not a passive depository of facts, but an active process of creation of meanings. [...] The specific utility of oral sources [...] lies [...] in the very changes wrought by memory. These changes reveal the narrators' effort to make sense of the past and to give a form to their lives." (Portelli 1998, 69)

Fischer (2014), in attempting to formulate an anthropological theory of wellbeing, contributes the insight that making sense of one's actions is crucial for realising the wish of living a good life: "Living up to the expectations of particular values is in many ways the stock and trade of human existence; and it is this forward-looking, aspirational quality that gives meaning to much of what we do" (Fischer 2014, 6). It is in the quest for meaning where looks into the past and looks into the future meet.

Phenomenology stands at the root of the modern way in which history and anthropology study meaning. One of its core thoughts is that the ways in which we see, conceptualise and understand elements of our lifeworld define what meaning we attribute to our experiences of the past. This makes the phenomenological perspective so important for the interpretation of oral history testimonies. Husserl commented on the fact that both the past and the future are graspable for every human only from the point of view of the present. In the following, he asserts that the present defines how we make sense of the past and of our projections of the future:

Our focus on the world of perception (and it is no accident that we begin here) gives us, as far as the world is concerned, only the temporal mode of the present; this mode

itself points to its horizons, the temporal modes of past and future. *Recollection, above all, exercises the intentional function of forming the meaning of the past.* (Husserl 1970 [1936], 168, my emphasis)

Here Husserl points at one of the tenets of oral history long before oral history as a discipline was established. When analysing our data, we need to start from the present in which we meet our interlocutor, and not from the past we are told about by him or her. Qualitative research, and most palpably interviewing, “operates in a complex historical field that cross-cuts multiple moments, all of which operate in the present” (Denzin 2009, 216). By exploring the lifeworld of the person in the here and now we can understand how and why people make sense of the past in certain ways, and only by taking this into account can we reconstruct past lifeworlds as well. The present lifeworld, which is temporarily shared between my interlocutor and me, is the ground for intersubjectively shared patterns of meaning. The meaning of actions of the past is created in an individual’s present reflection on this past. In other words, this meaning varies depending on the circumstances of this present reflection. Schütz gives a good definition of meaning in the same vein:

Meaning is not a quality inherent in certain experiences emerging within a stream of consciousness but the result of an interpretation of a past experience looked at from the present Now with the reflective attitude. As long as I live *in* my acts, directed towards the objects of these acts, the acts do not have any meaning. They become meaningful if I grasp them as well-circumscribed experiences of the past and, therefore, in retrospection. Only experiences which can be recollected beyond their actuality and which can be questioned about their constitution are, therefore, subjectively meaningful. (Schütz 1970, 1:210)

The interest in meaning-giving constructions is an originally phenomenological way of doing inquiry. It is phenomenology’s assumption that all action, including the creation of meaning (beliefs, motivations, representations, and theorising), is grounded in the present lifeworld. In the case of oral history and anthropological fieldwork, this means that not only the narration by the interviewee but also its interpretation by the researcher are constructions of meaning. Indeed, as Haumann notes, “In the process of critical and interpretive understanding of meaning [...] the historian creates new meaning and co-creates the reality” (Haumann 2006, 51).

There is, however, another assumption that needs clarification: in the lifeworld of everyday life, as well as in scholarly literature, the creation of meaning is most often implicitly understood as the making of *positive* meaning. This assumption is not often explicitly addressed in social sciences. The lifeworld-oriented historian Heiko Haumann puts the emphasis on meaning as follows: “Recollection shapes the self-conception of people and governs action. It substantially contributes to the praxis of

actors and bestows meaning upon it [...], whereby ‘meaning’ becomes nothing less than an existential question” (Haumann 2006, 44). It is implied that this existential quest for meaning is a quest for *positive* meaning. This reasoning points in the right direction but needs further clarification: it is an existential question because positive meaning is defined and required socially. The quest for positive meaning does not emanate from an isolated *ego*. As Davies and Harré (1990, 46, 59) state, “an individual emerges through the processes of social interaction”, and “we do struggle with the diversity of experience to produce a story of ourselves which is unitary and consistent. If we don’t, others demand of us that we do.” This is a reference to the common, unspoken intersubjective expectations within the lifeworld. Individuals tend to position themselves with reference to what they feel the current expectations are. This is of course not a law, but a tendency. Digressions tend to be seen by others as digressions from ‘normality’, which means that they leave the realm of the unspoken and will prompt attempts to explicate. The meaning that individuals are to associate with their actions is in most cases expected to be positive, and individuals usually take those expectations implicitly into account. When such expectations are not met, this may lead to a disruption of smooth coexistence, as exemplified in Article 3 in the account of contradicting depictions of the boarding school past by two different teachers and a former pupil.

According to Bourdieu (1987, 297), ‘life-history’ is one of those common-sense notions “which has been smuggled into the learned universe”. It presupposes that life is “a history”, “a path”, “a road”, “a track”, “a progression”, “a trajectory”, something consisting of a beginning, various stages and an ending. Bourdieu’s criticism is justified in relation to uncritical biographies depicting lives as consistent trajectories (the usual suspects being expressions like “from his earliest days...”). It is indeed tempting for both sides, biographer and biographee, to follow this order and thus obtain a fascinating story, and it requires a conscious effort from the scholar not to succumb to this temptation. However, what is common, shared sense is also likely to be “smuggled” into the oral narrative by the interviewee. It is the implicit and presumed common sense, the social frame that defines and requires an implicit meaning in past actions. What this meaning is “is defined through cultural values and a sense of purpose based on what matters most in life”, as Fischer (2014, 7) suggests in relation to pursuit of “the good life”. As biographical researchers, we need not blindly perpetuate the implicit common sense, but we need to recognise that certain constraints on creating meaning do exist. In sum, a well-constructed history, story or series of episodes giving the protagonist’s action positive meaning means constancy to oneself, a predictable, stable identity and, ultimately, “normality” (Bourdieu 1987, 299). In social interaction these are more often than not desirable qualities. What is regarded as positive is defined and implied not by the narrating *ego* alone, but in the wider society as well as in the immediate conversational situation. Positive meaning – as relative as it may

be – is both a social requirement, as stressed by Bourdieu, and a crucial element for personal wellbeing, as emphasised by Fischer.

Reflecting on his own life, Ingold came to a conclusion similar to Bourdieu's about the non-linearity of life:

My own work, over the last quarter of a century, has been driven by an ambition to reverse this emphasis: to replace the end-directed or teleonomic conception of the life-process with a recognition of life's capacity continually to overtake the destinations that are thrown up in its course. It is of the essence of life that it does not begin here or end there, or connect a point of origin with a final destination, but rather that it keeps on going, finding a way through the myriad of things that form, persist and break up in its currents. Life, in short, is a movement of opening, not of closure. As such, it should lie at the very heart of anthropological concern. (Ingold 2011, 3–4)

Phenomenologically inspired oral history research can directly embrace Bourdieu's and Ingold's thoughts on the zig-zag of life. Being aware of the teleonomic burden of biography by de-constructing and re-constructing this burden is a form of healthy phenomenological reduction. We stop seeing life as one line but rather see it at every stage and every moment as a point of departure for a potential myriad of different lines.

2.4. Evidence and truth

Notions of evidence and truth have been widely discussed in oral history and anthropology theory, where one sees them explicitly or implicitly informed by a phenomenological stance. Hastrup makes this point:

Anthropological reasoning [...] must be explicit of why particular instances are (perceived to be) connected in a certain way [...]. This displaces the problem of evidence from the ontological to the epistemological domain of knowledge. [...] 'Getting it right' is backed by anthropologists being *in touch* with reality – not by standing outside it looking for evidence. (Hastrup 2004, 460, 469, original emphasis)

Being “in touch with reality” means that together with our field partners we are living “the series of events that constitute social life, where there is no objective truth, but simply potentially exclusive versions of the truth that together constitute the event” (Jenkins 1994, 443). The multiplicity of truths and the embeddedness of evidence, as well as the relativity of both, have been extensively discussed in phenomenology. Both are tied to an utterly experiential understanding grounded in the lifeworld:

It is plain that I, as someone beginning philosophically [...] must neither make nor go on accepting any judgment as scientific that I have not derived *from evidence, from 'experiences'* in which the affairs and the affair-complexes in question are present to me as 'they themselves'. (Husserl 1982 [1931], 13, my emphasis)

Thus, *experiences are a form of evidence* that can lead us to truth. However, in order to accept this paradigm, we need to understand phenomenological notions of truth and evidence. This requires a constant effort to put aside other, hegemonic paradigmatic understandings of them: the objectivist and positivist understanding of truth and evidence that, stemming from the dualistic Cartesian worldview, has itself become part of the lifeworld in societies oriented towards constant technical progress and economic growth (Ingold 2000, 172–73). Such understandings of truth and evidence have a very strong standing down to the level of unconscious and unquestioned taken-for-grantedness, both in academia and outside of it. Well-established objectivist notions of evidence and truth, informed by apparently discourse-free disciplines such as the natural sciences, economics and statistics, as well as the assumed superiority of the written over the spoken in terms of objectivity, are the main grounds on which the validity of oral history and anthropology inquiry is questioned from the outside. This is why it is indispensable to clarify in more detail what evidence and truth mean in phenomenology and, as a consequence, in large parts of oral history and historical anthropology. Starting again from Husserl's writings:

The idea of objective truth is predetermined in its whole meaning by the contrast with the idea of the truth in pre- and extrascientific life. This latter truth has its ultimate and deepest source of verification in experience which is 'pure' [...] in all its modes of perception, memory, etc. (Husserl 1970 [1936], 124–25)

This, according to Husserl, has consequences also for 'objective' forms of truth in the classical scientific understanding:

The contrast between the subjectivity of the life-world and the 'objective,' the 'true' world, lies in the fact that the latter is a theoretical-logical substraction, the substraction of something that is in principle not perceivable, in principle not experienceable in its own proper being, whereas the subjective, in the life-world, is distinguished in all respects precisely by its being actually experienceable. The life-world is a realm of original self-evidences. That which is self-evidently given is, in perception, experienced as 'the thing itself,' in immediate presence, or, in memory, remembered as the thing itself [...]. All conceivable verification leads back to these modes of self-evidence [...] as that which is actually, intersubjectively experienceable and verifiable and is not a substraction of thought; whereas such a substraction, insofar as it makes a claim to

truth, can have actual truth only by being related back to such self-evidences. (Husserl 1970 [1936], 127–28)

Heeding Husserl's principal motto "back to the things themselves" (Husserl 1983 [1913], 35, see section 2.1., Reduction) here, we again see the lifeworld at play as a *paramount reality* (see section 2.2., *Lifeworld*). Experience-in-the-life-world stands at the basis of all evidence; it stands at the source of all truths, including those emerging from theorising, because all human conceptions of reality, no matter how 'primordial' or how 'objectified', are "originarily" grounded in our perception. In the Cartesian philosophical tradition of a separation of body and mind, and thus of the subjective and the objective, certainty and truth are related. In a phenomenological understanding there is no relationship between certainty and truth. Truth is not a static object, but a dynamic and processual understanding that is reflected for example in the dynamics of discourses.

Instead of being an "all-or-nothing affair", phenomenological truth emerges from "a complex and constant interplay between showing and hiding" (Van Manen 2014, 343). In qualitative inquiry, this results in an interest, for example, in discursive practices, positioning, that is, in an interest not in the ontology of truth but the epistemology of truth. In short, we want to know not what is true in absolute terms – because we maintain that such absolute terms do not exist in history and the study of human interaction in general – but why a particular person says that something is true. "We do not ask if the representation is true. We ask instead, is it probable, workable, fruitful?" (Denzin 2009, 229). Such insights, rooted in actual experiences, can indeed serve as new evidence, for example in the analysis of the relations between individuals and institutions. For instance, when it comes to the historical analysis of state-individual relationships, one focus in this thesis, what may be of interest "is the 'other half' of a process encompassing all of society: the history of how the expansion of commodity production, the state, and bureaucracy was experienced by 'the many'" (Lüdtke 1995, 8). This "other half" is crucial when looking for and respecting the multiplicity of truths. In this sense, different truths are different aspects – maybe as yet unknown – of known facts.

3. Generalisation and theorisation from qualitative data

Scepticism about fluid notions of truth and evidence (see section *Evidence and Truth*, 2.4.) is usually tied to a critical attitude towards the representativeness and generalisability of qualitative research. Questions about representativeness may be dismissed by proponents of qualitative case studies as wrongly framed, because they include notions of numeric value, whereas it is well-accepted that insights of qualitative studies need not necessarily be related to numbers or to significant groups of people. A broader notion is generalisation, and there is no way in which qualitative research can avoid questions about its generalisability if we accept the premise that qualitative research wants to make valid and widely received statements about human existence. The aim of this chapter is to show how my findings from empirical and hermeneutic research can be generalised and what kind of generalisations those may be.

We can and must admit that qualitative research poses problems when it comes to generalising from results. There is no single procedure for doing this, and there ought not to be one. The only general rule is that only careful argumentation, which will naturally be different for each case, can make the step from single case studies to broader patterns. At the same time, we have many reasons to say that qualitative studies can yield convincing and generalisable conclusions exactly because of their deep insights into specific cases.

Maxwell and Chmiel (2014, 540–41) identify three different ways of generalising, which they call (1) analytic-theoretical generalisation, (2) empirical-statistical generalisation and (3) generalisation from transferability. The first and second of these are largely inductive; the third involves making conclusions about more general patterns by transferring insights from one particular case about which there already exists some theorising to another, new case. This is neither strictly deductive nor strictly inductive. There are good reasons not to strictly separate induction and deduction and to acknowledge that they in fact go hand in hand.

Related to ways of generalising are ways of theorising, of which Kelle (2014, 564) identifies three in qualitative research: “1. Using general theoretical concepts to develop grounded middle-range theories, 2. Using qualitative data to challenge existing concepts and 3. transferring middle-range concepts to new research domains”.

I have applied all three types where it made sense to do so. For instance, looking into ‘grand’ theorising like Bourdieu’s on social reproduction and internal exclusion

(Bourdieu and Passeron 1990; Bourdieu and Champagne 1999) or Scott's (1998) on social engineering proved useful for my own theorising about the state's and the people's actions and interactions in the Soviet Arctic (Kelle's first type); challenging the dominant concept of 'community' as an etic designator for 'the' Russian Saami people was possible thanks to the qualitative data I collected in prolonged fieldwork (Kelle's second type); and transferring the concept of cynical knowledge (Goldner, Ritti, and Ference 1977), originally developed on the basis of a study of the American Catholic Church, to an apparently completely different setting revealed inherent systemic commonalities (Kelle's third type).

There is no reason why one should adhere to only one way of generalisation and theorisation throughout one piece of research. For example, in Article 2 I used Bourdieu's and Passeron's (1990) theory of social reproduction not because I found the abstract theory so appealing, and not because I had a hypothesis to test ("let's test their theory on another case"), but because I saw striking similarities between their underlying case (high schools in France) and mine. In Kelle's (2014, 566) words: "Often categories developed for a domain obviously remote to one's own field may prove rather fruitful on a second look". This is generalisation from transferability, according to Maxwell's and Chmiel's classification, but at the same time it happens to be a deductive – or retroductive (see below) – approach that successfully tested an existing theory in a setting other than the one from which the theory emerged.

Another distinction made by Maxwell and Chmiel (2014) is that between internal generalisation, where we generalise within a setting, case, institution or the like, and external generalisation, where we generalise from empirical knowledge or theories outside the studied setting.

An important aspect of *internal generalisation* is to adequately understand and reflect on the diversity in the phenomena of interest (Maxwell and Chmiel 2014, 542). For this reason, while it is impossible to ask every person in the field, we need to consider why this or that interlocutor is important and which other people should be interviewed. In other words, while we are staying in the frame of a qualitative study, it is beneficial to *think* in the frame of sampling when choosing our interlocutors (Miles and Huberman 1984, 36, 41). Building up a diverse set of field partners that adequately reflects the focal social group will probably lead to more fragmented and surprising results. Article 4, for instance, shows the diversity of positions within Saami society, which led me to the conclusion that using the widespread word 'community' is out of place. The discovery of fragmentation and diversity is also a form of generalisation and a basis for theorisation that is deeply peculiar to properly conducted qualitative research. In short, internal generalisation relies mostly on empirical generalisation (number 2, according to the conceptual classification in the beginning of this section), which means to extend the findings in individual cases to larger groups or settings (Maxwell and Chmiel 2014, 546); at the same time, it requires avoiding single claims about society at large in favour of

extrapolating the diversity among the single case studies analysed (informants) to a diversity within a given society or setting. The adequacy of the research findings hence depends a great deal on – as statisticians would say – sampling and selection issues or – as anthropologists would put it – on a long enough presence in the field and thick enough understanding of it.

In contrast to internal generalisation, *external generalisation* relies mostly on analytic generalisation and generalisation from transferability (conceptual categories 1 and 3 presented in the beginning of this section). External generalisation often also leads to the insight that there is more heterogeneity than initially assumed, or than suggested by other research, for example a pure legal or policy analysis. A good example is the case of boarding schools, the implementation of which was very different not only from region to region but even from school to school despite a centralist educational policy (see Article 2).

As already mentioned in the example of Bourdieu and Passeron, generalisation from other settings is not about the one-to-one applicability of an existing theory originally derived from another setting or about the generalisations possibly aspiring to ‘universal’ validity. Rather, generalisation from other settings involves using existing studies “to create heuristics for other studies” (Maxwell and Chmiel 2014, 546). Generalisation from transfer, and the ensuing theorising, does not follow a sampling logic, but a replication logic, concentrating on not only similarities but also differences. This is why it is not appropriate in this context to speak of deduction, which would mean to derive all ‘valid’ insights from a pre-existing theory. Kelle (2014, 561–62) suggests instead the term “retroduction”, which more accurately reflects the deliberate ‘playing’ with existing theories, taking them not at face value but as having possible added value. In addition to my application of Bourdieu’s and Passeron’s theoretical insights, I transferred to ‘my’ setting Goldner’s, Ritti’s and Ference’s (1977) findings on what they term “cynical knowledge”, a notion they base on their research on the Catholic Church in the USA (see Article 2). A set of observations and theories is brought in in order to facilitate “the search and discovery processes when examining other situations” (Eisner 1997, 270).

Generalisation in a qualitative inquiry does not necessarily need to follow the imperative of making the results representative of a larger population. It can be quite the opposite: generalisations may be grounded in “an account of a setting or population that is illuminating as an extreme case or ‘ideal type’, one that highlights processes that are found in less visible form in many other cases” (Maxwell and Chmiel 2014, 546). It is then through generalisation from transferability that such ‘ideal types’ can become plausibly seen as concentrated carriers of rather common patterns that may be present yet remain invisible in less extreme cases. The remedial school in Lovozero (Article 2) is such an extreme case within the vast and diverse landscape of northern boarding schools in the Soviet Union and Russia. Anthropological research with alcoholics undergoing treatment can be cited here as another example:

“An alcoholic is quintessentially guilty and an über-dominated individual.” Yet, “by breaking the code they exercise agency and thus can strategically manipulate the pressure and demands from outside” (Argounova-Low and Sleptsov 2015, 24). The generalisation here is that agency is an inalienable part of every human, even the – supposedly – most downtrodden, victimised and governed individual. In an analogous way, the Catholic Church was an extreme case through which Goldner, Ritti and Ference (1977) highlighted certain institutional patterns present in almost any organisation. In terms of biographical research, revealing extremes can be found in periods of personal or societal ruptures and transitions (see also the paragraph on epiphanies in section 2.2., *Lifeworld*).

To summarise, looking explicitly at an extreme case can contribute a great deal to the understanding of the general (Maxwell and Chmiel 2014, 547). With regard to my research setting, this is valid not only for the particular remedial school case, but also more generally for the entire Kola Peninsula as an extreme case of the Soviet Union’s northern policies: of all regions of the Soviet north, it was the one in which sedentarisation was implemented most extensively (see section 8.0., *Understanding population displacement on the Kola Peninsula as social engineering*).

Theorising about causal processes with the help of the transfer of theories from other settings while elucidating the contextual influences in a novel research setting can lead to specific, new insights. Generalisation from a single case, and generalisation from transfer, are certainly not unproblematic. As Erickson (2012, 687) states, “While certain causal processes may be at work in a local setting, the specific causal mechanisms in operation there may manifest differently in another setting, depending on the local social and cultural ecology of each.” Seeing and understanding such differences in circumstances should not entail the a priori dismissal of possible similarities. Quite the contrary, it can stimulate a search for higher-order similarities, which stand ‘above’ the often more visible differences. There are many examples of similarities of social phenomena between societies that are usually designated as either ‘Western’ or ‘Soviet’/‘post-Soviet’, evoking mainly the historical rift between the competing ideologies of capitalism and communism. However, as the transfer of the insights in Bourdieu and Passeron (1990) and Goldner, Ritti, and Ference (1977) has shown, validity of theories can be found across two apparently disparate societies such as these. We also see many commonalities between boarding schools (Krömer and Allemann 2016) and between resettlement policies (Krupnik and Chlenov 2007, 60) across the circumpolar world, which despite all their particular differences, lead to the ‘higher-order’ conclusion that all circumpolar states had a common attitude towards their indigenous minorities. The common denominator of these modern states, across communist and capitalist ideologies, is what Scott (1998) called a “high-modernist ideology”, that is, a “muscle-bound version of the self-confidence about scientific and technical progress” which originated as a result of unprecedented scientification and industrialisation of human life; what made it

an “ideology” was that it took legitimacy from science and technology, being itself “uncritical, unskeptical and unscientifically optimistic” (Scott 1998, 4–5).

Despite these explanations on *how* to generalise from qualitative data, the question remains of the *plausibility* of such generalisations in qualitative studies with single cases or non-random (in the statistical sense) samples. There is indeed no single answer or method to ensure plausibility. Rather, plausibility grows organically in step with the thickening, ever-finer meshwork of the research participants’ own assessments of generalisability (“We were all afraid of the commission”), the researcher’s contextualisation of such assessments (“How often and in which situations did I hear such assessments? What about contradicting information?”) and the researcher’s validation from other studies. Despite all efforts to ensure accuracy, plausibility remains a ‘soft’ category here. The relativity of plausibility, the conditionality of generalisations, due to the heterogeneity and diversity of experiences, are the price we have to pay for the deep and rewarding insights that qualitative, case-study-oriented research can give us. This price is justified, as it is a reflection and recognition of the complexity of the world. Instead of reducing this complexity for the sake of neatness, I try to accept the messiness – and this includes an acknowledgement that plausible and adequate insights do not, and cannot, aspire to give answers with broad general validity. My own theorising has grown, in a first stage, by applying my phenomenological awareness while taking an ‘internal’ look at my data and, in a second stage, in constant comparison with other existing settings, knowledge and theories while looking at the data ‘externally’. As we have been able to see, in particular “external generalisation overlaps substantially with the development of theory” (Maxwell and Chmiel 2014, 548). Transferability, as described in the section above, is a process of condensing in theoretical generalisation in the sense that each transfer is a filter after which the remaining common similarities lead to a more general theorising that overarches the different settings. In the case of Goldner, Ritti and Ference, my transfer can also be seen as a contribution ‘lifting’ their work, which was rather unpretentiously tied to the particular setting it emerged from, into the ‘higher’ realm of ‘grand’ theories. Conversely, Bourdieu and Passeron, rather immodestly, formulated the theoretical part of their book as a grand theory, even though it is based exclusively on their observations of French schools. In transferring Bourdieu’s and Passeron’s insights to the Soviet setting, my study can retrospectively confirm in large part their aspirations to a grand theory despite its flaws (see section 5.6., *Social reproduction*) and despite this not being one of my goals initially.

4. Credibility in oral history

As we have seen in the previous section, generalisation does not necessarily mean streamlining, unifying or eliminating contradictions. We can achieve our generalisations without denying a sometimes disorienting or even frustrating (for the researcher) heterogeneity. If we accept the widely used metaphor of history as a stream of events, we should not imagine a smooth, channelled stream. Rather, it is more productive to imagine a chaotic delta with numerous branches, some larger, some smaller, some trailing off into nowhere, some merging and some taking new directions.

Increased attention among historians to individual situations – and the ambivalences and multiple meanings in those situations – has its implications for the nature of *representation*. If ambivalences can be laid bare only by linking together a multitude of individual observations, or drawing on disparate sources and historical residua, then it is imperative to examine individual cases and their history. They provide far more than just local color, highlighting history as a process, as a plaiting of strands, a mosaic of (inter)actions. (Lüdtke 1995, 21, original emphasis)

Acknowledging heterogeneity means concentrating on individuals, and this is where listening to individuals lays a credible groundwork for further analysis. In oral history interviews “the operation of a system is reconstructed” from the perspective of individuals, and thus “insights into historical events are made possible that were barred by confining the perspective to more general structures” (Haumann 2006, 50–51). In order to answer how a system is working, we need to ask how individuals are “working the system” (Schubert 2017). This is why the deeply individual-oriented approach of oral and anthropologically influenced history does not stand in contradiction to approaches drawing on questions about the ‘larger’ flow of history and societal developments: If history is a river, everyday history tries first to zoom in, only to discover that the river actually consists of many branches. Anthropologically and phenomenologically influenced oral history jumps into these river branches, trying to adopt the perspective of the fish. One of the benefits of such a vantage point view is the ability to point out “the range of action [*Handlungsfreiheit*] of individuals in a normative social system” (Obertreis 2004, 24). Each system has a general direction of flow, but it offers quiet corners and counter-streams that are able to accommodate a wide array of variation.

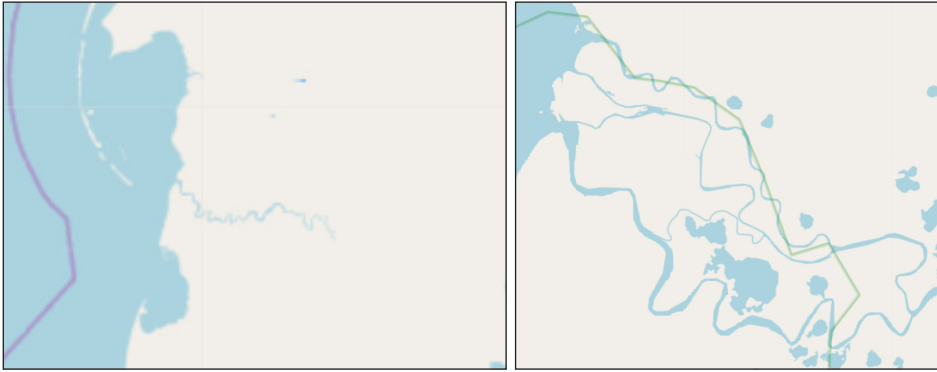


Illustration 1: The river Mordy-Yakha (Yamal Peninsula) exemplifies the metaphor of history as a river: Seen from a large scale, it looks like one big stream. Zooming in we discover that it consists of many branches. Actors, be they fish, reindeer or humans, are confronted with a variety of choices as to how to navigate or cross the river (source: OpenStreetMap.org).

Having different life trajectories, people develop different ways of making sense of their environment and thus deploy very different forms of agency. More structure-oriented or big-men-and-big-deeds ways of studying history, as well as terms like ‘community’ and ‘group’, tend to obscure this heterogeneity, perhaps for the sake of presenting conclusions that look like more convincing generalisations. However, such streamlined generalisations may prove to be teleological, serving, for example, to confirm a habitual and cherished worldview or to achieve political goals. By examining in-depth narrated life stories we can draw generalisations on questions such as non-hegemonic and hegemonic views on the flow of events, hidden mechanisms of power and the reproduction of social groups; the effects that experienced and narrated realities can have on respondents, such as shaping and “stabilising” (Sieder 2012) identities, mechanisms of “grouping” (Brubaker 2002) people, forming collective identities through collective memories; the instrumental use of memory and accordant mechanisms; and the healing effects of storytelling (Rosenthal 2003; Portelli 1991, 19).

Related to the credibility of oral history research is the question of how to *verify* information given in oral history testimonies. It goes without saying that factual information, such as dates, the temporal and spatial order of events, and so on, can and should be cross-checked with other sources, and that this information can prove to be wrong. However, as Portelli puts it in his typical, slightly provocative way:

Oral sources are credible but with a different credibility. The importance of oral testimony may lie not in its adherence to fact, but rather in its departure from it, as imagination, symbolism, and desire emerge [...]. The diversity of oral history [from more classical forms of historiography] consists in the fact that ‘wrong’ statements are

still psychologically 'true' and that this truth may be equally as important as factually reliable accounts. (Portelli 1998, 68, my emphases)

Studying subjectivity as an integral part of history, and not as something that needs to be excluded, does matter because it tells us about the motivations of people and hence about the reasons why they have tried to shape their lives and their environment in a particular way. Thus, we understand why *history* was and is shaped in this or that way. This radically changes the essence of evidence (see section 2.4., *Evidence and truth*), of the type of generalisations that we can make, and of credibility.

A case in point is the factually wrong story of a teacher who vehemently denied the claim by a former pupil that irregularities had occurred when sending indigenous children to the remedial boarding school. The pupil had mentioned these in a publicly presented interview (see Article 3). The irregularities mentioned can count as matters of fact, as they have been confirmed by multiple evidence, for example, interviews with former pupils and the headmaster and, more indirectly, written documents (see Article 2). However, evidently 'wrong' tales tell us very much about people's self-perception, about how they position themselves within their society (often in multiple ways) and about how make sense of the past in very different ways. Put simply: "What informants believe is indeed a historical *fact* (that is, the fact that they believe it)" (Portelli 1998, 67). This kind of fact is of interest if we want to understand how a society and its individuals are shaped by their past and why they shape their past in this or that way in their present narrations.

For this reason, it would be wrong to see oral history testimonies just as another source of facts, a potentially valuable one but one entailing the risk of obtaining facts of reduced reliability. Such an attitude misses the true potential that oral history has; that is, the speaker's subjectivity may become a source of additional facts. As Portelli notes in this regard, "Oral sources tell us not just what people did, but what they wanted to do, what they believed they were doing, and what they now think they did" (Portelli 1998, 67). These particulars are also facts, although they can hardly be cross-checked with, for instance, archival data.

Collectivisation, terror, relocations and their consequences have been well studied in terms of hard data, the range of questions addressed encompassing which minorities have suffered most from Stalin's terror; how many indigenous settlements were closed down and for which reasons; how many people were for and how many against their relocation to a proposed new place in a given village meeting vote; in which cases people were allowed to move their houses at the state's expense, in which cases not; where a cemetery was flooded; which pre-relocation kolkhozes (collective farms) had a Saami majority among their members; what was the non-natural death rate among relocated Saami men; how many Saami women opted for endogamy, how many for exogamy before and after the relocations; and how

many active and passive speakers of each Saami language there are. Indeed, these are important questions that have been answered before this thesis (for example by Scheller 2013; Afanasyeva 2013; Kotljarchuk 2012; Kozlov, Lisitsyn, and Kozlova 2008; Gutsol, Vinogradova, and Samorukova 2007; Vattonena 1989a). However, oral history sources add much other knowledge – knowledge about individual reactions to upheaval, which have usually ended up off the record, that is, written, archived sources; about forms of everyday accommodation and resistance; about the psychological costs of relocations, joblessness, a housing shortage and being in the position of newcomers and an ethnic minority in a new settlement; about conflicts and ruptures emerging from epochal disruptions like the relocations or the end of the Soviet Union; and about how and why different periods are differently framed by people as ‘good’ or ‘bad’ times.

This last point, for instance, has been a crucial basis for understanding societal ruptures among the Russian Saami: there is no monopoly on interpretation as to what ought to be seen as the ‘Dark Age’ and what as the ‘Golden Age’. On the contrary, there are highly competing versions of this determination, and their use can acquire a political and instrumental dimension or be a form of resistance to this dimension (see Article 4). Or, when many of my middle-aged and elder interlocutors use the word “perestroika” (meaning change, restructuring, realignment) for the whole period from 1985 until the end of the 1990s, they merge Gorbachev’s policy officially named *perestroika* (1985-1991) and the subsequent decade under Yeltsin, frequently referred to as the “wild 90s”. This tells us that retrospectively the big rupture turning order into chaos for many people did not come with the official end of the Soviet Union but with the beginning of Gorbachev’s rule. Positive aspects of democratic transformations went largely unappreciated due to the material cost of the changes. ‘Democracy’ as a term is, among many of my interlocutors, notorious and ambivalent, being invariably linked to the chaos of the times when it was introduced in the official political master discourse. This largely explains why Gorbachev’s reforms are, by tendency, emically evaluated in predominantly negative terms and merged into one era with the chaos of the 1990s under Yeltsin, while the etic, foreign view tends to see Gorbachev’s years more positively as an anti-totalitarian reform movement. Respecting and understanding negative evaluations of this time means adding additional facets to apparently known historical facts.

In a similar vein, Obertreis (2015) interprets the emphasis of Estonian respondents on the beautiful sides of life in the Soviet Union as alternatives to what was a rather negative dominant narrative. Such alternative views can be seen in terms of resistance to a hegemonic historiographic-political discourse of sufferance under Soviet occupation. This “right to happiness” (Obertreis 2015, 109) should have a justified standing alongside the right to talk about negative experiences, such as violence or deprivation. While in everyday life it might be more difficult to talk about negative experiences, an established discursive setting for institutions, political systems

or entire societies gone by can cause the opposite effect: those 'finished' stories usually already have a dominant script, one in which sharing negative experiences may become part of the act of liberation, but in which positive experiences may be perceived as undermining this liberation process and thus silenced. Not precluding but allowing and taking seriously also positive memories of the past in states like the GDR or USSR – states that have so often been etically described as plainly totalitarian – is an indicator of a more mature, relaxed and less politicised analysis of the socialist past (Obertreis 2015, 113).

The same kind of reflection may apply to the historical appraisal of other settings, such as the history of boarding schools for native children in North America. Reflecting on the discourses around truth and reconciliation processes, which often culminate in corresponding truth and reconciliation commissions, Thomson states: “Despite good intentions and many positive outcomes, the political compromises required by official truth-telling sometimes marginalise memories that do not fit their conciliatory aims.” (2007, 60). Long-term conciliatory aims are often preceded by political struggles where those who struggle are in need of creating a community with a univocal story in order increase their political strength. Far from taking on the role of truth commission, my research was free of such constraints. The topic of boarding schools, for instance, produced very heterogeneous narratives – in stark contrast to the testimonies about residential schools in Canada collected in the related commission report (Truth and Reconciliation Commission of Canada 2015). Anthropological methods in oral history allow enough time to develop an intimate relationship with one’s interlocutors, which in turn may embolden them to digress from the official ‘script’. Cruikshank comes to a similar conclusion: “An enduring value of informal storytelling is its power to subvert official orthodoxies and to challenge conventional ways of thinking” (1998, xiii). This subversion can go in both directions: it can uncover sufferance where it was previously denied, or it can reveal joy or (self-)empowerment where one would not expect them.

These have been some examples of oral history insights leading to added credibility and an extended understanding of what generalisations we can make, mainly by attributing positive value to subjectivity. Oral history’s understanding of credibility must necessarily be followed by a criticism of

the dominant prejudice which sees factual credibility as a monopoly of written documents. Very often, written documents are only the uncontrolled transmission of unidentified oral sources [...]. The passage from these oral ‘ur-sources’ to the written document is often the result of processes which have no scientific credibility and are frequently heavy with class bias. (Portelli 1998, 68)

Therefore, it would be wrong to see written documents as more ‘objective’ than oral data and to see their function in an oral history inquiry primarily as a

tool for testing the reliability of interview data. Leading oral history theorising commonly denies the supremacy of written sources over oral sources in terms of objectivity. Portelli mentions the records of trials, for example, which in Italy are not based on audio transcriptions or stenography but on the minutes taken by a clerk. I encountered the same situation in my archival study of Soviet-time records of administrative procedures or meetings. The document, which happened to be a good entry point into the topic of relocation, was a record of a meeting of the residents of Kil'din, a Saami settlement, dealing with their imminent relocation to their new village, Chudz'iavr, in 1935. This was the first Saami relocation organised in the Soviet Union. The old village had become unviable because it was located on the railway line and new road from the south to Murmansk in a place that was being rapidly settled and industrialised. According to the minutes, the village council "*decreed [that the villagers were] to vote unanimously* for the building of a new settlement [*pogost*]" (f.242 op.1 d.1 l.7-8, 1935, my emphasis). This opaque sentence signals that the outcome of the vote was decreed from above. It is a telling example showing that we need to approach written documents with a stance no less critical than that taken in the case of oral testimonies. As the record tells us upon a closer look, the inhabitants did not just spontaneously vote unanimously in favour of the new location of their village. It was "decreed" beforehand by the villagers present at the meeting that the vote was going to be unanimous. Moreover, the record reveals that this was done after some people who had opposed the relocation to the particular place had agreed "to withdraw their claims and join the majority". What the document does not disclose, however, are details about the circumstances under which those members of the village gave up their opposition. This is where oral history can play a decisive role. On the other hand, records such as this one could reinforce the credibility of oral accounts on such a conflict. The practice of working on *both* written and oral sources of history can eliminate potential distrust between oral and 'conventional' historians and inherent mutual accusations of bias: it becomes evident that both types of sources have their 'flaws' where credibility is concerned – flaws that are part of the very history that we want to uncover.

We can conclude that oral, anthropologically influenced history offers a credibility of a changed nature, one which starts from the premise that there is no one, single truth. Generalisation in such research is all but streamlining. Detecting a heterogeneity that is messy and difficult to handle is also a valid generalisation, one closer to lived experience. In oral history we see the multiple truths; it is worth exploring what counts as true for different people and why. Convincing explorations of such truths create added value in terms of new aspects of already known chapters of history and thus a surplus of credibility and generalisations.

5. Deliberate eclecticism as a method

There are problems with the common division between methods and theories in scholarly works. It is no more than a pretence, as it does not correspond to the real-life process of scholarly activity. Theories grow out of methods, and theories generate methods. Sometimes a single term can embrace both theories and methods. A good example is “discourse analysis”, which refers to a concept that is neither a uniform methodical school nor a uniform theory, but a pot in which a wealth of theorising and methods melt together and inform each other. In the following sections I will present the theoretical-methodical mix that, combined together in a deliberately eclectic manner, has had a great influence on my research. Before that, I will outline in detail the fundamental premises of what I call *deliberate eclecticism*, connecting it with what I have written above about credibility, generalisation, theorisation, and phenomenology.

Husserl’s and Schütz’s fundamental work on the lifeworld serves as a constant background, a pre-giveness to put it phenomenologically, upon which I have built the empirical research on my case. My empirical research relies on an array of different sources, methods and theoretical insights that are in part directly inspired by phenomenology, and in part not. There is plenty of social sciences research with a (claimed) strong adherence to a certain method or theory. Potential insights are often precluded by adhering closely to a limited and excessively strict methodological and theoretical horizon. Roland Barthes put this into trenchant words:

Some people speak of method greedily, demandingly; what they want in work is method; to them it never seems rigorous enough, formal enough. Method becomes a law [...]. The invariable fact is that a work which constantly proclaims its will-to-method is ultimately sterile: everything has been put into the method, nothing remains for the writing; [...] [there is] no surer way to kill a piece of research and send it to the great scrap heap of abandoned projects than Method. (Barthes 1986, 318)

Conversely, methodological – and also theoretical – openness, less sterile and less formal, keeps research alive by remaining open to surprising turns in the course of the research process. Certainly, openness does not mean “anything goes” but rather “being intellectually rigorous without succumbing to the rigor mortis of oversystematization” (Wolcott 1994, 176). Blumer (1969, 169) saw intellectual prestige at play: “I suspect that this steady production of new concepts arises from the effort to pose as scientific and to be judged as profound and learned”, a pressure

coming from the social acceptance of precision as the ultimate criterion of science. In general, “bricolage” of methods and theories is typical of research that has broken with positivist research traditions (Winter 2014, 249). Mayring (2002, 133) urges scholars to overcome separative, dogmatic thinking about different approaches and move towards an integrative attitude. In other words, a true orientation towards the topic is best served by a deliberately selective methodological and theoretical pluralism, not monism.

Phenomenology – as already clarified – is neither a method nor a theory. It is an attitude, and it is “more a method of questioning than answering” (Van Manen 2014, 27). Phenomenology is a form of reflexivity and “reflexivity is not a method but a way of thinking, a critical ethos” (May and Perry 2014, 111). Phenomenological awareness, as a fundamental scholarly attitude, thus invites one to stay open to a wide array of inputs and insights from existing theorising and research while interpreting data. I call this openness *deliberate eclecticism* – ‘eclecticism’ because I try to stay open to as many insights and inputs as possible, ‘deliberate’ because I reflect on the relation of those inputs and insights to the phenomenological foundations of my research. The phenomenological approach is the start. The phenomena made visible through this attitude are then further interpreted, with the help of existing concepts and theories. I do not consciously avoid theoretical frames, unlike a pure phenomenologist would.

Oral history is not only deliberately, but often also explicitly, eclectic. As Thomson has put it, “Oral historians began to use an exhilarating array of approaches [...] in their analysis and use of oral history interviews” (2007, 54). Among other approaches, he names those used in qualitative sociology, anthropology, biographical and literary studies, life review psychology, cultural studies, linguistics, communication and narrative studies, and folklore studies (2007, 63). Neither do anthropologists hesitate to use “such eclectic a selection of sources of inspiration” (Schubert 2017, 15). For instance, Konstantinov’s (2015) book on the rural part of the Kola Peninsula generated new and valuable insights – the results of which I refer to throughout this thesis – by using a combination of very heterogeneous approaches ranging from participant observation to structuralist linguistics.

Trying to force data into pre-conceived hypotheses often does not work in qualitative research. An example from my own research serves to exemplify this. When I embarked for my first period of fieldwork in Lovozero in 2008 for my master’s thesis, I arrived with a clear preconception of ‘the’ Saami as oppressed people. Re-listening to my first interviews today, I notice repeated attempts on my part to ask questions like: “Can you also remember instances when you were oppressed because you were a Saami?” While this example shows that there was still a lot of room for improvement in my interviewing techniques, it also indicates too stubborn a quest for oppression, trying, at times, to fit answers into the unquestioned preconception that people must have a strong perception of themselves as oppressed victims.

I suggest that reliable ways to avoid wanting data to fit preconceptions in qualitative research are phenomenological awareness, as described in the previous sections, and openness to analytic inspiration. It is indeed exciting “to see one’s empirical material coalesce in an unexpected or new way” (Gubrium and Holstein 2014, 37). This occurs in the present case, for example, when I see new, unexpected matches with existing theories I was not aware of and did not plan to use initially. Another instance is when it dawns on me that looking for a certain pattern was informed by a discourse which was limiting my perspective; here, I am thinking of the widespread discourses of victimisation in the literature that seeks to decolonise the history of minorities but in fact perpetuates disempowerment by staying blind to their agency (Chappell 1995).

In practice analytic inspiration often means that in the process of observation my questions gradually shift from what-and-why questions to how-questions (Gubrium and Holstein 2014, 43–47). For instance, when I started noticing the above-average Saami representation at the remedial school for mentally disabled children (Article 2), I initially asked “What is going on here? Why did so many Saami children go this school?” The more I looked into it, the more informed were the questions that I asked, examples being “How are boarding schools, relocations and social power relations in the host settlement interconnected?” or “How were the proceedings and decisions for transferring a child to the remedial school organised?”. This was analytic inspiration through fieldwork and it eventually provided answers to the what- and why-questions. Underpinning this approach are the ways in which questions in oral history interviews are asked. Here, too, I tried to ask how-questions rather than what- and why-questions. ‘Hows’ produce richer answers, both in informal or formal conversation and in a research situation.

Denzin (2014, 570) sees the differences between quantitative, positivist, “traditionalist”, on the one hand, and qualitative, non-positivist, “experimentalist” researchers, on the other, as “two poles on a continuum, a right pole and a left pole”. Traditionalists have an objectivist stance on methods and “focus their teaching on questions of design, technique, and analysis”. Experimentalists “take a more ‘avant-garde’ activist view of method, analysis and pedagogy.” They see a subjective, interpretive approach as legitimate, and they hope their methods will translate into social action. However, one should keep in mind that these are *conceptual poles* at either end of a *continuum*, on which my research is located toward the left, experimentalist side.

Left-pole research is a reflexive, flexible, at times messy, and possibly performative, poetic and political bricolage, where researchers need a broad set of skills in their methodological toolkit. According to Denzin (2014, 572), one form of such research is “ethnography that uses historical texts and the voices from the past to tell its stories.” As so often in writing by scholars who are not strictly anthropologists, ‘ethnographic’ has to be understood here as largely synonymous with ‘anthropological’; that is, it is

not mere description, but thoroughly reflective and speculative thinking about the conditions of human life in the world. The approach is akin to philosophising, but is one that is grounded in the world, in conversation with its diverse inhabitants (Ingold 2017). Thus, only a bricolage of sources, methods and theories, placed side by side, provides a spectrum of tools broad enough to understand the ambiguous messages that we take home from our messy, unstructured anthropological fieldwork. The polysemy of the field cannot be streamlined and simplified into a sleek study. It will necessarily need to be reflected in complex, multiple epistemologies, acknowledging that “one can never determine the meaning of a story, something that applies to the practice of both informants and researchers” (Leete 2018, 6). Most clearly, I have tried to show this multiplicity and flexibility of both narrator and scholar in Article 4, although it is present in all the interview-related parts of this thesis.

If phenomenological awareness best explains my approach to ‘real life’ as the basis of rather inductive theorising and interpretation, then deliberate eclecticism best explains my approach to the more deductive parts of the research process. It is a thoughtful, dialogic, methodical and theoretical eclecticism, one that moves away from scholarly monologue and determinism.

5.1. Grounded theory

Despite its somewhat misleading name, grounded theory (henceforth in this section: GT) is not in itself a theory but a method and attitude geared to generating theories. If followed strictly, it is a rather concise and narrow method. My doctoral research has not not applied GT strictly but has drawn significant analytical inspiration from it. A basic feature of GT is that data gathering and analysis take place in parallel. While arguably even in the most ‘scientific’, explicitly deductive way of working, new ideas and concepts can be born in phases of the research when they are not supposed to be born, GT made the intimate and permanent relationship between data gathering and data analysis transparent and thus legitimised the unplanned emergence of new ideas at any stage of the research.

Original GT by Glaser and Strauss (1967) and later Glaserian GT (1992) are rooted in a taken-for-granted vocabulary of positivism: they offer a rather utilitarian view of data as ‘speaking for itself’. Contrasting with this initial approach is the constructivist GT of Strauss and Corbin (2014) and Charmaz (2006), which acknowledges more the constructive and performative aspects of informants’ and researchers’ actions and their mutual interaction. One could speak of a more anthropologist way of practising GT.

Abduction – and, for that matter, its derivative concept of retroduction, which I discussed above (see chapter 3, *Generalisation and theorisation from qualitative data*) – plays a critical role in GT. For Aktinson et al. (2003, 149), abduction is “a way

of capturing the dialectical shuttling between the domain of observations and the domains of ideas.” It is a moving back and forth between data, pre-existing theories and developing theoretical thinking. Where there is pre-existing knowledge, this does not mean that I try to force the data into it but to use this diverse knowledge “as multiple possible lenses” (Thornberg and Charmaz 2014, 163). I use elements from constructivist GT, which – in contrast to classic GT’s approach of delaying literature reviews until the very end of the research for the sake of remaining ‘unbiased’ – avoids “the risk of reinventing the wheel, missing well-known aspects” and uses extant theories in a “non-forcing and data-sensitive way” (Thornberg and Charmaz 2014, 163). In short, constructivist GT offers a convincing rationale in favour of a healthy eclecticism, which relies on “previous knowledge, rejection of dogmatic beliefs and development of open-mindedness” (Thornberg and Charmaz 2014, 163).

Basic features of constructive GT as a post-positivist and phenomenological interpretive style that I espouse are “a naturalistic approach to ethnography and interpretation, stressing naturalistic observations, open-ended interviewing, the sensitizing use of concepts, and a grounded (inductive) approach to theorising, which can be both formal and substantive.” However, GT is also close to positivist understandings of science in the sense that, if followed strictly, “it provides a set of clearly defined steps any researcher can follow. It answers to a need to attach the qualitative research project to the ‘good science’ model” (both quotations Denzin 2014, 575).

In this sense, the ‘good science’ part of my research, as borrowed from GT, lies in the systematic way I have used to generate insights grounded in my field materials, that is, the process of coding. Some understand coding as something more mechanical, which is done prior to interpreting and theorising, but I favour the view that these processes go hand in hand. Thinking about the codes, changing and expanding them, and writing notes to accompany them (so-called memos), all this is already the analytic process in full swing. While it is not the whole analysis, coding is already a part of the interpreting and theorising process (Gibbs 2014, 283–85). With regard to the stages of coding, suffice it here to mention the rough categorisation between initial coding, focused coding and theoretical coding; I will not go into detail here about the procedural steps (for those see for example Thornberg and Charmaz 2014; Miles, Huberman, and Saldana 2013). For technical details on how the materials of this thesis were coded, see section 6.3.3., *Transcription, coding, translation*).

5.2. Discourse analysis and cultural studies

Denzin (2009, 29) suggests that “critical interpretive discourse must be launched at the level of the media and the ideological”, a level of analysis which I tried to remain mindful of throughout this thesis. Accordingly, several approaches, which

can be subsumed under the terms “discourse analysis” (henceforth in this section: DA) and “cultural studies” (henceforth in this section: CS), played an important role throughout my research.

CS, along with gender studies, has been a major multidisciplinary conceptual framework in the past forty-fifty years. In CS, culture is not regarded as a subsystem or a field but as an overarching element in all human interaction: “It penetrates and structures every aspect of social life and of subjectivity. [...] It is the medium by which shared meanings, rituals, social communities and identities are produced”. A CS perspective sees the task of every qualitative researcher as the “empirical examination of the relationship between experiences, practices and cultural texts in a specific context” (Winter 2014, 248). A fundamental feature of CS is that it has broken with any form of positivistic agenda, seeing in the research process a bricolage of various research methods and theories, such as participant observation, narrative or biographical interviews, analysis of newspapers, and the use of field notes. Since the beginnings of CS, resistance was a basic concept, with Gramsci’s analysis of hegemony and Foucault’s analytics of modern power being two main sources of inspiration. CS remains interested in the marginalised and the silenced, “the underclass and subjugated”, and it wants to give space to social transformation leading even as far as utopia (Winter 2014, 250). Despite cautioning against seeing oppression without accommodation, or seeing agency only as resistance (see section 5.4., *Agency*), I still espouse this basic characteristic of CS.

Part of the cultural turn against overly structuralist – and hence simplifying – analysis was the linguistic turn, which brought an interest in and awareness of discourses as a significant social shaping power. The epistemological position of discourse analysis (hereafter: DA) is constructionist and relativist. DA approaches made it clear that expressions, thoughts, feelings and experiences are not just the result of what is going on inside a person but also what is going on around him or her. The basic assumption is that in the human world everything comes into being through dialogue, and this is due to another basic assumption, that language does not merely reflect reality but creates it (Farnell and Graham 2015, 391–92). There are many different varieties of DA, but common to all “discourse-centered methods” (Farnell and Graham 2015) is an understanding of language as constructive and performative, as having the power to shape action. Two prominent scholarly works that took this stance as their point of departure to explore Russian/Soviet realities are Ries (1997) and Yurchak (2006). In this thesis, this stance comes to the fore mostly in Article 4 but has a general importance throughout my research for how I look at oral testimonies. DA is good at understanding how actors use and generate discursive resources and with what effects, but one rarely sees ‘pure’ DA used, as it is not very good at telling us what motivates actors to do what they do (Willig 2014a, 340–42). For this, we need to combine DA with other methods, which I did, making DA thus one part of my deliberate eclecticism.

The principal objectives of DA are “to discover the social meanings inhering in language forms and their relationship to social formations, identity, relations of power, beliefs, and ideologies” (Farnell and Graham 2015, 393). DA approaches seek to “dissolve the long-standing dilemma [...] of how to connect social structure and/or culture with human agency” (Farnell and Graham 2015, 393), which has been dichotomously seen by prominent scholars as structure and practice (Bourdieu 1977) or as structure and agency (Giddens 1995, see also section 3.3.4., *Agency*). Through this, DA approaches allow the researcher to understand more precisely how mechanisms of power and authority work (see especially Article 4, but also Article 2 and section 8.5.8., *Blaming the displaced people*). Through DA approaches, scholarly theorising about person, self and agency shifted from ethnocentric, individualistic psychologisms toward sociocultural dimensions of communication. Meaning is not created in internal and private mental structures but during “the dialogic, interactional processes within which meanings and identities are constructed” (Farnell and Graham 2015, 395). This seems nowadays to be a well-established stance in the social sciences, but it is noteworthy here because it is an important reason why DA approaches are especially well placed to contribute to the exploration of lifeworlds (see section 2.2., *Lifeworld*). Moreover, an awareness of these fundamental differences between individualising and socialising approaches to understanding people (in different disciplines and at different times) meant that I kept in mind that individualising approaches to the self were a hegemonic ‘pre-postmodern’ scientific tenet during those times and among those people at the focus of my historical analysis. This awareness contributed to my understanding the legitimising scientific origins of what I identified as individualisation of negative strategies (mostly in the form of medicalisation or scapegoating) and its use as a tool whereby representatives of state authority exercised power and denied responsibility for the negative outcomes of social engineering (see mostly Article 2, and section 8.5.8., *Blaming the displaced people*). What is more, as Davies (2017a) and Skultans (2007) argue, individualising approaches, stripped of social context, still seem to be widespread in psychiatry today.

Particularly influential in the long run for the linguistic turn and the subsequent discourse-oriented ways of seeing things were the Russian formalists and, after them, the Prague School linguists, centred around Roman Jakobson. Both emphasised the multifunctionality of language. Konstantinov (2015) and Yurchak (2006) used their linguistic work on speech categories as important tools for analysing discursive practices and, following from that, power negotiations and social arrangements in the Soviet context. Yurchak differentiated

between the “constative” meaning in discourse (using words or other signs to state facts and describe reality) and its “performative” meaning (using words to achieve actions in the world). Constative acts describe reality and can be true or false; performative acts

do not describe anything and cannot be true or false. They can only be successful or unsuccessful in achieving something. (Yurchak 2006, 285)

What Yurchak calls “constative” speech is called “substantive” speech by Konstantinov and “referential function” by Jakobson. Yurchak’s “performative speech” is called “poetic” by Konstantinov, following Jakobson (Konstantinov 2015, 75–83; Jakobson 1960). Being aware of and properly understanding the performative dimension of speech is a prerequisite for an adequate analysis of both oral history testimonies and public speech. It is here that Konstantinov’s and Yurchak’s work has been seminal for all my data analysis in this thesis. Most explicitly, I address these categories of discourse in Article 4.

Literary theoretician Mikhail Bakhtin can well be regarded as one of the first twentieth-century forerunners in the use of DA approaches, with his awareness of the socially conditioned diversity of speech types and his introducing terms such as polyphony and heteroglossia as opposing concepts to authoritative discourse and monoglossia (Bakhtin 1981; 1984; A. Robinson 2011). He did this in the apparently apolitical context of historical literary studies but when his works were discovered, their applicability to contemporary social and political context was patent. Bakhtin was a pioneer in explicitly stressing that in order to assess and properly understand somebody’s words we must not only know precisely *who* is speaking but also

under *what* concrete circumstances [...]. In everyday life, we do not separate discourse from the personality speaking it (as we can in the ideological realm), because the personality is so materially present to us. (Bakhtin 1981 [1934-35], 340–41).

Interpreting someone’s speech by making this distinction is what Gerbel and Sieder (1988) called “scholarly interpretation” as opposed to “everyday interpretation”. It is DA – the scholarly analysis of utterances – that started analysing speech by distinguishing discourses and individuals systematically: one person can follow and reinforce, influence and be influenced by different discourses depending on the factors present in a given speech situation (the audience, the place, etc.). Here, under the guise of literary analysis of former times, Bakhtin becomes very political. He asserts that a person’s consciousness always assimilates to the surrounding ideological worlds to certain degrees, noting “The ideological becoming of a human being, in this view, is the process of selectively assimilating the words of others,” and, moreover, that the tendency to assimilate others’ discourses is a fundamental part of an individual’s ideological becoming (Bakhtin 1981 [1934-35], 341–42).

Another source of inspiration for DA comes from sociology’s approaches that try to see “the negotiated and emergent quality of meanings” (Willig 2014a, 397), such as Goffman’s microsociology. Davies’ and Harré’s (1990) concept of positioning

is also based on DA approaches. I discuss these scholars' work in the next section (3.3.3., *Face, lines, position*).

Discourses fulfil instrumental functions, shaping and at the same time being shaped by people's motivations, interests and actions. In this vein, I highlight here one contemporary scholarly application of DA that influenced me in Article 4 in particular: Brubaker's (2002; 2004) concept of groupism. "Group" is one of the most basic categories in social sciences and, as a consequence, groups are often taken for granted. Groupism, then, is the tendency to group people together, to reify groups as entities as if they had homogeneous interests and agency (e.g. "the Blacks"). Taking groups for granted and as static is especially widespread and dangerous in the study of ethnicity, race and nationhood. Using a DA approach, Brubaker unravels and de-mystifies such groups. Ethnicity, race and nation should be conceptualised not as substances, things or entities but as relational, processual and dynamic. The basic analytical category is not the fixed 'group' but fluctuating 'groupness'.

Of course, Brubaker is not the only scholar using today's widespread constructivist stances and even theories in social sciences. However, it seems that in practice many scholars are tempted by groupism (e.g., those studying the struggles of ethnic or other groups) or consciously opt for it, such as Junkka-Aikio (2016), who condemns de-constructivist approaches as hampering the indigenous decolonisation agenda. By contrast, Brubaker maintains that scholars – if they want to remain scholars – should avoid mixing categories of ethno-political practice with categories of social analysis. Furthermore – and this is arguably one of the most threatening aspects to pro-activist and pro-primordialist scholars – the anti-groupist approach can help to detect the presence of *intra*-ethnic mechanisms (e.g., rifts, internal 'policing' and silencing, sanctioning processes, calculated instigation of conflict with outsiders and insiders) in shaping interethnic relations (see Valkonen, Valkonen, and Koivurova 2017).

Groupism should not be ignored by scholars: it can be part of the empirical data we acquire, but it would be wrong to let it be part of our analytical toolkit. Thus, ethnic primordialism is something we can analyse but not something we can analyse *with*. Such primordialism is, for example, reflected in words like "ethnic conflict"; it would be more precise to understand such conflicts as ethnicised or ethnically framed, prompting the question of who the actors are that are interested in propagating or following this frame. The reification of groups is a social-discursive process. It would be a "category mistake" (Brubaker 2002, 166) to criticise ethnopolitically active people for reifying their ethnic groups, as it is an instrument they use consciously for the achievement of a particular goal. As such, it is legitimate to accept this social process and, as scholars, to analyse it, but we should not reinforce such grouping and at the same time pretend to remain within the realm of academic scholarship.

Finally, I would like to briefly discuss the interrelation between hegemonic discourses and collective memory. Leinarte (2016, 14–15) claims that silence or

incoherent narratives among her oral history informants on a given topic can be explained by the absence of a public discourse on that topic, this being the case in Italy with the country's colonial past or in post-Soviet countries with traumatic experiences of everyday life. According to Leinarte, collective memory (Halbwachs 1992 [1925]) can come into being only on something that is publicly discussed. While I would not subscribe to this, this would seem to show that collective memory is not a very useful concept per se. We should look not so much for collective memory as for hegemonic discourses. Once we identify them, we will find a heterogeneity of memories ranging from more readily produced "front-stage memories" conditioned by those hegemonic discourses to less visible "backstage memories" beyond or behind hegemonic discourses (Stammler, Ivanova, and Sidorova 2017). The main requirement for accessing such memories beyond what appear to be diffuse bits and pieces is achieving cultural intimacy through long-term field commitment (see Article 3). Or, as Farnell and Graham (2015, 402) put it: "There is no single best method of collecting information on language and ideologies within a community. The starting point, though, is to seek, or discover, through participant observation, those forms of discourse that will form the data for analysis."

5.3. Face, Lines, Position

There are only different selves, different performances, different ways of being a gendered person in a social situation. These performances are based on different narrative and interpretive practices. These practices give the self and the person a sense of grounding, or narrative coherence. There is no inner, or deep, self that is accessed by the interview or narrative method [or for that matter, by participant observation, L.A.]. There are only different interpretive (and performative) versions of who the person is. At this level, to borrow from Garfinkel, there is nothing under the skull that matters. (Denzin 2009, 222)

What Norman Denzin puts so poignantly here, and what is so important to keep in mind consistently at any stage of doing oral history, has been theorised before him in great detail. The pioneering work in this respect includes that of Erving Goffman, whose microsociology was influenced by phenomenology. His notion of *face* is a helpful way to understand more clearly the ways in which different positive sense-making strategies (see section 2.3., *Creation of meanings*) can evolve in different situations, for instance in the course of an oral history interview. "The term *face* may be defined as the positive social value a person effectively claims for himself" (Goffman 1955, 222), whereby it is taken for granted that a 'normally functioning', not socially deviant personality is driven in his or her general behaviour by the wish to make "a good showing" (1955, 222). There are a large number of different drivers

for following a given social code and the implied “face-work”, such as self-esteem, pride, honour and power (on how these have affected the selection and the answers of interviewees in this research, see section 6.3., *Interviews*). While the weight, significance and shape of such drivers vary across societies and groups, Goffman claims a certain pan-human generality for his findings: “Societies everywhere, if they are to be societies, must mobilize their members as self-regulating participants in social encounters” (1955, 245). People everywhere, according to Goffman, are “taught to be perceptive, to have feelings attached to self and a self expressed through face” (1955, 245–46). “Face”, in this understanding, is a universal feature of human interaction: one can be “in face”, “in wrong face”, “out of face”, one can “lose” or “save” one’s face. “Following Chinese usage”, one can “give face”, which is “to arrange for another to take a better line than he might otherwise have been able to take.” The other “gets” face, so that he or she can “gain” face. Under ‘normal circumstances’ – a digression from those circumstances could be a conflict or a pathological disorder – a person is disinclined to his or her own or another person’s “defacement” (1955, 223–26). Related to oral history interviewing, this implies the following:

By entering a situation in which he is given a face to maintain, a person takes on the responsibility of standing guard over the flow of events as they pass before him. He must ensure that a particular expressive order is sustained – an order that regulates the flow of events, large or small, so that anything that appears to be expressed by them will be consistent with his face. (Goffman 1955, 224)

While “face-saving actions [...] often become habitual practices” (1955, 226), all in all, the term ‘face’ here should be understood neither too categorically nor too narrowly. We see in Goffman’s metaphorical use of ‘face’ its wider connotations related to a chosen line, to how a person positions him- or herself in a given situation. This line can be inconsistent during an interview; different lines or positions can appear in the course of a series of interviews or in the presence of different persons.

Constructing one’s self in a certain way in a certain situation is driven by a desire to follow the social conventions of keeping one’s ‘face’. These conventions can include, for instance, notions of self-esteem, pride, honour and power. The ‘higher’ the level of those indicators associated with a person by others, and the higher a person’s ambitions to keep or maximise those indicators, the more likely the person is to be concerned about showing a consistent face and thus offering more ‘streamlined’ narratives of his or her life. The more face there is at stake, the more face (or a mask) is likely to be constructed. In non-structured, narrative, collaborative biographical interviews, which formed the main source of data of this thesis, this has been a crucial insight, influencing not only how interviews have been analysed but also how they have been conducted. As Goffman observed, in ‘normal’ interaction, where the rules of face-work are followed, “The line taken by each participant is usually allowed

to prevail, and each participant is allowed to carry off the role he appears to have chosen for himself.” (1955, 225) In an interview situation, this means that I would not too harshly contradict a respondent or loudly state my own thoughts about the interlocutor’s statements; such actions might compromise the line chosen by the person, whereby I would risk destroying the current process of meaning-making in the narrative. This process should not be disturbed while ongoing because it is the main driver of the narrative flow that the oral historian tries to trigger. Goffman called this kind of giving way in a conversation “a ‘working’ acceptance, not a ‘real’ one,” since it is not necessarily based on full, candid and heartfelt agreement with all statements but “temporary lip service to judgements with which the participants do not really agree” (1955, 226). Thus, if I, as the interviewer, personally disagree with or would like to criticise a chosen line, I will step back. However, it is a mandatory part of any sound interpretation to make an effort to understand and discuss chosen lines, or positionings. Phenomenological awareness stands at the beginning of such processes of understanding, which have been further developed by approaches employing discourse and narrative analysis.

To continue with Goffman, “avoidance processes” (1955, 228–30) in oral history fieldwork can also be linked to “face”. Interlocutors or potential interlocutors may avoid contact, certain topics or common activities that are likely to threaten their chosen line towards me as a researcher. In interviews, this is most likely to happen by regulating and, where necessary, trying to change the direction of the conversation. In an interview a person can, despite first attempts of avoidance, find him- or herself “caught out of face” if, for example, strong feelings have disrupted a chosen line and the accordant expressive mask. In such a case, I, as interviewer, can “give face” by holding on or turning away and giving the person time to gather him- or herself, as suggested by Goffman (1955, 230). Depending on the situation, one can then go ahead and address the strong feelings.

Related to face-work is one’s positioning (B. Davies and Harré 1990). These authors mention that a dialogical mismatch is usually due to different positionings of the interlocutors. In a fictional example by the authors, such a miscommunication stems from one speaker’s “normative expectation that the poor both need and accept care” (1990, 58) and the other speaker’s deviance from this premise. In an interview, this could mean that I, as interviewer, may see my interviewee as belonging to ‘the victims’ and assume he or she is in need of help, whereas the interviewee does not perceive him- or herself either as a victim or as in need of help. As a rule, such dialogical mismatches result from a lack of contextual knowledge, which the researcher can only gain from prolonged socialisation in the field. Related to this is the “dialogic impasse” (Konstantinov 2015, 71) that I have elaborated on in Article 4.

5.4. Agency

Concentrating on lifeworlds implies acknowledging the agency of individuals, and it allows the scholar to explore this agency more closely. In the past, agency has been conceptualised and theorised in many different ways, and I will limit myself here to a few thoughts about how I understand and use the concept. Husserl (1970 [1936], 6) called the human-in-the-lifeworld “a free, selfdetermining being in his behavior toward the human and extrahuman surrounding world.” The main objection to this statement one could imagine today is that practices both reproduce *and* transform the very socio-cultural frames that shape them (see Ahearn 2001). In Husserl’s time, however, this was a novel scholarly gaze at humans in society, one that took decades still to achieve its strong standing in social sciences.

Alf Lüdtke and Hans Medick, the German historians who coined the term *Alltagsgeschichte* (everyday-life history) in the 1980s, were among those who worked with and developed the concept of agency most consistently. For everyday-life historians and subsequent oral historians, looking at agency became a key to understanding the links between concrete, down-to-earth individual experiences and broad socio-economic and political changes:

“*Alltagsgeschichte* – conceived as the history of everyday behaviour and experience – does not try to raise fundamental secular change to a level detached from human agents, occurring behind their backs, as it were. Rather, historical change and continuity are understood as the outcome of action by concrete groups and individuals. Human social practice is shifted into the foreground of historical inquiry. [...] The thrust here is to demonstrate how social impositions or stimuli are perceived and processed as interests and needs, anxieties and hopes” (Lüdtke 1995, 6–7).

However, the question remains of how we define agency. The interest of historians in a ‘history from below’ was inspired by anthropology, where agency – both individual and collective – had long been considered. James Scott (1985; 1989) introduced an understanding of agency mainly as a broad understanding of resistance, one including all forms of foot-dragging and subtle non-conformism. However, later scholarship criticised resistance studies as having a one-sided bias and as being imprecise and overused (for instance, Theodossopoulos 2014).

Not only resistance (however broadly defined), but also accommodation, collaboration, adaptation or collusion should be seen as instances of agency, since they are based on – socially conditioned – individual choices. Sherry Ortner (1995) offered one of the most compelling pieces of scholarship on this topic, calling blindness to this wide array of possible agencies “ethnographic refusal”. In her research, she shows how the old binary of oppression versus resistance has been increasingly refined in the wake of Foucault’s focus on less institutionalised and

more pervasive everyday forms of power and Scott's focus on less institutionalised and more pervasive everyday forms of resistance. These advances have complicated the question about what is and what is not power and what is resistance to it. Nevertheless, Ortner agrees that resistance is a "reasonably useful category [...] because it highlights the presence and play of power in most forms of relationship and activity. Moreover, *we are not required to decide once and for all whether any given act fits into a fixed box called resistance*. As Marx well knew, the intentionalities of actors evolve through praxis" (1995, 175, my emphasis). More recently, Ortner (2016) has continued defending resistance as a useful concept. I agree with her on this if we define resistance as something that *together* with accommodation leads to multi-layered forms of adaptations to circumstances.

Thus far I have tried to show why we should use a widened understanding of resistance and why we should nevertheless refrain from framing agency only in terms of resistance. Chappell (1995) proposed dispelling another widespread dichotomy: passive victim versus active oppressor. Based on his analysis, he claimed that being a victim is more often associated with passivity than with agency, which thereby remains a domain of the oppressors. As Chappell noted, "Victims need not be passive, nor the passive weak, nor actors free agents" (Chappell 1995, 315). Passive victim/active oppressor dichotomies are indeed untenable. They are untenable because in the exercise of power the opposition between victim and oppressor is often transcended; and they are untenable because behind the seeming passivity of victims there is less-visible 'small' agency as forms of resistance, accommodation, resilience or collusion. It is the anthropological depth of a historical study that can show this. This is a core idea underpinning all of the articles in this thesis; I discuss it in detail in Article 1.

A fine-grained and ambivalent understanding of agency also acknowledges that agency must be neither fully conscious nor effective. It emerges *nolens volens* through engagement with the lifeworld, and the most prominent means of this engagement is discourse, for language is social action, as "meanings are co-constructed by participants, emergent from particular social interactions" (Ahearn 2001, 111). Attending closely to linguistic structures and practices is one possible way to get away from static models of agency towards fluidity. Storytelling as agency, and ways of storytelling as conditioned by discursive settings, figures centrally throughout the thesis. Viewing agency in an interrelation with discourses (both conditioned by and shaping them) takes us away from a focus on agency as purely individual. It leads us away in two directions: to the supra-individual level and sub-individual agency. This insight prompts a number of questions: Can agency be exerted by families, communities, unions or whatever assemblages of people position themselves as a group? Conversely, can different agencies be ascribed to "dividuals" (Ahearn 2001, 112), when someone has different roles or feels torn apart? I reflect upon these questions in all four articles.

All these considerations mirror a shift away from impersonal, Foucauldian or Bourdieuan master narratives to more detailed and personal investigations. Such approaches leave more “room for tensions, contradictions, or oppositional actions on the part of individuals and collectivities” (Ahearn 2001, 110) and thus acknowledge that agency is possible not only by Husserl’s idealised and isolated, “free, selfdetermining being”, but also under the normal condition of being limited in myriad ways by “available opportunity structures” (Fischer 2014, 6). These encompass socio-economic relations, including formal and informal social norms, as well as the whole range of institutional and legal factors. Fischer thus sees agency as born within the tension between the *will* and the *way*. Oral history should take both into account.

5.5. Social engineering and displacement

James Scott (1998), a well-known scholar in anthropology, established an understanding of social engineering (henceforth in this section: SE) having a strong negative connotation with its megalomaniac, high-modernist state projects. Together with Scott, I apply this concept to the social transformations in the North that led to the mass displacement of people, with numerous indigenous communities affected. Before delving more into Scott’s work, however, it will be useful to outline the work on SE by the Polish-Canadian sociologist Adam Podgórecki and his followers, who, starting from the 1970s, managed to establish a scholarly dialogue about SE that spanned the Iron Curtain and was less condemnatory than Scott’s analyses. This group of scholars developed the discipline of sociotechnics, whose task is to analyse and unmask SE stratagems but also to give recommendations about SE independently of the widespread negative connotations of the term (Podgórecki 1996, 55). This implies that even if the outcomes of SE have indeed often been negative, they need not necessarily be so.

According to sociotechnics, SE can take place on not only a macro, but also a meso scale. “Social engineering means arranging and channelling environmental and social forces to create a high probability that effective social action will occur. The word engineering suggests the designing and erecting of structures and processes in which human beings serve as raw material” (Alexander and Schmidt 1996, 1). The macro-scale level is what is most commonly considered SE. At this level, entire nation-states operate in their attempts to reorganise societies, and this is the level Scott (1998) looks at. It captures many of the features of Soviet social reorganisation on the Kola Peninsula (see section 8.0, *Understanding population displacement on the Kola Peninsula as social engineering*). However, there is also meso-scale SE, which includes community struggles, grassroots tactics, single-interest social movements, and individuals or organisations pursuing specific social changes, examples being

corporate advertising strategies or the battles of indigenous peoples (Alexander and Schmidt 1996, 3). The snowmobile revolution in the Western hemisphere of the Arctic (Pelto 1973) can be seen as an example of corporate SE in which a meso-scale SE campaign burgeoned into a full-fledged macro-scale transformation. The meso level applies also to the ethno-political discursive strategies described in Article 4.

I will now go through the points that Scott makes about SE that seem to me to be most pertinent to indigenous displacement on the Kola Peninsula and elsewhere in the Soviet North. While he is very negative in his conclusions on the outcomes of most large-scale SE projects, throughout his book he correctly emphasises that such ventures are often well-intended development and welfare projects. This contrast between benign, albeit neo-colonial, goals and catastrophic outcomes certainly pertains to the post-Stalinist social engineering that led to the final sedentarisation of indigenous populations. Scott identified generic “family resemblances” of such high-modernist projects by developmentalist states: 1. A logic of ‘improvement’ through simplification, which should make the delivery of services more easily feasible (schools, hospitals etc. concentrated in one place); 2. Under the guise of the same key term, improvement, eco- and socio-structures are meant to be made more legible and manageable to the state through bureaucratic appropriation (people and property become more easily countable); 3. Also important for the planners is the aesthetic dimension as a representation of order and efficiency created by the state (in the Soviet Union mostly in the form of square, symmetrically placed and permanent apartment blocks); 4. Failures of such SE projects stem mainly from the fact that local knowledge and practices as well as local responses and cooperation are ignored, and they are ignored based on convictions about the rightfulness of a high-modernist ideology encompassing the belief in “scientific agriculture’s” superiority over local ways, which are not codified in any ‘scientific’ form (Scott 1998, 224–25).

Furthermore, Scott (1998, 187) observed that “non-state spaces” are more prone to artificial and harsh organising intervention by the state using SE. These tend to be “frontier” spaces, remote in distance or elevation, with dispersed and often migratory populations and relatively impenetrable landscapes. The Kola Peninsula – and the Soviet North in general – offers a perfect example of such a remote frontier and its transformation into state spaces; the process has been ubiquitous in the twentieth century and often traumatic for the local population.

Four elements, variously combined, can make state-initiated social engineering disastrous (Scott 1998, 4–5). All of them apply to the Kola Peninsula, as can be easily seen from the empirical findings presented in Chapter 8 and the thesis articles:

1. The administrative ordering of nature and society, with the goal to create legibility in areas that were previously poorly accessible by state authority. SE is often presented by the centre of power as ‘bringing civilisation’ to the people concerned. Scott, however, prefers to see it as an attempt to domesticate and to make a place and its people legible to the state. Relating to this point, important keywords are

“sedentarisation” and “concentration” and “radical simplification” (Scott 1998, 184), which apply unequivocally to the Soviet policies analysed in this thesis.

2. A high-modernist ideology, as a “muscle-bound version of the self-confidence about scientific and technical progress” (Scott 1998, 4–5) which originated in the West as a result of unprecedented scientification and industrialisation of human life; this technocracy can be called an ideology because it took legitimacy from science and technology, being itself “uncritical, unskeptical and unscientifically optimistic” (Scott 1998, 4–5), reflecting in many ways the dreams and desires of its conceivers and followers;

3. An authoritarian state that has the determination to implement the envisaged plans;

4. A civil society that is not strong enough to prevent such plans (however, post-factum resistance is very much possible and even essential in order to keep the new system running).

With these four elements present, SE in rural areas of the world has been marked by the “nearly unshakable faith in the superiority of monoculture over polyculture” (Scott 1998, 262). In the case of the Kola Peninsula, this faith, originating from thinkers whose offices and experiences were located in temperate climate zones, was often applied without much question to very different climatic conditions. For example, SE in the region concentrated almost exclusively on reindeer husbandry at the cost of virtually eliminating hunting and fishing, at least as official activities. In order to keep the front stage of monoculture functioning, polyculture has to persist in a marginalised back stage form. Ingold’s view criticising the typically Western human-animal divide is a plea for balanced polyculture and against streamlined, simplified and technocratic monoculture:

The farmer’s work on the fields, for example, created favourable conditions for the growth of crop plants, and the herdsman does the same for domestic animals. Moreover, granted that not all producers are human, it is easy to turn the argument around and show how various non-humans contribute, in specific environments, not just to their own growth and development but also to that of human beings. It follows that human social life is not cut out on a separate plane from the rest of nature but is part and parcel of what is going on throughout the organic world. It is the process wherein living beings of all kinds, in what they do, constitute each other’s conditions of existence. (Ingold 2011, 8)

The present thesis is an analysis of past events and their consequences, and I do not endeavour to formulate any policy recommendations for the future. Scott’s work is built up in a similar way, but at the very end of the book he briefly suggests “a few rules of thumb that [...] could make development planning less prone to disaster”. I quote them here as they are part of his theorising on SE: 1. “Take small

steps”; 2. “Favor reversibility”: prefer interventions that can easily be undone If they turn out to be mistakes; 3. “Plan on surprises”; 4. “Plan on human inventiveness”: Always plan under the assumption that the objects of SE will bring in their existing or forthcoming experience and insight to improve the design (1998, 345). I will now briefly turn towards two more SE theories which helped to shape my view on the events on the Kola Peninsula, although I did not engage with them in particular depth.

Scudder (2009) developed a theory of involuntary displacement, in which he conceptually divides the process of development-induced resettlement into four stages: recruitment, transition, potential development and handing over/ incorporation. However, in practice in many cases the process stops at the stage of transition, becoming thus a perpetual transition where people remain trapped due to poor planning and implementation. This is well reflected in a popular Russian saying: “There’s nothing more permanent than the temporary” (*net nichego bolee postoiannogo, chem vremennoe*). On the Kola Peninsula, the poor housing situation after the relocations should be seen in this light (see section 8.5.3., *Housing shortage*). Even where the four stages have formally been gone through and formally fulfilled, as in the case of the Skolt Saami (Mazzullo 2017), multiple dimensions of stress more often than not remain intact. One of the basic reasons why resettlement is often poorly conducted is that it is ‘only’ a by-product of some other development project, which means that it is a priori not at the centre of attention of those responsible for planning the development initiative as a whole. In the case of the Kola Peninsula, as in many other places in the Soviet North, the displacement of people was a ‘by-product’ of a range of priorities, with these including the hunt for ‘enemies’ and ‘kulaks’, collectivisation and sedentarisation, the development of industry, the needs of the military, and rural economic rationalisation. These were the primary goals, those mattered to the leadership, which held a prime position in statistics and for which mid-tier bureaucrats were held responsible.

Finally, Downing and Garcia-Downing (2009) have discussed the insufficient attention paid to psycho-socio-cultural impoverishment through involuntary displacement. The authors identify five fallacies of state-induced displacement that give those in power the opportunity to avoid responsibility: 1. The compensation-is-enough fallacy: monetary payments as a one-for-all compensation, supposedly covering not only economic but also moral and other non-material obligations; 2. the strict compliance fallacy: demonstrating formal adherence to all plans, policies and plans formulated prior to the resettlement leads to blindness and feeling of irresponsibility for unexpected issues; 3. (following from 2) the blame-the-victims fallacy, which assumes that the displaced population is unable to appreciate and take advantage of the new opportunities offered to them; 4. (following from 2 as well) the clock-stops-with-construction fallacy, asserting that responsibilities towards relocatees end with the completion of the primary development project, of which

the relocation is only a side-product; 5. the someone-else-should-pay fallacy, which holds that those who were responsible for designing and implementing the primary development project cannot be legally or economically liable for negative psycho-socio-cultural consequences. As we will see especially in the course of Chapter 8, all these points pertain in one way or another to displacement on the Kola Peninsula.

5.6. Social reproduction

Bourdieu and Passeron's (1990) "Reproduction in Education, Society and Culture" and Bourdieu and Champagne's (1999) "Outcasts on the inside" provide empirically grounded theorising that has been important in this thesis. Their research helps in understanding the mechanisms of and reasons for the reproduction of social inequalities, in educational systems specifically, and in societies more broadly. I will keep this section short, as Article 2 takes these works up in detail.

Exclusion within a system – especially a nominally highly inclusive social system like socialism in its idealised form – is more stigmatising than a priori exclusion from the system, because those who are excluded were apparently given 'their chance'. This means that failure can be more easily attributed to individual deficiency instead of collective deficiency or societal problems while maintaining the façade of a society of equals. This is a crucial element of the mechanisms which I subsume under the term *individualisation of the negative* (see section 5.8.). The works of Bourdieu, Passeron and Champagne show how educational exclusion makes a central but concealed contribution to consolidating a given social order. It was therefore especially eye-opening to apply their ideas to a comprehensive schooling system such as the one in the Soviet Union. The concept of reproduction works against vague ideas of the demise of class, whether this decline be an outcome of the idealistic belief of the dynamism of the American society, as criticised in Bourdieu and Passeron's study (1990, ix), or the declared classlessness of socialist societies, as shown in Article 2.

There is important criticism of Bourdieu's social theorising as lacking precisely the diversity that each real social setting has; that is, by introducing terms such as symbolic violence, habitus, exclusion and social reproduction, Bourdieu and his co-authors largely ignore the diversity of agency – in the ways explained in section 5.4., *Agency* – for the sake of boiling down all their observations to processes of reproduction of hegemonic structures (Strauss 1992; Ahearn 2001). There is indeed a risk of applying such theories in a streamlined way. Mindful of this, I have try to emphasise in this thesis that for many dislocated people social mobility was possible in both directions, thus defying notions of a rigid structure of perpetual social reproduction. I see, however, that Bourdieu and his co-authors also present such caveats. They are often overlooked by scholars who focus on the Bourdieuan grand theories without a close reading of the empirical parts of Bourdieu's work.

The basic message, however, remains: people tend to be shaped, limited and constrained by the habitus. The reason for this in turn is that humans are predisposed to think and act in a manner that reflects their previous experiences of the social order they were embedded in. As any social order is an order of inequalities, these inequalities are most likely to be perpetuated by the habitus of people. The problem is that Bourdieu and his co-authors do not explain that social reproduction is 'flawed' in that dynamics operate which subvert the perpetual flow of reproduction, eventually translating into social transformation. However, they do not preclude such transformation. The solution to the dilemma does not lie in rejecting Bourdieuan theories, but in refraining from falling into an excessively determinist exegesis of these theories; after all we should not forget that they are also grounded in ethnographic fieldwork. Thus, notions of power relations, habitus and social reproduction remain dominant, but I do not see them as impermeable structures (cf. Ahearn 2001; Ortner 1995; Humphrey 1994).

5.7. Cynical knowledge

Cynical knowledge is a concept introduced by Goldner, Ritti, and Ference (1977), who wrote a little-known analytic gem on the ways in which knowledge was distributed in the American Catholic Church and how this translated into power. The insights can be applied to any institution with some hierarchical organisation. In this thesis, I use the concept mostly in Article 2. Cynical knowledge, usually withheld by the knowing few, can eventually trigger dynamics of unintended force when released for whatever reason. For instance, this can be pressure from the outside induced by a discursive turn. Through incentives from the top, the reforms of *perestroika* unleashed dynamics similar to the ones described by Goldner, Ritti, and Ference in the case of the Catholic Church. In both contexts, in response to an urge to open a vent for a critical amount of accumulated knowledge on the previously concealed (albeit often common knowledge) and unmentionable downsides of a system (this is what the authors call "cynical knowledge"), the top tier of the hierarchy decided to introduce some changes that would allow people to discuss these downsides. However, the unleashed amounts of previously withheld cynical knowledge led to a discursive and political change far more radical than initially intended. These discursive and political changes in the end contributed to far-reaching changes in the social and political organisation of the entire country (in the case of Goldner's, Ritti's, and Ference's study, of the entire Catholic Church).

It is important to understand how these dynamics function. Cynical knowledge and increases in it may be both the cause and effect of radical changes. Independently of each other, and using different terminologies, Goldner, Ritti and Ference (1977), and Yurchak (2006) explained these amazingly similar processes of opening up to their

constituents in the Catholic Church and the Soviet Communist Party respectively. Cynical knowledge stops being withheld and begins to be shared publicly by those possessing it when the positive consequences for them of doing so start outweighing the negative ones. Whether one considers a priest in the American Catholic Church or an indigenous boarding school teacher in Lovozero, their cynical knowledge about the system they were working and living in had little effect for them in the past, because the immutability of the system and lifetime permanence of the individual in it was taken for granted. As Goldner, Ritti, and Ference point out, due to certain constraints, there were no viable alternatives to one's chosen career path:

The lifetime permanence of the priest's vocation prevented cynical knowledge from having the effect it now has [after the reforms of Vatican Council II] [...]. [After the reforms,] the priest – with viable alternatives to his present calling – can now afford to take seriously the cynical knowledge that comes from widely published and open sources. (Goldner, Ritti, and Ference 1977, 550)

We can see the same dynamics in the biographies of many who later became indigenous activists in Lovozero and had a background as a teacher, social worker or member of the statistics bureau staff. Thus, Liubov' Vatonena, an indigenous employee of the district's statistics bureau, was all of a sudden in a position strong enough to publicly decry catastrophic housing conditions or suicide rates that she had knowledge of long since (Vatonena 1988). And Aleksandra Antonova, an indigenous teacher remembered to this day as an ardent implementer of Russification, unexpectedly started pointing at the loss of indigenous languages and at assimilatory practices in boarding schools (Antonova 1996 [1988]).

After reforms, making cynical knowledge public is no longer a threat to one's own standing: people in positions of power have fewer possibilities or motivations to retaliate using the customary mechanisms such as stigmatising exposure of 'heretics'. The arguments for retaliation will have already lost their public credibility due to public access to the previously withheld cynical knowledge. Goldner, Ritti and Ference exemplify this point as follows:

Much of that cynical knowledge is generated by activities that take place in organized settings which provide an opportunity for extended discussion [such as newspapers, L.A.]. [...] [As a consequence,] we find a shift in explanations for the failure of commitment on the part of priests [or, in Lovozero, the social failure of relocated parents and children, L.A.] from those positing individual weakness to those pointing to flaws in institutional structure. (Goldner, Ritti, and Ference 1977, 550)

This process is relevant in the present case because the social problems I describe in this thesis (mainly Chapter 8 and the thesis articles) were the sources of the

cynical knowledge which laid the groundwork for engaging at a later point in the new discursive forms offered by *perestroika* (discussed mainly in Article 4).

5.8. Individualisation of the negative

Throughout the thesis – but especially in Articles 1 and 2 as well as in section 8.5.8., *Blaming the displaced people* – I discuss one very common type of cynical knowledge: the tendency to conceal social issues by diverting public attention from their true reasons towards selected individuals suitable to fulfil the roles of outcasts and scapegoats. I call this widespread phenomenon *individualisation of the negative*. Max Weber (discussed in Bourdieu 1971), Michel Foucault (discussed in Kharkhordin 1999) and René Girard (1986) have deliberated such forms of individualisation, but space precludes my indulging in a close reading of their original works here. With regard to the Soviet Union in particular, the stratagem of individualising social issues has been examined mostly in relation to Stalinism, for instance, by Bauer (1952), Conquest (1967), Madison (1968), Fitzpatrick (1993) and Halfin (2003). This is understandable, as Stalinist individualisation had the most brutal and noticeable consequences: decade-long labour camp or death sentences left a big scar on the entire Soviet society. In Article 1 of this thesis I address a gap in this otherwise much-researched area of Soviet history by looking into under-researched aspects of grassroots responses.

However, individualising negative social issues did not end with Stalinism. Kharkhordin (1999) was the first to posit that quite the contrary was the case. He identifies the pattern “reveal-admonish-excommunicate” as a constant feature of the state (and the Church) in both pre-revolutionary Russia and in the Soviet Union. He demonstrates that this pattern became more pervasive over the course of the Soviet period, becoming even more extensive in mature Soviet times than under Stalin: “Chaotic and sporadic fierce terror against some was replaced by milder but more extensive everyday bullying of all” (1999, 289). This development can be observed when comparing the Stalinist blaming of selected individuals on the grounds of invented charges (Article 1) with the more subtle but pervasive individualisation in post-relocation Lovozero; cases can be cited relating to housing issues (section 8.5.3), blaming socially deviant adults (section 8.5.8.) and applying special education policies predominantly to children of the individuals who were the target of individualisation (Article 2). Kharkhordin sees in the mutual surveillance within the *kollektiv* (the basic unit of peers in any organisation) an inversion of Foucault’s panopticon as a model of Western societies: while in Foucault’s model all are watched invisibly by one, in Soviet society the individual is watched visibly by all. This mutual surveillance forms public opinions around an individual rather than on an issue, thus venting collective distress in the form of wrath or scorn towards

scapegoats. Kharkhordin calls this mutual surveillance “the ever-present rock bottom that one reaches upon dismantling mountains of power” (1999, 110). In Kharkhordin’s view, if that core melts, all pyramids of power built on it melt, and he proposes this as a way to see the end of the Soviet Union. This perception is similar to Yurchak’s (2006) discourse analysis explaining the USSR’s collapse.

Kharkhordin’s study includes a cross-cultural comparison between ‘East’ and ‘West’, with two main hypotheses resulting from it: firstly, in the Soviet Union, organised horizontal surveillance by peers at work and in leisure time had a standing as strong as vertical surveillance, more so than in Western societies; secondly, the related penitential practices (blaming and self-blaming in public) were more widespread in the Russia/Soviet Union than in the West, which was (and still is) marked more by private confessional practices. The author locates both dimensions in a historical context rooted in how different Christian traditions – and, following from this, legal traditions – evolved over the centuries: “The farther back into Russian history we go, the more stress on publicizing aspects of *oblichenie* [public revelation of sins] we encounter” (1999, 220).

Other important sources of inspiration and knowledge on this topic have been Argounova-Low’s (2007) use of the scapegoating concept in her study of Soviet practices of accusing individuals of being nationalists, and Khlinovskaya Rockhill’s (2010) illuminating book on individualising approaches in the early-post Soviet context in which single mothers tended to be held responsible by the legal system for the socio-economically conditioned material hardships they encountered.

6. Collecting and organising materials

Today, in anthropology at least, it is obvious that new ideas, theories and concepts emerge already during the process of data gathering and analysis. I described above that we can acknowledge grounded theory (see section 5.1., *Grounded theory*) for explicitly doing away with the idealised separation of hypothesis formation, data gathering, data analysis, theory testing and theory building. In line with grounded theory, and with the general anthropological practice, I do not draw a strict line between the gathering of data ‘in the field’ and their analysis ‘in the armchair’. While my reflections about analysis in the previous sections apply to my work in both the field and armchair, in this section I will concentrate on a number of practical and theoretical points of fieldwork. In my research, I have combined several sources of data deriving from fieldwork in Russian Sápmi: narrative biographical interviews (documented through audio recordings and transcripts); participant observation (documented through field notes); and archival documents and newspapers (documented through excerpts and scans). I will explain here more about how I obtained these data and what inspirations and difficulties I had during this process.

The fieldwork for my research took place in the cities of Murmansk, Apatity and Kirovsk, in the villages of Lovozero, Revda, Loparskaia, Verkhnetulomsk and Umba, and in the tundra east of Lovozero in reindeer camp number eight of the Lovozero reindeer herding cooperative and the adjacent Ketkozero area. Additionally, I undertook one trip to another region of Russia to see one interviewee (the person wanted the place not to be disclosed). Here it is important to point out that the indigenous population of Russian Sápmi lives scattered over all of the Murmansk Region. There is a common tendency by outside visitors to limit their visits to Lovozero, where the largest number, yet less than half of all Kola Saami live. Especially when it comes to the history of displacement, it is important to cover a large number of settlements (for more on this see Article 4).



Illustration 2: The urban side of fieldwork. From left to right, top to bottom: Residential blocks in Apatity, Murmansk and Lovozero, 2003-2015; Tuloma, at a Saami festival, 2014; Lovozero, at a festivity in the ethnic culture centre, 2013; Apatity, at the former, so-called prophylactic labour-therapy camp (lechebno-trudovoi profilaktorii, LTP), a facility for alcohol addicts having a hybrid penitential-medical function, 2013. Top right image by Roland de Roo, centre right image by Julia Allemann, all other images by the author.



Illustration 3: On the rural side of fieldwork. From left to right, top to bottom: On Lake Lovozero; on the way back from the tundra, hitchhiking with ATVs from the ‘sovkhos’ Tundra of Lovozero; ATV breakdown with a part being forged back into its required shape using the heat from a campfire; tanning a hide at home in Lovozero; tea-time in the “hyper-gendered” (Konstantinov 2018) world of garages on the outskirts of Lovozero; two apprentices at one of ‘sovkhos’ Tundra’s camps making summer use of a snowmobile for transporting water from the lake to the hut. All images by the author, 2013-2014.

6.1. Overview of the collected materials

For this thesis I collected a total of over ninety hours of recorded biographical interviews, mostly but not exclusively with people of Saami (often mixed) origin. The interviews took place in the settlements mentioned in the previous section and lasted between one and seven hours, the most common duration being two to three hours. All interviews were annotated with metadata within the ORHELIA project’s

database and about ninety per cent of them were fully transcribed. I analysed a significant part of the transcribed data by coding them according to the method outlined in section 5.1., *Grounded theory*.

A second important part of the fieldwork was the month I spent gathering data at the State Archive of the Murmansk Region in Kirovsk and Murmansk. During that period, I collected and analysed documents authored mainly by the Lovozero District Executive Committee (*räiispolkom*) and its sub-committees; the Murmansk Region Department of People's Education (*OblONO*); the local collective farms (kolkhozes, later sovkhozes); the local village councils (*sel'sovet*). In addition, I undertook a systematic screening of all issues of the local newspaper *Lovozerskaia Pravda* from 1950 until 2010 in the Murmansk State Regional Library, looking for articles on the topics of relocation, housing, alcohol abuse, boarding schools, reindeer herding and relations between Saami and Komi.

The entire fieldwork phase was also an ongoing process of participant observation, experiences of and notes on which form a third corpus of data. In the following sections, I will look into these three kinds of data in more detail.

6.2. Participant observation and long-term field commitment

Phenomenology is not just a way of interpreting data. It begins before and continues while the data are being consolidated. In the stage of fieldwork, during data gathering and data creation, I strongly rely on my phenomenological awareness. It influences how I view interaction, ask questions to people and first look at documents in archival research. The backbone of my data has been generated in long, recorded conversations, but the participant observation that such conversations were embedded in is an inseparable part of those data. This entails a refusal of dichotomising conceptualisations of interviewing versus participant observation. Engaged and participant listening, during repeated and many hours-long conversations, is an essential part of participant observation (Forsey 2010). The aural component of observation is at least as important as the visual one. Through longer and recurring stays on the Kola Peninsula I built up the needed rapport and immersion, and thus the needed pre-conditions, for posing meaningful questions, understanding answers, and being understood. This “part-time socialisation” (Dudeck 2013b, 64) entails approaching and accessing the lifeworld of my interlocutors, maintaining this relationship and building up trust. Trusting relationships directly influence the depth and thus quality of the information, as one's own and one's partner's emotions and reactions become research instruments (Verdery 2018, 291). Conversely, without trust, the observer is afforded no more than superficial glances by the observed (Marvasti 2014, 357; more on part-time socialisation in the field see Articles 3 and 4).

In anthropological fieldwork, gathering and thinking about the data go on simultaneously and inform each other. As Hastrup (2004, 466–67) puts it, this approach does away with “the dualism of thought and action.” Understanding them holistically – and not as separate entities – forms “the basis for the self-evidence of incorporated ‘local’ knowledge”, and this is a prerequisite for “the skill at posing meaningful questions.” Participant observation and unstructured conversations – which, whenever recorded, we tend to call interviews – help in combination to develop an awareness that some categories that a researcher uses on a daily basis do not necessarily match people’s perceptions, memories and utterances (cf. Forbes and Stammler 2009). Initial assumptions and categories of the scholar about his or her interviewees and the research topic can dissolve in a high number of narrative and interpretational patterns. It has been noted by oral historians that this can result in “de-typification shock [Enttypisierungsschock]” (Niethammer 2012, 58), which can provoke in the researcher feelings of disorientation and frustration during intense interviewing ‘campaigns’ on short visits. Only long-term fieldwork and participant observation can turn such experiences into epistemological added value.

A popular assumption is that a cultural insider must have a greater, more intimate and shared understanding and knowledge than an outsider. Ardener’s comment below can be understood in this vein:

For the non-native anthropologist the act of interacting with an alien social space, even relatively successfully, forms the basis of that ‘daily experience of *mis*understanding’ [...] which is the undoubted source of our greater readiness to see the space as object (of study), and thus, like Durkheim, to see social facts as ‘things’. To treat the social space as object is almost literally child’s play, when it is located in unfamiliar scenes and is already, in any case, predefined as ‘other’ in relation to our own world. (Ardener 1987, 39, original emphasis)

Ardener sees in this form of interaction an ultimately phenomenological approach to the world. “Thing” should be understood here in the sense of the already discussed phenomenological vocabulary, which was also implied in Durkheim’s sociological use of the term ‘things’ (Belvedere 2015). While alluding to the risks of ‘othering’, Ardener sees in the outsider’s perspective, at the same time, one important advantage: A reductive gaze, in a phenomenological sense (“back to the things themselves”, see section 2.1., *Reduction*), can arguably be achieved more easily from an etic viewpoint because the effort of “de-familiarising” (*ostranenie* in Russian, Shklovsky 1965) things is reduced due to the a priori otherness of a non-group-member. An outsider in this context should be understood as a person who learned about the field partners’ lifeworld through a conscious, ‘phenomeno-logical’ learning effort. Conversely, an insider would be someone who shares major parts of the field partners’ lifeworld due to similar lived experience and local or ancestral background. However, a shared

lifeworld does not necessarily bring an advantage when it comes to understanding that lifeworld: “Universalising from shared experiences” (Ilic 2016, 6) can carry the risk of ‘home blindness’ with regard to shared and taken-for-granted discourses. Sometimes it is exactly the “skilled outsider” (Ilic 2016, 6) who can bring the ability and willingness to question established truths.

While it is common sense to think that an insider will be told certain things that are not shared with outsiders, the opposite is also often the case: the skilled and committed outsider is likely to hear about certain things that will be avoided in a talk among insiders. The outsider may be perceived as free from in-group social entanglements and prejudices and may thus become a welcome audience to whom an interlocutor can pour out pent-up experiences. This can be either connected to the – at times – comforting feeling that these stories will be carried away and not linger in the community or, conversely, to a desire to bring up these stories in one’s community with the help of an ally from the outside. Both cases are only possible if sufficient trust has been built up over a long enough time. An example is the history of the remedial boarding school in Lovozero (see Articles 2 and 3). Due to the shame and the stigma connected to being officially labelled “retarded” (*debil* in Russian), one of my field partners had left her village in her youth, as many other former pupils had as well. When she returned after a long period elsewhere, she avoided talking to anybody about this school experience, not mentioning it even to her husband or her children. According to Rosenthal (2003, 925–26), the reluctance of traumatised people to speak should not only be seen as a direct consequence of trauma. It also stems from the field partners’ respect towards a perceived need of the surrounding people not to have to deal with the pain involved in being confronted with disturbing experiences. The appearance of an external, unencumbered person can help to trigger talk because such worries do not apply. In the case of my main interviewee about the remedial school, the conversation triggered not only an account of previously suppressed memories, but also a desire to make them public in her village and in publications.

On the other hand, lacking insider status may lead to overlooking certain considerations, misunderstanding situations, or being barred access to certain spheres of life, hidden activities or secret topics. Discussing oral history interview situations, Rosenthal distinguishes between two types of accounts of traumatic events. The first type is when the perpetrator is publicly known and acknowledged (like the Shoah or Stalin’s terror years), and therefore there are no feelings of guilt or shame by the victims. The second type involves experiences of, for instance, physical or psychological childhood violence, which are accompanied by shame and stigma. In this latter case, there is a much higher risk that an inadequately prepared re-exposure to those memories may have harmful psychological consequences (Rosenthal 2003, 916). Here I see the potential danger of situations in which the outsider researcher could take it for granted that a traumatic experience falls under the first type, whereas

for the narrator it is of the second type. Returning to the example of the remedial school, especially for outsiders from the ‘Western’ North there is a risk of taking for granted that boarding school experiences prompt the first type of account because in the Nordic countries, and even more so in North America, discussions about boarding schools have long since gone public, culminating in the establishment of governmental truth and reconciliation commissions. This development may lead to wrong assumptions by the researcher that there is a shared and open understanding in the host society about the boarding school being an oppressive perpetrator and traumatic experience. However, there is no common understanding about this in the Russian North. Accordingly, a victim’s experiences are more isolated and sooner reflect Rosenthal’s second type of traumatic experience, which must be approached slowly and tactfully. For the insider this might be more evident than for the outsider, who will need to find this out by investing enough time and effort to explore the field site before prying into topics that may prove to be more sensitive than initially assumed.

To sum up, there are no grounds for claiming that either insiders or outsiders are by default in a more or in a less privileged situation in terms of accessing and understanding the people in the field. Both can be granted or denied ‘special’ access to some spheres of knowledge or memory, and both can be blind to certain things, or simply understand them in differing ways. Furthermore, for both outsiders and insiders it can be a difficult balancing act to adequately accommodate the agency of people and at the same time not minimise the destruction and sufferance which were a result of a power imbalance (Berg-Nordlie 2017, 16–17). This is especially the case when it comes to research on groups of people perceived as colonised or oppressed at some point in history, as questions of empowerment and disempowerment and of agency and oppression can be – whether we like it or not – tied to delicate political issues.

To return to the details of how I conducted the research, the fieldwork consisted of several trips lasting about three to four weeks and several shorter trips. Overall, the time spent ‘in the field’ was around six to eight months. It is not possible to precisely calculate the duration of the fieldwork proper because it was interspersed with periods of analysis and writing. As I used Lovozero and Murmansk as bases for writing retreats, I had occasion to meet and talk with my field partners again. Analysis and fieldwork thus continuously informed each other. In this regard the present study differs from a ‘classical’ ethnography, which is often based on one or two major field trips. My approach of recurring and rather spontaneous trips was also possible thanks to the proximity of my research institution to the field. Being located in Lapland, the Arctic Centre is not a faraway ivory tower, but a place at the crossroads of the academic and political discussions of Sápmi. Doing research at the Centre on the Saami in Russia creates a rather unique situation in that not only do I visit my field partners at their home, but some of them visit me at my

home, for example, on their way to Sápmi-wide workshops, conferences, meetings or other events. This, of course, does not apply in the case of all my field partners but those who are in one way or another involved in ethno-politics or cultural-linguistic revivalism (about the divide between ‘activists’ and ‘non-activists’ among the Russian Saami see Article 4). Other field partners visiting me at my home include those who travel to the Nordic countries as labour migrants or due to family connections.

6.3. Interviews

6.3.1. Finding interviewees

During my fieldwork in Russian Sápmi I met many interviewees, most of whom at least partly identify as Saami. However, being Saami was not a criterion which I used or would have used to limit my search for interviewees. Such an approach, as practiced for example by Afanasyeva (2019, 90), would form an unnecessary epistemological and ideological barrier, one that does not correspond to the reality of mixed and multiple identities and the multi-ethnic past and present of Russian Sápmi.

For fieldwork among potential oral history interviewees it is important to develop a feeling for where interlocutors stand within their community in terms of power, honour, pride, esteem by others and self-esteem. Such a feeling can only be developed in long-term fieldwork, as described in the previous section. Usually the people who become visible first to a visitor are those who stand higher within the community in terms of the above indicators, such as local political leaders, cultural activists or teachers. In order to complete the picture, however, we need to look also for the less ‘visible’ people, usually those with a lower social standing in the community. While the snowball system is the right approach to make one’s way, one must allow enough time to reach out to those people who are less often recommended by others. More than once, I heard the phrase “Oh, she’ll have nothing interesting to tell you”. On the other hand, even well-known people with a high social standing require longer and repeated approaching: due to their known “front-stage” positions (Stammler, Ivanova, and Sidorova 2017), it usually takes more time before they agree to show a more nuanced line, to reveal multiple positionings and to be less concerned about their ‘face’ in a Goffmanian sense (see section 5.3., *Face, lines, position*). More insights about good practices in interviewee selection are given in Article 4, p. 15–16.

A word about the gender composition of my sample of interviewees is in order here. I conducted proper, recorded narrative biographical interviews with 33 women and 11 men, which corresponds to precisely 75 per cent women and 25 per cent men. For the age category I focused on – as a rule of thumb, people after or shortly before retirement age – this seeming gender imbalance is representative. According to the 2010 census of the Russian Federation, among the Saami population aged

55 years and over, there are 472 women per 100 men, reflecting the huge difference in life expectancy between men and women precisely for the generation that experienced Soviet-time displacement when they were children or young adults (for more analysis and sources on this topic see section 8.5.6., *Violent death and substance abuse*). It was indeed a common situation among my female interviewees that all of their male siblings had already passed away, often in tragic ways.

In addition to the demographic factor noted above, there is another reason for the preponderance of female interviewees in my study. While a commitment to participant observation meant including the non-verbal aspects of human interaction, narrative forms of data gathering dominated this study. Clearly, it was easier to find talkative people among women than among men also because of marked differences in imported Soviet gender roles. Conversational skills have been promoted as especially desirable in typically 'female' educational paths and professions. I discuss this consideration in more detail in section 8.5.5., *Gender split and erosion of family structures*.

In informal, unrecorded situations, there was a higher proportion of men. On the one hand, this is because such situations occurred more readily with younger and middle-aged people, where the demographic gender imbalance is much less pronounced; on the other hand, such situations, where recording would be inappropriate or technically difficult, occurred more often outside of the village, in the tundra or at a lake, where typically more men are present.

6.3.2. Conducting the biographical-narrative interview

Hand in hand with the bricolage of methods outlined above comes the narrative collage of the oral history interview, which is not at all an interview in the classic sense. As Denzin describes it, "No longer does the writer-as-interviewer hide behind the question-answer format, the apparatuses of the interview machine" (2009, 223).

Biographical-narrative conversations are always interventions. They therefore need proper training and a sound method on the part of the researcher. This is necessary both to achieve good research results and due to the researcher's ethical responsibilities towards the research partner. I will therefore present some deliberations about how such an interview should be conducted and how questions should be asked. It is usually the researcher who is the first to propose that there be an interview. The interview thus is a priori a more or less unexpected intervention, with several effects on the interviewee. Certain distortions are unavoidable in the sense that interviewees might want to tell researchers "what they believe they want to be told" (Portelli 1998, 71). The interview – or simply conversation – should be structured as little as possible. As Portelli observes:

Rigidly structured interviews may exclude elements whose existence or relevance were previously unknown to the interviewer and not contemplated in the question schedule.

Such interviews tend to confirm the historian's previous frame of reference. The first requirement, therefore, is that the researcher 'accepts' the informant, and gives priority to what she or he wishes to tell, rather than what the researcher wants to hear, saving any unanswered questions for later or for another interview. (Portelli 1998, 71)

Such non-structured, non-directive, narrative and collaborative interviews require long conversations of several hours. The interviews consist of a first part, in which I interrupt the interviewee as little as possible. This is the main narration. The interviewees are encouraged to develop the recollections and structure the narration according to criteria they find relevant themselves. A process of self-understanding takes place already at this stage; indeed, an attentive and encouraging (but not interrupting) listener is already a great and rarely available motivator for initiating self-understanding. In this sense, narrative-biographical interviews can have a liberating and thus curative effect (Rosenthal 2003, 925). The main narration is followed by a second part, in which I ask more specific questions. This second part consists, on the one hand, of questions deriving from a general list of topics which the interviewee did not talk about in his or her main narration and, on the other hand, of questions deriving from notes that I will have taken during the main narration. A second or third encounter can often prove useful, both for the researcher and for the interviewee. Follow-up meetings allow the interviewer to gain insights into the effects the first conversation provided.

Especially in the main narration, I try to let my ways of asking questions be influenced by my phenomenological awareness. This means that I try to avoid questions that are abstract, theoretical or conceptual. Explicit asking for explanations or interpretations should be postponed to the second part of the interview. A phenomenological question asks about what presents itself in immediate experience and how it appears to us. Such questions should be used in the main narration because they have the best potential to trigger the flow of memory. A successful opening question for an interview could be: "I'm interested mainly just about your life. Your life, your environment includes all sorts of people, also those closest to you – your children, spouse, parents and the people you work with. All this is interesting to me. Maybe you can just start with where and when you were born and what life was like in the family home." Such a question unites two qualities: it is formulated openly enough so that the interviewee does not feel obliged to start from a certain topic, and at the same time it is concrete enough to trigger the desired narration and not make the interviewee feel lost. In most cases, such an opening question triggers a first, longer piece of narration.

One of the most difficult aspects of interviewing is to refrain from interrupting interlocutors with questions even when their account becomes unintelligible to me in terms of temporal or causal logic. Questions like "In which year was that?" force the interviewee to interrupt the narrative flow and switch into a year-counting

mode that he or she was possibly not prepared for. More than once I witnessed how such a type of question, carelessly thrown in by myself, damaged the fragile flow of emerging narrations. Good narrations are the result of a flow of remembrance set free, an extremely fragile process which can easily be disrupted by any kind of interference. In most cases, the interviewer's uncertainties caused by a dense and apparently chaotic narration are eliminated during the course of the narrative, and any lingering questions should be asked in the end.

Once the main narration is over, one may ask questions about topics that the interviewee did not bring up or only touched upon. These questions should be also asked in a phenomenological way. For example, if I should want to know something about the remedial boarding school (see Article 2), but the interviewee did not talk about it in the main narration, I might ask: "How about the remedial school? Have you ever heard about it?" I should avoid asking "What is your opinion about the boarding school?" or "Did you hear about instances of unjustified transfers to the remedial school?" or "Do you think there has been injustice or oppression at the remedial school?" or "Were your years at the remedial school happy?" Thus, good questions are as value-free as possible, an example being "Please try to recall the first time you did this or that"; also how-questions are generally a good way to trigger experiential narrations. Unfavourable types of questions to do so would be "What do you think about...? Why do you think that...? Is this or that a conflict? Did they oppress you?" (Van Manen 2014, 298–99).

Such questions may already point towards a prescribed mode of reasoning and can therefore pose a rather high risk of distortion. I do not rule out such questions completely, but they should be noted during the narration and asked only after the main narration and the topical questions, or in follow-up meetings. The idea here is to avoid slipping too early into a directive, teleological mode of interviewing in which the interviewee starts acting according to some (possibly imagined) expectations of the interviewer. Such expectations can be imagined by the interviewee, or they can be unconsciously present in the interviewer's mind-set, for example as part of a desire to "strengthen an attractive developing hypothesis or bolster a cherished worldview" (Levy and Hollan 2015, 328). Re-listening to one's own interviews can reveal such inadvertent 'approaches' and help avoid them in the future. Examples from my own research are my first interviews from 2008 for my master's thesis with an interviewee whom I met again five years later for this thesis. Before meeting her again, I re-listened to the old interviews and noticed how desperately I was trying to find instances of oppression. While the interviewee could not fulfil my wish for concrete stories of oppression, she supported the desired direction by telling the 'version for visitors' of the story about forced relocation, without delving into the details about the pros and cons of relocation that were discussed intensively before relocation. In the subsequent meetings five and more years later, more conscious ways of asking led to more differentiated answers, which included descriptions of

the disputes among the people to be relocated, and drew a picture different from the previous one, in which all residents had appeared as opposing their relocation.

To summarise, when, as a researcher, I have in mind a topic I want to ask about, it is important to think about how to frame the relevant question. Different framings of a question produce qualitatively different answers. Instead of asking for information about a category, it can be more productive to ask about a significant experience related to it (Hastrup 2004, 465). In this regard, Levy and Hollan (2015, 316) make a helpful conceptual distinction between the interviewee as informant and as respondent. In a question like “Please describe for me why so many people in your village were sent to the boarding school”, the interviewee is treated as a knowledgeable informant, an expert witness, and he or she might feel pleased by it. In a question like “Did you have any experiences with the boarding school in your village?”, the interviewee is treated as an experienced respondent: the question focuses on what the person makes of his or her own experiences. Both ways of asking are important, as they produce very different answers. However, by taking the detour via ‘subjective’ respondent-answers, we may obtain in the end more honest informant-answers, because there will likely be fewer wrong assumptions about the intentions of the interviewer.

Van Manen (2014, 299) posits that a proper phenomenological inquiry must be done only on the basis of experiential, pre-reflective narratives and that it cannot be performed based on data that consist of views, opinions, beliefs, perceptions, interpretations and explanations of experiences. This is a defence of the ‘pure’ phenomenological interview, which I consider unrealistic; the phenomenologically inspired biographic interview takes these principles into account but does not try to rule out consideration of interpretive sequences from the narration. It is true that one should not confuse concrete experience with interpreted experience, but in reality there are certainly no materials have captured pure concrete experience. Oral history interviews are always interpreted experience. The materials closest to direct experience are those gained in participant observation, and this is one more reason for the approach of oral history that integrates interviewing and participant observation. The phenomenological elements in my approach include the techniques of acquiring the materials and looking at them initially, but I do not ask the materials themselves to be as purely phenomenological as possible.

A successful narrative biographical interview also depends on an array of external circumstances. I will name a few here. I scrupulously avoid from the outset the word “interview” due to its possibly frightening associations with journalism or official situations, and prefer “talk” or “conversation”. The meeting should take place in an informal setting, throughout Russia the kitchen table typically being the most common place for sitting many hours with a pot of tea and listening to stories. The researcher must be personable and win trust; there should be no rush whatsoever. It is not necessary to ask many questions; tactful patience and silence

may be productive ways to build confidence and thus elicit deep narrations, and thus putting up with rather long sequences of silence is a necessary skill. Stimulating ways for moving on include remarks getting back to what was already said, such as “So you were telling that...” or returning to a concrete experience (“Can you tell an example...”). The presence of other people, be they family members, friends, colleagues, an interpreter or a second researcher will almost certainly influence how a person answers. Undoubtedly, the deepest interviews have always been in a one-on-one setting, where the psychological effect of having an opportunity to confide things to an unprejudiced and trustworthy person works at its best. Taking notes during the interview should be done in a balanced way. Taking too many notes distracts from paying due attention to the spoken words and may send unintended signals to the interviewee. The researcher should take notes so that he or she can keep track of questions that come up during the interview but would interrupt the narrative flow if asked immediately. With only minimal content-related notes, however, it is all the more important to write a summary as soon as possible after each interview. These summaries will form the backbone of the interview metadata, which are indispensable in order to maintain an overview of the collected materials. More details on the sequence of steps and advice on the do’s and don’ts of conducting the kind of oral history interviewing I have used in this research are presented in Allemann (2013, 23–26), Rosenthal (2003, 916–22), Van Manen (2014, 314–17) and Levy and Hollan (2015).



Illustration 4: “Kitchen-table talk and camp-fire gatherings” (Konstantinov 2015, 149) tend indeed to be the places where, in repeated and long conversations, cultural intimacy is most likely to develop and the usually suppressed topics appear, especially when no people other than interviewer and interviewee are present. From left to right and top to bottom: Galina and Semen Galkin, Mariia Popova, Emiliia Dobrynina, Pavel Fefelov, Lovozero; Elena Lokko, Verkhnetulomsk; Anastasiia Matrekhina, Murmansk. Some images include the author and his son. Middle left and bottom right images by Julia Allemann, middle right image by Vladimir Seliutin, other images by the author, 2013-2015.

During the interviews I was guided by a list of topics. I would usually address the topics in a free order in the after-question part of the interview if the interviewee had not broached them during his or her main narration. I never tried to make an interviewee talk about every topic, but rather skipped those which I thought were not appropriate in the given situation. However, in the average interview most of the topics from the list were covered. The list of topics is an adapted version of the

common list of topics of the ORHELIA project. Agreeing common topics and working with them formed an important basis for being able to exchange experiences from the different field sites among colleagues (see Appendix 2 for the ORHELIA list of topics).

As elsewhere, when it comes to interviewing indigenous people in Arctic Russia, there can be several language-related issues. Riessler and Wilbur (2017, 35–36), who are interested in oral histories from a linguistic point of view, criticise the lack of language awareness among oral historians from the social sciences, and specifically cite the ORHELIA project in this connection. Obviously, which language to choose for an oral history conversation is an important question. In Russian Sápmi, besides Russian, this could most likely be one of the Saami languages or Komi. However, in my case the language of conversation was always Russian. Contrary to, for example, Cruikshank (1998, 46), who did research among Canadian First Nations, I had reasons not to feel decreased acceptance by conducting interviews in the majority society's language instead of the local indigenous language. In the multi-ethnic setting of Lovozero, as well as in all other places of my fieldwork, all people, independently of their ethnic identity, speak Russian as their first language; the only exception is a number of older people whose command of Saami is on a par with their knowledge of Russian. Many people do not speak Saami at all, or have only a passive knowledge of it (Scheller 2013). While language revitalisation is important and has had some limited success so far among the Russian Saami, for me as an oral history researcher committed to a non-normative and non-political stance on language issues, Russian – how ever regretful one may feel about this in a socio-linguistic or an ethno-political context – was the way of communicating that was perceived as the most natural and obvious by all my interlocutors.

Language fluency and the command of linguistic nuances that comes with it are extremely important when formulating questions and when understanding answers. As Levy and Hollan (2015, 318) put it, “It is harder to be properly vague in a foreign language than to be properly precise.” Together with a long-term commitment to the field partners, language command on a par with theirs is a prerequisite for generating deep interviews and insights. Not only from a psychological and discursive point of view, but also from a linguistic one, an interpreter's presence would in any case have had a distorting effect.

As an epilogue to an interview, it is a good idea to ask for photographs. People are usually eager to show their collections. Pictures trigger additional remembrance about relatives and places and can thus extend the interview significantly. Only at the very end of the interview do I ask the interviewee to fill in the biographical data sheet, which helps me to keep oriented when working with the recordings and forms the second part of the metadata. Having interviewees fill in the sheet at the end and not at the beginning of the interview is essential. Starting with the data sheet at the beginning of the meeting may make the situation more formal and lead to unspoken

assumptions by the interviewee associated with scientists as representatives of the State. Survey approaches are widely associated with officiousness and social distance, invoking a trust-inhibiting dichotomy between ‘us’ (‘the people’) and ‘them’ (representatives of some form of authority) (Bloch 2004, 7); sociology has been seen as the discipline most reliably working in service of the Soviet state, informing it about its inhabitants’ condition (Slezkine 1994a, 348–52). By trying to rely as little as possible on written data collection during the meeting, I create trust, and make it implicitly clear to interviewees that mine is a purely qualitative study without any ‘scientific’ surveying and measuring and that I am not a sociologist. It is also at this point that I ask interviewees if they want their names to be published or anonymised. For more insights about the nexus between formality and informality, trust and distrust, and written or spoken consent, see Article 3.

6.3.3. *Transcribing, coding and translating the interviews*

About 90 per cent of the interview materials recorded for this thesis have been transcribed. Using transcriptions, as well as translating them, gives rise to additional issues which one should be aware of. Full transcriptions are useful and desirable. Despite many new technical possibilities to code and tag audio files directly, transcriptions are still the best tool to look for, easily find, and organise information, not least because of the possibility of full-text searches.

I performed the coding itself and the analysis of the coded segments with the help of the software MaxQDA. The ORHELIA project’s list of topics, which was formulated before the fieldwork (see Appendix 2), served as a starting point for my own coding tree. It was significantly modified and extended through open coding grounded in the data. The main advantage of coding software is that it helps to keep materials organised by making data easily searchable and retrievable. The use of software when working with codes also allows for more flexibility in reorganising data when needed, which aids in the transparency of the research process and preventing de-contextualisation. The retrieval of coded segments through software is an excellent way of grouping together data from different sources according to a selection of topics. Manually this would never be possible as reliably and efficiently. The advantages of such grouping – besides speed and comprehensiveness – are that it enables one to double-check across all the data whether the structure of codes (and hence topics) makes sense and to compare across data and find patterns.

The main risk of coding is de-contextualisation, as coherent narratives are chopped into pieces. However, it is precisely computer-assisted coding that can prevent the data’s detachment from the contexts in the process of code analysis, as such coding makes it possible to switch between context (the interview in which a retrieved segment is embedded) and code (the sum of segments from the entire corpus of data with the same code) at any moment. Software could, however, be misused if it should encourage a focus on numbers, a classic form of de-contextualisation: Looking at

the frequency of codes, although technically possible, is not a good idea because, as I have outlined in section 2.2., *Lifeworld*, rare or extreme experiences can have a crucial significance both as personal experiences shaping one's life ("epiphanies", Denzin 1989) and as indicators of similar, more common, but less visible patterns in other contexts.

Last but not least, when doing computer-assisted analysis, one is more likely to end up in the "coding trap" (Gibbs 2014, 286) – having too many rather descriptive codes – due to the ease of coding with software. Indeed, my codes in the software were rather descriptive, but as I did not have an overly large amount of data, this did not result in losing the big picture. I went outside the software with the broader analytic-theoretical categories such as 'individualisation of the negative/medicalisation of social suffering' (the value of this category can be seen in Article 3, for example). One tool for grouping together relatively larger amounts of descriptive codes is to make a hierarchy, forming a tree of codes. There is a retrospective need for such reorganisation from time to time, and software, again, can make this task much easier.

Computer-assisted linkage of transcriptions to the corresponding audio files to make it possible to read and listen simultaneously also eliminates the main drawback of transcripts, that is, their limited possibilities to convey emotions. An important advantage of transcriptions is that excerpts can be partially transmitted as 'raw material' to the reader.

In scholarly literature, direct quotations from transcripts contribute to avoiding a frequent drawback of social sciences research, one which Fabian has put so succinctly: "The other's empirical presence turns into his theoretical absence" (Fabian 1983, xi). Referring to Fabian's critical concept of distancing, Agarwal rightfully says, "Fabian suggests that ethnographic discourse 'rests upon personal, prolonged interaction with the other' but ethnographic knowledge 'construes the other in terms of distance, spatial and temporal'" (Agrawal 2002, 293). Recording and transcribing, and the use of longer excerpts from interviews brings the 'empirical presence' into the 'theoretical absence': it bridges this gap which is felt more, in my opinion, in works which rest mostly on classical field notes, where the relation between empiricism and theorisation/conceptualisation is a more intimate and less transparent personal affair of the researcher. Direct interview quotations, with all their limitations due to selection and editing, are pieces of evidence about the immediate interaction between informant and researcher. The process of theorisation and conceptualisation becomes more transparent and easier to grasp if quotations from field partners are used extensively and can ultimately make a piece of research more lively, convincing and thus appealing.

Yet, we must remain aware that evidence from transcriptions remains relative. It is a piece of narration that has emerged out of certain circumstances and that has gone through a multiple selection process: the narrator deciding what to say and how; the

transcriber deciding how to transcribe and the extent to which to ‘cleanse’ the speech of emotions and linguistic irregularities; and the researcher deciding which excerpts to select for direct quotation. An additional source of distortion when quoting transcriptions is their translation, in the present case from Russian to English. There should be a general concern about how to properly convey original cultural and contextual meanings (Roulston 2014, 301). Finding the right compromises between, on the one hand, closeness to the original and, on the other, understandability and plasticity in the target language is not an easy task (Esin, Fathi, and Squire 2014, 208). As the data are quoted in a language other than they were originally acquired in, layers are removed, and new ones added, for example in the case of metaphors or idiomatic expressions. In any case, questions about how to translate the numerous direct quotations from interviews require thoughtful treatment.

6.4. Archive and library research

I see in the relation between oral testimony and archival document the same relation that Ingold, invoking Heidegger, sees between the handwritten and the typed word: “Anyone who thinks that there is no difference between a typed word and a handwritten one [...] has failed to understand the essence of the word. This is to let us *be* in the world and, in being, to *feel*, and in feeling, to *tell*” (Ingold 2013, 122, original emphases). Like the process of handwriting, oral history testimonies carry the “human being-feeling-telling” (Ingold 2013, 122), whereas in the operation of typing – and most of archival materials I have analysed are typed documents – words are reduced to encoded information.

While I regard oral history interviews as the most important data in this research, I do acknowledge that written sources are an indispensable complement to them. Written documents from archives and oral sources from the interviews do not exclude but, rather, inform each other. This is why one month of archival work with paper proved to be an experience as valuable as the fieldwork with people.



Illustration 5: The helpful staff of the State Archive of the Murmansk Region in Kirovsk and the author at work in the archive. Images by the author, 2014.

Every type of historical source gains in value through comparison with other types of historical sources. In this vein, Portelli points out, “oral sources give us information about [...] social groups whose written history is either missing or distorted” (1998, 64). ‘Distortions’ may obviously be present in oral testimonies as well.

A more salient point is that when compared, the most useful features of both kinds of data are their ‘distortions’. Comparing different viewpoints – to put the process in more neutral terms – is one of the most fruitful epistemological practices of oral history. Many of the protagonists whom I interviewed, or who appear in oral narrations, also appear in archival materials. My fieldwork allowed me to delve into the lifeworlds of people whom I ‘met’ again during my archival research, for instance, while reading the minutes of a meeting from the 1970s of the Lovozero

district's commission on under-age affairs or a local newspaper article from the 1980s blaming a 'failed' mother for her alcohol abuse. A sound historical reconstruction and analysis should be based on a contrasting of perspectives, where available, on the same topic and protagonists.

Evidence in documents about phenomena "is produced under specific circumstances which affect how it should be read" (Fincham et al. 2011, 52–53); it is a form of evidence that tries to establish external verifiability in a certain way. This means that verbal statements and statistics are not just there; there is a selection of what to produce and how to present what is produced. This is why it is so important "to think about documents in relation to their production (authorship) and their consumption (readership)" (Coffey 2014, 377–78). Accordingly, essential questions to ask about a document are who wrote it, and for which readership it is intended. Additional questions I ask about a document are: What type of document is it (internal and external features)? What are its intentions? How close is it to the topic (temporally, spatially, socially)? What is its provenience? (Mayring 2002, 46–50). The anonymity of many documents, especially official ones, may obscure the fact that they, too, are authored. Each document has an authorship, either visible or concealed, and, connected to it, varying *authority*. Questioning the author's '*author*'-ity' means examining that party's power to influence the document's shape. We must always question authority within the larger question of how a document, its authors and its audience are socially situated (Coffey 2014, 375).

Archival materials also furnish a solid basis for anthropological fieldwork by delivering 'hard facts'. Providing selected numerical results in qualitative studies about the number of instances of a particular phenomenon based on numbers found in archival documents is not statistics and does not make a study quantitative. However, such "quasi-statistics" (Maxwell and Chmiel 2014, 545) can be used to convincingly underscore claims. This tool, while actively used by 'conventional' historiography, is under-used in many qualitative social sciences studies. I use 'quasi-statistics' mainly in Article 2 in order to support my claims about the high distribution of transfers from the regular to the remedial school and the ethnic bias in diagnosing mental disability. To give a full account of selection processes in education, pass/fail statistics are not enough, but they do offer an essential starting point on the way to understanding and describing how widespread or not a phenomenon is and serve to highlight certain patterns of distribution (cf. Bourdieu and Passeron 1990, 157). In addition to archival documents, an important source of statistics and quasi-statistics was an internal report made for the regional government by sociologists of the Academy of Sciences (Bogoiavlenskii 1985; Dobrov, Toichkina, and Korchak 1985; Lashov 1985). This booklet contains numbers and analyses by ethnic category on topics such as non-natural causes of death, alcoholism and general despondency as well as their links to relocation and sedentarisation, which at that time belonged

to the public unmentionables and were meant for official use only. I found this ‘gem’ accidentally in the local library in Lovozero.

All these documents reveal not only important numbers but also attitudes among state officials. They largely support the accounts I obtained from my interviewees, but from the ‘other’ side. They thus inform us also about ways of using authoritative discourse from above (Bakhtin 1981; Yurchak 2006) and how it is interpreted and applied on the lowest administrative level of the Soviet state, the *raion* (district).

One important feature of all Lovozero district archival documents from Soviet times should be kept in mind: they use the word ‘indigenous’ differently than it is used today. The term ‘indigenous population’ [Russian: *korennyye zhiteli*], unless otherwise specified, usually refers to Komi, Nenets and Saami people. Today, only the Saami people have indigenous status in the Murmansk Region. Being aware of this difference is crucial for avoiding a Saami bias where it would be inappropriate. Where I draw conclusions about tendencies or events relating only or predominantly to the Saami population, I make this clear by explicit mention in the relevant documents or in my oral history fieldwork.

Another important point is that everywhere in Soviet official statistics and reports Lovozero is classified as a rural area (as opposed to an urban area). However, in reality Lovozero to a large extent features the amenities of town life; the proper, perceived town-tundra rift starts east of the town (cf. Konstantinov 2015, 51). This is relevant, as statistically the proportion of indigenous people living in ‘rural areas’ in the Murmansk Region appears to be persistently high. This does not do justice to the experienced effects of relocations, which undoubtedly entailed a shift from perceived rural life to perceived urbanised life, accompanied by a large array of positively and negatively connoted changes attributed to this shift. This is a telling example of how statistics construct realities that often do not correspond to the realities ‘out there’, that is, the lifeworlds of people, and thus may give rise to wrong interpretations (cf. Obertreis 2004, 27).

7. Ethical considerations

Research is a “dirty word”, suggests Norman Denzin (2009, 297–99) due to its many instances of doing harm and misusing power relations, especially in the colonial past. The kind of participatory research I have pursued is far from those traditions, but being aware of this difficult past can help researchers protect themselves from repeating mistakes. Within a humanistic paradigm, I would define myself sooner as a scholar than as a researcher, thus emphasising the performative and dynamic dimensions and the fundamental structural differences vis-a-vis more positivist research. This has serious implications for how I have dealt with ethical questions in this research.

As Thomson (2007, 66) puts it, oral history scholars should “ensure that memory studies does not retreat into an arcane intellectual world of rarefied debate, but rather is informed by our relationship with the men and women who tell us their memories.” A long-term commitment to the field also means regular exchange, and thus sharing our research with our field partners. Within the group of anthropologists I am working with, we see this as both an ethical obligation and a part of the ongoing epistemological process of research: presenting and discussing our research with our field partners – instead of only extracting data – is a form of validating preliminary results, and of creating new ones. In my field site, I returned and discussed research in several forms – informally and as a continuous activity – through recurrent meetings and discussions with the same persons. This included sharing materials with interested legitimate heirs of people who had passed away. More formally, I organised a mid-term gathering in Lovozero in 2014 and two end-of-project meetings in Murmansk and Lovozero in 2015. These meetings were advertised publicly and through word of mouth such that everyone who wished to attend could do so. Moreover, in the ORHELIA project we created electronic media and distributed them among field partners (Allemann 2015) and we tried to publicise our project in local newspapers across all field sites (for example in Lovozero: Kuznetsova 2014a; 2014b). In Article 3, I discuss the ethics of bringing research back to the field, related questions about consent and authorisation, as well as harms and benefits involved. In this section, I will not dwell on this but rather discuss some additional questions.

In oral history, another much-discussed form of validating research is the question of letting interviewees authorise interview transcripts. For example, Kasatkina et al. (2018) opted for returning transcripts for validation to research partners. I did not do that, in the light of the following deliberations: People tend to feel disturbed

by the aesthetic factors of verbatim transcripts, such as broken speech, incomplete sentences, grammatical errors or lapses into vulgarity. Verbatim transcripts are internal working instruments and hard to read. Giving them back bears potential for conflict. Upon request I did share transcripts, but did not do so as a matter of course. Usually a person is happier hearing his or her own recording rather than reading the bumpy verbatim transcript. Moreover, there would be more need for post-factum authorisation of transcripts if, in a first stage, consent had been given *before* the interview, because the course of the conversation is not predictable. However, as Dudeck and I argue in Article 3, the best solution, in our opinion, is to discuss agreement or disagreement about working with the collected information right *after* the interview: the interviewee's memory of the conversation is still fresh, and the interviewer can give concrete instructions about possible exceptions from a general consent, anonymisations and like options.

A few words should be said here about anonymisation versus identification of informants. The reader will be correct in noticing that I had no consistent policy spanning the entire research process as to whether to anonymise or identify my research partners. This reflects my overall position that in participatory research no pre-formulated, rigid ethical criteria will suffice. While social sciences in general opt for anonymisation, oral history by tendency aims "to ensure the confidentiality, but not necessarily the anonymity, of the respondents" (Ilic 2016, 3). Thomson (2007, 57) detects a potential conflict here: on the one hand, a reflective approach means to critically deconstruct the memories of our interviewees, which may bewilder them; on the other hand, being committed to a wider dialogue between our field partners, a wider audience and researchers, oral historians also "need to write and speak in terms that make accessible sense". Based on my own research experiences, I cannot agree with the rather widespread and often unquestioned a priori assumption that anonymity is the state of affairs desired by interviewees. Not to anonymise our interviewees is a crucial part of the mutual wish to enter a dialogue and to empower both interlocutors: Field partners often want to get involved in a wider discussion, and in many cases they would perceive their anonymisation as not being put on par with the scholar; if we want our own authorship to be seen, we should not a priori deny this desire to our field partners. A prime example here is the Canadian collection of boarding school memories, "The survivors speak" (Truth and Reconciliation Commission of Canada 2015), which did not anonymise its informants. On the other hand, there are interlocutors who wish to stay anonymous, or for whom external circumstances require this, an example being the protection of third parties mentioned in the interviews. These processes are described in more detail in Article 3, and they are reflected in the mix of anonymisation and identification used throughout this research.

Getting back to questions about the validation of content, in addition to individual validation, there is also a notion of 'community' validation. This notion

is very a controversial one. At least in biographical research, ‘community’ validation is obsolete and only betokens potential conflict. A good example is my blog entry (Allemann 2018) with quotations in Russian about the boarding school past. I had asked each interviewee beforehand if the materials could be used and processed to anonymise all quoted materials due to the potentially extensive readership and the sensitive topic. About a year after the blog entry’s publication the leader of one of the two main political organisations representing the Saami in Russia called and asked if I could remove some passages in a few interviews that included cursing. The stated reason for the request was that the cursing sheds a ‘wrong’ light on the Saami people. In addition, the person who called me said that she was not happy in general to see these testimonies, not because they were not true but because people did not want to remember these bad things, it makes them feel depressed. The request posed serious ethical dilemmas: Firstly, how representative is this individual voice officially endowed with the power to be a ‘community’s voice’? No single voice can be representative of an entire community, as it would be naïve to mistake a community as being a group of people who all have the same opinions (see Article 4). Secondly, does this official voice have any moral or legal rights to override the individual wishes of the field partners, who gave their consent to publish their interviews and who acknowledged the importance of raising the topic? I had to answer this question, too, in the negative: there was no legal basis, nor was there any ethical review board or other body to whom I would have been obliged to present my findings prior to their publication; and there is no moral basis justifying why one individual equipped with more political power should be able to override the wish of another individual with less political power. And yet I did agree to make partial changes, by removing single curse words. There are several reasons for my decision. Firstly, the use of such words in Russian written text, according to the common perception, looks and sounds harsher than in English. In view of the large readership, I decided to remove those words as part of the process of smoothening verbatim transcripts for specialists into publishable, easily readable text. Secondly, and more importantly in the context of ethical dilemmas, I agreed because of my past and future long-term engagement with the society and the place where I have done and will continue to do research. A good relationship with the people involved, including the one who had protested, is crucial for my future activities. Moral and pragmatic factors merged here into my decision to partially give in. Any engagement with the social world constantly requires trade-offs in order to be viable, and mine is no exception. No guidelines or rules whatsoever would have been able to tell me what the best compromise would have been in this situation.

This story touches upon current heated debates about the quests for adequate ethics of research among indigenous peoples in general, and, more specifically, questions of indigenous control over the planning, conduct and dissemination of research. Institutional review boards or research ethics boards

are institutional apparatuses, regimes of truth and systems of discourse that regulate a particular form of ethical conduct [...]. This is a historically specific form suited to a 'neoliberal world of legislative controls, legal responsibilities, and institutional audit and accountability'. It is clear that this regulatory formation is no longer workable in a transdisciplinary, global, postcolonial world. (Denzin 2009, 277)

Such apparatuses are increasingly adopted by indigenous groups as well; that is, an established institution known from the majority societies is appropriated as a group's own instrument of power. While already well established in Canada, for instance, and being increasingly discussed in Finland, such boards are completely lacking in Russia for the time being. In the case of the blog entry described above, I can openly say that it was beneficial to my research partners that there was no institutionalised mechanism able to veto publication of the testimonies they wanted.

Instead of the utilitarian ethical model of review boards, Denzin calls for a dynamically evolving, communitarian, multi-track research ethic, a "performative ethic, grounded in the ritual, sacred spaces of family, community and everyday moral life. It is not imposed by some external, bureaucratic agency" (2009, 278), no matter which kind of representational aspirations this agency has. This is the position I advocate as well. To apply fixed standards is a well meant, but in the end paternalistic way of tackling ethical issues, one that incapacitates individuals while seeking to protect them. The ethical issues that are meant to be addressed by an ethical board have arisen from colonial pasts of cultural hegemony, but such boards will always risk becoming toolsets for new or old hegemonies. In Article 3 we show that it is an ethnocentric, legalistic 'Western' assumption that written documents and legally sound language equal empowerment of the people such regulations seek to protect. What is even more disturbing is that, in the case of research with indigenous people, the utilitarian, hegemony-perpetuating review board model tends to be increasingly used as an instrument by indigenous representational institutions. It can become their hegemonic tool for internal, intra-ethnic colonisation, thanks to the formalised power to include and exclude both researchers *and* research partners; such institutions do not necessarily represent in uncontested ways the interests of all people who feel that they belong to the group that an institution claims to represent. Thus, failing to see and deconstruct these mechanisms, or consciously keeping silent about them, may increase internal power inequalities. This is something that for instance Junkka-Aikio (2016) omits when she claims that deconstructing internal indigenous affairs – as I do in Article 4 in relation to the Saami in Russia – vitiates the power of indigenous groups that they have achieved through claimed unity. Pursuing "sanitising politics" (Ortner 1995, 176–80) which suppress voices of internal opposition for the sake of advancing decolonisation arouses a false consciousness of indigenous empowerment and, what is more, does so using "colonial and neo-colonial discourses that inscribe 'otherness'" (Denzin 2009, 278).



Illustration 6: At the mid-term informational gathering with field partners and researchers of the ORHELIA project, Lovozero 2014. Towards the end of the project, two final gatherings followed, in Murmansk and Lovozero. Images by Nuccio Mazzullo.

Part II: Soviet social engineering and displacement on the Kola Peninsula

8. Understanding population displacement on the Kola Peninsula as social engineering

Together with the thesis articles, this chapter and its component sections form the empirical core of this dissertation. I will present here facts and analyses about displacement in the Soviet Arctic North. I will use interviews here more for empirical evidence, combined with archival materials; in the articles I focus more on scrutinising the interviewees' accounts in terms of narrative analysis, looking at motivations, positioning and discourses.

Heidegger's phenomenology offers concepts of dwelling based on a broad, environment-related understanding of what *building* as a human activity means. These are reductive – meaning 'original' – perspectives on the interrelation between building and dwelling, and they are highly relevant for understanding the effects of relocation projects and social engineering in general: "We do not dwell because we have built, but we build and have built because we dwell, that is because we are dwellers [...] To build is in itself already to dwell. (Heidegger 1971, 148, 146; quoted in Ingold 2000, 185–86). Ingold extends this thought:

Opposing the modernist convention that dwelling is an activity that goes on within, and is structured by, an environment that is already built, Heidegger argued that we cannot engage in any kind of building activity unless we already dwell within our surroundings. 'Only if we are capable of dwelling, only then can we build' (Heidegger 1971, 160). (Ingold 2000, 347)

After resettlement, as so often happens, people do not find ready conditions to *dwell*, but just to *be* (many examples in Oliver-Smith 2009b). Hence, in the new post-relocation environment, it can become very challenging, though not impossible, to build up a satisfying life. As Konstantinov (2015, 163) puts it, "existing pre-dispositions, i.e. the at least partial attractiveness of the proposed changes to at least parts of the population, mean that grass-roots agency was certainly there, but with actual events and consequences that were far from what was imagined." The challenges of re-emplacement after displacement, and the ambiguity of seizing opportunities and encountering problems, are the starting points from which I believe we should examine the relocations and post-relocation phenomena.

I see the radical social reshuffling on the indigenous and para-indigenous (see section 1.5., *Terminological disambiguations*) side of the Kola Peninsula through Scott's (1998) well-known theory on social engineering, which is grounded in a detailed analysis of several large-scale social experiments by nation-states in the

twentieth century. In many ways, what happened on the Kola Peninsula looks like a showcase implementation of this theory, as if its conceivers had Scott's book in hand and wanted to present a premier example.

For instance, Scott concludes that the Ujamaa relocation and villagisation campaign in Tanzania in the 1970s was "a point-by-point negation of existing rural practice, which included shifting cultivation; polycropping; living well off the main roads; kinship and lineage authority; small, scattered settlements with houses built higgledy-piggledy; and production that was well dispersed and opaque to the state" (Scott 1998, 238). Every single point in this enumeration finds its equivalent in the Kola Peninsula relocations, as I will show in the upcoming sections.

The concept of social engineering will thus guide us in this chapter through the intricacies of Soviet social upheaval in Russian Sápmi; indeed, displacement and its consequences form the common denominator of the thesis articles.

In the fragile Arctic, social engineering was backed by two interconnected ideological tenets, epitomising the Soviet version of high-modernist thinking as a basis for all modern social engineering. I mention them here only in passing, as they have been explored in detail by other scholars. The first is the 'conquest of nature' or 'conquest of the North' discourse, in Russian embodied in the key term *osvoenie* (appropriation, lit. 'making ours'). Here the North was (and largely still is) presented to people as a hostile environment to be fought against. Settling there was seen as a battle to be won (Bruno 2016; Bolotova 2014; Josephson 2014; Slavin 1982). The second tenet, the Soviet New Man as the ideal type, was crafted for the new forms of social organisation and the task of mastering the elements. This figure was a disciplined, conscious toiler "in control of and responsible for his or her behaviour, dominated by reason and able to change his or her environment" (Khlinovskaya Rockhill 2010, 269; see also Hagemester and Richers 2016; Kharkhordin 1999; Kelly and Shepherd 1998; Bergman 1998; 1997). These may be seen as the deepest ideological premises of all Soviet social engineering. Additionally, as I show in several of the upcoming sections and in Article 2, it was precisely these traits of self-control and self-responsibility that became the ideological grounds for scapegoating: while they were supposed to be the prerequisites for building a new society, these high moral requirements of individuals made it possible to shift the responsibility for the failures of social engineering from the state to the individual.

The Kola Peninsula is not only the most densely populated region of the circumpolar North, but since the end of the 1960s within the Soviet North it had already become the region where sedentarisation had been carried out most consistently. As stated in an unpublished sociological report, intended for official use only by the regional administration, "in the Murmansk Region the sedentarisation of the former nomadic population has been accomplished. [...] Similar processes can be observed among other northern peoples, but among most of them they did not go that far" (Bogoiavlenskii 1985, 92–93). In the typical style of self-congratulatory

reports, the Kola Peninsula is proudly presented in the report as a place where all the ‘problems’ of the local indigenous population have been solved in an exemplary manner:

Currently the indigenous people of the North [in the Murmansk Region] live in comfortable villages and settlements with modern conveniences. For example, in Lovozero, Revda, [etc.] the social infrastructure is well-developed: there are apartments with modern facilities, schools and kindergartens, shops, canteens, hospitals, health centres, Houses of Culture [*doma kul'tury*] and clubs. *This guarantees the high level of adaptation [prizhivaemosti] of the indigenous population in the extreme conditions of the Kola North.* (Balakshin 1985, 6, my emphasis)

In the logic of high-modernist colonialism in the Arctic, it was not just innovation that Soviet power brought to the North. Its purpose was deeper: it aspired to bring the local northerners proper *adaptation to* the North, implying that until that moment they had led a maladaptive life. To central planning from the outside, the fine-tuned, organically formed ways of *habitation in* the North (Ingold 2019; 2011) looked simply too messy, intransparent, heterogeneous and irrational to be included into its initial design. Local knowledge was rationalised away under the pretence of doing proper scientific planning, while in fact these were fanciful dreams about the almost playful mechanisation of intricate social and environmental adaptations, and thus completely unscientific simplifications of reality (Scott 1998, 253).

Remembering the village meetings before the relocation and the persuasive orations of the bureaucrats, one elderly witness of the Voron'e relocation shows little respect for the self-declared wisdom of the officials:

In 1963 they already started to close down everything- [...] They came [to our village], those people, how should I say, who think they are knowledgeable. Maybe they knew less than we did, but they pretended to be smart. “You will be relocated”, and “you will live in such and such houses and you won't pay anything.” (AI 2013)

If anything, this document and this testimony show us well what Scott expressed in his characteristic, sometimes acerbic style: “The progenitors of such plans regarded themselves as far smarter and farseeing than they really were and, at the same time, regarded their subjects as far more stupid and incompetent than they really were” (1998, 343).

The same interviewee continues her account about the “smart officials” empty promises:

Of course, “you won't pay. You will be transported at the expense of the resettlement [programme]!” So they should pay for that. There was nothing of the sort. Absolutely

nothing. When they brought us here [to Lovozero] [...] we all lived in one little house. We and my mum, and her sister's family. [...] Altogether nine people in one room, that's how it was. And we waited for several [five] years until they built this building [referring to the apartment block where the interview was held]. (AI 2013)

Getting back to Balakshin's (1985) saluting of the Soviet measures guaranteeing the adaptations of native northerners to the harsh north, the authors of the report, as well as the authorities, *did* have knowledge about the problems mentioned by this interviewee. The convictions of planners and scholars must not be necessarily understood in a literal way, but rather through the lens of vertically transmitted social-discursive constraints. In this case, and in most of the archival documents which will be discussed in the upcoming sections, this is the "normalized, ubiquitous, and immutable authoritative discourse of the Brezhnev years", which, it should be pointed out, includes the period after Brezhnev's death until the discursive turn triggered by Gorbachev's reforms (Yurchak 2006, 32). Within this discursive environment, addressing serious problems was possible, but only by enmeshing them into laudations about resounding successes according to the formula 'We have had indisputable successes but there are still certain difficulties to overcome'.

Indeed, the internal report quoted above offers contributions of contradictory character. Typically for this genre of documents, the accolades are placed at the report's beginning and end, playing on the assumption that political leaders usually do not have the time for deep reading. The critical content is discreetly placed on the middle pages of the publication, where it may be noticed only by the more deeply concerned reader. Considering that the report was written shortly before the great and sudden discursive turn of *perestroika* and *glasnost*' (Yurchak 2006; Slezkine 1994a, 371–72), the same report contains amazingly overt statements about the negative consequences of social engineering on the Kola Peninsula. Bogoiavlenskii (1985, 92–93) intersperses his declaration of the 'one hundred per cent sedentarisation victory' with alarming figures one could be hardly proud of: the rate of violent deaths among all deaths in the years since the end of relocations and full sedentarisation had been about fifty per cent, with the rate among men of working age being as high as around eighty per cent. At the same time, the birth rate was one of the lowest among all Arctic indigenous peoples. The author concludes that not only sedentarisation but also the negative consequences of Soviet reshuffling of populations in most of the other regions of the Soviet North did not go as far as it did in the Murmansk Region. Among the usual "delirium of numbers, targets, and percentages," (Scott 1998, 245) some of the contributors also mention issues related to "labour and employment" as one of the main social problems among the Saami population (Lashov 1985, 49), or point out that "the regressive mortality structure among the Saami population, especially the men, is a source of serious concern" (Dobrov, Toichkina, and Korchak 1985, 98). One former member of the Party and

of the local statistics bureau, who did statistical calculations along ethnic lines and specifically about relocated people, recalled:

Well, whatever family you take, all had difficult fates. Especially the displaced ones. There [in the statistics] I concluded that the highest percentage of suicides and violent deaths can be seen among those who were displaced. ‘They pulled them up by the roots.’ It’s there exactly like this: those from Voron’e, those from Varzino [and so on]. (LA, EP 2015)

Until the late 1980s, it was common for all these alarming figures and statements to exist in a kind of vacuum. They consistently avoided explicit linkage to the exercise of sedentarisation, rationalisation and displacement; this became the reader’s responsibility. Slezkine (1994a, 348–52) mentions the problem in his history of the Soviet indigenous Arctic. The work is also a history of Soviet social science in the North inasmuch in the 1970s and 1980s, after the dismissal of ethnography, the role of being more critical was taken over by sociology, whose quantitative methods were more compatible with the paradigm of scientific preciseness. Clearly showing – at least to the political elites – social ills like widespread bachelorhood, de facto unemployment and alcoholism, sociology did not clearly state that the social transformations had been a fundamental mistake, but rather that their implementation was poor and should be improved. Based on surveys, one could see that a majority of those who received a successful urbanised upbringing and a good education were happy with their professions and lives, while ‘unskilled’ workers, reindeer herders and fishers had extremely low rates of satisfaction with their life situation. For instance, according to Slezkine (1994a, 348–52), on Northern Sakhalin 80 per cent of natives with a higher education declared that they were satisfied, as compared to 3.4 per cent among fishermen. The creation of better job or housing conditions for the latter segment of the population was the preferred suggestion. In other words, in Soviet social sciences of the North, negative outcomes were cited – even on the Kola Peninsula, as we have seen in the 1985 report – but fundamental criticism that something was wrong with the recent policies of social transformation remained virtually absent. I will take a closer look at what it was that went wrong in the following sections.

8.1. Displacement on Kola: an extreme and unique case?

I will answer this question straight away: the Kola Peninsula is an extreme but not a unique case. Starting from the premise that common patterns are crystallised through extreme cases (see section 3.1., *Generalisation and theorisation from qualitative data*), the Kola Peninsula is an especially valuable case epistemologically.

I have already provided some evidence in the section above that the Kola Peninsula is one of the Arctic regions – if not *the* Arctic region – where Soviet social engineering has been most thoroughly implemented. However, many other regions have gone through similar developments. This section has two aims: firstly, I undertake to show how extreme the Kola Peninsula is as a Soviet Arctic social engineering case by bringing together numbers and other information scattered in a wide range of literature; secondly, I seek to pre-empt what I have called above the uniqueness bias (see section 1.3., *General quality criteria and pitfalls*), that is, to avoid the risk whereby using extreme accounts or figures may make me feel that ‘my’ field site is something unique. Many texts about the Kola Peninsula fail to establish a link to similar developments elsewhere. I will illustrate such links here and thus show how an extreme case can help us to better see and understand phenomena that may be less visible yet widespread elsewhere (see section 3.1., *Generalisation and theorisation from qualitative data*).

The Kola Peninsula has in many respects been justifiably depicted as an extreme case of Russian imperial colonisation and Soviet *osvoenie*. In many ways, this is due to its relative geographical closeness to central Russia compared with all the other Arctic regions of the country. By the end of the Soviet period, the Murmansk Region was the most industrialised, most militarised and most populated area in the entire Arctic (Josephson 2014; Hønneland 2013; Overland and Berg-Nordlie 2012; Fedorov 2009; Hønneland and Jørgensen 1999; Luzin, Pretes, and Vasiliev 1994; Doiban, Pretes, and Sekarev 1992). It is for these reasons that indigenous territorial autonomy and identification of settlers with the local indigenous culture has been less pronounced throughout the entire past century on the Kola Peninsula than in indigenous territories elsewhere in Russia. Moreover, today the Murmansk Region continues to show a particularly weak tradition of indigenous minority policy and representation (Berg-Nordlie 2015).

To a large extent, this has to do with simple demographics. The incoming population of the Murmansk Region has grown exponentially in the last 100 years or so, as shown in the following overview (Shavrov 1898; “Vsesoiuznaia perepis’ naseleniia 1926 goda” n.d.; “Vsesoiuznaia perepis’ naseleniia 1939 goda” n.d.; “Vsesoiuznaia perepis’ naseleniia 1959 goda” n.d.; “Vsesoiuznaia perepis’ naseleniia 1979 goda” n.d.; “Vsesoiuznaia perepis’ naseleniia 1989 goda” n.d.; “Itogi Vserossiiskoi perepisi naseleniia 2010 goda.” 2010a):

Table 2: Population dynamics in the Murmansk Region, 1897-2010

Year	Overall population	Saami population	Percentage
1895	8,690	1,940	22.3%
1926	23,006	1,708	7.4%
1939	291,178	1755	0.6%
1959	567,672	1687	0.3%
1989	1,146,589	1615	0.15%
2010	795,409	1599	0.2%

Additionally, the all-Soviet censuses show that throughout the Soviet period the non-indigenous population in the Murmansk Region grew faster than in any other Arctic region. At the peak of Soviet settlement in the North, in 1989, all Arctic regions together had about 184,000 indigenous and more than nine million non-indigenous inhabitants (Bogoyavlensky 1996, 35). While this is *per se* an impressive number, the non-indigenous population in the Murmansk Region at that point was proportionately about ten times smaller than that in, and the smallest among, all Soviet Arctic regions. These numerical realities explain the persistently small political weight of indigenous interests and their weak presence in the region's identity. This in turn is crucial for understanding the above-average power of Soviet social engineering on the Kola Peninsula.

Consequently, the indigenous population of the Kola Peninsula was in above-average ways impacted by different forms of displacement. In terms of punitive displacement during Stalin's times (imprisonment and executions), arrest and death rates among the Saami populations were much higher than the overall average, but on a level similar to that among other Soviet transnational minorities (Article 1). Due to its proximity to the national border, the Kola Peninsula has in several respects been an extreme case but not a unique one. Even in the non-punitive variations of displacement, the proximity of a national border arguably played a role. The coastal Saami were especially hard hit by the relocations. At one time around 40 per cent of the Kola Saami lived on the coast (Kal'te 2003, quoted in Afanasyeva 2013, 20), and none of these communities exist anymore. There are estimates that 70 to 80 per cent of all Saami adults living in the Soviet Union were relocated (Bogdanov 2000). Being relocated in one's life more than once was not uncommon, as the biographical interviews as a whole collected in this research show. The only region with a similarly high share of relocated indigenous people is found at the other end of the country, in Chukotka. Krupnik and Chlenov (2007, 74) estimate that two-thirds of the Yupik were relocated, and many families had to change their place of residence three or four times. Comparing Chukotka with other locations in the Arctic, the authors conclude that only the Caribou Inuit of the Canadian Keewatin Region were subject to relocation on a similar scale.

That relocations happened to such an extreme extent at the Soviet Union's westernmost and easternmost borders may have two explanations: firstly, the country wanted to use its areas bordering the capitalist world and the transnational minorities living there to showcase its 'exemplary' and breakneck implementation of revolutionary ideals; secondly, in the Cold War confrontation, these were the regions where the coastline was subject to an above-average militarisation and had to be cleared of civilians. The first reason will remain pure speculation here, due a lack of empirical evidence in my research materials. The only hint in the materials I have may be the picture of a Norwegian Saami delegation invited in 1961 to visit Lovozero and its local boarding school (see Illustration 13), an event about which unfortunately I could not find more information. The second reason will be discussed in more detail in section 8.3., *Legibility: mono-settlement and mono-industry*.

Despite the Kola Peninsula being an extreme case, there is plenty of research documenting the implementation of similar policies in other regions. Displacement of native people took place across the entire Soviet North, with heavily traumatic consequences everywhere. Importantly, relocation and urbanisation of Arctic indigenous populations has been a common policy among *all* modern Arctic nation states, North America and the Nordic countries thus being no exceptions, and with strikingly similar social consequences everywhere (see section 1.4., *Literature review and gap analysis*). This means no less than contextualising Kola relocations within global tendencies of urbanisation and industrial rationalisation: on the one hand, "more people were involuntarily displaced in the twentieth century than in any other in recorded history" (Oliver-Smith 2009b, 3); on the other hand, urbanisation "was also a process of increasing access to new career paths, as well as marriage possibilities offered by the rapidly emerging towns" (Konstantinov 2015, 167). At any rate, displacement policies almost never played out as originally intended, having consequences that were beyond the imagination of both the relocated people and the social engineers. Between 1960 and 1980, after the bulk of Soviet social engineering had been implemented, life expectancy among all northern indigenous people dropped by about twenty years on average, to about 45 for men and 55 for women (Slezkine 1994a, 375).

Being aware of all these instances of social engineering, which feature similar motives, implementation and outcomes, helps to avoid a uniqueness bias towards one's own research site.

8.2. A short historical overview

Although history as a scholarly discipline does not know the subjunctive mood ("what if.."), it remains at least questionable whether the large-scale incoming settlement in the Murmansk Region during the twentieth century is due solely to

the advent of Soviet power. We should not forget that ‘bringing improvements’ to the natives is a historical continuity bridging the revolutionary rupture between the Russian and Soviet empires, as shown in detail by Slezkine (1994a). The economic value of the Kola Peninsula’s natural resources, as well as the priority of their extraction over the interests of the local population, was already recognised by state officials towards the end of the nineteenth century. For example, Governor Engelhardt wrote the following after an extensive expedition: “Yes, the time will come when the Kola Peninsula is accorded the commercial and political significance that nature herself intended for it.” (Engel’gardt 2009 [1897], 135). The railroad from St. Petersburg to Murmansk, completed in 1916, was a major project of the Russian Empire and literally laid the path for the large-scale industrial expansion of the region (Nachtigal 2007).

I will not look into the history of the Kola Peninsula before the twentieth century. Works dealing with the topic are mentioned in section 1.4., *Literature review and gap analysis*. By way of exception, however, I will take up a particularly influential event of the nineteenth century: the immigration of Komi and Nenets herders from the Izhma Basin (part of today’s Komi Republic) to the Kola Peninsula. The Izhma Komi had adopted the practice of reindeer herding from the neighbouring Nenets by the sixteenth century. As an expansion- and trade-oriented way of herding, theirs it was very different style of herding from that of the Kola Saami. In the nineteenth century, large herds, environmental pressure and animal diseases led to the search for new territory. The Kola Peninsula seemed appropriate, but only to a small group. Purportedly 65 people, including some Nenets who used to work for the Komi as hired herders, set out with several thousand animals to Kola in autumn 1883 and arrived the following spring. After a few itinerant years they settled down in the then tiny Saami settlement of Lovozero. Towards the end of the century other Izhma Komi families joined the new community. Whereas Lovozero used to be a mid-sized regular Saami *siyt*,⁴ after the Komi immigration the situation changed. By 1915, Lovozero had become a village with 690 inhabitants, of whom 167 were Saami, 493 Komi and 30 Nenets. Moreover, the Komi founded three new villages: Ivanovka, Kanevka and Krasnoshchel’e. The immediate consequences of Komi herding methods on the Kola Peninsula included what seems to have been an astonishing over-seven-fold increase in the reindeer population between 1883 and 1914; conflicts with the Saami regarding

⁴ The *siyt* (Kildin Saami), or *siida* (North Saami), designates the pre-sedentarisation way of settling. A good definition is given by Kuchinskii (2008, 96): “The *siyt* is: 1) a socially important centre; 2) a community of people interconnected by kinship, culture, religion, animal husbandry and history; 3) self-identification; 4) from a historical point of view an aggregation of people (also in a religious sense), to which the life of many generations of the same community can be connected.” The *siyt* as a social institution was in many ways remarkably stable for about 400 years (Kuchinskii 2008:134–38, 145–46, 182–86).

the overuse of grazing lands (especially lichen) and about Komi appropriating the freely roaming reindeer of the Saami; and partial adaptation by the Saami to Komi ways of herding (Mankova 2018a; Bruno 2016, 128–30; Allemann 2013, 36–41; Kiselev 2009; Gutsol, Vinogradova, and Samorukova 2007; Bryleva 1996; Konakov and Black 1993; Kotov 1987).

Two main implications for the subsequent Soviet-time changes can be identified here. Firstly, the growing dominance on the Kola Peninsula of Komi reindeer herding ways was an inspiration and precursor for the Soviet plans for collectivised and industrialised reindeer herding (Habeck 2005, 75–77). Secondly, when Soviet power started the large-scale closure of outlying Saami villages and their relocation to Lovozero, the settlement had long since ceased being a regular Saami village and, in addition, was no longer the ‘Saami capital’; it had de facto become the ‘Komi capital’ of the Kola Peninsula.

Another important feature of pre-revolutionary life on the Kola Peninsula was the range of regular international trade contacts maintained by the local population. In the excerpt below, a field partner recalls what the life of her ancestors in Voron’e, located about halfway between the centre of the peninsula and the Barents Sea coast. The answer was triggered by my deliberately provocative questioning about the coerciveness of relocations:

Q: Well, back then [in 1963] you moved away from your settlement. When you were about to leave Voron’e, did you already know that no flat would be waiting for you here [in Lovozero]?

A: We knew. We knew that nobody was waiting here for us. Nobody.

Q: Why leave then?

A: That’s how it was. We had to. We left because the nearest shop was sixty kilometres from us, and there was no road. You had to go there on foot. If they had only left a kiosk to buy bread and the most indispensable things.

Q: I see. I dare to ask, however, without pretending to be the smartest person here, didn’t your ancestors also live without any shops?

I: Yes, they lived without a shop.

Q: And you didn’t consider that option? I mean, you or maybe other people in Voron’e, could say: Why should we need a shop? And what do we need a city for? Let’s just stay in the tundra and live like our ancestors did.

I: Earlier it was like that. For example, my grandpa Erofei. When they were alive there was no shop. They had a sledge; they had everything they needed and they brought goods for sale you know where? To Teriberka [on the Barents Sea coast]. They would go there to meet the Englishmen. Steamboats. They knew when the steamboats would stop there, and they would go there then. The Englishmen would barter [their goods] for meat. They took meat. And they [the Saami] would take clothes and so on.

Q: Your people?

A: Yes, our people. And they would also take wheat, sugar, as much as needed. He [Grandpa] would calculate how long it would last. They'd buy it and live on that. That's it; they had stocked up.

Q: I see. And later this option was no longer available.

I: No, there was no such possibility anymore, because of the Revolution.

(AI 2013)

The quoted interview brings us to the main topic of this chapter, the relocations of the 1960s and 1970s, as it embeds them in a historical perspective reaching into pre-Soviet times. The interviewee sums up all the main reasons why it was hardly possible to resist relocation and to stay in the old places of habitation. On the one hand, relocation was a matter of collectivisation and sedentarisation as policies aiming directly at socio-economic changes among the indigenous population. On the other hand, it was about developments that were not specifically directed towards the indigenous population but rendered impossible previous paths, rhythms and ties: industrialisation, militarisation and the concomitant infrastructurisation were the main aims dictating how the Murmansk Region was developed (Fedorov 2009, 149–366). These developments must be seen as external constraints that made it very hard to resist relocation – even though in their second, post-Stalinist stage in the 1960s and 1970s they were not *formally* coercive.

Early Soviet times were marked by a policy of indigenous empowerment, one unique in the world at the time (Kotljarchuk 2012, 61). By organising education and aiming at limited autonomy and self-government, the policy sought to convince the northern natives how well-meaning and beneficial Soviet power was compared to the overthrown imperial power, and thus create loyalty (Slezkine 1994a, 142–43). Nonetheless, it must be seen as a 'top-down' empowerment, where the rulers determined the forms and scope of such empowerment (Berg-Nordlie 2017, 66). Starting in the 1930s, however, the discourse of *osvoenie* started challenging and eventually surpassed indigenous empowerment (Slezkine 1994a, 338–40). For the next several decades, all the local people who had been living on the Kola Peninsula since pre-Soviet – and hence pre-urban – times experienced considerable changes in their lives.



Illustration 7: A relative (probably the father) of one interviewee posing in a mock-up car for a photograph in Leningrad, where he was sent to a crash course in literacy in the wake of the “liquidation of illiteracy” campaign (likbez, likvidatsiia bezgramotnosti). Probably 1930s. Photographer unknown, private archive of Nina Mironova, Umba.

The most visible of the changes was the conspicuous displacement of people and entire communities induced by the state between the 1930s and the 1970s for several purposes. Many indigenous communities were displaced multiple times, in which respect, as I have already shown, the Kola Peninsula is an extreme example, even in comparison to other Soviet Arctic regions.

Both before and after the Second World War, a repeated reason for relocations was industrial development, which necessarily led also to infrastructural development and a steady population influx (see Table 1). Already by the 1940s, the entire western part of the Murmansk Region had been cleared of rural and indigenous livelihoods and become the main arena for extractive and other industries (see Table 3, relocations 5, 7, 10, 13, and 14). In the 1960s, the increased need for electricity led to the construction of two dams, with several indigenous settlements being inundated (see Table 3, relocations 12, 15, and 16).

Before the Second World War, the reasons for displacement included collectivisation and sedentarisation. This entailed displacement of both a punitive and socio-economic character (Article 1; Konstantinov 2015). It is important to mention here that, according to existing research, the Komi on the Kola Peninsula suffered from the brutal sides of collectivisation under Stalin more than the Saami

and the Nenets, the latter being a very small group. The reason was straightforward: there were more relatively wealthy Komi families who could fall victim to dekulakisation (the political campaign aiming at eliminating wealthy or supposedly wealthy peasants or *kulaks*) (Bruno 2016, 145–47; on the anti-kulak campaign in general see Conquest 1986). While de-kulakisation aimed at often arbitrarily and randomly chosen individuals (Kharkhordin 1999, 289, 299), sedentarisation and rationalisation systematically aimed at entire village communities. Here more Saami than Komi settlements suffered from displacement due to sedentarisation, both in its first phase under Stalin and in its second phase under Khrushchev, for the simple reason that the Komi were sedentary, while the Saami fell into the category of semi-nomadic people.

After the war, Khrushchev's policy of consolidation (*ukrupnenie*) was the main force responsible for relocations. This policy, variously translated as "consolidation", "agglomeration" or "amalgamation", was a hastily planned, centralised economic rationalisation of the whole agricultural sector of the Soviet economy, but without the punitive component of its predecessors. In the 1960s, throughout the Soviet Union central agencies sent ministerial specialists, mostly young, out into the countryside equipped with guidelines to identify 'viable' (*perspektivnyye*) and 'non-viable' (*neperspektivnyye*) rural settlements. The result was that 120,000 villages were deemed worthy of investment and development, against an immense number of 580,000 villages scheduled for liquidation (Melvin 2003, 64). This means a ratio of one surviving village to five eradicated. In the country's Arctic regions, the ratio of village closures was similar or higher than one to five: on the Chukchi Peninsula, the number of villages dropped from 90 to 12 (this number probably includes instances of the widespread post-Soviet depopulation) from 1937 until the end of the twentieth century, on Sakhalin from about 1000 native settlements to 329 between 1962 and 1986, and in the Khanty-Mansi District from 650 to 126 by 1980 (Holzlehner 2011, 1961). The one-to-five ratio applies approximately also to the Kola Peninsula.

In addition to the consolidation policy, specifically for the indigenous North another policy played a crucial role in drastically reducing the number of settlements: the 1957 resolution "On the Measures for Further Economic and Cultural Development of the Peoples of the North", with its core Article 5 urging "further improvement of the economic and administrative territorial organisation of national raions and okrugs [...] to consider the question of simplifying the structure and improving the work of the economic, Soviet and Party organisation in the North" (quoted in Vakhtin 1992, 18–19). "Simplifying" and "improving" in practice meant dismantling and relocating in order to obtain simpler structures.

All over the Soviet North, these measures led to a concentration of people in a few "hubs" (Krupnik and Chlenov 2007, 62), selected to become urbanised centres from which rural economic activity would spread out into the wild tundra and

bring its fruits back to civilisation by means of shift work. The villages deemed non-viable were closed down (Vakhtin 1992; Slezkine 1994a, 340). In many regions, this amounted to the full sedentarisation of the indigenous population, a goal which the state had not been able to fully achieve before the Second World War had broken out. Industrialisation, a rapid influx of residents and infrastructure development of the Murmansk Region were further reasons for relocating local people (Allemann 2013; Afanasyeva 2013; Gutsol, Vinogradova, and Samorukova 2007). In a wider sense, all displacement was connected to social reorganisation and the ideal of the Soviet New Man, as mentioned above.

The 1957 decree provides a formal explanation for the ethnic misbalance of village closures on the Kola Peninsula: within the Soviet hierarchy of nations (Slezkine 1994b, 445–46), Pomors and Komi did not belong to the ‘small’ northern native peoples and were thus not subject to the decree. Accordingly, most of the predominantly Pomor villages along the White Sea coast and the predominantly Komi villages of Krasnoshchel’e and Kanevka, including their collective farms, survived consolidation despite their being as remote and disconnected as Saami settlements.⁵ Another reason why Komi and Pomor villages were spared is the low degree of militarisation of the White Sea coast and inland Kola: from the perspective of military structures, there was no need to remove civilians, unlike on the Barents Sea coast, where many Saami communities were located.

A non-exhaustive list of relocations has been put together by Afanasyeva (2013, 31; see here Table 3, rows 1 to 12). My fieldwork and review of other information sources has shown that several village closures can be added to this list (according to entries in *Kol’skaia Entsiklopediia* 2016; Lokhanov 2013; Wheelersburg and Gutsol 2010; 2008; see Table 3, rows 13 to 17). The settlements mentioned can also be found on the map in Appendix 1.

5 In the case of the Komi, only Ivanovka (Chal’mny Varre), and in the case of the Pomor only Ponoï were closed, the latter being historically a mixed settlement that included Saami families (see the respective toponymical entries in *Kol’skaia Entsiklopediia* 2016).

Table 3: List of Kola Saami relocations within the Soviet Union

	Year	From	To	Reason for relocation
1.	1934	Lumbovka winter settlement	Lumbovka summer settlement	Sedentarisation, collectivisation
2.	1950	Lumbovka year-round settlement	Iokan'ga	Sedentarisation, collectivisation
3.	1938	Iokan'ga winter settlement	Iokan'ga summer settlement	Sedentarisation, collectivisation
4.	1963	Iokan'ga year-round settlement	Kanevka, Sosnovka, Gremikha, Lovozero	Consolidation
5.	1933	Motovskii (entire <i>siyt</i>)	Titovka, Zapadnaia Litsa	Sedentarisation, collectivisation
6.	1931-1934	Kamenskii (entire <i>siyt</i>)	Chal'mny Varre (=Ivanovka)	Sedentarisation, collectivisation
7.	1938	Babinskii (entire <i>siyt</i>)	Iona	Sedentarisation, collectivisation, creation of the Lapland Nature Reserve ¹
8.	1937-1938	Semiostrov'e winter settlement	Varzino summer settlement	Sedentarisation, collectivisation
9.	1968-1969	Varzino year-round settlement	Lovozero	Consolidation
10.	1935	Kil'din (entire <i>siyt</i>)	Chudz'iavr	Industry-induced (construction of railway)/collectivisation
11.	1959	Chudz'iavr year-round settlement	Lovozero	Consolidation
12.	1967	Voron'e year-round settlement	Lovozero	Consolidation and industry-induced (construction of hydroelectric power plant)
13.	1920s	Ekostrovskii (Iokostrovskii) (entire <i>siyt</i>)	Gradually dispersed to several settlements	Industry-induced (mainly due to construction of the St. Petersburg-Murmansk railway)
14.	1920s	Masel (entire <i>siyt</i>)	Gradually dispersed to several settlements	Industry-induced (mainly due to construction of the St. Petersburg-Murmansk railway)
15.	1956-1967	Chal'mny Varre (=Ivanovka)	Lovozero and Krasnoshchel'e	Consolidation and industry-induced (construction of hydroelectric power plant)
16.	1962	Ristikent year-round settlement	Verkhnetulomsk	Industry-induced (construction of hydroelectric power plant)
17.	1969	Ponoi year-round settlement	Lovozero	Consolidation

This long list is likely to make one agree with Konstantinov (2015, 147) when he says: “The principal rationale of agglomeration, in the local application of this programme, can be likened to the macabre adage, attributed to Stalin: ‘No person – no problem’ (*Net cheloveka, net problema*). In other words: ‘No village – no problem.’”

So what was the problem? I repeat the most prominent reasons which led to the displacement of people: sedentarisation in its pre-war stage (collectivisation) and post-war continuation (consolidation, [*ukrupnenie*]) as well as rapid industrialisation and militarisation, both of which were accompanied by an influx of population and infrastructure development. These changes were grounded in an unshakable belief in the supremacy of technocratic, streamlined planning by the state for its inhabitants. As already mentioned, the prospective benefits for the population were backed by the ideologies of the Soviet New Man and the human-nature dichotomy, in which nature was not to be lived *with* but fought *against* (*osvoenie*). Hence, we can say that sincere convictions – at least partially – were at play, based on a high-modernist ideology that allowed little space for local knowledge and practices.



Illustration 8: Upper image: Voronë, late 1950s or early 1960s. The settlement was inundated due to a hydroelectric project in 1967; lower image: Lovozero, on the day of the yearly Festival of the North, probably 1986. Photographer unknown, private archive of Apollinariia Golykh, Lovozero.

As already mentioned, Scott (1998) undertook a close examination of the Ujamaa relocation campaign in Tanzania. This was an attempt to permanently settle most of the population in villages whose layouts, housing and local economies were centrally planned. In addition to the temporal parallels, its motivations and implementation show striking similarities with what happened in the Soviet indigenous North. A report from a relocation bureaucrat Tanzania shows the general attitude: “The African has neither the training, skill, nor equipment to diagnose his soil erosion troubles nor can he plan the remedial measures, which are based on scientific knowledge, and this is where we rightly come in” (quoted in Scott 1998, 226). A 1985 report on the state of affairs in Russian Sápmi offers similar telling examples of such technocratic, high-modernist language:

The most pressing economic and demographic problem is to finalise the sedentarisation of the nomadic population of the North. A nomadic way of life [...] is still maintained by more than twenty per cent of the indigenous population of the North [the author refers to other, less ‘successful’ places than the Murmansk Region, L.A.]. A semi-nomadic life is still common among hunters and fishers. Under these conditions, it is impossible to fully use labour resources; it becomes more difficult to realise the technical perfection of production processes and to meet the needed conditions for improving residential, medical, trade, household and cultural services. The mentioned shortcomings slow down the development of production capacities in the areas where the People of the High North live, and the improvement of their living conditions. At the core of the problem lies the elimination of these shortcomings by completing sedentarisation. (Balakshin 1985, 5)

The emphasis on sedentarisation and subsequently the concentration of indigenous populations in a few settlements was connected to sincere convictions about the benefits for the local population, namely, the state being able to provide services and goods, most prominently in the spheres of education, healthcare, hygiene and ‘civilised’ living conditions. An idealistic belief in the eventual fusion (*sliianie*) of all participants of the Soviet experiment into a Soviet nation (Slezkine 1994a, 343–44) served as a basis. However, benign intentions and ideological convictions justifying displacement and sedentarisation are only one side of the coin. Cynical knowledge (see section 3.3.9.) and idealistic belief went hand in hand, as the confidence in the path towards a united, communist nation conspired with several other, but not explicitly stated, ambitions on the part of the state. Scott (1998) subsumes them under the umbrella term *legibility*. In the following section, I will discuss several aspects of legibility pertaining to indigenous displacement on the Kola Peninsula.

8.3. Legibility: mono-settlement and mono-industry

Berg-Nordlie has rightfully suggested about pan-Saami ethno-politics that “the self-image of having once constituted one unitary people, forcibly separated and repressed by alien states, serves as an ideological and rhetorical basis” (2011, 32). Interestingly enough – and without denying the fact of separation through state borders – in Kola Saami narratives about displacement quite the opposite idea prevails; that is, the state has not forcibly separated but forcibly unified people who were originally living far apart from each other.

Indeed, collectivisation, sedentarisation, and reorientation towards monoculture have primarily meant concentrating a previously scattered population in fewer but bigger settlements. The assumption is that this will make the population more legible to the state: the actions and movements of people, domesticated animals and all their belongings become more easily controllable and countable, and education, healthcare, logistics, taxation and many more duties and ambitions of state power become more tractable. As we will see in the course of this section, what had occurred in the Arctic rural regions was nothing less than social re-organisation by the rationale of industrialisation; the process is described by Eric Hobsbawm in the following:

Industrial labour – and especially mechanized factory labour⁶ – imposes a regularity, routine and monotony quite unlike pre-industrial rhythms of work, which depend on the variation of the seasons or the weather, the multiplicity of tasks in occupations unaffected by the rational division of labour, the vagaries of other human beings or animals, or even a man’s own desire to play instead of working. (Hobsbawm 1968, 85)

In social engineering, there is a correlation between radically simplified designs for social organisation and for natural environments. Failures of monocultures or mono-industries correlate with the failures of collective farms and/or planned settlement. Conversely, there is a correlation between social and natural diversity as a source of resilience. Concentrating people in one big settlement (Lovozero) and elevating reindeer herding to a state of monoculture form an example of both: we can see the evident failures, but we can also see how diverse practices persisted and emerged as ‘grey’ resilience under the surface. These forms of resilience include, on the one hand, a wide array of grey tundra activities (meaning those that are not part of the planned economy and thus marginalised or even declared illegal, such as hunting or fishing) and, on the other hand, accommodating and making the best of what the

⁶ The most eloquent example in the Soviet Arctic is the terminological transformation of the ‘traditional’ reindeer herder (*olenevod*) into the corresponding formalised Soviet profession called *olenevod-mekhanizator* (*herder and machine operator*).

new urban life can offer. While Konstantinov (2015) has focused mainly on aspects of resilience, my research focuses more on the failures, but without losing sight of resilience; in fact, I see failure and resilience as being as inseparable as displacement and (re)emplacement are.

According to Scott's analysis of the Ujamaa campaign, "administrative convenience, not ecological considerations, governed the selection of sites; they were often far from fuelwood and water; and their population often exceeded the carrying capacity of the land" (1998, 235). Once again, the commonalities of high-modernist social engineering are confirmed in the comparison with the Kola Peninsula. Examples can be found already in the early-stage relocations before the Second World War, when the migration between summer and winter settlements was discontinued for the sake of sedentarisation. An interviewee recalls this:

Later [at the beginning of the winter], the Saami – my parents among them – moved to a winter village [...]. We moved into the forest where we could find more firewood and better moss for our reindeer [...]. When the Communists came to power, as early as the 1930s they created collective farms and started converting the Saami to a sedentary lifestyle. **That's why** the village of Semioströv'e was closed [...] [Table 3, relocation 8]. Because there were no roads leading to it, you could use reindeer to get there in winter, but you couldn't do that in summer. That's why they decided to create a permanent settlement on the Barents Sea coast. This is my native village ((shows a photo)). It was a summer village [...]. That's where I was born. It was called Varzino [...]. You ask why Saami didn't live there before [all year round]? Because it was a very cold place [...]. One couldn't survive there in winter, that's why Saami didn't live there. But the Soviet authorities were building a new life and hadn't a clue how the settlers would survive in a location where there was no firewood and no trees. There was a forest [in Semioströv'e], so one could go and fell a couple of birches and cut them up for firewood [...]. That's why life was so difficult there [in Varzino] in winter. (NE 2008)

In this case, the failure of the Soviet planners to take local knowledge into account meant that in winter the locals experienced a shortage of firewood. Moreover, they were exposed to a windy and damp climate while the hinterland would have offered better conditions. From a centralised state's vantage point, however, this was not simply thoughtlessness but a matter of priorities: sedentarising people in one place, and connecting them to a year-round transport infrastructure (in this case, the never-freezing Barents Sea), meant control over the production of goods and the movement of people, including potential 'spies' (see Article 1), in what was a sensitive border zone. In short, it meant increased legibility in the state's eyes.

Even by Arctic standards, during the Hot War and Cold War confrontations of the twentieth century, the Kola Peninsula became one of the most heavily militarised areas in the entire the world (Hønneland 2013; Heininen 2010; Fedorov

2009; Hønneland and Jørgensen 1999; Luzin, Pretes, and Vasiliev 1994; Doiban, Pretes, and Sekarev 1992). While there is no direct written proof at hand in openly accessible sources about any direct link between the resettlement of indigenous communities and military activity, many accounts, events and circumstances in sum point to such a causal connection. The sensitive security situation, especially in the Cold War, has been variously mentioned as a factor prompting the relocations, and I will bring together these references here and combine them with evidence from my fieldwork.

In the literature, Hønneland and Jørgensen (1999) state that reindeer grazing areas were designated as closed zones for developing the navy and the country's nuclear arsenal. Although they provide no documentary evidence, Gutsol, Vinogradova, and Samorukova (2007, 52) also state that the needs of the military in the context of the Cold War played a role in the relocations. Mentioning the strong military presence on Chuktoka, Holzlehner (2011, 1967) acknowledges that "direct evidence of military induced resettlements is rather limited". It seems, however, to be hardly a coincidence that the coast was most systematically cleared of civilians precisely in the two Arctic regions of the Soviet Union bordering on NATO countries.

Nevertheless, I also need to mention here that in the lived experience of many interview partners, the relationship with soldiers and border guards was a relaxed one. Working as a nanny in an officer's family, going to school with children from military families, being taught at school by officers' wives, having common dancing evenings and intermarriage all figure among the positive memories of many of my interlocutors. In passing this issue has been also taken up by Heininen (2010, 19), who mentions that the relationship between the environment and the military has been a contradictory issue among northern peoples, since armies are also seen as having brought benefits to northern indigenous peoples in the form of additional employment, infrastructure, transport connections and other services. As a result, many northern residents were not against the military presence in the North.

As already mentioned above, sedentarisation meant more legibility for the state, and hence knowledge about and control over the population. This pertains also to security and military issues. The pre-war collectivisation had led to a first stage of sedentarisation by closing winter villages (see Table 3, at least relocations 1, 3, and 8). Happening exactly at a time when all over the country there was an active search for spies, saboteurs and other 'enemies of the state', sedentarisation made the population structures more transparent to the authorities. In the sensitive border area of the Barents coast, these early instances of sedentarisation arguably had the side-effect of making it easier for the authorities to pursue their security interests and 'find' spies (see Article 1).

After the war, most remaining civilian settlements along the Barents Sea coast were eliminated, with some transformed into military ones. No indigenous settlement survived. As already mentioned, the stated reasons for their closure were always

other than military ones, which means that there is not much material to add here. From my corpus of interviews, I will cite only one out of many similar statements on the topic of the military:

A: Not so much the [restricted] border zone but the nuclear [submarine] fleet was to be developed. That's it. The inlets around our Drozdovka [settlement] never froze: they could all be kept at [the mouth of] the river.

Q: The military-

A: Yes, the military, they make their plans – it's the most important reason why Varzino was closed down [...]. So the conclusion is a simple one. Developing the nuclear fleet required freeing up certain spaces. They closed down the coast, not only Varzino, but the whole coastline, you know. Iokan'ga, Varzino, Kharlovka – they closed down everything up to Teriberka. (NE 2013a)

This secretive topic is sustained by one piece of written evidence found during my archival research: a report of the Varzino village assembly on the eve of its closure in 1966 stating that civilians and border guards were bothering each other: “The village population has been informed about the border zone rules. At the same time, there are still shortcomings. [...] There are instances when people take out a boat or walk into the tundra without informing the border guards.” (f.302 op.1 d.134 l.4, 1966). As we already know, shortly after these occurrences, the problem of such disturbances by civilians was solved by simply removing the settlement altogether.

This is only one example of the Soviet administrative report as an established and fixed stylistic genre, which uses the same tropes: a list of long achievements comes always first; the transition to the downsides, signalled by the formula “...that being said, ...” (*vmeste s tem*); and the end, which includes a list of recommended measures, sometimes combined with a number of scapegoats who are supposed to bear personal responsibility for prospective improvements. Among the documents examined in my archival fieldwork, this fixed form applies to virtually all reports on the district and village level, as well as to the 1985 sociologists' report cited several times above. These formulaic, entrenched ways of addressing problems have been described by Yurchak (2006) with reference to the example of leadership speech during the Soviet period from the late 1960s to the early 1980s. The cited Murmansk Region reports and records at the district and village level show how these stiff discursive tropes looked after having travelling all the way down the vertical chain of power. Yurchak calls the discursive setting in which this genre flourished hypernormalisation:

This normalizing shift can be observed especially clearly at the level of language. [...] The production of political discourse among the leadership, and following them, on all other levels, became increasingly organised through collective writing and personal imitation, leading to a hypernormalization of that language [...] that made it increasingly

more fixed, predictable, citational, and cumbersome. The same process took place on all other levels of ideological discourse, from visual propaganda [...] to the organization of routine practices of everyday life. (Yurchak 2006, 284, my emphasis)

Hypernormalisation was the discursive mirror of a period usually called the period of stagnation. After Stalin's death and some of Khrushchev's correctives, this period was marked by the absence of an external voice putting itself above the frames of conventions. This state of affairs was left untouched until Gorbachev came to power. This excessively normative and unwieldy language is not taken seriously in its originally foreseen content. In the Varzino document about infringements of border zone restrictions, this meant that "*a decision was made to recommend*" to the kolkhoz chairman that he inform the border guard unit every time a village inhabitant planned to leave the village. Besides the sheer impossibility of implementing such a recommendation, the nested and formulaic construction leaves open the question of whether the recommendation will be ever seen as binding or whether it will be even delivered to the kolkhoz chairman. In short, the problem had been mentioned, but nobody expected any change until, eventually, a draconian measure would drop from the sky. In the case of Varzino, it took less than a year before that measure would come: the village was closed down in a sudden move coming from the regional authorities (see section 8.4., *Implementation: pull factors and push factors*).

Krupnik and Chlenov (2007, 67) describe similar limitations on the freedom of movement in the border areas of the Chukchi Peninsula; there, too, the situation was resolved by removing civilian settlements altogether. The formalised control of movement of every single citizen outside of his or her own village, as stipulated by the border control regulations, and their increasing confinement to concentrated settlement represents the culmination of efforts to achieve legibility: as hard as it may have been to control the movements of a population in a border zone village, it would have been an altogether impossible task if the intransparent holism of semi-nomadic family life and work in a fluid multitude of places had been allowed to continue.

Mono-settlement, however, was not only the solution to security concerns. To be sure, such concerns were important, but they were a territorially limited peculiarity of some parts of the Kola Peninsula and other areas of outstanding strategic importance, such as the Chukchi Peninsula. Far more representative for the countrywide rationalisation of rural settlement was the relation of mono-settlement to mono-industry and its rural equivalent, monoculture. To reiterate, the Kola Peninsula was the pinnacle of Soviet rural socio-economic transformations in the Arctic.

Full sedentarisation also meant that nomadism as a livelihood (*bytovoe kochevanie*) had been replaced by so-called 'production nomadism' (*proizvodstvennoe kochevanie*) (Slezkine 1994a, 341); that is, reindeer herding was done only by paid herders as shift work without their family members. This had been fully implemented in only a

few places in the Russian North, Russian Sápmi among them (Bogoiavlenskii 1985, 92–93; Eidlitz Kuoljok 1985, 127). The development meant that all Soviet Saami were living permanently in a settlement – roughly half in Lovozero (Bogoiavlenskii 1985, 93) – with only few of them, mostly men, periodically leaving for long-term shift work on the tundra. The logic behind organising work in such a way can be compared to that behind the work of sailors in Murmansk (Konstantinov 2015, 23) or the long-term shift workers in the Siberian gas fields (Saxinger 2016). What is more, production nomadism also meant that reindeer herding was defined by planners as the main economic activity of the rural part of Russian Sápmi. Reindeer monoculture officially replaced the previous polycropping, which among the Eastern Saami encompassed not only herding but also fishing, hunting, gathering and keeping sheep. These activities were largely confined to the realm of free time and were partially illegalised.⁷ In the first stage of collectivisation, however, before the small local kolkhozes had been consolidated into sovkhoses⁸ and the people relocated, the polycropping principle was kept. Polycropping is one of the principal tropes of pre-relocation remembrance among the field partners of the oldest generation. I quote here only two of a wealth of similar accounts:

We had a club, a library – oh what a **library!** A hospital, a bakery, a village council, a kolkhoz office where our chairman worked, and a sheep farm. And a dairy farm, too. We had maybe three hundred cows. And about a hundred sheep. Calves – I used to tend the calves, yes. That was our kolkhoz; it was regarded as a **millionaire** kolkhoz. Tons of fish we sent to the front. ‘And how many of our men left and never came back?’. Yes, we lived our lives until suddenly – hop! – and it was no more. What a village we used to have! (NN 2013; see Table 3, relocation 4)

That’s called consolidation of the farms; they were deemed to be of little promise. But how could they put us in this category if our farm was a millionaire kolkhoz? We had salmon, and the reindeer were in good shape. We had cattle and we grew potatoes. Not for us but as fodder for the cows. Turnips and potatoes. (NE 2008; see Table 3, relocation 9)

7 As this issue is not a focus of this research, it will suffice here to say that turning hunting and fishing livelihoods into poaching can also be seen from the vantage point of creating legibility as a desired side-effect. Poaching as a crime is less a result of a change in everyday practices than of a change in the law and state presence. Sudden declarations of what is state property prompt state-created crime, and the advent of such rules turns subsistence routines into forms of crime and resistance. This pattern has been repeated in a large number of societies (Hobsbawm 1968, 105; Scott 1989, 9–10).

8 On the Kola Peninsula, the remaining kolkhozes (*kollektivnoe khoziaistvo*, “collective farm”) were formally renamed sovkhoses (*sovetskoe khoziaistvo*, “state farm”) in 1971 (Konstantinov 2007, 4–5; Vladimirova 2006, 142). For the workers, the most significant difference in practice between these two operational models was the introduction of stable state salaries instead of the previous mix of pay in kind and in money depending on the farm’s success.

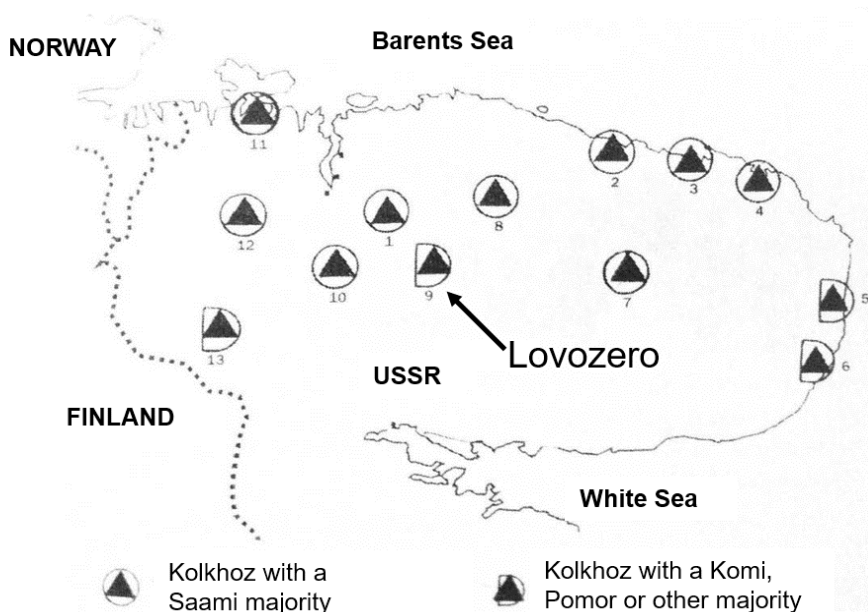
In terms of economic activity, consolidation meant first and foremost an orientation towards intensified reindeer monoculture. After the collective farms of the closed villages had been merged into only two collectives, those in Lovozero and Krasnoshchel'e, fishing and hunting were dropped altogether. Cows were kept for local production of dairy products, but reindeer herding remained the only large-scale activity. The indigenous and quasi-indigenous population of the Kola Peninsula had been assigned its small but clearly defined place in the large mosaic of the Soviet planned economy. This is something that still evokes extreme indignation especially among interviewees of the oldest generation: it does not seem rational to them that fishing in particular, a profitable activity, was dropped.

There was, however, a rationale behind these transformations. From a high-modernist socio-economic planners' point of view, it consists in creating a canon of monocultures as the presumed key to maximum efficiency: designated entities – be they entire republics or single villages – were assigned clearly defined and limited roles within the Soviet economy, the best-known examples being probably the Uzbek cotton and the Ukrainian wheat and corn monocultures. On the Murmansk regional level, sea fishery on an industrial scale was based in Murmansk, with this reaching a ten per cent share of the countrywide fish production by the end of Soviet era (Fedorov 2009); salmon fishery as a specialisation had been assigned to the Pomor kolkhozes on the White Sea coast, which did not interfere with military activity and were not closed down (according entries in *Kol'skaia Entsiklopediia* 2016); and the remaining central sovkhoses in Lovozero and Krasnoshchel'e were responsible for reindeer herding. In short, collectivisation and consolidation were one large “program designed to establish central control over food production”, part of a worldwide trend towards monoculture (Bruno 2016, 142, 157).

That precisely Lovozero and Krasnoshchel'e were categorised as viable and chosen as the future centres of reindeer monoculture was no coincidence. First of all, they were located inland, far from interference with external conflicting interests such as military and industry development. There were also Saami villages inland, but they were nevertheless closed down. Secondly, and this concerns only Lovozero, it had been easy to build a road providing access to the town thanks to the proximity of the mining town of Revda. Thirdly, and most importantly for all post-relocation events and relations, these villages and their kolkhozes were Komi-dominated. As already mentioned, Krasnoshchel'e was founded by Komi and in Lovozero the Komi outnumbered the Saami population by far already by the beginning of the twentieth century (Konstantinov 2015; Gutsol, Vinogradova, and Samorukova 2007).

Historically the Komi and Nenets were more oriented towards expansive and intensive reindeer herding than herding based on extensive smallholder production. This suited the monoculture orientation of the Soviet planned economy. It has been shown by previous research that the Komi way of herding, containing future Soviet features such as expansivity, sedentarism and a strong gender separation, has been

the preferred template for collectivised Soviet reindeer herding (Habeck 2005, 75–77; Povoroznyuk, Habeck, and Vaté 2010, 17–18; Konstantinov 2015, 116–17; 2005a; Bruno 2016, 125). Such views represent a historical continuity contradicting the often exaggerated Soviet/pre-Soviet rupture. Governor Engelhardt, who had supported Komi settlement on the Kola Peninsula in the nineteenth century, emphasised the close bonds between the Russian and Komi people, regarding them as ‘strong peoples’ who had the duty to civilise smaller native peoples. These views were embraced in the Soviet evolutionary hierarchisation of ethnoses (Jääts 2009; Slezkine 1994a, 120; 1994b, 445). Thus, among other things, “in the Komi reindeer-herding families, we find the [Soviet] blue-print for the spatial and labor division that was imparted upon other peoples of the Soviet North in the 1930s to 1970s” (Povoroznyuk, Habeck, and Vaté 2010, 17–18).



Map 1: This map lists the first-stage, pre-war collectivisation of Saami kolkhozes, which were territorially based on the earlier siyts. Most of them have been merged during the 1960s consolidation with the Lovozero collective farm, which was Komi-dominated. Map taken and adapted from Kalstad (2009, 35).

Thus, when the time came to put an end to the ‘irrational’ polyculture⁹ of the small Saami-dominated kolkhozes and to implement this blueprint, on the Kola Peninsula it was obvious that this would mean merging those kolkhozes with farms already in Komi hands (the founding members of the Lovozero collective farm had been mainly Komi; see Konstantinov 2015, 117–18). Accordingly, on the Kola Peninsula, Lovozero became the epitome of rural legibility by creating compact settlement in the village and reindeer monoculture around it.

8.4. Implementation: pull factors and push factors

I will focus in this section on how displacement was implemented, trying to identify the push factors (constraints) and pull factors (incentives) of moving. I will confine myself here to the non-punitive displacement of the 1960s and 1970s only, and not take up its punitive counterpart in the Stalin era. Straight away, I can say that a clear distinction between push and pull factors is not possible, as I will show in more detail below. The changes leading to large-scale displacement happen on several levels.

In the first stage, especially in a one-party system with a strong political leadership, decisions at the top can be taken very quickly. An example in the present context is the adoption of a countrywide consolidation policy, which within one year produced up a plan to close down over half a million villages (already discussed above, see section 8.2., *A short historical overview*).

The second stage consists of announcing the planned changes to the population and “manufacturing consent” (Herman and Chomsky 1988; Lippmann 1922). In the case of the Soviet Union, there were no institutionalised civil society mechanisms at hand acting as a strong counteracting negotiating party. However, even in such settings forms of negotiation do take place, but they have to deal with a decision already taken at the top (see next interview quotations), or they are ‘grey zone’ post-factum negotiations, such as the sovkhoist accommodations (Article 4; Konstantinov 2015; Scott 1989). Several interviewees described the negotiations pertaining to their imminent relocation:

9 The common denominator of all polycultures is that they “may not be as productive, in the short run, as single-species forests and fields [and animals, L.A.]. But they are demonstrably more stable, more self-sufficient, and less vulnerable to epidemics and environmental stress, needing far less in the way of external infusions to keep them on track” (Scott 1998, 353). Such arguments against the Komi-Soviet monoculture would have been available already at the time of its introduction, based on historical experiences: it had been exactly that style of intense breeding that in the nineteenth century produced overgrazing and epidemics in the Izhma-Komi homelands and led to the wave of emigration to the Kola Peninsula.

And so there was this directive from above, a decision by the Murmansk Region Executive Committee, this and that date, this and that year, that the village of Varzino, as a non-viable settlement, was to be closed down [see Table 3, relocation 9]. That was it! And when this decision came, where was it first discussed? In the kolkhoz board. The board first agrees, the board members [...] discuss, and they start working with the people. You see, in any settlement there are always those who say: “Oh, great, let’s go, it will be better there!”. But there were others who were against it; they had to work with the population, in order to prevent too much noise ((chuckling)). (NE 2013b)

The things they promised us at the village meeting! ‘You will have good housing, with glass windows, with running water, with heating, with everything. Like in civilised places. In Varzino [see Table 3, relocation 9] we had nothing of the sort. We had no electricity; the lines had not been built. And so it was promised, we will be given housing, and we will be given jobs. But when we arrived there were no houses. Where should we go? To whom? (NE 2013a)

[About relocation 12, Table 3:] Q: Do you remember, were there people who did not agree at all and straightforwardly rebelled?

A: There was, of course, no rebellion. But there were people like, for instance, our family. There was my mum and there were those Matveevs – they didn’t want to go. They simply didn’t want to leave. They were against it. But everything was removed [from the village], and where should we go then? No means of transport were left, so where should we go? Should we starve or what? So we had to [leave].

Q: Yes, you had too. And then there were also those who liked the idea of living in flats – it’s great, it’s progress, a leap forward.

A: Yes, there were such people, and they left first. They left first. Yes. Because the children would be able to go to school in the same place. Because in Voron’e there was only a school for the first four grades. All others, from the fifth grade, had to go there [to the Lovozero boarding school].

[...]

Q: So this was also a factor for the parents. That they could live where their children go to school.

A: Yes, it was like that, that’s how it was.

[...]

Q: All in all, in your village, do you think more people were for or against the relocation to Lovozero?

A: God knows. Well, I think it was about fifty-fifty. Yes. (AI 2013)

The quotations show that, at the stage of announcement and negotiations in village meetings, the main goal of officials was to present the new location – Lovozero – as an improvement compared to the old place of habitation. Pull factors were listed in

order to create consent, many of which were exaggerated or made up. Later many of these pull factors turned out not to correspond to the promised reality. Among the fabricated pull factors, the most prominent was housing: to many people electricity, gas and running water were important and real benefits. However, there was an immense gap between the promises and the reality (see section 8.5.3., *Housing shortage*). Another pull factor named in the quotation above was education: parents from remote villages had to send their children to the boarding school in Lovozero. Living in the same village was supposed to terminate this separation and was a significant pull factor. However, here, too, the reality lagged behind the promises: children of relocated families were kept at the boarding school despite having their parents in the same village (I explain the reasons for this boarding school paradox in Article 2; see also sections 8.5.3., *Housing shortage*, and 8.5.5., *Gender split and eroded family structures*). Road access was a fulfilled promise and a pull factor for some – but not for everyone, as the contemporary ‘happy roadlessness’ of the remote Kola villages shows (Konstantinov 2009). Improved sanitation was an undoubted and uncontested advantage. Finally, yet importantly, the new career and marriage opportunities that a much bigger settlement could offer were certainly pull factors as well. The highly gendered difference in marriage and career patterns after the relocations clearly shows that women profited much more from these pull factors than men (see section 8.5.5., *Gender split and eroded family structures*).

Presenting all the pull factors in favour of planned settlement during village meetings was not intentional lying, but it concealed “important goals of appropriation, security, and political hegemony” (Scott 1998, 191). As a result, apparently all relocations were formally supported by a majority of the concerned villagers (several interviews). In the case of Varzino, for instance, at the kolkhoz meeting – the kolkhoz was the only employer in the village – twenty people voted for relocation, four were against it and five abstained (Gutsol, Vinogradova, and Samorukova 2007, 42).

The third stage consists of the actions following the manufacture of consent: due to strong vertical power structures in the bureaucratic apparatus, often combined with an emphasis on strong personal responsibility, officials are urged to show results swiftly. In its most extreme form, with “a dictator [...] who wanted results, the normal bureaucratic pathologies were exaggerated” (Scott 1998). While in the Soviet Union this pathological condition had reached its peak during Stalin’s rule (see Article 1), we can clearly identify it in later displacement as well. Interviews and archival materials show that with each relocation there was less time and attention set aside for negotiating, informing and manufacturing consent. For example, a look through the village council records of Varzino – one of the last and the most slapdash relocations – shows business as usual until the very last moment before the relocation in 1968, with no signs of an imminent kolkhoz or village liquidation (f.302 op.1 d.145, 1967-1968). Interviewees draw a similar picture:

Here in Varzino [relocation 9, Table 3] we all had good houses [...]. We had to leave like refugees; we left everything behind [...]. My parents could bring quite a bit of goods and property with the reindeer. But those who had no reindeer, they just packed a bag. (EK 2014)

They sent the tractors and said: “Three days to get ready. [...] Pack your belongings, you have three days to get ready, here is the transportation so you can get to Lovozero.” That’s how they relocated us [relocation 12, Table 3]. (AD 2013a)

The same pattern was applied in Chukotka (Krupnik and Chlenov 2007, 69) and in the Ujamaa campaign (Scott 1998, 234). Thus, hastiness as an external push factor seems to be another basic trait of many social engineering projects, with the goal to curtail negotiation and resistance opportunities, and with the result that there is no proper relocation programme.

For these reasons, as I have already suggested in the beginning of this thesis, the term *displacement* should be preferred to *relocation* and *resettlement*. Displacement is broader: there is often displacement without proper relocation if we define the latter as a dedicated programme offered and organised by the development project that initially made it necessary to move a settlement (Oliver-Smith 2009b, 8). In this sense, displacement is not only broader, but it depicts the reality of the Arctic village closures more accurately. On the Kola Peninsula, the early cases when *sijys* disappeared (Table 3, relocations 13 and 14) can certainly be defined as gradual displacement by circumstances without any formal relocation: the people dispersed to other places by themselves due to outside pressures. In the 1960s and 1970s, as I will show in more detail, there were nominal relocation programmes for every village closure, yet these varied in attention to detail and the amount of benefits offered and were marked by inconsistent implementation. The construction of pull factors (as shown above), but also of endemic push factors (problems of non-viability supposedly not connected to the world outside of the community, such as remoteness or poor economic performance) could serve as a justification for offering minimal resettlement programmes.

This is illustrated in the following interview segment:

It was winter; she had to give birth soon, but the infirmary had been already closed down, and the shop too. So why did the people begin to leave themselves? I mean, they created the conditions [to push out the people]. That’s how it was. (AD 2013a)

In planned displacement, both push and pull factors, presented to the population as reasons for moving, are often invented or exaggerated by the people who stand behind the initial idea to move a settlement. For example, *before* the actual relocation, the village’s slow death is induced by removing, one by one, its vital organs: the shop,

school, infirmary and other components of the infrastructure are closed down, thus exacerbating the non-viability that is presented to the population in village meetings as a reason to move. From the state's point of view, such constructed non-viability is likely to make more people agree that moving away is the only option (push factors are created) and that the new, prospective settlement is by comparison a more attractive option (pull factors are created). The manufactured consent, in its turn, offers the advantage of minimising logistic support and compensation, as the relocation can be 'spun' in such a way that it is at least partially desired by the relocated people themselves. In the case of Voron'e and Varzino, while some monetary support was foreseen for the relocated people, many families, especially those with children, could not benefit from it. The reason for this is that they had to leave before the relocation started officially because social services were cut or completely discontinued before the kolkhoz was and thus before the village had been formally liquidated (Gutsol, Vinogradova, and Samorukova 2007, 52). Thus, in a state-driven economy, compared to private economy, the state has a crucial advantage: it has the power to create non-viability if it is suitable for its goals, such as improving the legibility of a population (as seen in the previous section).

In the case of the fluidity of push and pull factors, and the relativity of resettlement support, we once again find striking similarities with the Ujamaa relocations in Tanzania. The campaign there started smoothly, with an emphasis on conviction rather than coercion; later, once general resistance to villagisation became evident, the state started to apply more coercive methods, and more directly, trying to impose on the 'ignorant' peasants what was good for them. As Scott notes, "Positive inducements were, apparently, more typical for the early, voluntary phase of villagization than the later, compulsory phase" (Scott 1998, 236). The same pattern is visible in the series of relocations in Soviet Sápmi in the 1960s and 1970s. Growing coercion meant fewer consultations, fewer promises, fewer benefits and a more hasty, sudden, order-like resettlement. In an apparent paradox, while coercion was increasing *de facto*, relocation was voluntary *de jure*.

Gutsol, Vinogradova and Samorukova (2007, 42, 50, 53) have observed, and my interviewees confirmed, that there were enormous differences in how Chudz'iavr (1959), Voron'e (1963) and Varzino (1968) were relocated to Lovozero. Block houses from Chudz'iavr were disassembled, moved and reassembled at state expense. In Voron'e, the people's houses were not moved; new housing was promised, although ultimately provided with a five-year delay. For Varzino, nothing of the sort was promised.

Filling up a "viable" village and its single enterprise with people from a number of "non-viable" villages requires, with each relocation, an increased effort to provide housing and meaningful work. As one interlocutor put it, Lovozero "wasn't made of rubber" (NE 2013a), meaning that expansion had its limitations. However, local administrators had to somehow make the dreams of distant planners come true, by

hook or crook. The easiest solution, from that point of view, was to turn the tables on the non-viable villages and make their people and farms petitioners: it should be *their* wish to move because *their* village became non-viable and this is why *they* should bear the bulk of responsibilities.

The last relocation of a Saami village, Varzino, illustrates this best. In the light of the non-viability verdict from Moscow, the members of the kolkhoz *Bol'shevik* in Varzino voted for a merger with the Lovozero kolkhoz *Tundra* and relocation (Gutsol, Vinogradova, and Samorukova 2007, 42). The Lovozero kolkhoz, which by that time had already absorbed several other smaller kolkhozes, was concerned about the limited pastures, but they had to agree, half-heartedly, as there was no way around the formal fulfilment of the consolidation directives from above. However, Lovozero, caught between the centralised consolidation planning and local constraints, set conditions that were probably far from what the original consolidation planners in Moscow had imagined: it would not be the Varzino relocatees but the *Tundra* people who would be given priority to move into new housing and the relocation costs would not be paid. As a consequence, the Varzino relocatees met the most difficult situation among all relocated groups (more details on this story see section 8.5.3., *Housing shortage*).

8.5. The consequences of displacement

Liubov' Vatonena's well-known newspaper article, the first publication to appear about the Kola Saami relocations in the new discursive *perestroika* genre, put the issue in a nutshell: "The people were torn away from their *habitual life*, and nothing better was given to them in exchange" (Vatonena 1988 my emphasis). While for a not-so-small part of the population, especially women, the relocation brought new opportunities, Vatonena's statement still accurately reflects the general sentiment of being disappointed and deceived. Even those who realised benefits professionally in terms of family life and living standard usually had to confront despair and despondency among family members and fellow villagers. Moreover, Vatonena's choice of words in this early sample of the new victim discourse still reflects a rather non-politicised attitude, as met among the sovkhoist, non-activist 'half' of my field partners (see Article 4). What is perceived as the main loss is the previous *habitual* daily life, which consisted of having a meaningful occupation, a family, a house, in short, habitation or dwelling (Ingold 2000; 2011). In its subsequent 'activist' version, quickly emerging at the time, 'loss of *habitual* life' was invariably changed to a 'loss of *traditional* life' trope (Article 4; Konstantinov 2015; Vladimirova 2011; Kuchinskii 2007), as can be seen in Antonova's open "Letter to the Central Authorities of the RSFSR":

Newcomers to the area and Russians are surprised [...]. To them, it seems mad that I prefer a cup of reindeer blood to a piece of fruit, and that after having eaten vegetables, for three days I don't know what to do to wash them down and take the taste away in order to feel right again. Who is capable of understanding all this? Probably only a Saami. [...] The Saami regret that they have lost their nomadic way of life. [...] We want to survive, to sing our songs, dance our dances. (Antonova 1996, 176, 179–80)

This sample belongs to the performative, neo-traditionalist talk (Konstantinov 2015) – a rhetorical turn with little credibility if understood literally, especially taking into account that the author is remembered by an entire generation as the former headmaster of the remedial boarding school and as an indigenous boarding school teacher who most ardently forbade the use of the Saami language (see Article 2). Conversely, in substantive, culturally intimate talk (Herzfeld 1997), it is not the loss of a supposed traditional life but simply of habitual life that matters to most elderly field partners, including Antonova. 'Before-and-after-tradition' talk is instrumental, politicised discourse – with its own legitimacy – while 'traditional' livelihoods on Kola changed gradually, bridging what is etically often thought of as sudden historical ruptures, like the Bolshevik Revolution or collectivisation (see also section 8.2., *A short historical overview*).

From this point of view, it is a justified claim that neither the Revolution itself nor collectivisation constituted the greatest ruptures for the northern native population: the Revolution arrived belatedly and gradually (Fedorov 2009); the transition to collectivism in the economy entailed very many compromises, and certain elements of the material and social security offered by the new arrangements are things that to this day people are not ready to give up (Konstantinov 2015). When it comes to displacement in people's lives, there is, however, a clear 'before' and 'after'. Displacement – not seldomly more than once in a lifetime – formed a disrupting epiphany (Denzin 1989; see section 2.2., *Lifeworld*) in every person's life whom I interviewed and who experienced displacement; what is more, it was a rupture that in many ways has been passed on to subsequent generations. The manifold aftermaths of displacement form the common denominator of all the articles of this thesis. The following sub-sections are an attempt to present them in a systematised and detailed way.

8.5.1. Under Stalin: Terror, imprisonment and orphanhood

While the development-induced indigenous resettlement on Kola Peninsula and its consequences stand at the centre of this thesis, we should not forget that during Stalin's time there was another type of mass displacement, which we may call punitive displacement. Article 1 is devoted to this kind of 'person removal' and its consequences. Punitive displacement was trained on trans-border ethnic groups to a much larger extent than it did purely internal nationalities (Martin 1998), in

the case of the Kola Peninsula those affected being Finns, Norwegians, Swedes and Saami (Kotljarchuk 2012; 2014). Uprooting and displacement impacted not only the arrested individuals but also their family members, above all their children. As orphans or children of a state enemy, they often faced relocation to relatives' homes or orphanages and had to cope on their own from a very young age. In addition to traumatic experiences, these times show the powerful agency that the victims of Stalinism deployed in order to re-empower themselves after the turmoil; that is, they set about building up an existence, determining the whereabouts of their families and fighting against stigma and for justice. Article 1 tells about these reactions in the aftermath of punitive displacement.

8.5.2. The hierarchy of nations becomes visible

I have already discussed the dominance of the Komi style of reindeer herding as a model for Soviet reindeer herding and the numerical dominance of Komi in Lovozero (see sections 8.2., *A short historical overview*, and 8.3., *Legibility: mono-settlement and mono-industry*). More generally, concentrating people in a few selected villages was a concrete step towards merging nationalities into one Soviet nation. Ethnic neighbourhoods that had developed over time became replaced by ethnic mixes. However, as the intended eventual disappearance of nations was still a very distant goal, being together in an amalgamated village primarily meant actualising the Soviet hierarchy of nations. This constructed hierarchy was ideologically conceived as a temporary condition, which, like so many other things, became the status quo during the period of stagnation (Yurchak 2006).

Slezkine has emphasised the deep historical roots in the eighteenth-century Enlightenment of what was often perceived as an encounter “between perfection and crudity”. In a text from as early as 1788, this took the form of bringing to the Siberian natives “‘better buildings, more profitable lifestyles, more convenient hunting and fishing tools,’ and ‘easier ways of satisfying one’s needs’” (quoted in Slezkine 1994a, 56). This reads like the programmatic speech on Soviet relocation from the 1985 report quoted above (see section 8.0., *Understanding Kola resettlements and displacement as social engineering*). As we have already seen, social engineering was the high-modernist, Enlightenment-driven incarnation of the perfection-versus-primitiveness opposition.

In the Soviet array of nations, ‘small’ Arctic indigenous groups were seen as the least-developed groups; they were originally not seen as nationalities proper, because, as the dominant rhetoric stated, they lacked a national ‘culture’ and ‘consciousness’. By the mid-1930s the hierarchy of nationalities was already firmly established, based on evolutionary principles. While this Darwinist ideological position was not far from pre-Soviet attitudes, it served as a motivation for previously unseen affirmative action (Martin 2001). As Slezkine (1994a; 1994b) explains, over time the pragmatic realisation prevailed that organising a state with 192 different languages, bureaucracies

and the like was not possible. Over time, more and more ‘national’ soviets, villages, districts, schools and other small units were disbanded. One of the central claims by Slezkine is that, while collective affirmative action for most nationalities was eventually abandoned, a few of the larger, selected ones were ‘enhanced’. As he noted, “The nationality policy had abandoned the pursuit of countless rootless nationalities in order to concentrate on a few full-fledged, fully equipped ‘nations’”, meaning that the Soviet multi-ethnic state, metaphorically imagined by Slezkine as a big communal apartment (*kommunalka*), had now fewer, but more decorated rooms (Slezkine 1994b, 445–46). In this process, the Saami eventually lost their ‘room’ with the elimination of the Saami district in 1962 (Agarkova 2016), while the Komi, as a numerically important group, had reached the highest level a non-Russian nation could achieve; that is, they had their own republic. Although the Komi in the Murmansk Region were not ‘citizens’ of the Komi Soviet Socialist Republic (which was located on the other side of the White Sea), the high status of their ethnic group had considerable significance in practice, as I will explain below.



Illustration 9: Meeting of members of the pre-consolidation kolkhozes of the Saami district, 1950 or earlier. Photographer unknown, private archive of Anastasiia Matrekhina, Murmansk.

At the same time, and despite protracted scholarly discussions about merging, the northern natives remained ‘*narodnosti*’ (populations) all the way into the 1980s, a vague term indicating a stage of development inferior to a ‘*narod*’ (people) proper,

a term which everybody was using but which also lacked any precise definition. Hierarchical thinking was not dropped, with the general tendency that the more developed a group was, the less 'ethnic' it was. The ethnic dynamics supposedly triggered by socialism and eventual communism were described in a scientific terminology ranging from consolidation, assimilation, integration, merging to eventual fusion (*sliianie*) (Slezkine 1994a, 144–47, 343–48).

Consolidation and assimilation were the stages at which Arctic indigenous people found themselves after the relocations of the 1960s and 1970s. Moreover, for these people, moving to the consolidated villages meant the final rollback of the early Soviet affirmative action-based empowerment in the previous, self-administrating collective farms. Vakhtin (1992, 27) points out that most of the key positions in the concentrated villages and districts at the time were occupied by non-natives, and this trend has continued to this day. In Lovozero, the presence of the Komi as middle layer between the indigenous *narodnosti* and the incoming population from the South ('the Russians') meant that to this day both the district leadership and the sovkhos have been either in Komi or in incomers' hands. Not once since the relocations has a person with Saami background been in the position of chairperson of the farm or the district, a fact many field partners remember.

A comparison with Ujamaa is, once again, helpful for identifying significant patterns influencing the course of development-induced resettlement. In Tanzania, there were areas where villagization was carried out later and in a laxer manner, often by simply leaving villages as they were but pro forma including them in the category of new villages. One reason was that they were already relatively populous villages and the existing crops provided an indispensable source of revenue for the state; another was that the groups from those areas were overrepresented among the bureaucratic elites (Scott 1998, 236). As Scott explains, "In the actual process of creating new villages, the administrators and party officials [...] effectively evaded all those policies that would have diminished their privileges and power while exaggerating those that reinforced their corporate sway" (1998, 245). On the Kola Peninsula, we can see this same pattern: the Komi and Pomor villages happened to be overrepresented on the list of viable villages, while Saami settlements were overrepresented on the list of non-viable villages. Along with military and industrial considerations, the stronger power of the 'higher' nations to lobby their interests cannot be underestimated: it is a power deriving, on the one hand, ideologically from the higher respect they enjoy thanks to their imagined 'higher' evolutionary position and closeness to the leading Russian nation and, on the other, bureaucratically thanks to better representation within the administrative apparatus.

While I will not look into the Pomor case in this thesis, the better administrative resources of the Komi were relevant and conspicuous. Firstly, these were relevant prior to the relocations: the only two roadless inland communities that were left untouched during consolidation were the Komi villages of Kanevka and

Krasnoshchel'e; all Saami settlements were closed down. Unfortunately, in my research I did not discover archival evidence of the circumstances that might have explained why precisely these villages were put on the 'viable' list, but the higher status of the Komi and of their reindeer herding methods is likely to have played a key role. Secondly, the hierarchy of power became most visible after the relocations in the distribution of housing – the rarest commodity in Lovozero (see section 8.5.3., *Housing shortage*) – and 'real' jobs (see section 8.5.4., *Lack of meaningful occupation*). The ethnic hierarchical structures were cemented by secondary effects such as the relocated people's children being kept at the boarding school (see Article 2), substance abuse (see section 8.5.6., *Violent death and substance abuse*) and ever-stronger stereotypes, generally following patterns of social reproduction (Bourdieu and Passeron 1990). Similar instances of post-relocation social descent due to the proximity of 'higher' nations have been observed, for instance, by Bloch (2004) and Ssorin-Chaikov (2003) among the Evenki, by Krupnik and Chlenov (2007) among the Inuit, and by Grant (1995) among the Nivkhi.

The hierarchy of nations unquestionably played a role in terms of differences in relocatees' living standard. However, it would be completely wrong to imagine this as a sort of apartheid. In practice, the hierarchy of nations was subverted on a daily basis: interethnic marriage, much easier to realise after the relocations, turned out to be the easiest and a very common way to reach the Soviet final goal of ethnic 'fusion'; education and career opportunities were in principle open to everyone, constraints in this regard not being formal and absolute but due to structures of social dominance, which were not impermeable; and children of all ethnicities went to school and played together.

However, the effects of the 'invisible' hierarchy of ethnicities partially last to this day: among the tundra-connected population of the Kola Peninsula, most leadership and ownership elites are Komi and not Saami (Konstantinov 2015, 119–20, 242). Nevertheless, lower-status Komi and people of mixed ethnicity are in a way an even more invisible group than lower-status Saami: the latter are at least periodically mentioned in the victim discourse of their ethno-political leaders (see Article 4), although they do not directly benefit from this. This means a partial inversion of the contemporary ethnic hierarchy. While Komi still tend to dominate the local establishment in terms of power and wealth, during the past quarter of a century the Saami have overtaken them in terms of outward social capital, thanks to legislation and activism. This leads to new tensions, as reflected in this interview with a person with a Komi background:

A: The Saami are more **pampered** with **attention**, they demand this and that, their culture and all this blah-blah. [...] The Komi, they have a clearly defined rhythm of life, they have a clear-cut order, their own language [...], their own behaviour, which is different, very different from the Saami's. The Saami are somewhat spoiled by all the

attention they get. That they are Arctic people that need to be taken care of somehow.

Q: Attention by whom?

A: Attention in general. They demand it. The Komi don't demand in such ways, while the Saami demand something all the time. Give us this, give us that. Well, for example from our local government [...] they got privileges that Russians, for example, don't get. [...] The Komi stand aside, while Saami are somehow higher. What difference is there, I don't know any difference – Komi, Saami, Russians, Finns – I think that's all the same.

Q: When you were still of working age, was there also this attention?

A: There was less then, of course, less. [...] That's a long time ago. Saami, **Lapp**, dirty, yes. That was there, there's no way around it. But they really behaved like that. The dirt – you get inside their dwelling, well, house, oh my God! I don't even know how to tell you where and what. Those were no living conditions. Not only was there nowhere to sit down but there was nowhere to stand either. (LI 2014)

The interviewee, who worked for the police in Lovozero, names the old stereotypes and new sentiments of the Komi toward the Saami, located somewhere between feeling superior and feeling envious. At the same time, however, throughout the interview she showed a lack of awareness of the social hardships endured by the relocated people, which is common among non-Saami locals and those who have moved to Lovozero from the South. This lack of awareness is expressed here, for instance, in ascribing the higher level of language preservation among the Komi compared to the Saami – which is a fact (Blokland and Riessler 2011) – simply to the orderliness of the former. Conversely, she attributes several social problems, such as language loss, heavily and chronically overcrowded housing, 'deficient' families and substance abuse, to the 'disorderliness' of the Saami.

Similar vague illusions about inborn traits of the Saami are widespread and offer common 'explanations' for deviant behaviour without connecting this behaviour to the social issues that emerged from the relocations. Here is another example, from a non-Saami and non-relocated person who went to the boarding school in Lovozero and remembers some Saami children as inherently aggressive.

A: They were like that, **aggressive** young people; many were aggressive, I don't know why.

Q: You mean specifically among the Saami kids?

A: Yes, in my class there were many such youngsters. [...] That Vasia, the slightest wrong word, and he could start spitting at you and insulting you all over the place, or he would kick you. There were many such boys. [...] Tolik was also a psycho, and Valia was. [...] They were always so high-strung; you couldn't say the slightest wrong thing to them. (GP 2015)

The story goes on, ending tragically with one of those aggressive classmates

murdering a girl when he had been drinking. The interviewee does not consider that the described aggressiveness could be due to social circumstances, and she admits that she does not know why her classmates were aggressive. Aggressiveness was often a mirror of the highly precarious living conditions of the relocated people. It is widely acknowledged today that above-average aggression and violence can be a consequence of social upheaval likely to be transmitted across generations (J. Atkinson, Nelson, and Atkinson 2010; Bombay, Matheson, and Anisman 2009). The high violence and mortality rates among the relocated groups confirm this (see section 8.5.6., *Violent death and substance abuse*).

The quoted oral history testimonies confirm the point I made above that widespread despondency among the relocated people – without its causes being understood by the majority population – only reinforced stereotypes connected to the ideologised Soviet hierarchy of nations. In a vicious circle, misunderstood despondency and despair acted as a social reproduction mechanism, exacerbating the despondency. The following sections will show that the main reasons for these social problems need to be sought in the relocations rather than in innate characteristics of the relocatees.

8.5.3. Housing shortage

A wry saying that I heard during my fieldwork in Lovozero many a time when agreeing about a meeting in a flat was that the ground and top floor are commonly called “the Saami floor” (*saamskii etazh*). This refers to the typical five-storey apartment blocks. Without exaggeration, I can confirm that most of the interviewees from relocated families whom I visited at home lived either on the ground floor or the top floor. Why?

Overcrowded houses and flats, fights over promised flats, endless waiting times until construction was completed, in short an enormous and chronic deficit of living area – this is the most prominent of all post-relocation issues named by field partners throughout my fieldwork. Issues relating to housing and property in the wake of relocations have been widely acknowledged by other scholars (Konstantinov 2015, 164) and for other Soviet Arctic regions (Vakhtin 1992), but nowhere do these references go beyond a short mention.

For Josephson (2014, 239), urban planning and settlement in the North looked “messy, irrational, and disorderly”. I would add “depending on which criteria and from whose perspective”. For mid-tier state bureaucrats, nominally reaching the primary, countable goals that mattered to the leadership had priority over the secondary goal of creating comfortable conditions for the population; hence, one sees waiting times of many years – and sometimes decades – for the relocated people to receive a flat. Dwelling space per resident statistics looked gloomy. By the end of the 1970s, it was between three and seven square meters per head among relocated northern people across the Soviet Arctic (Vakhtin 1992, 26; Pika 1996, 15). This

range is comparable to that for in Lovozero, calculated in the late 1980s by Liubov' Vattonena, former head of the district's statistics bureau and later a Saami activist, and shared with me in an interview. The numbers according to ethnic affinity were three square metres per head for the Saami, seven on average, and twelve for the Komi (LA, EP 2015). The high number for the Komi can be explained by their historical presence since the nineteenth century, when they built relatively spacious houses, many of which survive to this day. The average number includes the incoming Soviet population, who usually reside in flats. The low average among the Saami is due to the relocations, in the aftermath of which the Saami remained for decades at the bottom of the housing statistics, waiting in overcrowded conditions for the promised flats.

This is the answer to the ground-and-last-floor question. These floors were (and still are) considered the least popular, due to potential burglars and leaking roofs. In the light of the local power hierarchies after the relocations (see section 8.5.2., *The hierarchy of nations becomes visible*), the relocated people, often in the position of supplicants, were more often than not given those floors, while the better floors were more likely to be distributed among those with better connections, as suggested in the following excerpt:¹⁰

Q: In the beginning, did they promise you that they [the authorities] would also move your own houses for you [relocation 12, Table 3]?

A: They said that they would, then they [went back on their word and] said that you will live there and they are already constructing a building for you. That building was being built indeed, but then they all moved in themselves. That four-storey building behind the river.

Q: Who moved in there?

A: The bosses. They all crawled in. All went there.

Q: You mean those from the district executive committee?

A: Yes-yes-yes, they all went there. Basically, all the bigwigs moved in. (AI 2013)

And when we moved to Lovozero [relocation 9, Table 3], who do you think was given a flat? The kolkhoz chairman. (NE 2013b)

Archival documents and oral evidence bring those details of relocation to light that led to a catastrophic housing situation. Thus, once it was publicly known that the Voron'e settlement was going to end up under water due to a new hydro-electric power plant, the administration decided to pay for the transportation of the villagers' personal items to Lovozero but at the same time obliged them to "tear

10 The third floor is commonly considered the best one and, in a typical form of unconscious everyday racism exploiting another 'national' stereotype, sometimes called the "Jewish floor" (*evreiskii etazh*).

down [*proizvesti likvidatsiiu*] their private houses by their own efforts and to write them off according to the due procedure” (f.146 op.5 d.3 l.116, 1965). The “due procedure” meant that the block houses had to be handed over to the kolkhoz *Tundra*, which re-used some of them elsewhere as temporary housing and sheds in the herding camps (LA, EP 2015). Moreover, as the Voron’e kolkhoz had been prosperous in previous years, several families had only recently built new log houses for themselves, for which they took out loans from the *Sberkassa*, the Soviet savings bank. These families were obliged to keep paying off these loans – after having torn down their houses, handing them over to the Lovozero kolkhoz, moving to Lovozero with a minimal amount of personal goods and while living for years in extremely overcrowded conditions in randomly found places (MA 2013; LA, EP 2015). Two field partners recall this in the following:

A2: Our [parents] had just finished building their house. Mum and Dad had built a new house in Voron’e, and after a year they resettled us [relocation 9, Table 3]. That house was moved to reindeer herding camp five. But our parents, you know, they had taken out a loan in order to build this house. And when they moved here, to this kolkhoz [in Lovozero], their [previous] house was being used by this kolkhoz [at the herding camp], while the parents **still had to pay off their loan!** They were still being ripped off ((speaking heatedly)).

A1: The kolkhoz took away the house.

A2: Which was no more [in possession on the parents].

Q: I heard the same from Agaf’ia [another interviewee].

A2: Well yes, she’s my relative [...]. They were our neighbours, their house also-

Q: Yeah, they also took their house to the herding camp.

A2: Yes.

Q: And they also kept paying off the loan for it, a house that was being used by others.

A2: Yes, yes=yes. For the Voron’e people it was like this. [...] If they had told people well in advance that they were going to be relocated, that-

A1: The people wouldn’t have spent the money [on building a house]. [...].

A2: And our [parents] say, we wouldn’t have started building [if we had been warned about the coming relocation], we would have kept living in our parents’ home, the entire family, like in old times, [until being relocated]. (LA, EP 2015)

From the grassroots perspective, this was a cynical and humiliating course of action. From the point of view of the state, however, we find another, legalist logic: people were offered supposedly better housing in Lovozero, and a majority accepted this offer in the village vote about the merger with the Lovozero farm and relocation to the village of Lovozero. Giving people new flats for free was seen as appropriate compensation for lost houses, no matter whether they would be flooded or taken over by the kolkhoz and dismantled, moved and put back together for the kolkhoz’s

own purposes. Formally, this would have nothing to do with people's previous financial obligations, which would therefore not be waived.

There turned out, however, to be a major flaw in this plan: upon arrival to Lovozero, people did not receive any flats. The house for the Voron'e relocatees was completed only several years later. Even then, for the first years the house was not connected to the gas network and people cooked in the flats with improvised firewood ovens (AM 2015; EP 2015). Moreover, receiving a flat often involved struggles, as the better units of the new housing tended to be distributed among local elites. Sometimes, writing to the higher authorities in Moscow or the mere threat of doing so could yield positive results (AI 2013; ZP 2013). In the meanwhile, people had to find a way to cope: living with relatives or squatting and improving old, empty houses were the common options, as noted in the following:

[You remember] Liuda, my cousin, I already talked about her. So, their family, ten people, and our family, we were nine people, we all lived in one house in Lovozero [two-rooms, about sixty square metres]. (AD 2013b)

I belong to the [local] Lovozero Saami, yes. But those who were relocated, when they came here, they lived, for example, at the homes of both my grandmothers. My mum even keeps telling how my sister and me slept under the table because they resettled those people but didn't give them housing. And so the Saami here slept literally on each other, so-to-speak, or in shifts. Those were of course terrible times, yes. (LP 2013)

As already mentioned, for the people of Varzino the situation was even worse. Vatonena (1988), herself from Lovozero, remembers how eleven relatives from Varzino moved into her home. All in all, they were sixteen people in two rooms for five years. As in the aftermath of the Voron'e relocation, so, too, after the Varzino relocation a new apartment building was built. The catch was that it was not built for the relocated people but for the local members of the *Tundra* kolkhoz. There was a formal explanation for this. Non-viability meant that the "the members of the kolkhoz *Bol'shevik* [Varzino] submitted a request [*pros'ba*] that they be included in the kolkhoz *Tundra* [Lovozero] and *Tundra* gave its consent" (f.146 op.5 d.16 l.74, 1968; see Illustration 10, lower document). With this consent, *Tundra* kindly ceded its own, dilapidated communal dwellings to the relocating people from Varzino, while moving its own, local staff into a new apartment block (f.146 op.5 d.16 l.96, 1968; see Illustration 10, upper document). Both documents show how the relocatees were put into the role of supplicants and recipients of aid.

СОБРАНИЯ УПОЛНОМОЧЕННЫХ ЧЛЕНОВ КОЛХОЗА
"ТУНДРА" от 29 апреля 1968 года.

1У.СЛУШАЛИ:ПОСТАНОВЛЕНИЕ ЧЛЕНОВ КОЛХОЗА
"БОЛЬШЕВИК" от 14апреля 1968 года.
ОБ"ЕДИНЕНИИ В КОЛХОЗ "ТУНДРА"

ПОСТАНОВИЛИ:

1.С об"единением колхоза"Большевик"в колхоз "Тундра"
с 1января 1969 года согласиться;

а/решении вопроса о передаче оленепастбищ бывшей
территории М.В.О.С.колхозу"Тундра" занимаемых
сейчас колхозом В.И.Ленина.

б/с освобождением в с.Ловозеро коммунальной жи-
лой площади, занимаемой колхозниками сельхоз ар-
тели "Тундра"при переселении в благоустроенный
жилой дом,передать ее для жилья членов колхоза
"Большевик".

Исполнительный комитет Ловозерского районного Совета де-
путатов трудящихся р е ш а е т :

1. Учитывая экономическую целесообразность и просьбу
колхозников колхоза "Большевик" о присоединении их к колхозу "Тун-
дра" и согласие колхоза "Тундра" о принятии их в свой колхоз реше-
ния собраний утвердить и рекомендовать правление колхоза "Тундра "
/председатель колхоза тов.КУЗНЕЦОВ Г.С./:

а/ принять колхоз "Большевик" по годовой инвентаризации
в IУ квартале 1968 года;

б/ переселение колхозников из с.Варзино осуществить до
конца 1968 года по мере освобождения жилья;

Illustration 10: Two examples of archival documents from the Murmansk Regional State Archive (f.146 op.5 d.16 l.96, 1968; f.146 op.5 d.16 l.74, 1968) dealing with the merger of the 'non-viable' Varzino kolkhoz with the 'viable' Lovozero kolkhoz and ensuing housing issues.

Thus, the chronic housing shortage after the relocations led to overcrowded flats and generally difficult material and psychological circumstances for the relocated families. I have shown in Article 2 that placement in boarding schools was a consequence of these hardships: an easy way for the local authorities to – apparently – alleviate these problems, and to massage the housing statistics, was to send the children of relocated families to the boarding school. Afanasyeva (2019) came to the same conclusion. In Article 2, I call this “the boarding school paradox”, as the parents of boarding school pupils lived in the same settlement. Again, Vattonena was the first to locally denounce in public the previously unmentionable connection between the housing shortage and boarding school policies: “It was easier to take

away the kids than to give housing” (1989b). However, this way of ‘solving’ housing problems was neither unique to the Kola Peninsula nor was it always concealed. Krupnik and Chlenov (2007, 70) quote one pro-Soviet Yupik activist praising on the circumstances in Chukotka after the relocations: “All the Eskimos live in pretty, comfortable frame houses. [...] A big school has been built. Now pupils live in the boarding school, supported completely by the state.” The apparent paradox – at least from an outsider’s perspective – is seen here as a completely obvious way of handling the situation. Afanasyeva discusses the phenomenon as partially welcomed by relocated parents; on the one hand, the boarding school was a resource offering shelter, food and clothes after the relocations but, on the other, it increased post-relocation social stigma and emptiness (2019, 79, 186, 191–92). Clearly, for all my interviewees it would have been preferable to have been given the promised housing on time instead of being offered ‘help’ in the form of removing their children from overcrowded temporary dwellings:

When we [the rest of the interviewee’s family] moved to Lovozero [from Voron’e; relocation 12, Table 3] we lived ... they gave us a wooden hut. [...] There were two families living there: my parents’ family and my mother’s sister’s family. The smallest kids lived with them, of course. But we, all the others, studied and lived at the boarding school because with only two small rooms [for both families] there was simply not enough space. (AS 2014)

There was some kind of order that went out to get the children away from their families. That was really bad. Ideally, the boarding school should have been for kids whose parents were in the tundra, who were **not** in the village. And that’s how it was initially. Or for the children from remote settlements like Kanevka, Sosnovka and Krasnoshchel’e.

Yes-yes.

But then they introduced this rule that all kids should sleep at the boarding school. That was, of course, a bit on the heavy side. (NA 2013)

There was no space to live [referring to the shortage of flats for the relocated families]. When I was visiting my mum [during boarding school leave on weekends], I saw how they lived there. My mum was happy, of course [that I visited her]. (SG 2013)

School statistics confirm this picture. For example, for the 1974-75 schoolyear, the list of pupils at the Native Boarding School in Lovozero shows 160 Saami, 57 Komi, 6 Nenets, 13 Russian children from remote settlements, and 10 Russian children from other districts (f.146 op.5 d.194 l.126-132, 1974) – despite the fact that all remote Saami settlements had been already eliminated and all families of those Saami children resided in Lovozero. These numbers also show that, while Lovozero had slightly more inhabitants of Komi descent than of Saami descent,

there were far fewer Komi children living at the boarding school; they tended to go to the regular daytime school, as there was no pressing need to solve any housing problems for them. In the school year 1977-78, the boarding school in Lovozero had 290 pupils, 43 of whom had parents working in the tundra and 51 of whom were from the remaining distant villages. 40 had parents whose parental rights had been administratively revoked, 11 were orphans and 4 had parents serving a prison sentence (such pupils could come from anywhere in the Murmansk Region, see Afanasyeva 2019, 255-266). The remaining majority, 141 pupils, were children with 'regular' parents in Lovozero, that is, ones who had all or some of their children in the boarding school due to the shortage of living space (f.352 op.1 d.60 l.4, 1978).

It took years for more apartment blocks to start appearing and gradually relieve the housing shortage. With the construction of new apartment blocks, several other noteworthy settlement dynamics came into play. Some areas of the old Lovozero village were torn down in order to make space for new apartment blocks. These were located mainly on the side of the village that since the Komi immigration of the previous century had become the Saami side. The Komi side could keep its houses (see Illustration 11). This was presented by several interviewees with Saami roots as one more piece of evidence showing who had a say in Lovozero at the time (the Komi). While it is hard to make a final assessment, I could observe normative values among a Komi interviewee about what counts as good housing (Komi) and what as bad housing (Saami), implying that the latter is less worth being preserved.

Of course. Well, here [in Lovozero] they [the Saami and the Komi] lived separately, too. The Saami, for instance, used to live on that side of the river, the Komi on the other ((chuckling)). You could see it straight away, the Saami had midget houses with low windows, while the Komi houses were much taller ((appreciatively)). (SI 2014)

Besides normative values, material interests were certainly present, and power hierarchies at play. However, it is impossible to make an unequivocal determination whether it was discrimination or not to build apartment blocks on the Saami side of Lovozero. The dominant narrative of discrimination among my interviewees has it that the local Komi in Lovozero did everything to keep their old houses, explaining thus why old buildings were torn down to give space to apartment blocks only on the Saami side of the village. The old houses had the advantage of offering more space for living, for tundra gear and for 'parking' draft reindeer, features that many relocatees missed in their flats. An alternative narrative claiming discrimination has it that blocks of flats, offering amenities like electricity, gas and running water, were initially intended only for the relocated Saami, leaving aside possible wishes of the locals to obtain a flat as well. This narrative gains in plausibility when we consider how actively locals tried to secure a flat for themselves in the new apartment blocks. Once the new blocks were built, many locals (mostly Komi) wanted to trade their

old houses for a flat with some relocated Saami family. On balance, without wanting to understate the hardships endured by the relocated people, we can say that these widespread barter relativise the dominant account of discrimination to a certain extent. At any rate, the two versions of discrimination need not to be seen as mutually exclusive.

In any event, barter did take place and there are several accounts in my data about improper deals and subsequent loss of property, to the disadvantage of some relocated families (LA, EP 2015; AD 2013a; AS 2014; however, there is no space here to describe these in detail). These stories of being short-changed usually ascribe the losses, on the one hand, to insider knowledge and the connections of some locals and, on the other, to the general stress situation, credulity, low literacy and hence lower legal proficiency of some relocated people. While to some extent these narratives exploit local ethnic Saami-Komi stereotypes, we may also interpret the relocated people's credulity and nonconfrontational demeanour as the habitual, pre-relocation way of avoiding conflict in a very small community where people depended and relied on each other heavily. Trust as well a tendency to give in rather than let a conflict escalate are qualities likely to have been exploited by some devious individuals, especially in Lovozero's economy of shortage (cf. Stammner 2011).



Illustration 11: Top: Views from the fifth floor (the “Saami floor”, saamskii etazh) of apartment blocks towards two opposite sides of Lovozero. The picture on the left shows one of the areas with new apartment blocks built between the 1960s and 1970s, for which single-family houses had to be torn down. Before the blocks were built, this side was predominantly (but not exclusively) inhabited by local Saami families. The picture on the right shows part of the village on the other side of the Virma river, traditionally inhabited predominantly by Komi. No apartment blocks were built here and the area has a large number of old houses stand to this day. Bottom: Interview partner Apollinariia Golykh in Lovozero. In the background some of the apartment blocks intended for the relocated people. Left image by Nuccio Mazzullo, 2014; right and bottom images by the author, 2013.

In sum, the more the number of relocations that took place, and the more the viable village of Lovozero became ‘saturated’ with the ‘non-viables’, the less eager the district administration and the *Tundra* kolkhoz/sovkhoz became to implement the consolidation and relocation directives from above. Towards the end of the

relocation cycle, everything was done to make it look like the people were not *being* relocated but that they simply *were* relocating. As a result, the displaced people had to confront a catastrophic housing situation.

Many people felt deceived and deeply regretted the relocation. In the following, I quote a field partner who expressed these regrets:

A: And so we married, he [the husband] worked in the club [in Voron'e, relocation 12, Table 3]. Then they invited him to Krasnoshchel'e to work there in the tundra [one of the roadless villages, which survive to this day]. He shouldn't have refused. Later, of course, we regretted that we didn't go there.

Q: So he got a job offer there?

A: Yes, they had offered him [an opportunity to work there]. We could have moved there and we would have lived there in a house of our own. And we would be living there to this day.

Q: You regretted that after you were moved here [to Lovozero]?

A: Yes, yes. When we became more adult. We were so young back then. I was twenty-one, he was twenty-five. We were looking for the wrong things back then, maybe. Later, Grandpa [meaning her husband] kept saying: "We shouldn't have come here, not for anything. We should have moved to Tumanka [another surviving village, at the coast], there we would have had a house [like in Voron'e]. There, he would say, there's fewer people around, but there's also a road. We would have lived there, and we would be living there now. (AI 2013)

However, Lovozero did become the new place of residence for most of the relocated people. The housing situation did not improve until the 1980s, when additional houses were finally built. The discrepancies denounced by Vatonena had a certain effect on a commission that came from Murmansk, the regional centre, which was confronted with Vatonena's numbers during a meeting: "A commission from the Regional Committee came [to Lovozero], and they invited me. [...] But I was a member of the Party, so there was no way for them to ignore me." (LA, EP 2015). Today we have the reverse situation: people may well have a range of flats, boat-houses and other property that is useful but expensive to maintain.

Fifty years after the last relocations, housing issues as the main stressor are a thing of the past. However, the damage done by the secondary consequences of these housing issues should not be underestimated, for it can be seen to this day. Protracted housing issues had an utterly detrimental effect on the mental and physical health of the relocated people. It is beyond doubt that precarious housing conditions and, for some, the lack of meaningful work (see section 8.5.4., *Lack of meaningful occupation*) lay at the root of the widespread despair that followed, with all its dark consequences. These have included substance abuse, removal of children by the authorities, violence, suicide and homicide (see section 8.5.6., *Violent death and*

substance abuse), and the concomitant stigmatisation (see section 8.5.8., *Blaming the displaced people*).

8.5.4. Lack of meaningful occupation

Many interviewees talk about unemployment in the aftermath of the last relocations to Lovozero.

Covert unemployment arrived [*neglasnaia bezrabortitsa*], if you understand what I mean. Lovozero was no exception because the town was overpopulated. They closed the Kil'din and the () settlement, then they went on to dismantle Voron'e, Iokan'ga and Varzino. But they had to do something with the people. So they decided to cram them all into Lovozero. But this town had its own people. That's why jobs were too few. (NE 2008)

Here we didn't take root. If they - I mean how they resettled us - If they had built housing here, if there had been work. Well then, yes, the population could have been saved, especially the older generation. And the male population. But nothing worked out here, they let matters take their course: whoever can [work] has to look for work; if he finds it, he will live; if he doesn't, he won't, that simple. (AA 2014)

Unemployment must not be understood literally, because in the Soviet Union there officially was no unemployment. Formally, everyone had to have a job, and the local authorities tried hard to fulfil this obligation. The problem with this task was simple: there was not enough work. Sociologists of the late Soviet times calculated that a typical consolidated 'native' settlement could offer real jobs to about one-third of its residents, while others were employed as unskilled, auxiliary, administrative or temporary personnel with very little to do (Slezkine 1994a, 347). Pika (1996, 17) called this phenomenon the "lumpenisation" of indigenous people: resettlement and labour reorganisation turned many indigenous people from highly skilled specialists into unskilled casual labour.

A: When we moved in '68 my Tolik [brother], he didn't have anywhere to live. He used to work as a horse-keeper. His habitual work in Varzino [relocation 9, Table 3] was to be a horse-keeper and a reindeer herder. In practice, [after the relocation] he had to become a temporary worker. Do you understand what I mean?

Q: He would not be given a permanent job?

A: There was no permanent work. There are campaigns. For example, it's reindeer slaughtering season, they need working hands, so for that time they invite him to work. For the slaughtering campaign. Yes, of course, he knew well how to do that, he worked with reindeer. And he knew how to skin them; in short, he was a much-needed person when he was at work. [...] But when there was no work, he was without work. (NE 2013a)

This is, of course, only one side of the coin, the other side being the new career opportunities following from the expanded educational opportunities. If we dwell here on the topic of de facto unemployment, it must be said that it concerned mainly people with a low level of formal education and with less proficiency in urban activities, which means mostly both sexes among the older generation and men among all generations (see section 8.5.5., *Gender split and erosion of family structures*, and Article 4). Every able Soviet citizen had the dual legal status of having a right and being obliged to work; this status was anchored in the pervasive discourse of labour (*trud*) and enshrined in Article 12 of the Soviet Constitution (Vladimirova 2006, 109–10). In post-relocation Lovozero, which was saturated with people and excess labour from the merged kolkhozes, the administration tried to solve the problem with coercive job allocation. It was a practice, however, which was resisted both by employees and employers. Archival evidence speaks volumes about this. For instance, a ruling by the local administration states:

Citizen Ivanov I.E. [name changed], born on [...], living in [...], leads a parasitic life, drinks, has not been working for more than four months, disturbing the public order. [...] According to the Decree of the Supreme Soviet of the RSFSR [...] on the 'Fight Against People, Who Sheer Away From Socially Useful Work and Pursue an Anti-Social, Parasitic Lifestyle' send the citizen Ivanov I.E. to the sovkhos *Tundra* for compulsory job placement. Ivanov I.E. must appear at the sovkhos *Tundra* and seek employment before 17 April 1974. The director of the sovkhos must employ citizen Ivanov I.E. (f.146 op.5 d.210 l.202, 1974)

Such rulings are frequent discoveries among the archival documents of those years that I analysed, and in Lovozero most of these rulings bear the names of Saami people, thus confirming the oral evidence from narrative interviews. The pattern is always the same: the political administration formally fulfils its responsibilities by forcing employees and employers to enter into an employment relationship. The question whether the employer in question can offer actual work to the person in question is silenced. It should be no wonder, then, that employers resisted this practice, as stated in this report:

The administrative institutions of the district [...] are working towards employing and re-educating people who do not carry out socially useful labour. [...] However, their work has to be improved. [...] The Employment Commission is not demanding enough in dealing with the leadership of enterprises [...]. There are cases when the administration of enterprises refuses to employ people sent to them for compulsory employment. (f.146 op.5 d.192 l.6-7, 1973)

To summarise, post-relocation employment issues led to tensions between the political leadership, the local administration and the relocated people. De facto unemployment transformed many people, especially men, from previously respected community members with needed skills into members of society with the lowest status. It is the sum of long-term employment issues and housing issues that led to widespread illbeing after the relocations. What to most of my interviewees must sound like a truism has been put elsewhere into the concise scientific language of psychology:

Of all forms of social class change, long-term unemployment [and I would add: a long-term housing shortage, L.A.] is likely to be the most damaging because it combines the undesirable direction of social mobility (downward) with the fact that the change is outside one's control and often unexpected (psychological shock). (Simandan 2018, 259)

As we have seen already from the archival examples quoted above, which show the alternating rhythm between idle phases of “parasitism” and coercive employment, this downwards post-relocation social mobility had extremely negative consequences for the mental and physical health of large parts of the relocated population. I will look more closely into these negative consequences in the sections 8.5.6., *Violent death and substance abuse*, and 8.5.8., *Blaming the displaced people*.

8.5.5. Gender split and erosion of family structures

If we see displacement and sedentarisation as part of a macro-scale social engineering project (see section 5.5., *Social engineering*), namely, forming a new Soviet society, we can explain the strong erosion of previous family structures and gender roles and instatement of new ones that followed the relocations. The main pillars of this erosion are conspicuous: a strong gender division in professional life resulting from a top-down labour reorganisation; the exogamy of women; the resulting widespread bachelorhood of indigenous men; and the practice of sending children to boarding schools. Article 4 discusses at length the erosion of family structures and the new gender roles. I refer the reader to the relevant section of the article (pp. 10-12) before proceeding to the following additional commentary.

The basic message in Article 4 on gender dynamics can be summed up as follows: The losers of Soviet social engineering in rural Arctic places like the eastern part of the Kola Peninsula were mainly men. Where consolidation (*ukrupnenie*), relocation, sedentarisation and a reorganisation of labour and education were the main features of social transformation, men tended to suffer a loss of status in society. The winners tended to be women, with a likely gain of status thanks to the positively connoted ‘female’ roles they were offered by the majority society in the new urbanised setting. These tendencies have been recently confirmed by Vladimirova and Habeck (2018)

in their overview of gender in Arctic social sciences, in which they caution, however, that the Russian North is diverse and that regional or local situations can differ from the general picture. In the Soviet Union, indigenous women were more likely to find an activity in which they could define themselves in a meaningful way according to discursive categories offered them relating to ‘culture’ and status in society. Indeed, as Konstantinov (2015, 162) contends, the new living circumstances “placed ambitious young women on career tracks undreamt of in the context of pre-agglomeration semi-nomadism [...]. Agglomeration, despite its devastating consequences, should be examined critically from this vantage point also”. posits Konstantinov

As shown in Article 4, scholarship has established that women being ‘natural allies’ of Soviet power resulted in attractive new educational and professional options, and the social rise of women – at least according to the new, imported social values – can count as a widespread feature of Soviet power. We should, however, be careful not to overgeneralise. There is no rule without exceptions, as for example shown by Florin’s (2015) study of the Kyrgyz intelligentsia. In his doctoral thesis, he suggests there is a “domination of Kyrgyz men in the cultural and intellectual life” throughout and after the Soviet times, while “women and ethnic minorities have been and are heavily under-represented in these areas” (Florin 2015, 262).

Throughout the Arctic at least, we can conclude that promoting the rise of ‘the educated woman’, in combination with the other radical socio-economic changes already mentioned – has been a rather consistently realised goal of Soviet power (important contributions here, in addition to those cited in Article 4, are Tuisku 2001; Sherstyuk 2008). The new opportunities, however, came at a cost. Young people’s ambitions – mainly those of girls, for the reasons explained above – developed in boarding schools, when families were destroyed by more or less forceful separation or as a desire to fight and to escape post-relocation despondency and despair (Article 2; Allemann 2019; Afanasyeva 2019). Boys, on the contrary, tended to feel less comfortable with the kind of care, tasks and challenges offered by the boarding school in an urbanised life instead of the family in a rural life. Many interview partners recall such differences, as the following example indicates:

I wouldn’t say slow progress [about bad grades among pupils]. They were simply not interested at all in studying. Absolutely not interested. They just wanted to go home, to their parents, where they could be in their usual environment, where they could speak their own language, where they didn’t feel subject to any prohibitions. But we [my friend and I], when we came to the boarding school, we decided to study hard. Me and her, we finished [high] school. Already when we were in about the seventh grade, we told each other: “Okay, we will study well. We will finish [high] school, and we will get a higher education, no matter what.” We kind of swore it to each other. And we did it! (AS 2014)

But my [younger] brother, maybe the times had already changed a little bit, he successfully fought for his right to live at home. They were so fed up with him! [...] He would just go away, away from the village, where his mum was in the fields for the haymaking. Or in winter, he would take his boots and his hat and walk all the way, fifteen kilometres. And so finally they gave up, so the child wouldn't go completely ... [mad]. He was simply stubborn. (LA 2013)

Not only interviewees discuss the gender imbalance between insurgency and docility; it is also reflected in archival materials. Thus, documents from the Lovozero administration show that in the second half of the 1970s, among minors caught for law infringements, about 90 per cent were boys (f.146 op.5 d.215 l.6-7, 24-28, 53-54, 1978). There were and are of course also boys with ambitions to achieve educational heights. However, tundra work – which requires skills that are not provided in formal education – was conceptualised through the majority society's norms (see the *osvoenie* discourse mentioned in sections 8.0, 8.1., and 8.2.) as purely male work, and hence the educational system also pushed young men to 'turn to the tundra'.

As Vitebsky has put it: "Village girls say that they could not imagine marrying a herder, because of their lack of conversation: Television and magazines portray life as a torrent of words and easily revealed passions [...]. To them, the men's communication with animals, rivers and mountains which make much speech unnecessary seems maladaptive. Rather than a talent, it is now interpreted as a deficiency." (Vitebsky 2010, 42). Ulturgasheva offers a convincing approach to understanding this apparently higher 'dysfunctionality' of the male part of the population, which resulted in its lower resilience in urbanised settings:

Unlike the forest, the village is a space which mostly involves human–human (rather than human–animal) relations. Forest challenges and risk situations shape the survival skills and resilience of an adolescent which are compatible with the space of forest; however, even competent forest youths remain more vulnerable in the face of socially risky situations associated with the space of the village (separation from parents, bullying, interpersonal violence, and alcohol abuse). Hence, those who are viewed as resilient in the forest may be vulnerable when facing the necessity of staying in the village, and those who may cope well and show signs of resilience in the village may become quite vulnerable in the forest. (Ulturgasheva 2014, 646–47)

This is a crucial observation. We do not see one gender being resilient, while the other is not. Rather, different genders develop resilience in different areas, depending on the social norms absorbed by them through education and discourses. Many women admit that they avoid the tundra now, because they have lost their 'forest resilience', as these field partners remark:

But I would like to add, you know what? That my dad was a reindeer herder, he was a reindeer herder, but mum would never go with him to the tundra. And as my mum never went to the tundra, we never lived there. I don't know how to live in the tundra. I never was out in the tundra. And this maybe somehow had an influence on us, I don't know. (AS 2014)

Q: Have you ever thought of going back to the lifestyle of your parents?

A: Oh, no. I've never even thought about it. I wouldn't mind visiting and spending some time where my ancestors used to live. But to work there, clean up after somebody, no thanks. I'm quite squeamish about these things, you know ((laughing)). (VA 2013)

Thus, the gender rift was initiated by the separation of labour and dwelling places after the relocations and resulted in very different male and female lifeworlds. This rift was then transmitted to the first post-relocation generation by inculcating the expected gender roles through educational, professional and cultural norms in the new settlement. Those men who had been relocated as adults soon died away (see next section). In the next generation, the men again started dying off earlier, because the gender roles inculcated from school age left them without prestige and many of them without spouses. The bachelorhood of herders affected not only Saami men, but also Komi and those of mixed origin who engaged in herding after the sweepig social reorganisation (Konstantinov 2015, 234). However, even before any ethnic activism had awakened, sociologists (Dobrov, Toichkina, and Korchak 1985) had recorded exceptionally high exogamy and bachelorhood rates for the Saami in particular. This arguably relates to the especially low social status of relocated Saami men, as a combination between, on the one hand, the generally devaluated rural professions and, on the other hand, the notoriously poor material and psychological situation of the relocated groups. The following interviewee names these factors as a reason why women tended towards exogamy:

A: All this [drinking] everywhere, in Varzino [relocation 9, Table 3] we didn't have that. I don't remember that in Varzino or Kanevka people would linger around with bottles, we didn't have that. [...] From the young Varzino men of that time, I cannot tell you one who did not lose himself in the bottle. They all went on the bottle. [...] The women turned out to be stronger in this respect.

Q: So, could we say that the most vulnerable part, if we make such a conclusion, the most vulnerable part of the population became the men among the relocated Saami.

A: Yes. That's how it was. Here you can't think of or add anything to that. I can't tell you about anyone [men] who would have married here, about any weddings. The girls yes, they married.

Q: Others, not their own men?

A: Well yes, in the sense that they moved here [to Lovozero] and married other men. If

those drink like that, who will want to marry a drunk? Drunkards, that's what they said. "Bugger off, you drunkard". (EK 2014)

Indeed, between 1975 and 1979 of all marriages among Saami people, 82.1 per cent were mixed. In these mixed marriages, in 79.5 per cent of the cases the woman was Saami, the other 20.5 per cent the man (Dobrov, Toichkina, and Korchak 1985, 98). Bogoyavlensky put the same problem in more general terms: among all northern indigenous people, significantly fewer men than women get married, due to the preference for exogamy among women. The low incidence of marriage among men contributes to their social ills and higher mortality (1996, 37–38). Among the Komi, exogamy has been a much lesser problem (multiple evidence from my own fieldwork).

In the following section, I will look in more detail into the devastating morbidity and mortality connected – both as causes and consequences – to the post-relocation decay of family structures.

8.5.6. Violent death and substance abuse

There is a rather large corpus of literature and statistical sources on indigenous mortality and morbidity, usually to be found in the work of scholars from medical or quantitatively oriented social sciences. While they offer very valuable data, these studies reveal a common problem: they do not delve into detailed interpretations of the social factors behind the numbers. This is where the present section seeks to make an additional contribution. I will do so by combining the grim picture provided, on the one hand, by figures from such literature and directly from censuses and archival materials and, on the other, by evidence collected in narrations by field partners.

Precisely while the Soviet Union was celebrating its achievements in bringing healthcare to the remotest corners of the country, a major shift occurred in causes of mortality in the indigenous North. While the 1950s still saw infectious diseases as a major cause of death, by the 1970s injury, suicide, murder, and other forms of violent death prevailed as causes, entailing a general shift of the mortality from children and the elderly to middle-aged people, especially among the male population. Vinokurova (2010) calls this phenomenon male “ultramortality”, mortality at working age being three to four times higher among men than among women. In their confidential report to the local administration, Dobrov et al. (1985, 98) pointed to the following pronounced gender imbalance among the Saami population in the 1979 census:

Table 4: Ratio of men to women in the Saami population in the Soviet Union according to the 1979 census.

Age	Men	Women
0-19	100	99
20-54	100	<u>119</u>
55+	100	<u>182</u>

The significant disproportion in the oldest age category in 1979 was attributed in the report mainly to Second World War losses, whereas the less pronounced gender imbalance in the middle-aged category was attributed to a high rate of violent death among males, albeit no explanation of the reasons was given. A quick look at the 2010 census reveals the following picture (“Itogi Vserossiiskoi perepisi naseleniia 2010 goda.” 2010b):

Table 5: Ratio of men to women in the Saami population in the Russian Federation according to the 2010 census.

Age	Men	Women
0-19	100	107
20-54	100	101
55+	100	<u>472</u>

Compared to 1979, by 2010 the middle-aged category stabilised, pointing to a recovery from the male population’s worst social distress. Conversely, compared to 1979, we can see in 2010 a dramatically increased gender disproportion in the oldest age category. This tendency is consistent with the temporal difference between the two censuses: the oldest generation in 2010 is the one that in the 1979 census was in the middle-aged category; over time, the 1979 middle generation’s elevated male mortality translated into the older generation’s male ultramortality of 2010. As we know, the bulk of the relocations hit in the 1960s, which means that the people of today’s oldest generation were young adults or children at the time of the relocations. The oldest generation’s disproportionate male mortality in 2010 (mainly deriving from social ills after relocations) was several times higher than that of the oldest generation’s in 1979 (mainly deriving from the war, but also from Stalin’s terror). These numbers sustain my previous claim based on oral history evidence (Allemann 2010; 2013) that it was not Stalin’s terror but the relocations under Khrushchev that constituted the most severe trauma for Russia’s Saami population – especially the male population – in the twentieth century.

To show the full weight of post-relocation ultramortality, I must take this palette of numbers even further. The figures on Arctic populations in Russia's North presented in various sources speak for themselves, and Russian Sápmi is no exception. The share of violent death (trauma, accident, acute intoxication, suicide and homicide; in Russian sources usually termed "external" [*vnesbnie*] or "unnatural" [*neestestvennye*] reasons) among the Saami in the Lovozero District developed remarkable dynamics over the decades after the consolidation policy had started: in the years 1958-59, on the eve of the relocations, the share was 22.6 per cent. From 1965 to 1969, simultaneously with the first relocations, it grew to 34.4 per cent. After all the relocations had been completed, the level grew quickly and remained at between 50.6 per cent and 52.2 per cent from 1970 to 1984. Between 1985 and 1988 it declined again to 34.0 per cent. It took four decades for the numbers to return to the pre-relocation level: between 1998 and 2002 the share of violent death reasons was again at 22.4 per cent (Kozlov and Bogoiavlenskii 2008, 78). I will round out the grim picture with one useful quantitative micro-scale study about death reasons conducted by Gutsol, Vinogradova and Samorukova (2007, 54–55): between 1968 and 1988 the proportion of natural deaths among the relocated people from Voron'e was at an extraordinarily low 1.8 per cent and from Varzino at 4.3 per cent, while for those from Chudz'iavr, who had not encountered the same housing difficulties (see section 8.5.3., *Housing shortage*), the corresponding figure was around 50 per cent.

To summarise, the numbers mentioned tell us bluntly that relocations were the main factor in producing widely acknowledged but poorly explained social ills: there has been a more than twofold jump in non-natural causes of death between pre-relocation and post-relocation times. While the 1958-59 figures are relatively low, in the years 1965-69, when the relocations had been partially completed, they were already higher. The period 1970 to 1984, with the highest figures – more than half of all deaths being violent deaths – corresponds exactly to the period between the last relocation and the beginning of perestroika, when, in an abrupt discursive turn, problems ceased being silenced, people were not blamed personally as much for their problems and the 'spring' of Saami ethnic revival and self-esteem began. Post-Soviet times saw a return to pre-relocation levels, which – at least in plain numbers – speaks for a comparatively positive direction of post-Soviet social development among the Russian Saami in spite of the widespread Soviet nostalgia (see Article 4).

In relation to post-relocation ultramortality, a closer look at increased alcohol abuse is unavoidable. Schrad (2014, 253) cites several scholars who consolidated scattered data and estimates indicating that alcohol consumption per head in the USSR rose sixfold between 1952 and 1980. Accurately or not, my interviewees largely confirm this trend, with the major jump in alcohol consumption being connected to relocations. Locally, as archival documents show alcoholism as a major social problem was already well known decades ago among specialists. In a report to the local administration (f.146 op.5 d.188 l.48-50, 1973), the chief physician

of the Lovozero hospital Nikita Shamlian – a person remembered fondly by many field partners of the oldest generation – claims: “Parental alcoholism arguably has an influence on the higher share of indigenous children among the remedial school pupils” (according to my findings presented in Article 2, this is only one of many reasons). A similar observation came from Bloch’s (2004, 205) fieldwork among the Evenki, where she quotes the head of the boarding school’s division for special education as estimating that “the majority of the children under her supervision were there because of foetal alcohol syndrome”. This is a further link between social despondency and boarding schools, as “many economically and emotionally broken parents could no longer provide for the well-being of their children after their relocation” (Afanasyeva 2019, 185). ‘Helping’ parents by removing the children, however, led to a recursive loop: while social destitution after relocations motivated the administrative separation of children from their families (Konstantinov 2015, 148–49), the separation itself in many cases contributed to a further emptying of the lives of parents, deprived of their children, and hence to more despair.

Alcohol abuse was also discussed as a serious issue impairing work and productivity at production sites. In 1982, of the 329 workers at Lovozero’s sovkhos *Tundra*, 48 at least once spent a night at the police’s sobering-up station (*vytrezvitel’*, a cell in the police station where drunken people were kept under custody and medical observation until they sobered up) (f.146 op.5 d.468 l.103-104, 1982). In the report quoted above, the physician Shamlian calls indigenous alcohol dependency “a true public catastrophe”, reporting that confinement to psychiatric clinics and to so-called prophylactic labour-therapy camps (*lechebno-trudovoi profilaktorii, LTP*) as a treatment of last resort was applied to 22 Saami individuals in a one-year period between 1972 and 1973 (see also section 8.5.8., *Blaming the displaced people*). For comparison, another source states that across the entire district and all ethnic groups, 1972 saw 15 orders of commitment to the LTP (f.146 op.5 d.156 l.80-83, 1972). This speaks for the higher share of indigenous alcohol abuse compared to the district’s population at large. Twenty years later, Vladimir Kaminsky, Shamlian’s successor, published numbers of “patients under observation for alcoholic psychosis” in the period 1992-1994 in Lovozero District. A comparison between Komi and Saami for all three years consistently shows a ratio of one Komi to three to four Saami under treatment among a population of 1269 Komi and 940 Saami in the year 1995 (Kaminsky 1996). This, in its turn, speaks for alcohol abuse as being also a long-term consequence of indigenous displacement.

Excessive drinking has been extensively discussed by many field partners. Beyond the well-known popular notions of hard-drinking ‘marginal’ people – which may also serve as self-reassuring mechanisms for those who point at ‘the drunkards’ (Khlinovskaya Rockhill 2010, 19, 289; see also section 8.5.8., *Blaming the relocated people*) – there are hard facts proving that in many parts of the circumpolar world alcohol addiction does have an ethnic dimension. Statistical data come mostly from

medical researchers and practitioners. At best, they deconstruct essentialist notions about the ‘drinking native’ by suggesting a mix between genetic and social reasons, although looking for the social reasons usually does not go beyond thin descriptions (an example being the volume edited by Young and Bjerregaard 2008).

In their analysis of “non-natural death and deviant behaviour” among the Saami in Russia, Kozlov and Bogoiavlenskii (2008) offer a convincing biomedical deconstruction of a presumed genetic inclination towards alcohol addiction, a notion widespread among both the general population and medical practitioners (Argounova-Low and Sleptsov 2015, 12). While there are observations suggesting that alcohol metabolism among northern populations is slower and alcohol tolerance lower than average, the authors state the following: firstly, such genetic differences are not observable along a strict indigenous/non-indigenous divide; and, secondly, it is a wrong popular assumption to see low physical alcohol tolerance as an incentive for excessive drinking, for it sooner acts as a deterrent. At any rate, the authors conclude that even if there are some physical differences, the main reasons for excessive alcohol consumption and all its further negative consequences must mainly be sought in the field of social relations and not physical peculiarities.

As mentioned, the figures on violent death clearly show the correlation between relocations and social distress, a dominant topic raised by a majority of my field partners. While increased alcohol consumption is likely to be an expression of the same stress factors, it has less visibility in statistics than violent death; only a small proportion of registered deaths state alcohol poisoning as a reason. In fact, as Bogoyavlensky (1997, 64) points out, in the case of Arctic populations “a great many drownings, freezings, suicides, and homicides occur in connection with alcohol abuse”. The author concludes with an estimate that between 60 and 70 per cent of all violent deaths among people of the Russian North in the 1980s were related to alcohol abuse. The corresponding figure among the relocated people in Russian Sápmi has been calculated to be 68 per cent between 1968 and 1988, the period between the last relocation and the first open discussions on their negative social consequences in the wake of *perestroika* and *glasnost*’ (Gutsol, Vinogradova, and Samorukova 2007, 55).

Thus, the temporal succession of relocations and a rise in mortality and substance abuse suggests that Soviet-time displacement lies at the root of the overwhelming rise in alcohol abuse and violent death. Overland and Berg-Nordlie (2012, 49–51) conclude that alcohol abuse among Russian Saami was due to the ties with their land being broken: “Alcoholism was what filled the divide between the Sámi and the very basis of their culture: their land.” This is an excessively essentialist way of putting the problem. As I have discussed above, the attitude towards relocations was ambivalent, and the prospect of living in a modern settlement was at least partially appealing to at least a part of the population. A closer look into the matter shows, however, that it was not the relocations as such but several circumstances in the aftermath of

the poorly implemented relocations that triggered self-destructive behaviour. I have already discussed these circumstances in the previous sections: ethnic disparities in local power hierarchies, housing issues, lack of meaningful work, an organisation of production favouring gender and family segregation, disorientation after boarding school and difficulties in creating a family.

In the following interview quotations, I wish to illustrate the connectedness of these circumstances to alcohol abuse. Additionally, we can see from some of the excerpts that social despondency, including suicide and homicide, spread to the relocated families' children, reflecting a pattern of transgenerational trauma (Bombay, Matheson, and Anisman 2014; J. Atkinson, Nelson, and Atkinson 2010).

And even if I look at Gremikha [a larger settlement at the Barents coast, to which some families from the discontinued settlement of Iokan'ga moved, see relocation 4, Table 3], our young men from Iokan'ga, those who were a bit younger than me, they hit the bottle. You know, maybe, it could be that- People were used to living in an environment where you are among equals. First of all, they didn't use to get an education, I mean, yes, they used to finish four to seven classes, meaning that they were simple people who did not get much formal education; they stayed, they felt good at their kolkhoz. They worked there, were respected and honoured. And here they come to Gremikha [after their village was closed down], you most probably have no proper education, nobody needs you. You get only the worst of the jobs, but you were used to working on a farm, for instance, as an outstanding milkmaid [referring to the Soviet badges of honour for outstanding workers]. And here you come and you're nobody; you go and slave away as a cleaner somewhere. What does that all mean? No respect, the people will lose their self-respect. It seems to me that's most likely the reason. (VIa 2013)

A: As a person from Lovozero, did you notice any worsening of the social situation in Lovozero [in the wake of the relocations]? Were there any conflicts, maybe?

Q: Of course, we did notice that. How **not** to notice! We know how they lived there [in the discontinued village], we know what they did for a living there. (3) When they came here, they were like disoriented, didn't know where to start, what to do, where to get and find what, how to resume the life they used to have there. Very difficult. Very complicated. And so the suicides started. (AA 2014)

Later [in the 1980s], Pasha hung himself. They were also planning to send him to the LTP; he had been taken into custody by the militia three times in March. The lad was twenty years old. He was afraid; his father had already died, his mother too. His brother had hung himself. That's already the second generation of people, you see? (NE 2013b)

Q: Thinking about all those violent deaths among the Saami, did they occur more among men or among both men and women?

A: It seems to me more among the men. I was already saying that I have almost no male classmates left [alive]. Many guys left into the hereafter. [...] For some reason, many passed away from illness. Quite likely they drank all kinds of swill. Most probably.

Q: From illness, I see.

A: One's liver or stomach doesn't last long this way.

Q: Plus all those who drowned, who-

A: Who shot themselves, who hung themselves. That same Petia who had shot Ania; he hung himself later.

Q: Himself.

A: He was to serve a long sentence, they say, but he hung himself. [...] Also Mitya hung himself, Volodia died, Sergei died, all Saami [...]. 'Almost none of the guys are left' ((sighing)). (GP 2015)

Of course, the relocated people lost themselves in drinking more quickly. The Ivanovs used to be a wealthy family, they had a good house, they had just built a five-walled house¹¹ in Varzino [relocation 9, Table 3]. And they were resettled [to Lovozero] in a house that was rotting. They barely managed to caulk the chinks in that house. And also they could not bring much with them on the ship. They took what they could of their belongings. Their older sister, Polia, she poisoned herself with vinegar and died. And Lavrentii, he was a herder, he, by the way, went through this LTP, or maybe there weren't any LTPs yet and it was the nuthouse where they put him with his alcohol intoxication. It was to the LTP, if I remember right, that his wife, after all, sent him once. He also died early. For God's sake, he barely reached forty-five. (LA 2013)

Those were **good** and strong families, good houses, and they left it all behind. One [older] woman told me: "When they came here [to Lovozero], the people lost everything, and they hit the bottle." We lost a generation, we can say that. We lost a generation. The generation of child-bearing age, many of the people passed away, began to abuse alcohol. People were thrown out of their nests. How to cope? You don't have your own roof, nothing [referring to the years of waiting time until a flat was allocated]. You lived a bit here, a bit there. Hosts are happy about guests twice: when they arrive and when they leave. But here people had to ramble endlessly and- and families broke apart. A whole generation broke apart. (VA 2013)

A: We were 13–14 years old. We were already smokers. And so, she took us to her home [in Lovozero]; there were her drunken uncles. Her grandmother [...] was always at work, 'cause she had to feed them, but those [uncles] didn't want to work, they drank. And so, they would give us drink. Well they were doing nothing else with us; they knew

11 Russ. *piatistenok*, meaning a log house with a middle wall, thus creating a divided space with two separate rooms.

they could go to jail for that. But they were making fun of observing us when we became drunk. And all of a sudden there was somebody knocking at the door. We checked; it was the teachers. Our educators already knew where to look for us. They knocked: “Open this door, quickly, what’s that, we have to fetch the children!” [[laughing]]. Finally, they opened the door, and we were drunk, imagine, drunken children. [...] And then, after the school years were over, they arrested Masha. She wasn’t working anywhere and she was put in jail for being jobless.

Q: Parasitism [Russian: *tuneiadstvo*].

A: By then [when Masha came out of the correction camp] her grandma had already died, Sasha [one of the drinking uncles] had hung himself, and Vasia [the other drinking uncle] had died from tuberculosis. And Masha ... it was very strict then about being registered [Russian: *propiska*, a permanent, official address], [...], three months without registration and they could arrest you. And so, she started living with an elderly man [...], he was over fifty then. But she had no choice, because of this registration, and she gave birth to a boy from him. [...] She was twenty-one when she came back from the camp. And one [day], they were drinking, and their boy was seven months old. And they went to check the [fishing] nets with the baby. And they all drowned. The baby was never found [...], but they found him and her. She was also Saami. This kind of death was all around, imagine.

Q: Yes, plenty. (GP 2015)

A: [About her relatives in Lovozero:] In 1982, their son, Andrei, died. He hung himself, and in '82 Fedia was already dead, too, or maybe it was in '80-'81. And Anatolii hung himself in '82. In those years, Pasha also hung himself. Their entire family fell to pieces. I don't know why, maybe because the parents drank and let things slide. In Lovozero, you know, I can't really explain it, but there was a kind of tacit roughness, when there's physical violence among the kids towards each other. Beating each other up, and so badly that it hurt.

Q: The kids among themselves or the parents towards the kids?

No-no, the kids among themselves. And I don't really understand the reasons, but Pasha was such a quiet boy, and he did this to himself. (NE 2013b)

In my fieldwork it was a common situation when meeting elderly, mostly female interviewees, that all their siblings, especially the male ones, had already passed away. One interlocutor from a relocated family, for instance, told that all of her brothers and sisters had already died, from tuberculosis, homicide, suicide, freezing or drowning, most having histories full of clashes with the authorities, including forced medical treatment and prison sentences. Across all interviewees, such biographies of social destitution were not uncommon among their dead siblings. Another interlocutor mentioned several times that she wants to actively remember mainly the many little positive things in her life, and not so much the negative trajectories of

the biographies of so many people of her generation. But upon request she remarked that she knows practically no men of her or her parents' generation from her village who did not spoil their lives through alcohol abuse. According to her, men generally coped much worse with the resettlements, whereas the women generally did better, but had no choice but to marry outside the community (exogamy) in the light of the general despondency among the men from their previous communities. This closes the vicious circle between alcohol abuse and the destruction/erosion of family life, which I discussed in the previous section.

8.5.7. Loss of language

Although this thesis does not focus on language issues, indigenous language loss must be mentioned here as one of the most striking consequences of the social engineering and displacement of the Soviet Saami. There are many reasons for the decimation of the Saami language during the Soviet times. All of them are ultimately connected to social engineering and the displacement of people. The reasons include:

- Under Stalin there were ubiquitous fears of being denounced as, among other things, a Saami nationalist (Article 1; Scheller 2013);
- as a long-term psycho-social legacy of Stalin's terror, fears of standing out from the crowd persisted and were transmitted through generations (Scheller 2013; confirmed by many interviewees);
- the policy of consolidation resulted in Lovozero becoming a melting pot of different Saami groups with their own Saami languages, and of different ethnic groups. Hence Russian became both an inter- and intra-ethnic lingua franca (Afanasyeva 2019, 210–11; Gutsol, Vinogradova, and Samorukova 2007, 43; confirmed by many interviewees);
- boarding schools contributed to the Russification of the youth, especially one entire generation between the 1950s and 1970s. They thus were responsible for a missing link in language transmission not only within school curricula, but also within families (Afanasyeva 2019; Article 2).

In addition to Soviet-time reasons, and causally connected to them, there are a number of contemporary reasons:

- An ongoing intra-ethnic elitist linguistic quarrel, starting in the 1980s and continuing to this day (for more details see Scheller 2013; Overland and Berg-Nordlie 2012);
- combined with a lack of support by the authorities, this means that language tuition today is in a worse state than it was in the 1980s;
- in a situation where new generations of speakers are not trained or developing 'naturally', most speakers belong to the older generation and are rapidly dying out;
- new and young speakers are attracted by the North Saami language, which has the most speakers internationally and has established itself as a kind of lingua

franca; today, on the Kola Peninsula North Saami, an exogenous language, is the second most common spoken Saami language (Scheller 2013; Overland and Berg-Nordlie 2012).

We can glean from this list that, in addition to a focus on Russification in education, there were many circumstances that put pressure on the Saami languages without a special intention to do so, but rather as a side-effect. Only boarding schools implemented an explicit policy of suppressing the language and establishing the dominance of Russian. Even there, however, Russification was less a nationalist goal than a utilitarian instrument for Sovietisation. The goal was to create “one holistic Soviet nation, which should be united not only linguistically, but also politically and, most importantly, economically” (Afanasyeva 2019, 203). There never was any official prohibition of indigenous languages. The suppression of the Saami language – and in general of the ‘small’ indigenous languages – was more a matter of practice: teachers punishing children for using their indigenous language and convincing parents of the supposed harmfulness of bilingualism, and parents and young people seeing the necessity of a good command of Russian for success in life under the social circumstances they were born into (Liarskaia 2003; Afanasyeva 2019; Article 2).

On the Kola Peninsula, we can generally see a higher level of language preservation among the Komi as compared to the Saami (Blokland and Riessler 2011). While this may today lead to cementing certain stereotypes about a supposedly ‘superior’ social organisation on the part of the Komi when compared to the Saami (see section 8.5.2., *The hierarchy of nations becomes visible*), the concrete reasons for this difference lie in the Soviet past: the Komi minority experienced clearly less assimilatory pressure. This has, on the one hand, to do with the absence of mass relocation: without being dislocated, family structures remained more intact, with less exogamy; and children, whose parents were in the same village, were usually not sent to the boarding school, unlike in the case of Saami children (see section 8.5.3., *Housing shortage*). On the other hand, the Soviet Union’s ambivalent policy of supporting attitude of supporting and oppressing languages other than Russian (Grenoble 2003; Liarskaia 2003) implied a compromise: languages with few speakers were not supported and their speakers were more actively ‘converted’ to Russian than speakers of the ‘bigger’ languages. In the case of the Kola Peninsula, this meant that throughout the ups and downs of Soviet language policy, the Komi language was rather left alone, while the Saami language was either the object of active suppression by teachers (in the 1960s-1970s) or of desperate attempts at revival (in the 1980s) (Scheller 2013; Afanasyeva 2019). Diminished attention towards the Komi language has thus been beneficial to its survival; that the language received comparatively little attention can be attributed on the one hand to there being less social upheaval among this ethnic group, and on the other to their higher standing in Soviet society and ‘closeness’ to the “great Russian people” (Antonova 1996, 180).

The Komi's better opportunities to choose Komi as their language is similar to the strong standing of the Nenets language on Yamal: most parents have kept their indigenous language at home, which is a very different situation compared to the case of most other northern indigenous peoples in Russia and elsewhere. Liarskaya (2004, 79–80) emphasises the responsibility of parents, pointing to Yamal as an example showing that the survival or disappearance of the indigenous language depends at least as much on the decisions made by the parents as on the school's language policy. While it is certainly true that personal choice – and thus agency – of parents plays a crucial role, my field site shows that external pressures have heavily influenced parents' ideas about which language choice is better for their children. Home visits by persuading teachers – sometimes indigenous – and fears about the lack of success in the educational system and the dominant society are important factors. Such fears are greater if the weight of the dominant society is heavier; on the Kola Peninsula this weight pressed more heavily on the Saami than on the Komi minority.

Lastly, I would like to present an important caveat mentioned by Vakhtin (1992, 32) in relation to indigenous people of the Russian Arctic: “The complete loss of their native language by the younger generation does not necessarily signify total assimilation and loss of ethnic identity.” The author mentions Evenks in Buryatia, ‘komified’ Nenets, some Inuit, and other peoples as examples. However, I would add that self-identification is never strictly endogenous, but depends on exogenous identification, that is, on who a person is perceived as by others and with whom else a person is grouped together by others (Brubaker 2004). From this point of view, the most widely perceived external markers for outsiders are language, clothing and physical appearance. In terms of etic identification, the last of these is the ‘predicament’ of the Saami. Siberian minorities are easily identified by European newcomers through their differing physical appearance, even if they speak Russian (I found one of the most extreme expressions of these stereotypes, combined with a lack of knowledge about the Saami people, in the ‘inuitising’ depiction of Saami on urban souvenirs sold in Murmansk). However, when not exoticised in such ways, for many inhabitants of the Murmansk Region the Saami simply look Russian, speak Russian and hence are ‘not really’ indigenous. This simple circumstance may lead to popular conclusions that the Saami instrumentalise indigeneity and are ‘not really’ indigenous. The potential, easily noticeable markers that remain are language and traditional clothing. For these reasons, keeping the language alive language plays an especially important role for the indigenous identity of the Saami.

8.5.8. Blaming the displaced people

I start this section by recalling that blaming the victims is named as one of the five fallacies in Downing and Garcia-Downing's (2009) theory on the psycho-socio-cultural consequences of development-induced displacement (see section 5.5., *Social engineering*). These fallacies are recurring patterns, permitting those in power to

avoid responsibility and thus causing or exacerbating the negative outcomes of social engineering mega-projects. The blame-the-victim fallacy describes the top-down assumption that the displaced people are unable to appreciate and take advantage of the new opportunities offered to them, thus individualising social issues (see section 5.8., *Individualisation of the negative*, for the theoretical background).

Patterns of blaming were ubiquitous in Stalinist terror. Thus, besides the direct targets of state terror, spouses and children of arrested people had to suffer due to their status of being related to an enemy of the state, encountering repression themselves or having to conceal their ancestry (Article 1). In mature Soviet times, blaming individuals was an even more widespread, though less physically violent, stratagem for diverting attention from the true causes of social problems (Kharkhordin 1999). Thus, in post-relocation Lovozero, the strategy of individualising social issues was directed towards those who met with different kinds of difficulties in their new place of residence. I will shed light here on several instances, partially referring to cases mentioned already in the sections above. First and foremost, again, housing and employment issues stand at the centre here, as they affected virtually everyone who was relocated. I will only briefly recall here that the relocated people were from the outset put into the position of petitioners, having to ask for both housing and jobs (see sections 8.5.3., *Housing shortage*, and 8.5.4., *Lack of meaningful occupation*). Furthermore, these primary difficulties, especially the catastrophic housing situation, led to widespread distress among the relocated people (see sections 8.5.5., *Gender split and erosion of family structures*, and 8.5.6., *Violent death and substance abuse*).

As shown in Article 2, and in the sections above, for many children of relocated and distressed families the blame-the-victim fallacy meant at best a good school career in the boarding school and at worst the ruin of good prospects for the future by being sent to the remedial boarding school for mentally disabled children. I will not dwell more here on the consequences for children, but will point out that sending children to the boarding school due to the poor housing conditions or unstable mental state of the relocated parents reflects a clear post-relocation blame-the-victim approach.

What still needs a closer look is the attempt to crack down on the adults among the ‘problematic’ post-relocation families and individuals, that is, those, whose behaviour was deviant from the expectations transmitted in top-down discourse about being a socially useful member of society. The most frequent marker of unacceptable deviance from these expectations was drinking. Today, alcoholism is rarely addressed by the Saami ethno-political elites. Konstantinov (2015, 138–39, 273) is correct in counting drinking among their “unmentionables”, even in the canon of the need and misery discourse, which otherwise makes active use of the upheavals of the past (Article 4, Berg-Nordlie 2011).

However, stories about drinking and blaming the drinkers did figure heavily in grassroots narratives collected during fieldwork. In official discourse, drinking could serve as a departure point for ‘explaining’ disturbing social facts, such as appalling

housing conditions, negligence in child rearing and non-observance of the Soviet work ethic. The public exposure of drinking people is certainly not something peculiar to Lovozero or to indigenous people. It was a widespread practice throughout the country, and one which intensified during the 1970s. I will show, however, how, in Lovozero, these general dynamics acquired an ethnic dimension through their local application to the people relocated from the closed-down villages. The oral accounts, the media and the archival documents that I have collected are full of ‘blame-the-drunkard’ stories. They have been mentioned in passing by Konstantinov (2015, 90) as extremely traumatic for parents and children alike. Here I will take a closer look into these humiliating and traumatising strategies, and into their goals and effects.

The 1985 article “Shame on the drunkards” in the local newspaper *Lovozerskaia Pravda* is an illustrative example. The names in the article reflect the local ethnic hierarchy mentioned earlier: two of three culprits have Saami surnames (judging from the surname, the third was likely a newcomer to the region) while chairman of the comrades’ court and author of the article is a local Komi (section 8.5.2., *The hierarchy of nations becomes visible*). Comrades’ courts (*tovarishcheskii sud*) were extra-judicial Soviet social institutions consisting of elected employees or residents at a given place of work or residence. They could hand down limited corrective sentences, such as public admonishment, or turn the accused over to the law-enforcing authorities (Prokhorov 1978). I will quote this newspaper excerpt here at length in order to convey the atmosphere of public humiliation that such announcements involved:

The comrades’ court of the sovkhos *Tundra* asks to place in the newspaper *Lovozerskaia Pravda* the note “Shame on the drunkards”.

In the end of April, the cases of A.K. Galkina, T.F. Zakharova and G.A. Rudakov have been heard – crop-growers, who systematically abuse spirits and maliciously goof off. [...] It looks like [Galkina’s] friendship with booze is stronger than her promises. The colleagues are indignant and no longer ready to put up with the whims of this lazy-boots drunkard.

Also Zakharova should feel ashamed: She’s well over forty, it’s high time to think of a future as an esteemed and well-respected pensioner,¹² to leave behind good memories of herself. But no. The woman did not learn anything from her previous experiences. She keeps skipping work as she has done since 1973. [...] She used to have children but her parental rights were removed, and she drowned both her motherly feelings and her holy obligations in the plonk.

[...]

The comrades’ court ruled that a public reprimand [...] was to be issued in the local press. Additionally, the materials have been handed over to the Commission for the fight against drunkenness.

12 The pension age in Arctic regions of the USSR was 50 for women and 55 for men.

A saying has it that only the fool gets angry about justice, while the wise will draw conclusions. We hope, after all, that these people will be able to be critical towards their own behaviour and to rectify themselves. (Rochev 1985)

ПОЧТА ЛОВОЗЕРСКОЙ ПРАВДЫ

ИЗ ЗАЛА ТОВАРИЩЕСКОГО СУДА

ПОЗОР — ПЬЯНИЦАМ

Товарищеский суд совхоза «Тундра» просит поместить в газете «Ловозерская правда» заметку «Позор пьяницам».

В конце апреля рассмотрены дела систематически употребляющих спиртное, злых прогульщиков — полеводов А. К. Галкиной, Т. Ф. Захаровой, Г. А. Рудакова.

Галкина и раньше трудилась в совхозе, при этом частенько загуливала. Терпели, терпели ее выходки да и уволили. Пришла через время снова, попросилась, обещала работать на совесть, соблюдать дисциплину, отказаться от выпивок. Но, видимо, дружба с зеленым змием у нее крепче, чем обещания. Началось старое. Коллеги по работе возмущены и не желают потакать лодырю-пропойце.

Стыдно должно быть Захаровой. Ей много за сорок, пора бы подумать о том, как достойно, с хорошей репутацией выйти на пенсию, оставить по себе добрую память. Но нет. Из предшествующего опыта женщина ничего не вынесла. Как прогуливала, начиная с 1973 года, так и ныне продолжает. Цепь нарушений разрывалась время от времени

вынужденными увольнениями. Имела детей — лишилась родительских прав, утопила и материнские чувства и свои святые обязанности в бормотухе.

Пятидесятилетний Г. А. Рудаков в совхозе десять лет. Был хорошим специалистом, уважаемым человеком, но залил все водкою. Пропорционально степени пристрастия к алкоголю развивалось безразличие к профессии, к обязанностям по работе. Обычным для него стало посещать вытрезвитель, приходиться на смену под мухой.

Решением товарищеского суда А. К. Галкиной, Т. Ф. Захаровой, Г. А. Рудакову объявлен общественный выговор с опубликованием в печати. Кроме того, материалы переданы в комиссию по борьбе с пьянством.

Говорят, на справедливое слово только глупый рассердится, умный сделает выводы. Мы надеемся все-таки, что названные сумеют критически отнестись к своему поведению, исправиться.

Р. РОЧЕВ,
председатель товарищеского суда совхоза «Тундра».

Illustration 12: Publicly humiliating the 'drunkards' in the local newspaper Lovozerskaia Pravda, 1985.

This article shows the official line: personal blame should bring people to their senses. Alcohol addiction, employment or family problems were strictly framed as individual issues, detached from the social environment. This condition, in which the individual had to bear the full moral and legal responsibility for his or her situation, reflected the state's ideological and legal position throughout the entire country:

Firstly, there was officially no unemployment: as nobody could be unemployed, the designation in all official documents for people not working or not properly working – independent of their actual situation – is by default “non-working” (*nerabotaiushchie*) people or “social parasites” (*tuneiadtsy*) (for example f.146 op.5 d.226 l.63-66, 1976; see also Konstantinov 2015, 324; Kheveshi 2002, 130).

Secondly, there was no alcohol addiction as a reflection of social problems. There was only drunkenness (*p'ianstvo*) as a personal weakness of the poor and uneducated (a common trait of modern societies on both sides of the Iron Curtain, according to Struchkova and Ventsel 2015, 177–78; Rouse and Unnithan 1993, 220–23). The punitive approach and the fact that healthcare for alcoholism was not free deterred people with a drinking problem from seeking professional help (Field and Powell 1981, 43).

Thirdly, the preference for discouraging through punishment, rather than promoting prevention and treatment, included frequent removal of child from the family. Interview materials and archived records show us that this approach was mainly intended to protect children and to punish parents (see also Article 2 and section 8.5.5., *Gender split and erosion of family structures*). The pattern fully corresponds to the practices analysed in detail by Khlinovskaya Rockhill (2010) in an early post-Soviet context, where individual parents were also held responsible for the social and material hardships induced by larger socio-economic changes.

Child being taken into care, public humiliation, and forcible treatment of excessive alcohol consumption were enforced from the top down through periodical decrees and campaigns. We can follow this chain of command in the unchanging, formulaic introductions of archived documents at the lowest administrative level. For instance, the 1973 decision of the Lovozero District Executive Committee “*On stepping up the work [...] on the fight against people, who sheer away from socially useful work and pursue an anti-social, parasitic lifestyle*” refers to the 1971 decision of the next higher administrative tier, the Murmansk Region Executive Committee, “*About the serious flaws in the work [...] in the fight against people who sheer away from socially useful work and pursue an anti-social, parasitic lifestyle*” (f.146 op.5 d.192 l.6-7, 1973). Another document skips that tier and refers directly to the next-highest level of power, the 1970 Decree of the Supreme Soviet of the RSFSR “*On the fight against people who sheer away from socially useful work and pursue an anti-social, parasitic lifestyle*” (f.146 op.5 d.210 l.202, 1974). The 1972 joint decision by the Communist Party's Central Committee and the USSR Council of Ministers

“On measures to step up the drive against drunkenness and alcoholism” (Raikhel 2016, 190) completes this series of top-level decisions fostering individualising, de-contextualising, medicalising approaches to social issues so characteristic of modern, complex industrial societies (J. Davies 2017a; Skultans 2007, 156–74; Rouse and Unnithan 1993).

A report from 1972 (f.146 op.5 d.156 l.125-153, 1972) about the condition of youth affairs in the district turns out to be a key document, locally explaining the intensified measures against ‘problem families’ after the last relocations. In a telling example of late-Soviet hypernormalised discourse in Yurchak’s (2006) sense, the reader first sees twenty-eight pages of cumbersome reporting about achievements and shortcomings in all kinds of youth-related issues. At the end of the document, a mere half a page of recommendations demands that all state representatives involved are to “give special attention to the work among problem families”.

At the lowest tier of law enforcement and state authority, the district and village level, this intensified authoritative discourse (Bakhtin 1981) about individualising social issues meant quite simply that results had to be delivered rapidly. The early 1970s thus saw a surge of punitive activity in Lovozero, which – while stemming from countrywide decrees – were used locally as a welcome instrument for doing away with the highly disturbing social issues among the population which had been recently relocated from the small Saami settlements that had been closed down.

For instance, in the record of a hearing at the Commission on Under-Age Affairs, Lena (name changed, L.A.), a police officer from the so-called “children’s room” (*detskaia komnata militsii*, the police unit supervising juvenile affairs and child protection) states:

[...] Belykh [name changed, L.A.] keeps drinking, neglecting the education of her children and violating the work discipline. The Commission on Under-Age Affairs came to the conclusion that in the Belykh family the atmosphere is extremely unfavourable to the children’s upbringing. If last-resort measures aren’t taken now the kids could become law-breakers themselves. The Commission [...] rules that a proposition should be put forward to the People’s Court to remove citizen Belykh’s parental rights. (f.146 op.5 d.193 l.1-3, 1973)

Lena conducted most such hearings throughout the 1970s in Lovozero. In one of them, a teacher is invited to testify against a ‘bad’ mother. The unequal confrontation culminates in this statement: “Pinchuk [name changed, L.A.] is a very difficult child. This is 99 per cent your, his mother’s, fault” (f.146 op.5 d.224 l.1-2, 1975). In another hearing, several individuals – judging from the surnames, most from the recently relocated communities – are threatened with various punitive measures: “She is nineteen. She has a child. If we don’t want this child to be hurt permanently [*ne iskalechila ego do kontsa*], I ask the Commission on Under-age Affairs to send to

the court a request to withdraw [...] the mother's parental rights." Another mother, accused of "drinking systematically", is put into the situation of supplicant: "I will get a grip on myself and stop drinking. At home I will put things in order. I will work well. All will be fine. Just don't send me to the LTP, don't take away my son." A reindeer herder is accused of introducing his son into drinking: "We need to write in the newspaper *Lovozerskaia Pravda* about this father's behaviour. He deserves disdain and condemnation by the people." (f.146 op.5 d.224 l.4-8, 1975). Overall, a few years after the countrywide decrees ordering the intensified battle against anti-social individuals were issued, the local authorities could be proud of themselves about having "actively engaged in the fight against drunkenness and alcoholism" (f.146 op.5 d.192 l.98, 1974). The individuals to whom this battle should be directed had been readily found in Lovozero thanks to the recent relocation disaster. Pressure was kept high throughout the 1970s and early 1980s, with periodic reminders such as "the People's judge and chief editor the *Lovozerskaia Pravda* should be more vigilant in these matters" (f.146 op.5 d.226 l.63-66, 1976).

I met Lena, whom I knew from archival documents, 'again', now in person, as a pensioner in Lovozero. Rather astonished about how I found out about her, she agreed to talk with me about the past, despite being ill and addicted to alcohol.

A: Earlier there were lots of those common phrases, you know – "intensify", "improve", "increase".

Q: "Step up".

A: "Step up", [phrases] that, basically, who knows who picked them, who wrote them. Some smart people, probably, who were sitting there and didn't know how to come to grips with all this [*s kakogo kontsa za eto brat'sia*] ((chuckling)). There were such phrases, and so what? (LI 2014)

Lena referred in this way to the authoritative discourse vertically transmitted and reproduced over and over again in decrees, decisions, reports and official records as a discourse ultimately coming from a distant, anonymous source, demanding

that we acknowledge it, that we make it our own. [...] Its authority was already *acknowledged* in the past. It is a *prior* discourse. It is given in lofty spheres, not those of familiar contact. Authoritative discourse may organize around itself great masses of other types of discourses (which interpret it, praise it, apply it in various ways), but the authoritative discourse itself does not merge with these. [...] It demands our unconditional allegiance. [...] [It] permits [...] no spontaneously creative stylizing variants on it. It enters our verbal consciousness as a compact and invisible mass; one must either totally affirm it, or totally reject it. It is indissolubly fused with its authority – with political power, an institution, a person – and it stands and falls together with that authority. (Bakhtin 1981, 342–43)

External, publicly visible adherence to such discourse is its main requirement. As long as sticking to this discourse will bear rewards to those doing so, the discourse will thrive. Ascribing to actors machine-like adherence to this discourse, however, would be tantamount to a denial of agency. Bakhtin contrasts the monoglossia of the authoritative discourse with the heteroglossia of the internally persuasive discourse: “When thought begins to work in an independent, experimenting and discriminating way, what first occurs is a separation between internally persuasive discourse and authoritarian enforced discourse” (Bakhtin 1981, 345). The preponderance of internally persuasive or of authoritative discourse in influencing a person’s actions should always be seen in the light of current circumstances, by evaluating the rewards that one or the other can bring. The last words of the quoted interview section with Lena show a certain degree of critical distance (“who were sitting there and didn’t know how to come to grips with all this ((chuckling)). There were such phrases, and so what?”). They show that she does oppose alternatives to the authoritative discourse. We cannot say with certitude, however, to which extent this opposition was there at the time of the events and to which extent it is a result of her reflections in the light of more recent events, and in the light of my own presence as a researcher. The interview partner further emphasised that the system was not purely inhumane and that before it turned to denouncing and punishing it contained elements of support. At the same time, she stressed that the police were the only ones entrusted with taking care of people in difficulties. The unequal terms on which the authoritative discourse had to be imposed was materially and visually engendered by the police uniform, which had to be worn during house visits. Certainly, the law enforcement organisations are a perfect ground for authoritative discourse to thrive, as the merits of its members will be judged by how closely they adhere to its monistic ideological orientation and to their and the organisation’s affinity for unambiguity, embracing in the process an aversion to subcultures of deviance and dissent.

Treating alcohol addiction followed discursive patterns and punitive approaches similar to those seen in the child removal policy. The anti-alcoholism campaigns of 1967 and 1972 addressed the social problem of alcoholism by medicalising and individualising it.

The first campaign led to the introduction of prophylactic labour-therapy camps (*lechebno-trudovoi profilaktorii*, LTP) throughout the country (see also section 8.5.6., *Violent death and substance abuse*). LTP sentences were usually a half year up to two years in length. They comprised work therapy and medical treatment at the same time. Room, board, work on site or in a factory and payment according to normal standards were provided. As in the sobering-up stations, in reality the patients paid for their treatment, as 30 to 60 per cent of the salary was withheld. The patients could pick up the accumulated money at the end of their term. Most of the LTPs worked as enterprises and some even made a profit (AP 2014; AE 2013; Allemann 2014; Field and Powell 1981, 41–42). LTPs were heavily criticised in the final years

of the communist era for their harsh conditions and infringements of human rights (Alekseev 1989; Ivanets, Pelipas, and Nikiforov 1991), as together with psychiatric clinics this was the only way to imprison a person without a criminal sentence. By 1994, the year of their abolishment, there were 244 LTPs in Russia with an estimated capacity of 70,000 to 100,000 inmates (Plotkin 2015; Shved and Chiknaeva 2019).¹³ The treatment was and still is widely regarded as useless or even harmful, and people usually quickly started drinking again after release. Such opinions were reiterated by all interlocutors with whom I broached the topic.

The second campaign was mainly a discursive one, putting pressure on state authorities and organisations to reveal and admonish alcoholics (Raikhel 2016, 64, 190). With narcology as a sub-discipline of Soviet psychiatry, we must also keep in mind that Soviet psychiatry's paradigm was the Pavlovian approach – a positivist approach linking all deviance from 'normality' to biological reasons rather than social ills and dismissing psychotherapeutic approaches as 'bourgeois' (Bloch 1978). The 1960s and 1970s saw an expansion of Soviet specialist discourses on drunkenness, hooliganism and other forms of social deviance. As a result, practices of compulsory treatment for noncriminal alcoholics were introduced, and seemingly widespread approval of them could be created (Raikhel 2016, 65).

The LTPs and the so-called sobering-up stations both operated under the Ministry of Internal Affairs, meaning the police, while the rest of narcology was under the Ministry of Health (Raikhel 2016, 66). Importantly, alcohol addiction was excluded from the list of illnesses entitling people to free healthcare and other benefits (Field and Powell 1981, 43). Another former local police officer in Lovozero, Sergei (name changed, L.A.), recalls what this meant in practice:

A1 (Sergei): It was in '72, yes, stepping up the drinking [meaning the fight against it], we were **fighting** here. They straightaway put a sobering-up station here. There wasn't any before here. And so they put up the sobering-up station and whoever was drunk would be brought there swiftly. In the beginning the fellows were even eager to see it ((laughing)) [...]. The service cost 25 roubles.

Q: A fine, of course.

A1: The fine was 10 roubles.

Q: I don't understand, so the 25 roubles were not a fine?

A1: No, that was for-

A3 (his daughter): For the accommodation.

13 According to former inmates and police officers, there were at least three LTPs in the Murmansk Region for men, but none for women. Women kept being sent either to psychiatric clinics or to LTPs in other regions (GP 2015; LI 2014; SI 2014; before the creation of LTPs, 'drunkards' of both genders used to be sent off to psychiatric clinics, which led to their chronic overcrowding Raikhel 2016, 66). As of 1987, in the Murmansk Region there were 3,878 people who had been in an LTP (Konstantinov 2015, 327).

A1: For the services of the medical sobering-up station.

Q: So that was seriously called a service?

A1: Yes, why not? Clean linen, a **doctor** who examined the patient. A doctor was always on duty there. (SI 2014)

Thus, the “clients” – a frequent way among former personnel to call the hybrid patient-inmates – had to pay for their treatment in detention. The payment consisted of two components: the fine, as the punitive part, and the fee, as the client’s contribution to keeping up the institution. In this context, Raikhel (2016, 69) mentions allegations that “the sobering-up centres, like medical facilities, were sometimes subject to pressures to fill quotas, which were dubious indexes of success.” He quotes the details of a case in the Turkmen SSR stating that in the course of one year a certain number of people had to be serviced by the sobering-up stations. His information about quotas is based exclusively on oral evidence. Raikhel concludes by guessing that “arguably it stands [...] as an index of a system that was less interested in accuracy than it was in exerting a general kind of social control, [...] a kind of social pressure on potential drinkers.” My oral history interviews provide similar evidence. In support of this argument, I will present two longer excerpts from interviews. Sergei explains here how and why quantity mattered in his work:

A1 (Sergei): So then- there was this separate decree from ’72, they used to write “drunken, according to this-and-that decree from this-and-that date.” There was a fight, the government had decided about it ((chuckling)). [...]

Q: And so this resulted somehow in some pressure? Pressure from above to-

A1: You bet!

Q: From above that you must fulfil, must-

A1: Absolutely.

Q: Must show some results.

A1: Yes, yes. First of all, our sobering-up station in Lovozero operated at a loss. It did not pay its way. Because of that we would get additional funding from Murmansk.

Q: I see. [...] Interesting that state-run places, not only the LTP, also the sobering-up station, had to pay off.

A2 (his wife, also a former police officer): Yes, the sobering-up stations had to.

Q: So does that mean that there were some quotas, you had to bring in so-or-so many people in order to make the place profitable, and you had to fill the quota?

A2: The more the better.

A1: Indeed.

Q: So we are saying that this had to pay off, there were some quotas. How would these quotas be communicated to you, to the police staff? Were there any documents of the sort “this year so-and-so many people need to be taken into custody”?

A1: No, there wasn’t such a thing.

Q: Some sort of plan? Or was it rather a soft pressure to-

A1: Well, they would say that in this month- Once a month, when we would get our pay, they would for instance tell us: “This year you’re not working well enough, we had to ask for subsidies from Murmansk, so do a better job.” Only like this, this way.

Q: I see.

A1: They did not give [numerical] quotas because- how? We cannot make a person drunk only to-

Q: Yes, of course. (SI 2014)

The main requirement thus was that the hybrid punitive-curative institutions serving “drunkards” had to be financially self-supporting. They adhered to the principle that alcoholism was not an illness but a vice, and thus not covered by the healthcare system. The individuals had to bear the full costs of their behaviour, financially and symbolically. It is inherent in the para-legal nature of these quotas that evidence of them cannot be found in written form, as they do not even exist. As Sergei explains, pressure was put forward in words, not in numbers. If numeric quotas were involved, it is very likely that they were communicated only orally. With the numbers at hand about the yearly costs of running the sobering-up station and the set price for a ‘stay’, it was an easy job for each police chief to calculate how many people the officers had to deliver.

This brings us to the second excerpt, which is from an interview with Liubov’ Vatonena, the former head of the district’s statistics bureau and later a Saami activist, a formerly public person whom I have already mentioned several times. She recalls how a relative of hers had been sent to the LTP:

I’m repeating myself now, this procedure with the LTP, filling it according to allotments [*raznariadka*] transmitted from above. An allotment, that is, a kind of a plan, how many people you have to deliver in this or that period of time, so that this LTP would have a high enough occupancy. Say, the rybkoop’s [a cooperative in Lovozero] leader would get an allotment list, I don’t know, by phone, or maybe there were some papers. I’m telling you just from lived experience, having in mind my cousin [a bachelor] [...]. He ((sighing)), a winner of the socialist workplace competition [*sotssovevnovanie*], who had been solemnly given a lapel pin [*znachok*], he worked at the pig-breeding farm [part of the sovkhov] looking after the pigs, he **did drink**, I won’t deny that, but it never was unrestrained drinking [*ne zapoinyi*]. Work, at least, came always before drinking. And so, a week before the ceremony [of the socialist workplace competition] he comes, almost crying. He asks us [the interviewee’s family] that in one month we should write an appeal to get him released from the LTP, that we had to vouch for his sobriety and so forth. He says- And I say: “How could it be that they are sending you [to the LTP], you’re just about to get this prize.” He says: “They coaxed me into it, because they absolutely have to send somebody, but everyone else has children, wives, only I’m alone.

And so they're asking me." I say: "So was it the same also earlier?" Because that was already the third time [for him to serve an LTP sentence]. They used to lock them up there for a long time, at least a year. [...] They asked precisely him to agree to go there, because they **had to** send at least someone. And as he was alone, well, how should I say, no one else would suffer from that, only he would. (LA 2013)

This account relates to the pressure exerted from above to deliver people to the LTP. That such pressure was not only exerted on police and courts but also on employers is not a far-fetched conclusion, considering the decisive role of comrades' courts – working directly at workplaces and places of residence – in issuing 'recommendations' as to who would be suitable LTP candidates. This pattern is confirmed by archival materials. Thus, in 1972 the district's Executive Committee directly ordered several enterprises to fulfil the recently issued Communist Party's decree against drunkenness and alcoholism. The order directed towards the sovkhos *Tundra* as the main employer in Lovozero put direct personal pressure on its director, demanding, among other things, that he "improve the work of the comrades' court", "to list the families of employees in which there are alcohol abusers who initiate conflicts", "to develop by 1 January 1973 an action plan about stepping up the drive against drunkenness and alcoholism", and "to send by 1 February 1973 to the district's department of internal affairs [i.e., the police, L.A.] a request to start proceedings to send serious drunkards to the LTP" (f.146 op.5 d.141 l.179,181, 1972). Combining Raikhel's and my findings on the topic of quotas, from distinct sets of data and distinct cases, grants the story about quotas in the 'fight against drunkards' a fair level of credibility. We can conclude that, if not in numbers then at least in words, pressure was maintained to deliver a 'reasonable' – from the state's point of view – number of people to the corrective-curative institutions. This is an ideal type of generalisation from comparison with other case studies, as I have discussed in section 3.1., *Generalisation and theorisation from qualitative data*.

Vatonena's interview also informs us about *what kind* of people would most likely become the targets for those with an urgent need to fulfil the plan: unmarried men. She mentions the main reason: an unmarried man was seen as comparatively free of social obligations and thus more suitable to be sent off to an LTP for a longer period; the usual duration of an LTP sentence was from half a year up to two years (Field and Powell 1981, 42). As I have shown above (see sections 8.5.5., *Gender split and erosion of family structures*, and 8.5.6., *Violent death and substance abuse*) men from the relocated Saami groups were especially prone to bachelorhood and alcohol abuse. Other interviewees – inmates, relatives, police staff (AP 2014; NE 2013b; LI 2014; SI 2014) – provided similar insights about this local bias in supplying people for the punitive-curative machinery (in section 8.5.6., *Violent death and substance abuse*, I have cited numbers along ethnic categories related to LTP treatment). Another often-remembered way to recruit people was the notorious prison van patrolling and

picking up people who had been drinking. It was a normal practice to sort the people caught depending on their status: party members, executive committee members, etc. were always swiftly released (SF 2013; SI 2014).

Two more aspects of the system are still in need of at least a brief mention. The first one is the sales quotas for alcohol by the state-owned shops (Sokolova 2017), which were never reduced until Gorbachev's campaign of 1985 (Schrade 2014). These quotas may have posed a moral conflict of interest with a fight on *alcoholism*, but not with the fight on *alcoholics* as needed "outcasts on the inside" (Bourdieu and Champagne 1999, see Article 2 for the same principle applied to the boarding school setting). The state had an interest to keep alcohol sales steadily high – and even increase them – as they very significantly contributed to the state budget. It has been convincingly proven by Schrade (2014) that this has been one of the most consistent historical continuities of the Russian Empire and the Soviet Union. The Lovozero case shows that the state's campaigns against drinking aimed at socially isolating alcoholics rather than eliminating alcoholism. Creating a category of "drunkards" as outcasts – locally epitomised by the relocated people – served the goal of keeping alcohol sales steady: by clearly singling out the few, the majority could keep buying and consuming alcoholic drinks in habitual amounts without remorse.

The second aspect I still need to broach is the recurring nature of the LTP terms: once in the system, one was likely to become a returning "client", thus firmly entering the role of society's outcast and scapegoat. Vattonena mentions the practice in the interview cited above. She had already denounced this vicious circle during the late *perestroika* times: it is difficult to get a job after a stint in an LTP, and many of those committed are either sent to the LTP again or even sentenced to prison terms for social parasitism (*tuneiadstvo*) without taking into account what led to their situation (Vattonena 1990). Aleksandr, one of the very few non-married, relocated Saami and former LTP inmates who is still alive,¹⁴ recalls:

A: How should I say, I drank at work, yes. They asked me to leave, they gave me one and a half years [in the LTP] ((chuckling)). And so, when I came back from my work [at the LTP], came here [back to Lovozero], they wouldn't take me back to work. [...] They wouldn't take me and said: "We don't have enough places." That's how it was.

O: [...] So one more reason to take up drinking again.

¹⁴ The popular opinion held by most relatives of former inmates, and the few former inmates who are still alive, is that the treatment at the LTP actually shortened the lives of many inmates. The long-term effects are commonly seen as heightening one's body's sensitivity to alcohol without increasing aversion. Moreover, the medicines are commonly seen as damaging one's system, which is a reason why many inmates only pretended to swallow the pills they were given. What can be said with certainty is that being released from the LTP was often 'compensated' for by excessive drinking. The accumulated wage handed out upon release hastened the relapse into drinking. The survivor Aleksandr is one of the few who became a teetotaler; however, as he says, this was not thanks to the LTP but in the 1990s through religion.

A: Sure, yes. (2) After half a year they sent me there again. (2) [...]

O: Meaning you were fired by the sovkhos, or where did you work?

A: Yes, I worked at the sovkhos. “Dismissed”, yes. “Dismissed.” Wait, now [I’ll remember]. “Dismissed in connection with dispatchment to the LTP,” yes. That’s how it was written, it was written [in the employment record booklet]. “Dismissed in connection with dispatchment to the LTP,” and that’s it. (AP 2014)

Comparing the Soviet LTP case presented in this research with Foucault’s deliberations on prisons reveals that the LTP looks like a quintessential form of Foucault’s definition of prison. Even more than in a conventional prison, in an LTP the body is constantly “trained and retrained”, its forces are applied to labour, and “medicine, as a science of the normality of bodies, found a place at the centre of penal practice (the penalty must have healing as its purpose).” Hence, imprisonment becomes “ambiguously therapeutic and punitive.” Moreover, Foucault urges us to understand delinquency as a “coupled penalty-delinquent system”: The modern penal system “manufactures a category of individuals who form a circuit with it: Prison does not correct – it endlessly calls the same ones back.” This system gradually leads to “irregularity or illegality toward the infraction, with the help of a whole process of exclusions and parapenal sanctions” (all quotes Foucault 1997, 35–36). Indeed, a stint in an LTP was not only a parapenal sanction; the LTP was an entire parapenal institution. Ultimately, its perfectly ambiguous curative-punitive status did not truly aim at healing its charges. While the body was not marked physically, symbolic marking kept being the system’s crucial function: by creating its outlaws, the LTP did not really correct but rather created a permanent category of outlaws-as-scapegoats. The scapegoats keep fulfilling their function through their periodical returns to society. Thanks to the on-and-off rhythm, instead of being locked up and thus rendered invisible, the person stays noticeable and visible in the society where he or she is supposed to fulfil the role of a scapegoat.

Under the pressure to provide culprits to the system in a quantity that stood in stark contrast to previous practices, it is evident that the law enforcement system had to act in the grey zone of legality, and hence targets had to be selected based on social status, following the universal principle that “a poor person is always easier to rob than a rich one” (Foucault 1997, 36). Together with the boarding school children and the ‘bad mothers’, single men became the most likely targets of the individualising measures taken in Lovozero in the wake of the countrywide crackdown on alcoholics as deficient individuals (instead of alcoholism as a social issue). Locally, this crackdown rushed in in a timely way to provide one more tool for concealing the failures and dilemmas created by state-induced displacement.

8.5.9. Finding work-arounds

We have seen so far that both material and immaterial dimensions played a role in the dramatic decrease of the population's wellbeing after displacement. The material dimensions of the problem chiefly related to housing and employment. The immaterial dimensions were in many ways connected to the 'politics of blame' described above. Finding ways to cope with these post-displacement problems means deploying agency. Such a perspective "recognizes the strategic element of choice, often overlooked in studies of poor and marginalised peoples" (Fischer 2014, 6). Obviously, the effectiveness of aspiration and agency is limited by available opportunity structures. All aspirations and agency are relational, and there is no absolute value indicator for different degrees of agency.

With his kaleidoscope of examples from around the globe, Scott suggests that state plans for sedentarisation and planned settlement have rarely gone as anticipated, and the population has always found ways around obstacles in their path (1998, 191). Thus, the long existence of collectivised agriculture over many decades is not thanks to its thorough implementation but, quite the opposite, thanks to the improvisations partly compensating for its failures in the background, that is, the grey market, barter economy and other informal arrangements, often outside of the law. This is the inevitable "dark twin" of the planned innovations, far more complex and seemingly disorderly, compensating for the deficits of social engineering's crude simplifications. In the context of the Kola Peninsula – and with a wider validity for Soviet society – these dimensions have been closely examined by Konstantinov (2015) and labelled using the umbrella term "sovkhoism". On what he concedes is a speculative note, Scott assumes that the more rigidly planned an economy is, the more it will be in need of informal practices that supply what the planned part fails to and that are needed to keep alive the pretence of a functioning planned economy. He goes on to suggest: "When this [shadow, L.A.] economy is ruthlessly suppressed, the cost has often been economic ruin and starvation" (1998, 261). Examples are the Ukrainian famine or the Great Leap Forward in China. Reading Konstantinov's analysis we can conclude that in mature Soviet times the leadership had learned their lessons from the past and allowed informal practices to a greater extent than earlier as a means to keep the system as a whole, as well as the pretensions and aspirations of the elites, running.

While many aspects of the informal economy have been extensively discussed by Konstantinov, there are many more ways in which people responded that would merit closer exploration. Examples include staying in the old place of residence as long as possible, gradually slipping into subsistence and self-reliance (some interviewees remember that they or their parents stayed longer than most); achieving success in the new place by studying well and working hard (a heavily gendered pattern, see Article 2 and section 8.5.5., *Gender split and erosion of family structures*); and fighting for housing using legal and para-legal means, such as writing to the higher

authorities in Moscow. I will not dwell here on these examples, and this list is far from being exhaustive.

Instead, the main focus of this section is on yet another set of reactions to imposed change: non-compliance. It cannot remain unmentioned that in the patterns of non-conformism which I will discuss below, the male population was heavily overrepresented compared to the female. I repeat here that we can identify different patterns of coping along gender lines (see section 8.5.5., *Gender split and erosion of family structures*). Not strictly, but as a clear tendency, comparatively female-dominated accommodation met comparatively male-dominated non-conformism. Again without going into detail, I reiterate that I see this difference as chiefly socially conditioned by the gendered requirements and expectations within the majority society, which offered incentives, on the one hand, for deviant, non-conformist behaviour as a marker of virility and, on the other hand, for docile, conformist behaviour as a marker of femininity.

Among the sites where struggles of compliance versus non-compliance are fought rather conspicuously are schools. This is probably connected to the fact that schools can count as a testing ground where consequences of deviant behaviour are less serious than in adult life. In Article 2 I describe several ways in which pupils and parents sometimes challenged decisions about pupils being put into a school for mentally disabled children, the so-called remedial school. Quite often I came across accounts of resistance by pupils to the boarding school system in general. As I have already illustrated in section 8.5.5., *Gender split and erosion of family structures*, there was a pronounced gendered difference between accommodating or resisting boarding school education. I present here one more testimony:

A: No=no=no, [in our family] we all had to go to the boarding school. Only that my brother, he didn't accept this. He ran away **regularly**, and they gave up and let him live at home. My sister and I, we were more amenable, to some extent maybe we liked it ((laughing)). Of course, in the beginning we also ran home, but then we calmed down.

O: So there were things you liked.

A: We found some merits for ourselves. You know, women always adjust more easily. But he, no. He would run away, he would not go to school and- I remember, grandma, when they had already sent him to the remedial school, she got after him, used to sit at the entrance until lessons were finished so he wouldn't run away. That's how he hitty-missy managed to finish school ((chuckling)). After that he graduated from our vocational school. (LA 2013)

If we talk about non-compliance, we should avoid thinking of it mainly as open insurgency. While a schoolboy's little insurgency may still be met with relative mildness, open rebellion is in principle a risky measure that rarely pays off. The mother of a remedial school child was summoned to the local administration, where

the teacher might accuse the mother in the following terms: “You humiliate me in front my pupil, you have no culture of education. ‘Don’t go to the morons’ school, those are your words” (f.146 op.5 d.224 l.1-2, 1975). These accusations led to a fine of thirty roubles, a considerable sum at the time.

Non-compliance much more often occurred in unspectacular and subtle ways, such as foot-dragging, tacit support of the scapegoats, or deriding and carnivalising the system in hidden transcripts (Scott 1985; 1989). Stories around the LTP do not only illustrate the politics of blame as described in the previous section (the seeing-like-a-state perspective) but also the multiple dimensions of non-compliance (the grassroots perspective). In a popular alternative decoding of the abbreviation, familiar to most of my older-generation interlocutors, an LTP becomes the sarcastically-proudly sounding *letno-trenirovochnyi polk* (flight training regiment), instead of the original *lechebno-trudovoi profilaktorii* (prophylactic labour-therapy camp). Demonstratively not taking seriously the corrective institutions set up to discipline the deviants is a pervasive feature of stories about being threatened with an LTP term, about being inside an LTP, and about being released from an LTP. Thus, while being in an LTP, according to several interviewees, it was common to resist the medical treatment by only pretending to swallow the pills. Upon release from the LTP, the accumulated wage was often demonstratively spent on rather large amounts of alcohol. One former taxi driver from Apatity remembered having picked up a released inmate at the door of an LTP who had ordered two taxis: one for himself, and one for his bag. Before hitting the road to Lovozero (180km) he ordered the driver to stop at a shop and bought a box of vodka bottles (own fieldnotes, 2013).¹⁵ The act of self-staging a triumphant comeback in a convoy of taxis, equipped with the amount of booze needed to throw a decent party, must be seen as a gesture of symbolic defiance towards the ever-regulating and ever-optimising state. This gesture presupposed moral approval from at least a part of the people in the village the former inmate was returning to. This kind of tacit support is confirmed also by stories about events preceding a convict’s departure to the LTP. Thus, one former ‘client’ remembers:

Q: How was the trial?

A: That’s all very quick – half an hour and it’s over. “So he drinks?” – “Yes, he drinks.”

The district police officer is there. (4) When they convicted me for the first time, the district police officer was there, and the guys from my workplace.

A: As witnesses?

15 In Finnish Lapland I was told by a Saami reindeer herder an amazingly similar story: After an exceptionally successful slaughtering campaign, his father wanted to celebrate in the city. Equipped with a good amount of money, he ordered two taxis to Rovaniemi: one for himself and one for his hat. I have no means here to explore the possible common roots of this pattern.

Q: “How does he work?” – “He fulfils his work obligations.” Well, they were on my side. “We haven’t seen him drunken” ((laughing)). So what? He does his work, but all the same, all the same. (AP 2014)

The crucial message here is that the other members of the *kollektiv* were on the defendant’s side. They covered him, undermining the ‘reveal’ part of the reveal-admonish-excommunicate (Kharkhordin 1999) mechanism. Archival documents confirm this deplorable – from a state’s point of view – condition: The district’s commission on healthcare and social issues, for instance, complained to the district’s leadership that the sovkhos *Tundra* is “one of those enterprises that do not at all cooperate in the fight against drunkenness” (f.146 op.5 d.226 l.96,99, 1976). Similar claims appeared in the newspaper as well, such as: “Certain [enterprise] leaders thwart the proceedings to send people to the LTP” (*Lovozerkaia Pravda* 1988).

A former police officer remembers another story of non-compliance, this time in the village of Krasnoshchel’e, to which he had been flown together with the judge in order to demonstratively try three ‘drunkards’:

We brought everyone in [to Krasnoshchel’e], the judge says: “Okay, so today after lunch let’s make the trial” [...]. While we walked from the airstrip, all [locals] showed compassion. First, one [villager] stuck a bottle into [one of the defendants’] pocket, then another one, and so on. And while we were deciding with the judge when the trial should take place, they already managed to quietly have some drink. [...] I see all this and **seize** the stuff. In the end their pockets were filled with five or six bottles. On the flight back to Lovozero they sobered up more or less. They started fidgeting, looking for something. What should I do? They’ll run into the woods, finding them will be hard. So I get a bottle, here we go. They got some drink and calmed down. [...]. Those were sweetie-pies from Krasnoshchel’e. I mean, from Krasnoshchel’e they rarely made it there [to the LTP]. (SI 2014)

Here, in the isolated village with only sporadic presence of state authority, the subversive support by locals became rather overt when they were giving bottles to the show-trialled ‘drunkards’. Popular support of those labelled as outcasts and criminals (Hobsbawm 2000) is a form of second-tier resistance. In the first stage, the non-conformism of a relatively few ‘irregular’ citizens undermines the plans of getting the reformed society to work without friction, but they are numerically limited and needed as scapegoats for covering up fundamental flaws in the design of the social engineering (as explained in the previous section). In the second stage, the popular support for the non-conformism of outcasts – from colleagues to the police officer to the sovkhos chairman – becomes an undermining non-conformism in itself. The post-displacement setting in the rural part of the Kola Peninsula shows us how the ‘regular’ citizens were caught in an ambivalent condition of taking part in the play

of outrage and exclusion versus having feelings of support and compassion. This is not an epistemological inconsistency, but the way in which agency is played out in practice: relationally and situationally, and hence often ambiguously (see section 5.4., *Agency*). The acts of involved people can be ambivalent because “the dominant often has something to offer, and sometimes a great deal” (Ortner 1995, 175). Following the same reasoning, Humphrey’s (1994) case study on Soviet Mongolia transgressed the binary by showing how almost everyone can be – in differing proportions and within nested hierarchies – both ‘oppressor’ and ‘oppressed’, both ‘dissident’ and ‘collaborator’. Clearly, non-conformism can be a more or less active condition, but it can be also a rather passive state of resignation. Patterns of behaviour around drinking are both: resignation and resistance. Drinking itself is sooner resignation, with a non-deniable component of passivity: the physical addiction takes hold of the person. However, practices relating to this addiction can count as active non-conformism: villagers supporting the ‘drunkards’, patients not swallowing the tablets, deriding and carnivalising the system. After all, we can say that drinking patterns reveal a wide array of agency and at the same time are a prime site of loss of control over one’s own agency due to addiction. Different shades of non-conformism, both caused by and mitigating social engineering’s flaws, are all part of an enriched understanding of everyday resistance. Understood in this way, the term has lost none of its validity since Scott (1985) coined it.

9. The articles

The previous chapter has provided an exposition of the rich oral testimonies and written materials collected in the course of this research. Based on these materials, I tried to systematise my findings on the dislocation of indigenous people in the Soviet North by, firstly, showing its causal relation to social engineering and, secondly, listing its far-reaching effects and their interconnectedness. More than the articles, Chapter 8 shows in practice that oral history interviews are co-produced knowledge, as it features quotations from the interviews more extensively than the thesis articles. Here it is worthwhile quoting Portelli once again:

The final result of the interview is the product of both the narrator and the researcher. When interviews, as is often the case, are arranged for publication omitting entirely the interviewer's voice, a subtle distortion takes place: the text gives the answers without the questions, giving the impression that a given narrator will always say the same things, no matter what the circumstances—in other words, the impression that a speaking person is as stable and repetitive as a written document. (Portelli 1998, 71)

I tried to heed this insight as much as possible in Chapter 8 and in the articles. However, in the articles I was constrained by journals' requests to write compactly. This is why I regarded Chapter 8 as a forum where I should compensate for these spatial constraints. The present, ninth, chapter features the published articles on which the thesis is based. Commentaries on each of them follow in Chapter 10.

**Part III: Consolidated anthropological-
phenomenological reflections on doing oral
history on the Kola Peninsula**

10. Discussion

In the first, theoretical-methodological part of this thesis I put forward my view of a phenomenologically and anthropologically inspired oral history. The second part of this work was an empirical-hermeneutical case study of Soviet social engineering and displacement on the Kola Peninsula. In it I used open-ended narrative biographical interviews, combined with archival materials and newspaper articles, in presenting facts about and analyses of displacement in the Soviet Arctic North. Rounding out the empirical core of the thesis presented in Chapter 8, the articles (Chapter 9) scrutinise oral history testimonies and insights from long-term fieldwork with a stronger focus on consideration such as motivations, positionings and discourses. In the present chapter, I proceed to discuss the articles, including additional thoughts and literature, summarise the common threads running throughout this thesis and present the broader insights I have gained in the process.

10.1. Article 1: Commentary

Article 1 was published in the journal *Oral History* (2019) and investigates the early forms of Soviet displacement, which were mainly of a punitive nature. Thematically, at least at first glance, the article is less closely connected to the others, as its timeframe (the 1930s and 1940s) is outside of the main temporal focus of the thesis (the mature Soviet times). However, it is important in two ways: Firstly, it shows us the pervasiveness of displacement, with its broad array of underlying reasons, as a dominant force throughout the lifespan of the Soviet empire. Under Stalin, the ethnic minorities of the Kola Peninsula – and elsewhere – fell victim to the Great Terror in above-average proportions. The biography of Gidrun Aleksandrovna, a woman of Norwegian and Saami descent, reveals this in an exemplary way. Secondly, the article reveals that people did not and do not feel like purely passive victims of an all-powerful regime. Whatever the circumstances, people try to achieve a sense of relative control over their lives. While Lüdtkke, the pioneering German historian of anthropologically inspired history of everyday life, finds it “*obvious* that the historical actors were (and are) more than mere blind puppets or helpless victims,” (1995, 5, my emphasis), this is not necessarily noticeable in many of the historical inquiries based exclusively on archival data. One of the goals of the article was to demonstrate the power of individual case studies in revealing dimensions of agency and dignity. The article thus shows some of the main advantages of single

case studies: the possibility to check the adequacy of insights from broader studies against individual examples; to make studies based on archival data, surveys or other ‘big’ data more interpretable, accessible and understandable; and to offer deeper and additional insight into fields with difficult access. Single case studies are not only analytical accounts of individual lives, but institutional analyses; through them, we can see the less visible lines of interaction with and within institutions.

10.2. Article 2: Commentary

Article 2 was published in the journal *Acta Borealia* (2018). Both from my own field materials and from comparative discussions within our research group in the ORHELIA project, it quickly became clear that boarding schools are a ubiquitous topic. Indeed, “almost all indigenous people in the Russian North remember the boarding school as an important experience in their childhood but with varying evaluations” (Dudeck 2013b, 72). The article lays bare the causal connections between practices in boarding schools and the realities of population displacement. I show that the tundra-connected population of the Kola Peninsula in mature Soviet times – from 1970 onwards, when all relocations of Saami people had been carried out – was served in Lovozero by two boarding schools. One was the native boarding school, which had some special ‘ethnic’ elements in its curriculum in keeping with the official Soviet understanding of indigenous cultures as being limited to the realm of museum-like folklore. The other was the remedial boarding school, belonging to a countrywide type of educational institutions for mentally disabled children. Both schools showed an over-representation of children with a Saami background compared to the regular day school in the settlement (see section 8.5.3., *Housing shortage*, for additional numbers). What is particularly remarkable is that many Saami children living in one of the two boarding schools had at least one parent living in the village, contrary to the idea of boarding schools being for children whose parents live somewhere far away. I call this the *boarding school paradox* and the article reveals the multiple reasons for this.



Illustration 13: From left to right and top to bottom: Inside the Lovozero Native Boarding School, probably 1970s; children being transported on the river from Voron'e to the boarding school in Lovozero at the beginning of the school year, 1950s or early 1960s; in the native boarding school's dormitory, probably 1970s; first visit by a Norwegian Saami delegation at the native boarding school, 1961 (I could not clarify the circumstances of this visit); the buildings of the former remedial school and contemporary daytime school, 2013; one of the buildings of the native boarding school with a playground, probably 1970s. Bottom left image by the author; all other images: photographer unknown, archive of the Lovozero Museum.

Between the publication of Article 2 in 2018 and the submission of this thesis, two more academic publications about boarding schools on the Kola Peninsula appeared. One publication, my anthology of boarding school testimonies (Allemann 2019), was conceived as a specimen of truly participatory research; namely, I broke somewhat away from academic conventions and dedicated an unusually large space to the *primary* interpretations given by the witnesses themselves and, correspondingly,

reduced the *secondary* interpretations on my part. For this reason, this anthology is a good complement to Article 2, in which there was little space for directly quoting oral history testimonies.

The other publication was Afanasyeva's (2019) doctoral thesis on Saami children in Soviet/Russian boarding schools. Having already engaged with her work in several sections of this thesis, I will add a few more points here. Afanasyeva avoids taking a closer look into the boarding school paradox, and she is well aware of this. Not focusing especially on boarding school children whose parents lived in the same village is in keeping with her stated goals: her aim was not to analyse each group of children but rather to systematise the wide range of "collective experiences without presenting all the nuances and variations between the situations of the various groups of children residing at the boarding school" (Afanasyeva 2019, 186). Moreover, her thesis does not have a particular focus on the relocations and the following hardships or take up the topic of the remedial school. With express reference to the 1970s and 1980s – the two decades when the remedial school operated in Lovozero – the author concludes that

it is important to note that there were two separate schools operating in Lovozero – the boarding and the secondary school [meaning the regular daytime school]. Sami students received education in both of these institutions, and understanding the interconnections between these two schools is essential for the current analysis. (Afanasyeva 2019, 265)

The complete absence of the remedial school in Afanasyeva's thesis supports my claim that "only locally confined, long-term qualitative research could bring to the fore the usually hidden topic of the RBS [remedial boarding school]" (Article 2, page 3), while Afanasyeva's doctoral research was based on a mere twenty hours of interviews arranged in rather formal conditions (appointments arranged through a field assistant who was often also present during the interviews, formalised consent paperwork done before the interviews, see Afanasyeva 2019, 88-90).

In this regard, our work, following different approaches to fieldwork, was complementary, hers giving a valuable overview of what were very diverse boarding school experiences, mine looking into special aspects of the boarding school system related to the relocations. While Afanasyeva does describe the boarding school paradox, she does not refer to it as a finding that had been taken up in Article 2, published a year before her thesis. One completely new contribution by Afanasyeva to scholarship on the issue was the chapter on the effects that children transferred from orphanages all over the Murmansk Region had on the boarding school system in Lovozero. The transfers, undertaken in the end of the 1960s and the beginning of the 1970s, were motivated by two considerations: reducing juvenile delinquency among children from orphanages and solving the problem of the poor material

conditions in most orphanages. Afanasyeva points out that many of her informants maintained that this additional group of children from outside the village had a negative effect on the boarding school (Afanasyeva 2019, 255–66). Arguably, in addition to the reasons stated in Article 2, this additional amount of children is likely to have contributed to the decision to open a remedial school in Lovozero in 1970.

Afanasyeva mentions another, simple and convincing explanation, beyond all ideology, for the widespread practice of quite arbitrarily referring children either to the daytime school or the boarding school: a lack of places in one or the other, depending on the year, depending on how many children came from the outside and other dynamic factors. My interview materials confirm this arbitrariness of referrals, which saw families end up with children going to each of the three schools (daytime, boarding and remedial) and enormous differences in individual adaptations and achievements. As Bloch (2004, 95) has noted, the “affirmative action approach to solidifying the nation-state anchored the allegiance of many.” Indeed, the general boarding school system should certainly not be seen in purely oppressive terms. I have focused on the remedial school here as only one, albeit exceptionally oppressive, part of the picture.

Today, inclusion in education, as opposed to any form of segregation whatsoever, is a widely accepted but also highly controversial paradigm (Allemann-Ghionda 2015). In concluding this section, I would assert that there is nevertheless still a frightening amount of indigenous segregation by special boarding schools throughout the world, pointing to instances where political and economic interests prevail over good pedagogy (Woodman and Kroemer 2018). From this point of view, Article 2 is not only a retrospective piece of research but also a forward-looking contribution to discussions about ongoing boarding school practices for indigenous pupils.

10.3. Article 3: Commentary

Article 3 was published in the journal *Qualitative Inquiry* (2017 “online first” publication, 2019 in print). The article is a reflective piece of research on the ethical consequences of the prolonged and repeated presence of a researcher (myself) whose ambition goes beyond extracting results from the field. Namely, I embrace as my guiding principle that I should bring results back to the locals. These results may include possibly disturbing aspects of the past. This is a crucial part of the ethical integrity espoused by the research team I am working on, although it can pose serious ethical dilemmas. Frequently returning to field sites and bringing back research results in a form accessible and interesting to field partners enable us to pursue deeper conversations about aspects of issues that they otherwise might not

have shared. Thus, observing ethical considerations is not only an act of political and ethical correctness but one that yields an epistemic surplus. This was decidedly the case with the discussions triggered and controversies described in the article, for engaging with the field partners multiple times uncovered additional facets of the interconnection between boarding schools and population displacement that served to augment the analyses in Article 2.

A scholar's ethics should be first and foremost the ethics of the people he or she talks with, and only in the second place the ethics of regulatory bodies and funding agencies. I was guided by this insight while conducting all my research, and all thesis articles reflect this attitude towards flexible, situational research ethics. I made this point most explicitly in Article 3, together with co-author Stephan Dudeck, connecting our practical experiences from fieldwork with theoretical deliberations on such flexible ethics. In Chapter 7, I have summed up these insights and made several additions. One of the main findings was that rigid rules and procedures, symbolised by the written word, do not necessarily – and unlike is often assumed – provide the concerned research partners with a feeling of empowerment. In many a setting, signing a document or discussing formal considerations before what should be informal conversation evokes hierarchical relations and disempowerment.

Article 3 is largely about an ethical dilemma: the question of whether to include or to exclude a topic from public presentation and discussion. Deciding about this means to “weigh the gains for some against the pains of others” (Oliver-Smith 2009b, 18). Having to weigh competing notions of good and evil against each other may mean to be confronted with a situation where it is *impossible* to apply a set of ethical guidelines consistently. Interestingly, Oliver-Smith (2009b) and de Wet (2009) describe the same kind of ultimately unresolvable ethical dilemma in relation to decision makers who need to consider infrastructural development projects requiring resettlement. In the work of both authors displacement stands historically at the root of all the major events and experiences discussed in their research. This is also one of my initial claims in this thesis and figures significantly in examining the sensitive issue of the remedial boarding school in Lovozero ensuing questions of the gains and pains of the people involved – ultimately, as consequences of policies that had led to the displacement of people.

Article 3 also makes a plea to not apply rigid FPIC (free prior and informed consent) procedures. While it is a widespread assumption that FPIC papers signed *before* a conversation create trust, our conclusion in the article is exactly the opposite. This insight is based on practical research experience in post-Soviet settings. Regarding oral history research in a post-Soviet society it has been confirmed, for instance, by Ilic (2016, 9). The depth of the materials gained – including views on hidden topics like the remedial school (Article 2) or the LTP (prophylactic labour-therapy camp, see section 8.5.8., *Blaming the displaced people*) – are a confirmation of our claims advocating flexible, discursive and post-

factum consent based on a relationship of trust rather than on written forms and strict legal procedures.

In indigenous studies, academic discussions favouring strict consent and ethical review procedures are rooted in traumatic events of the past with blatant examples of unethical, colonialist research. What tends to be forgotten in these discussions, however, is that rigid ethical procedures – even if their application is increasingly demanded in social sciences or humanities – have their origins in bio-medical research designs, where they are more justified than in deeply qualitative inquiry, as the bullet points below indicate. Universities tend to support strict FPIC proceedings and IRB (institutional review board) rulings because they are risk-averse institutions caught in Western legal frameworks. Increasingly – and paradoxically, in light of ubiquitous discourses of indigenous views as alternatives to Western perspectives – indigenous representational institutions also try to enforce this approach. In a gratifying response to this trend, Denzin refers to the following recommendation by the American Association of University Professors from 2006:

Research on autonomous adults whose methodology consists entirely of collecting data by surveys, conducting interviews, or observing behavior in public places should be exempt from the requirement of IRB review, with no provisions, and no requirement of IRB approval of the exemption. (quoted in Denzin 2009, 293)

This position has since been officially endorsed by the American Oral History Association (Denzin 2009, 294–95). Its reasons are the following:

- In oral history, and in social anthropology, no large samples are used, no hypotheses being tested, no large-scale generalisations formed. No underlying principles or laws with predictive value are being looked for. In short, it is not a positivist science.
- Oral history explains particular settings and events and studies the lives of individuals.
- The participating individuals are neither randomly selected, nor is the relationship an anonymous one. Oral history narrators engage willingly in a process of co-creation.

Obviously, exclusion from rigid consent procedures and formal ethical review does not mean being exempt from ethical issues. Rather, there is more freedom and flexibility to rely on a responsibly built, dynamic and reciprocal relationship as a basis for sound, ongoing judgement. Respecting a professional association's standards (for instance, the Association of Social Anthropologists of the UK and Commonwealth 2011; and the American Anthropological Association 2012) is still imperative, and signing a legal release at the conclusion of an interview is a good idea if the circumstances permit. But throughout the research process we should be allowed to let ourselves be guided situationally and flexibly by our own sound ethical

judgement if our goal is to produce truly co-productive research that shows respect for, as well as tolerance and recognition of, whatever the needs of our research partners may be.

10.4. Article 4: Commentary

Article 4 was published in the journal *Arctic Anthropology* (2017). While its insights are rooted in oral history evidence, among all articles of this thesis it is the one with the strongest relation to the present. The research is a discourse analysis of today's Russian Saami society in which I deconstruct the widespread etic notion of a homogeneous community with unitary interests, this being a position represented by a few mouthpieces of the 'community'. Largely invisible to the numerous short-term visitors to Russian Lapland, who tend to write about the Russian Saami in terms of sweeping generalisations, the social rifts criss-crossing Saami society are rooted to a large extent in the social engineering of the twentieth century. Seeing these fissures is a major exercise of contextualisation, requiring, as Lüdtke points out,

an exact, more 'profound' probing look at social situations and relations, as well as their intertwining and rhythms of change. Only in this way is there any chance of recognizing lines of discontinuity and fissure between and within classes and social strata. [...] Because conflicts over aspects of class and power are synchronous with gender-related polarization as well as generational conflict, the social field to be explored is always multilayered. (Lüdtke 1995, 20–21)

While I maintain that I have achieved this goal in Article 4, I will – with the help of a corpus of established scholarship – try to give an answer to a fundamental question: Why should it be important or beneficial to recognise discontinuities and rifts within a social entity? My answer is also intended as a response to Junka-Aikio (2016), who advocates abstinence from de-constructivism as it may contribute to atomising indigenous groups and thus harm their political struggles against dominant societies. In a critical position on the issue, presented in an often-cited study on Bedouin women, Abu-Lughod has written:

A serious problem with generalization is that by producing the effects of homogeneity, coherence, and timelessness, it contributes to the creation of 'cultures'. [...] The appearance of a lack of internal differentiation makes it easier to conceive of groups of people as discrete, bounded entities, like the 'cultures' of 'the Nuer', 'the Balinese', [...] populated by generic cultural beings who do this or that and believe such-and-such. (Abu-Lughod 1993, 9)

Such a picture is in many cases – also in relation to the Russian Saami – created not only by outsiders, but also by internal elites. While such groupism (Brubaker 2004) is a common state of affairs, this is not to say it does not merit critical examination. Connected to this, Bloch (2004, 24–25) identifies two traditions of thinking about ethnicity. The first is the primordialist approach, in which ethnic categories are construed as rooted in a common past and shared heritage; this is the “nature of the stuff on which these groups feed” (Eriksen 2010, 64). It is invoked by many indigenous activists. The second is the instrumentalist approach, in which political aims justify the maintenance or resurrection of and emphasis on ethnicity. In practice, indigenous activists use a mixture of both approaches. Scholars have mostly focused instrumentalist approaches. Thus, it is the instrumentalist perspective which makes it visible that primordialism becomes an instrument in its own right. If indigenous leaders invoke primordialist views on their ethnicity, they are at the same time applying an underlying instrumentalist approach: they are grouping together people according to created categories on the grounds of primordialism, and this is done with political aims. In relation to several peoples of the Pacific region, Keesing noted the following:

the ancestral ways of life being evoked rhetorically may bear little relation to those documented historically, recorded ethnographically, and reconstructed archaeologically – yet their symbolic power and political force are undeniable. [...] Perhaps it matters only whether such political ideologies are used for just causes. (Keesing 1989, 19)

Indeed, I see this as the crux of the matter. My task as a scholar is to make it evident to outsiders that certain claims are political and instances of rhetoric, and thus not to be understood literally. Essentialist and reductionist understandings of culture and indigeneity can be seen as political tools, possibly legitimate, but not as absolute realities. As a scholar engaged in a long-term field relationship, I have felt as much obliged to those people among whom the ideas, rhetoric and acts of leaders, gatekeepers and mouthpieces hold little sway. As long as there is no perceived community internally, laying bare ruptures to which respondents themselves point is not an instance of weakening a community and playing into the hands of outside powers. Berg-Nordlie, himself an indigenous scholar, sums up the issue as follows:

When researchers aim to “strengthen the nation,” blindness to internal divisions may lead them to strengthen certain groups to the detriment of others. If they simply adopt the discourse about what constitutes “the nation’s interest” as presented to them by the group’s internal authority figures, they may end up supporting internal elites against internal marginalized groups. Researchers are not doing nations under research any favors by strengthening internal elites against oppressed subgroups, or by not shedding light on internal problems. If research is to benefit the people and not the elites, then it

must investigate critically the nation's own power structures and internal antagonisms. (Berg-Nordlie 2017, 54–55)

Indigenous elites may dominate indigenous discourses, disseminate certain understandings and suppress alternatives by mediating or even blocking contact between outsiders and grassroots actors. I was guided by this reasoning while writing Article 4 as an overview of the effects that the historical events of the twentieth century have had on the contemporary structure of Saami society as a heterogeneous entity and not as a unity.

10.5. Social engineering in the indigenous Soviet Arctic

By looking into oral history testimonies, combined with the analysis of archival and media documents, I have endeavoured to make a contribution to the knowledge about the modern history and contemporary situation of the Kola Peninsula. As I have written in the introductory chapter, I was especially interested in the displacement of rural populations as a significant part of Soviet social engineering, which on the Kola Peninsula mostly impacted the Saami communities that at one time could be found throughout the region. Based on an open-ended process of collecting materials, upon completion of my fieldwork I felt that the manifold consequences of displacement are omnipresent in people's lives and narratives. These impacts had to be brought together and analysed systematically. This I have done in Chapter 8, a detailed account of the Kola Peninsula that is illustrative of social engineering across the entire Soviet Arctic and even beyond. The negative consequences of displacement induced by social engineering include a chronic housing shortage, lack of meaningful occupation, gender split and erosion of family structures, violent death and substance abuse, terror and violence, educational obstacles, loss of language, and hierarchisation along ethnic lines. The mass closures of settlements and large-scale relocations of the 1960s clearly correlate with the subsequent rise in mortality and substance abuse. This suggests that Soviet-time displacement lies at the root of the overwhelming rise in alcohol abuse and violent death.

Drawing on the analysis of local materials from the Kola Peninsula, I have tried to cover the two salient perspectives mentioned above: grassroots perceptions and "seeing like a state" (Scott 1998). Yet now, in concluding this piece of research, I prefer to avoid their strict separation. The two are intertwined in my sources of data (oral history interviews, participant observation, archival documents and newspaper articles), bringing to the fore amalgams of agency and constraints, of making positive sense of one's life and resignation or desperation, and of having power and being powerless. I have tried to look into the broad range of options which people tapped in responding to social engineering. Articles 1 and 2, as well as Chapter 8, tell

about this wide array of strategies, which included actively seizing opportunities or resisting the new order, quietly accommodating or resigning oneself to one's lot to the point of plain despair. Articles 3 and 4 show how far-reaching the consequences of social reshuffling have been, figuring to this day in societal and discursive fault lines that are manifested through a multitude of competing discourses and visions of the past. The present oral history research has succeeded in revealing these thanks to its long-term field engagement. At the end of the day, the research has shown that Soviet social engineering is an important historical reason why it is especially hard today to speak of a community in the case of Saami society in Russia (see especially Article 4).

Owing to these findings, social engineering became one of the dominant concepts of this thesis. This was not planned at the outset, and this is why the term – but not the idea behind it – is lacking in Articles 3 and 4. Nevertheless, social engineering spans all the articles in two respects. The first relates to Alexander and Schmidt's (1996) distinction between macro-scale and meso-scale social engineering. While Soviet social reshuffling doubtless belongs to the macro level, the indigenous ethno-political activism in the past three decades, with its attempts to construct past and present realities (see especially Article 4), belongs to meso-scale social engineering. As a conceptual extension of this reasoning, this doctoral research may add here a micro-level, that is, what happened at the individual level. We may identify as micro-scale social engineering the multiple ways in which individual oral history research partners present themselves, their past and their present to me and other interlocutors and thus influence views on the past and the present of their society.

In a second, more prominent way, Soviet social engineering is present in all the articles through the topic of displacement and the concomitant social transformation and problems. Displacement, as one of the most pervasive dimensions of Soviet social engineering, occurred throughout all regions of the Soviet Arctic. And, as I have written at the beginning of this thesis, displacement is necessarily followed by a (re)replacement of people. In Chapter 8, I showed that the Kola Peninsula has been a place where social engineering among indigenous communities took place to an extreme degree – something proudly stated by the Soviet leadership itself. Following the logic that extreme examples can be a valuable means to highlight phenomena that may be widespread but less visible elsewhere, I use the case of the Kola Peninsula to show how far social engineering went in the Soviet North (Chapter 8) and look more closely into some aspects of it: Stalin's terror (Article 1); boarding schools (Articles 2 and 3); gender (Article 4); and sedentarisation, mono-culturisation, and the creation of legibility (Chapter 8).

One of the conclusions in my first oral history inquiry about the Eastern Saami was: "The greatest evils for many Sami were not collectivization per se nor the Stalinist terror, but the resettlements" (Allemann 2013, 134). Replying to Habeck's (2015) critical review of my book, I would like to put this differently

now: population displacement should be seen as intrinsically connected to Stalinist terror and collectivisation, and not separate from it. Collectivisation and terror both prompted displacement as a direct consequence (in the 1930s and 1940s) as well as later displacement in the wake of the consolidation policy (in the 1960s and 1970s). In order to emphasise this, I have suggested in the present thesis that the broader term *displacement* is more suitable for describing this multiple uprooting than *relocation/resettlement*.

All “social bulldozing” – a term coined by Scott (1998, 218) paraphrasing his “social engineering” – by the state, including collectivisation and terror under Stalin, ended in population displacement. Plain terror as a means of displacing indigenous people – with its physically more violent deportations, long-term imprisonment and death sentences – remains quantitatively behind the multiple forms of non-punitive displacement in the North. Displacement and uprooting due to collectivisation began in the 1930s, but it was in the late 1950s under Khrushchev that moving people in the Arctic entered its most pervasive phase. The Kola Peninsula is an extreme case of indigenous displacement both in terms of extent and of consequences, as countrywide and Arctic-wide policies were conflated with a number of local factors. Without repeating the details, the reasons for displacement included economic rationalisation and final sedentarisation (under the cover term of *ukrupnenie*=consolidation); industrialisation, infrastructurisation and mass colonisation (under the dominating term of *osvoenie*=appropriation, ‘making ours’). Well hidden behind the *osvoenie* and *ukrupnenie* discourses were two more reasons: creating legibility, meaning that the population and resources should become easier to control, and militarisation in the light of the Cold War and the Kola Peninsula’s proximity to the West. Presumably, this geographical closeness and the transnational status of the indigenous population were among the motives for showcasing the Kola Peninsula as an exemplary case of Soviet reforms.

Oliver-Smith (2009a, 4) suggests in his definition of development-forced displacement and resettlement (DFDR) that “people in DFDR are ‘pushed’ to move rather than ‘pulled’ or attracted by better possibilities elsewhere.” This is not so evident in the case of the dislocated Saami groups in Soviet times: there were both push and pull factors (see section 8.4., *Implementation: pull factors and push factors*), meaning that people did not only feel coercion but also saw opportunities that they wished to seize. However, we should also bear in mind that push and pull factors can be deliberately constructed and exaggerated. Engineering the consent to planned social engineering action is part of social engineering: communication strategies, propaganda technologies and the resulting discourses are engineered. Creating consent meant engaging with the local population. Those who saw opportunities for themselves did not necessarily or fully disagree with being relocated. I have shown that there were more evident incentives for moving for the female population, mostly connected to how the majority society’s gender roles were “created and enacted

through talk” (Farnell and Graham 2015, 397). I have elaborated on this in Chapter 8, in Article 2 in relation to boarding school education (and the concomitant inculcation of the majority society’s gender roles), as well as in Article 4 in relation to the contemporary consequences of this gendered difference.

10.6. Individualising the negative, and ways of coping

A particular focus of this thesis has been the top-down measures to conceal social hardship caused by social engineering, and the ways in which the people affected dealt with these measures. I have discussed the main instrument to achieve this goal: individualising and medicalising emerging social issues. Based on empirical materials collected as part of this inquiry, as well as existing research (among others, Goldner, Ritti, and Ference 1977; Bourdieu and Passeron 1990; Kharkhordin 1999; Argounova-Low 2007; Khlinovskaya Rockhill 2010), I proposed that these mechanisms be subsumed under the term *individualisation of the negative*. I have shown how these measures were received, implemented, moulded, adapted and coped with by all parties involved. By attributing material and mental hardship deficiencies in individuals instead policies, it becomes possible to maintain a façade of a society of equals with a few deviants. Collective mutual surveillance (as promoted by Soviet workplace and neighbourhood institutions like the comrades’ courts) and public top-down blaming (for instance, the symbolic pillorying of “drunkards” in newspapers) generate public opinions surrounding an individual rather than an issue, thus transforming collective distress into public anger or scorn towards scapegoats. A topic for further research could be to look into the evident converse of this approach, that is, to investigate the extent to which there was a commonalisation of the positive, understood as a tendency to highlight positive societal developments as common achievements of the *kollektiv*. While there certainly was commonalisation of positive developments, successes were often also attributed to idealised individuals like, for instance, in the praising of toilers overfulfilling the production plan.

Instances of individualisation discussed in detail in this thesis include invented charges of ‘wrecking’, sabotage and espionage (Article 1); boarding schools (Articles 2 and 3); and anti-drunkenness measures (section 8.5.8., *Blaming the displaced people*). All these patterns should be seen as instances of an individualising approach that created a certain number of scapegoats among the population as an instrument to obscure the fallacies and failings of social engineering, and scapegoats are easiest to recruit among low-status members of society. Thus, in post-*ukrupnenie* Lovozero, displaced people living in poor conditions, their children in the boarding schools, and single men became the most likely targets of individualising measures. This was the dominant social order in the post-*ukrupnenie* (consolidation) Lovozero of the

1970s and 1980s, with deviants being recruited predominantly among the relocated people and ideologically constructed as outcasts. In a Bourdieuan spirit, we may assert that any social order implies inequalities (to a greater or lesser extent), and these inequalities are most likely to be perpetuated by the habitus of people. This is crystallised in the ways in which individualisation techniques over time became more pervasive (although less physically brutal) in Soviet society.

In Soviet society, and also in Lovozero at the time, by far not everyone could be considered a victim: scapegoating grounded in the *kollektiv* requires a critical number of people who engage in it. Additionally, and in contrast to Stalinist punitive displacement and uprooting (Article 1), later Soviet social engineering (Articles 2, 3, 4, Chapter 8) was – in theory – benevolent. It was not officially coercive, and no organised violence was used. Despite the fact that relocation often turned out to be forcible and psychologically violent, we should refrain from using “forced” as a *constant* epithet of Soviet resettlement in the Arctic: it does not do justice to the multiple ways in which displacement was received, made sense of and coped with by most of the eyewitnesses I talked with. As Lüdtke (1995, 16) has pointed out, this type of inquiry “can disclose and pinpoint the specific ‘patchwork’ of impositions and incentives.”

As the empirical materials of this doctoral thesis clearly show, the ways of coping with Soviet-time displacement and forging one’s path through its consequences were very diverse. Ortner (2016) makes a convincing point that one should not see ‘dark’ and ‘bright’ aspects of life in an antagonistic relation and asserts that anthropological research should not focus exclusively on one or the other but reflect their mutual interplay in lived life. Engaging in projects of care, love and empathy, taking responsibility, creating goals to pursue and making positive sense of one’s current life circumstances – these are things that are always present and hence relevant, no matter how dire one’s surrounding conditions may be. Understanding the Soviet population as a passive and oppressed mass misses important points of rather subjective, perceived senses of agency, and practicable ways of achieving various ends.

As Alexander and Schmidt (1996, 3) claim, later victims’ abhorrence of extreme and/or failed social engineering measures does not mean that these very measures lacked their future victims’ public assent. I would add to this that, most commonly, the eyewitnesses of displacement feel that they are simultaneously victims of displacement failures and beneficiaries of Soviet reforms and opportunities. Indeed, they inevitably embraced developments as a given supra-structure, making sense of it and their lives in it in a variety of ways. Such a historiographic perspective is doubtless useful in uncovering both suffering and agency among the ‘victims’, but it also explores “the ‘inner perspective’ of the acquisition and exercise of power. [...] In the light of such inquiry, the gaping distance between rulers and ruled is reduced” (Lüdtke 1995, 4). Accepting and taking seriously the coexistence of positive

and negative memories of a past in a state so often (etically) described as simply totalitarian is an indicator of a more mature and composed, and less politicised, analysis of the socialist past (Obertreis 2015, 113); this is what I have tried to offer in this thesis.

The heterogeneity of grassroots action has figured as a crucial element throughout the articles. In other words, “the focus is on the forms in which people have ‘appropriated’ – while simultaneously transforming – ‘their’ world” (Lüdtke 1995, 7). The protagonists of Article 1 deployed an agency that had gone unnoticed in previous historical literature on minorities on the Kola Peninsula under Stalinism. Articles 2 and 3 show that placing and educating children in boarding schools presented different challenges and opportunities to different pupils and teachers, with the result that the protagonists might make sense of the boarding school system in very different ways. Article 4 shows that the contemporary Russian Saami ‘community’, largely as a result of the social engineering of the twentieth century, includes a large array of ruptures and attitudes and is by far not as homogeneous as is frequently assumed.

As Ortner has rightfully noted, subordinate groups are not unitary but always “internally divided by age, gender, status and other forms of difference” (1995, 175). In relation to the Saami in Russia, this is the core message of Article 4, with the addition that such distinctions also ‘divide’ individuals and make them deploy different varieties of agency depending on the situation. Fegan talks about being “constantly baffled by the contradictory ways peasants talked about the tenancy system in general.” (1986:92). The ways in which, depending on the outcome in their case, many of my interlocutors alternate back and forth between negative and positive assessments, between stories of resistance and accommodation in Soviet times, can be as baffling, an example being their accounts of the boarding schools (Article 3, Allemann 2019). This makes it hard to firmly associate agency with a narrow understanding of resistance.

Another example of the ambiguity of resistance may be the general social despondency in Lovozero in the wake of the relocations (section 8.5., *The consequences of displacement*, and subsections). While it is the most straightforward choice to see excessive drinking and other forms of self-destructive behaviour as acts of resignation, one can also see them – and not in a mutually exclusive way – as resistance to attempts to streamline social organisation according to a high-modernist ideology. Resistance here should be seen in an expanded perspective. Alcohol abuse and, as a consequence, indolence and absenteeism, for which so many people were blamed in Lovozero, turn out to be forms of resistance to the smooth functioning of the planned socio-economic machine. In the end, it is a matter of how one defines the term whether a relatively conscious intention to resist must be present or not in order to call a phenomenon resistance. As a result, however, we can see that what have elsewhere been called self-destructive strategies (Povoroznyuk,

Habeck, and Vaté 2010, 16) had negative effects not only on individual well-being but also on the functioning of the socio-economic model masterminded by other people.

10.7. The Soviet Union as a ‘Western’ power

In this thesis I have invoked Scott’s (1998) very generally formulated idea of the Soviet Union as a ‘Western’ power in the light of its high-modernist affinity for technical and scientific progress. I set out to breath empirical life into this idea, as I found that this affinity was very visible on the ground. It is obvious that the Soviet Union was a European power in its intellectual roots of anti-capitalist criticism. However, it seems that through the Iron Curtain divide a creeping othering of the Soviet Union took place in much of non-Russian scholarship. While it was not a central or initial goal of this thesis, an interest arose in seeing the ‘Western’ aspects of the Soviet Union from a multitude of observations that appeared in the course of the research. I will discuss these in what follows.

Ortner (2016) identifies the roots of what she calls “dark anthropology” in the works by Marx, Durkheim and Weber, and Foucault and Bourdieu, which I have referred to in this thesis. According to Ortner, focusing on the dark sides of human existence has become necessary and unavoidable, first with a need to unravel the colonial past of the West, and later on to criticise capitalism, especially in its harsh neoliberal forms. However, Ortner omits a third important dimension: critiques of socialist states. Defining the scope of dark anthropology beyond what is typically framed as ‘the West’ gives us a holistic perspective on the dark sides of high-modernist societies, a view independent of their organisation as nominally capitalist or socialist. All too often authors slip into seeing the dark sides they analyse as ‘typical’ of one or the other form of polity and social organisation, overlooking the commonalities with their ‘significant other’. A strong exception to this is Scott’s *Seeing Like a State* (1998), in which he elaborates on a range of twentieth century social experiments across the world, showing their striking commonalities. Among other things, Scott emphasises the ‘Westernness’ of the Soviet Union, or rather the common denominator between Soviet and ‘Western’ societies: a high-modernist approach to human existence driven by technocracy and simplification (portrayed as rationalism), albeit with one difference: “For capitalists, simplification must pay” (Scott 1998, 8). I would qualify this statement by saying that profitability – no matter how efficiently achieved – was at least theoretically an important driver of many Soviet social engineering projects as well. Consolidation (*ukrupnenie*) in its entirety was conceived on the tenets of economic rationalisation, and sedentarising indigenous people was both ideologically and economically driven. Thus, *ukrupnenie* as rationalisation makes the Soviet Union a state more ‘Western’ than is commonly assumed.

Another dimension spanning the capitalist-socialist divide is what I have called in this thesis the *individualisation of the negative*. Indeed, blaming the victim is identified by Downing and Garcia-Downing (2009) as one of the five fallacies of development-induced displacement. They see it as a recurring pattern, permitting those in power to avoid responsibility. This frequently met attitude exacerbates the negative outcomes of social engineering mega-projects instead of trying to solve them. The blame-the-victim fallacy reflects the top-down assumption that the displaced people are unable to appreciate and take advantage of the new opportunities offered to them, and thus individualises social issues.

As Davies (2017a) argues about capitalist societies, under the political and economic circumstances of neoliberal reforms, a widespread way of tackling psychological – often socio-psychological – issues is to apply the reasoning chain of individualisation, medicalisation, pathologisation and, finally, pharmaceuticalisation. In the past three or four decades this reasoning has been able to flourish due to powerful economic interests despite poor clinical evidence and outcomes. Davies’ arguments about societies with strong neoliberal influence are very convincing, but they succumb to a uniqueness bias typical of criticisms of capitalism by framing these tendencies as the spawn of capitalism. This thesis indicates that Soviet society, too, became supportive of individualisation, medicalisation-pathologisation and pharmaceuticalisation. While this happened under very different political and economic circumstances, we can identify on both sides of the ideological divide similar reactions to the undesired effects of ideologies and policies whose stated purpose was to advance economic prosperity.

Some of Foucault’s work is also conceived of as a critique of modern capitalist societies: “Inadequate wages, disqualification of labor by the machine, excessive labor hours, multiple regional or local crises, prohibition of associations, mechanism of indebtedment – all this leads workers into behaviors such as absenteeism, breaking of the ‘hiring contract,’ migration, and ‘irregular’ living” (Foucault 1997, 33–34). As countermeasures Foucault names an “immense worker moralization campaign” and attempts to establish “regularity” in the form of diligent workers (1997, 33–34). While Foucault aimed at naming factors typical of Western societies, this picture sums up many of the features which I have described in this thesis. I have discussed the moralising, normative discourses, and the resulting politics of blame. We have also seen how ‘irregular living’, or collusion with outcasts pursuing this irregular lifestyle, and subtle forms of breaking the Soviet labour ethic, are reactions to the modern state’s attempts to radically reorganise society. Furthermore, linked to these mechanisms of individualisation and exclusion, I have discussed in detail (Chapter 8, Articles 2 and 3) the social reproduction (Bourdieu and Passeron 1990) and cynical knowledge (Goldner, Ritti, and Ference 1977) as concepts developed for ‘Western’ societies yet applicable to Soviet society as well.

In addition to these bridges across the capitalism-socialism divide, many topics related to displacement in this thesis find parallels in research about other

circumpolar regions or other indigenous groups than the Saami. For instance, state-induced relocations among the Saami people have also taken place in Sweden (Lantto 2014). The most explicit assimilation policy among all states where Saami live took place in Norway (Minde 2003). Similarly, Riva et al. (2014) point out that Inuit have experienced inadequate, overcrowded housing across North America and the Soviet Union/Russia. In all these circumpolar countries boarding schools for indigenous children have been a common feature (Krömer and Allemann 2016), with amazingly similar recollections among former pupils. While Lomawaima (1994) relates that boarding school recollections among Lakota Sioux are rather painful, she also mentions different forms of accommodation and resistance, from violating the dress code to pupils reaching academic heights. As was the case in Soviet reality, due to the gender stereotypes proffered by the state, girls were embraced by the system of urbanised upbringing and thus turned out to be more compliant with the system. This in turn fuelled the gradual withdrawal of the women “from the land” (Vitebsky 2002) (see section 8.5.5., *Gender split and erosion of family structures*, and Article 4). This trend has been investigated also for Arctic indigenous peoples outside of Russia, for instance, by Kuokkanen (2009) for Finland, Hamilton and Seyfrit (1994) for Alaska, and Hamilton et al. (1996) for Greenland. A final point I would mention is that discussions about scholarly deconstruction versus strategic essentialism (Article 4) are widespread among and about indigenous groups globally (for example, Junka-Aikio 2016; Shah 2007; Keesing 1989), and can be regarded as the long shadow of the changes brought about by modern states and their social transformations.

All these commonalities, which become manifest in the comparison between my locally grounded empirical insights from ‘the East’ and existing literature about ‘the West’, underscore the need for a more holistic view of the high-modernist twentieth-century state and its relationship to Arctic indigenous minorities, beyond the widespread socialism-capitalism dichotomy. I have tried to contribute to the further advancement of such views by highlighting some of the most significant commonalities: in a first stage, large-scale social engineering in the Arctic regions of modern nation states and, in a second, the creation – in considerable variety – of the non-responsible and deviant subject made accountable for disturbing society’s smooth functioning and planned move towards progress.

11. Conclusion

As I note in the beginning of this thesis, significant contributions to oral history have come from both anthropologists and historians. In the past, however, the dialogue between historian and anthropologist oral history writing has been limited. I thus set myself the goal of melding insights from both disciplines. I have tried to do this by combining, on the one hand, the rich theorising by historians on the analysis of written sources and on oral history interviewing techniques (which was in its turn inspired by anthropology and psychology) with, on the other hand, insights from anthropology about long-term fieldwork and participant observation. I have done this where practice was concerned, and the results are the empirical insights presented in all the thesis articles and in Chapter 8; I have also done this where theory was concerned, the results of which are discussed in the articles and in Chapters 3 to 6. In addition, combining the two disciplines has received its legitimatising common philosophical foundation through my turn towards phenomenology as an underlying but rarely discussed common basis of anthropological and historical curiosity.

As a foundation underpinning the goals stated and fulfilled in this thesis, I formulated one basic, initial question: How did and do people experience Soviet-time displacement and (re)emplacement in Arctic indigenous regions, with particular reference to the Kola Peninsula? This was a phenomenologically driven question and the answers have proven to be every bit as broad as the question. I combined the individual experiential dimensions stemming from this phenomenological approach with perspectives represented by sources close to a state perspective.

The conclusions of this endeavour may be summarised as follows: Firstly, displacement in all its facets is both a tool and an outcome of modern-state social engineering. Secondly, there was no explicit repressive component trained on ethnic groups in any of the countrywide, centrally decided policies discussed in this thesis. Distinct effects along ethnic boundaries emerged, however, on a local scale, either as unintended outcomes or as creative adaptations. Thirdly, social engineering induced by the state was prone to multiple failures. These flaws were compensated – from the state's point of view – with a strategy of individualising the negative. The responsibility for the spiral of negative social developments – among other things, a housing shortage, alcohol abuse and other self-harm, and poor school performance – was shifted from the state to the individual, from the public to the private sphere. Fourthly, displacement is remembered both as being forcible and non-forcible, with

this depending on the narrator, the narrated situation, and the narrative situation. What is remembered negatively for the most part is not the coercion to move but the slipshod implementation. Also negatively remembered are several dimensions of social reorganisation in the aftermath of displacement. Within the scope of this study, this means that throughout the entire Soviet period and whenever people were moved, displacement as a term is not only broader than its more euphemistic counterparts, such as sedentarisation, consolidation, relocation or resettlement, but also more accurate.

This thesis set out to understand biographies and their surrounding lifeworlds. My interest lay in how people make sense of their experiences and what effects are produced by their social interaction. In the theoretical chapters of this thesis I have drawn on storytelling as agency by looking more closely into sense-making and meaning-giving constructions as a constant feature of the oral history testimonies I collected. I have observed connections between this meaning-making and questions of situational self-representation and self-empowerment (all thesis articles, sections 2.3., *Creation of meanings*, and 5.3., *Face, Lines, Position*), and to the relativity of truth and evidence (all thesis articles, sections 2.4., *Evidence and truth*, and 4., *Credibility in oral history*). Furthermore, I have pondered a broadened understanding of agency and resistance. This broadened understanding also entails the insight that storytelling in itself is agency and has the power to transform (Ahearn 2001; Basso 1996). This is something I have tried to show throughout the thesis articles. Portelli's (1998, 72) statement about his own research on insurgent movements in Italy reflects a dominant methodological feature of this thesis: "The recounting of a strike through the words and memories of workers rather than those of the police and the (often unfriendly) press obviously helps (though not automatically) to balance a distortion implicit in those sources." Whether discussing displacement and upheaval through punitive terror or through 'benevolent' social transformation, I have tried to counterbalance events and circumstances that are well documented by archival sources with oral testimonies. The resulting accounts which I present convey heterogeneous perspectives that can hardly be streamlined into one argument or one cause.

Thus, this oral history inquiry does not meet the frequent expectation that "victims' must talk like 'victims'" (Briggs 2007, 558). They may do so or may not. The "right to happiness" (Obertreis 2015, 109-111, originally in German: "Glück") should have a justified standing alongside the right to shame, anger or trauma. This echoes Fischer's (2014) strong case in anthropology for not missing out the projects of happiness and wellbeing that individuals and collectives pursue, independently from their bare socio-economic and socio-political living circumstances. The prerequisite for all these 'rights' to materialise in historiographic accounts based on oral evidence is to give enough time to develop an intimate relationship between the respondent and the researcher. This allows digressions from the official 'script',

this often being one which creates dichotomies between victim and oppressor. This is especially the case in research about societies, political systems and states that are gone by – as usually a dominant script has already managed to establish itself for those already ‘finished’ stories. The “right to happiness” in oral history accounts should be seen as the need for a positive sense of one’s life that every human wants to create for him- or herself within the circumstances he or she happens to live in. On the Kola Peninsula, and related to displacement and its consequences, instances of this need can be seen in the narrative resistance by the ‘sovkhoists’ against the strong victimisation proposed in ‘activist’ perspectives (Article 4); by the multiple ways of coping and living with the remedial boarding school (Articles 2 and 3); by the ways of dealing with blaming strategies related to post-relocation social despondency (section 8.5.9., *Finding ways around*); and by strategies of both resisting and humanising the oppressors during Stalin’s terror (Article 1).

The essential theoretical insights of this thesis include the following: Firstly, I suggest a combined approach to oral history research using anthropological traditions of long-term fieldwork and participant observation, and historiographic traditions of document analysis and source criticism. Secondly, I propose that phenomenology could furnish the foundation of this approach. A phenomenological gaze often serves as a powerful lens, but it is rarely made explicit. I have done so, concluding that phenomenological awareness as a powerful reductive – that is consciously de-contextualising and re-contextualising – lens lies at the core of any (de)constructivist, postmodern scholarship, but is rarely addressed and theorised as such. Thirdly, while I opted for phenomenological awareness as a basis, I suggest that in history and anthropology it should be complemented by a flexible use of existing theories and methods. I called this approach *deliberate eclecticism*. Fourthly, I make a strong point for inclusive, flexible and situational research ethics going beyond rigid regulations and rules.

In this thesis, I have also tried to tackle questions of generalisability and representativeness. In his discursive analysis of the collapse of the Soviet Union, Yurchak (2006) formulated an important caveat relating to his qualitative research: it focuses on urban, educated people and the discursive shift analysed in the book on the example of these people is “not necessarily representative of an average social experience.” But what would actually be representative? Is there an average social experience at all? At the same time, Yurchak suggests that his analysis serves “as a powerful lens through which emerging internal shifts in that system become visible.” This is the crux: a study like the present one, starting from a phenomenological perception of things that people say and do, is not necessarily representative of an “average experience”, because there is no such experience. This is a fundamental insight engendered by an attitude that I have called *phenomenological awareness* in the beginning of this thesis. Rather than creating homogenised views and streamlined conclusions, a phenomenological approach has the advantages of going in the

opposite direction: I have pursued this by focusing on the heterogeneity of local implementations of and adaptations to centrally planned approaches. Through this, the most human aspect of any policy becomes visible: experiencing it. At the same time, as I have written in the beginning of this thesis, these fundamental insights from individual-oriented research are an indispensable component for assembling broader pictures about the past and the present, as the only way for the universal to speak to us in depth is through the individual.

Such understandings are based on a lifeworld-oriented, fundamentally phenomenological approach. In this thesis, I have tried to systematise this inherent phenomenological basis of my and many other pieces of research. I showed in Chapter 2 that the phenomenological philosophical school of thought in many ways anticipated the theories and practices of many oral history scholars, be they historians or anthropologists. As many other oral history publications, my thesis articles do not explicitly refer to this phenomenological background – in a historical or anthropological article, where the main goal is to present results linked to a concrete case, there is usually no space to elaborate on the phenomenological fundamentals. Linking oral history more explicitly to its deep but often unspoken phenomenological roots was therefore made an explicit goal in Chapter 2. In his typical provocative style, Denzin describes scholarly writing in qualitative inquiry as a means to make sense of one's own and, additionally, maybe of another's life, as "in the end it is a matter of storytelling and the stories we tell each other" (Denzin 2014, 581). Thus, the cycle of sensemaking through storytelling, which I utilise to describe my research participants, revolves on myself through their stories. What I tell about other subjects is at the same time about myself and my very research. Indeed, a common denominator of all reflexive, critical, postmodern research is that "the world of human experience must be studied from the point of view of the historically and culturally situated individual", and that "qualitative researchers will persist in working outward from their own biographies to the worlds of experience that surround them" (Denzin 2014, 581).

Phenomenological awareness has been a constant backdrop and underpinning here when looking into the lives of people, their historical experiences and interpretations. With all their apparent incongruencies and 'wrong' tales, the field partners of this research generated insights that would otherwise not have been possible in such ways and such density. We have seen how people drive history, and are driven by it.

References

Literature

- Abu-Lughod, Lila. 1993. *Writing Women's Worlds: Bedouin Stories*. Berkeley [etc.]: University of California Press.
- Afanasyeva, Anna. 2013. "Forced Relocations of the Kola Sámi People: Background and Consequences." Master thesis, Tromsø: University of Tromsø. <http://munin.uit.no/handle/10037/5241>.
- . 2019. "Boarding School Education of the Sami People in Soviet Union (1935–1989): Experiences of Three Generations." Doctoral thesis, Tromsø: University of Tromsø. <https://munin.uit.no/handle/10037/15101>.
- Agarkova, T.V., ed. 2016. "Saamskii Raion." In *Kol'skaia Entsiklopediia*. Apatity, Murmansk, Sankt-Peterburg: IS, KNTs RAN. http://ke.culture.gov-murman.ru/slovník/?ELEMENT_ID=259244.
- Agrawal, Arun. 2002. "Indigenous Knowledge and the Politics of Classification." *International Social Science Journal* 54 (173): 287–297.
- Ahearn, Laura M. 2001. "Language and Agency." *Annual Review of Anthropology* 30: 109–37.
- Alekseev, S.S. 1989. "Osnovnye printsipy otbora bol'nykh alkoholizmom dlia prinuditel'nogo lecheniia." In *Voprosy kachestva psikiatricheskoi, nevrologicheskoi i narkologicheskoi pomoshchi*, edited by V.V. Kovalev, 173–81. Moskva: Moskovskii nauchno-issledovatel'skii institut psikiatrii MZ RSFSR.
- Alexander, Jon, and Joachim Schmidt. 1996. "Social Engineering: Genealogy of a Concept." In *Social Engineering*, edited by Adam Podgórecki, Jon Alexander, and Rob Shields, 1–19. Ottawa: Carleton University Press.
- Allemann, Lukas. 2010. *Die Samen Der Kola-Halbinsel: Über das Leben einer ethnischen Minderheit in der Sowjetunion*. Menschen und Strukturen, Bd. 18. Frankfurt am Main: Peter Lang Internationaler Verlag der Wissenschaften.
- . 2013. *The Sami of the Kola Peninsula: About the Life of an Ethnic Minority in the Soviet Union*. Tromsø: Septentrio Academic Publishing. <http://dx.doi.org/10.7557/sss.2013.19>.
- . 2014. "Vstrecha v 'letno-trenirovochnom polku.'" *Moskovskii Komsomolets v Murmanske*, March 12, 2014.
- . 2015. "A Memory Stick Packed With Memory – Karta pamiat'iu." *Arctic Anthropology* (blog). October 19, 2015. <https://arcticanthropology.org/2015/10/19/a-memory-stick-packed-with-memory/>.
- . 2018. "Vospominaniia korennykh zhitel' severa o natsional'nykh i vspomogatel'nykh shkolakh-internatakh – Testimonies about Boarding Schools among Indigenous People in Russia's North." *Arctic Anthropology* (blog). March 25, 2018. <https://arcticanthropology.org/2018/03/25/some-testimonies-about-boarding-schools/>.
- . 2019. "The Perspective of Former Pupils: Indigenous Children and Boarding Schools on the Kola Peninsula, 1960s to 1980s." In *Sámi Educational History in a Comparative International Perspective*, edited by Otso Kortekangas, Pigga Keskitalo, Jukka Nyssönen, Andrej Kotljarchuk, Merja Paksuniemi, and David Sjögren, 83–104. Basingstoke: Palgrave MacMillan.

- Allemann-Ghionda, Cristina. 2015. "Dealing with Diversity in Education: A Critical View on Goals and Outcomes." In *Governing through Diversity: Migration Societies in Post-Multiculturalist Times*, edited by Tatiana Matejskova and Marco Antonsich, 125–42. Global Diversities. New York: Palgrave Macmillan.
- American Anthropological Association. 2012. "Principles of Professional Responsibility." <http://ethics.americananthro.org/category/statement/>.
- Anderson, Benedict. 1983. *Imagined Communities: Reflections on the Origin and Spread of Nationalism*. London: Verso.
- Anderson, David G. 2000. *Identity and Ecology in Arctic Siberia: The Number One Reindeer Brigade*. Oxford University Press.
- Antonova, A.A. 1996. "A Saami Woman's Letter to the Central Authorities of the RSFSR." In *Anxious North: Indigenous Peoples in Soviet and Post-Soviet Russia: Selected Documents, Letters, and Articles*, edited by Alexander Pika, Jens Dahl, and Inge Larsen, 81:171–83. IWGIA Document. Copenhagen: International Work Group for Indigenous Affairs.
- Ardener, Edwin. 1987. "'Remote Areas': Some Theoretical Considerations." In *Anthropology at Home*, 25:38–54. ASA Monographs. London, New York: Tavistock Publications.
- Argounova-Low, Tatiana. 2007. "Natsionalizm: Enemies and Scapegoats." *Sibirica* 6 (1). <https://doi.org/10.3167/sib.2007.060102>.
- Argounova-Low, Tatiana, and Platon Sleptsov. 2015. "Breaking the Code: Strategies of Alcohol Addicts." *Folklore: Electronic Journal of Folklore* 61 (August): 11–26. https://doi.org/10.7592/FEJF2015.61.argounova-low_sleptsov.
- Armstrong, T., G. Rogers, and G. Rowley. 1978. *The Circumpolar North*. New York: Methuen.
- Association of Social Anthropologists of the UK and Commonwealth. 2011. "Ethical Guidelines for Good Research Practice." <https://www.theasa.org/downloads/ASA%20ethics%20guidelines%202011.pdf>.
- Atkinson, Judy, Jeff Nelson, and Caroline Atkinson. 2010. "Trauma, Transgenerational Transfer and Effects on Community Well-Being." In *Working Together: Aboriginal and Torres Strait Islander Mental Health and Wellbeing Principles and Practice*, edited by Nola Purdie, Pat Dudgeon, and Roz Walker, 135–44. Canberra: Australian Institute of Health and Welfare. http://aboriginal.telethonkids.org.au/media/54847/working_together_full_book.pdf.
- Atkinson, Paul, Amanda Coffey, and Sara Delamont. 2003. *Key Themes in Qualitative Research: Continuities and Changes*. Walnut Creek, CA: AltaMira Press.
- Bakhtin, Mikhail. 1981. "Discourse in the Novel [1934-1935]." In *The Dialogic Imagination: Four Essays*, edited by Michael Holquist, 269–422. Austin, Tex: University of Texas Press.
- . 1984. *Problems of Dostoevsky's Poetics*. Theory and History of Literature 8. Minneapolis: Univ. of Minnesota Press.
- Balakshin, Yu.Z. 1985. "Perevod korennykh zhitel'ei severa na osedlyi obraz zhizni – Aktual'naia zadacha partiinykh, Sovetskikh i khoziaistvennykh organov." In *Sotsial'no-ekonomicheskie i demograficheskie problemy zaversheniia perevoda kochevogo naseleniia na osedlyi obraz zhizni*, edited by B.M. Levin, S.N. Batulin, and F.S. Donskoi, 3–6. Apatity: Akademiia nauk SSSR / Murmanskii oblastnoi ispolnitel'nyi komitet.
- Barthes, Roland. 1986. *The Rustle of Language*. New York: Hill and Wang.
- Basso, Keith H. 1996. *Wisdom Sits in Places: Landscape and Language among the Western Apache*. Albuquerque: University of New Mexico Press.
- Bauer, Raymond A. 1952. *The New Man in Soviet Psychology*. Cambridge: Harvard University Press.
- Belvedere, Carlos. 2015. "Durkheim as the Founding Father of Phenomenological Sociology." *Human Studies* 38 (3): 369–90. <https://doi.org/10.1007/s10746-015-9357-1>.

- Bergman, Jay. 1997. "The Idea of Individual Liberation in Bolshevik Visions of the New Soviet Man." *European History Quarterly* 27 (1): 57–92. <https://doi.org/10.1177/026569149702700103>.
- . 1998. "Valerii Chkalov: Soviet Pilot as New Soviet Man." *Journal of Contemporary History* 33 (1): 135–52.
- Berg-Nordlie, Mikkel. 2011. "Need and Misery in the Eastern Periphery: Nordic Sámi Media Debate on the Kola Sámi." *Acta Borealia* 28 (1): 19–36. <https://doi.org/10.1080/08003831.2011.575657>.
- . 2015. "Two Centuries of Russian Sámi Policy: Arrangements for Autonomy and Participation Seen in Light of Imperial, Soviet and Federal Indigenous Minority Policy 1822–2014." *Acta Borealia* 32 (1): 40–67. <https://doi.org/10.1080/08003831.2015.1030849>.
- . 2017. "Fighting to Be Heard – in Russia and in Sápmi. Russian Sámi Representation in Russian and Pan-Sámi Politics, 1992–2014." Doctoral thesis, Tromsø: University of Tromsø. <https://munin.uit.no/handle/10037/11405>.
- Bloch, Alexia. 2004. *Red Ties and Residential Schools: Indigenous Siberians in a Post-Soviet State*. Philadelphia, Pa: University of Pennsylvania Press.
- Blokland, Rogier, and Michael Riessler. 2011. "Komi-Saami-Russian Contacts on the Kola Peninsula." In *Language Contact in Times of Globalization*, edited by Cornelius Hasselblatt, H. P. Houtzagers, and Remco van Paren, 5–26. Studies in Slavic and General Linguistics, v. 38. Amsterdam ; New York: Rodopi.
- Blumer, Herbert. 1969. *Symbolic Interactionism: Perspective and Method*. Englewood Cliffs (N.J.): Prentice-Hall.
- Bodrova, O.A. 2007. "Saamy glazami V.I. Nemirovicha-Danchenko: K probleme spetsifiki etnograficheskogo opisaniia." In *Regional'noe soobshchestvo v period sotsial'nykh transformatsii: Kolskii Sever, nachalo XXI veka: sbornik statei.*, edited by V. P. Petrov and I. A. Razumova, 135–46. Apatity: Kol'skii nauchnyi tsentr RAN.
- . 2008. "Saamskaia kul'tura skvoz' prizmu 'kolonial'noi etnografii' (k probleme zhanrovi prirody etnograficheskoi literatury)." In *Naselenie kol'skogo severa v period sotsial'nykh transformatsii: Problemy i praktiki kul'turnoi adaptatsii: Sbornik statei*, edited by V. P. Petrov and I. A. Razumova, 164–80. Apatity: Kol'skii nauchnyi tsentr RAN.
- Bogdanov, N.B. 2000. "Protsess urbanizatsii koren'nogo naseleniia kol'skogo poluostrova: Saami v XX veke." *Lovozerskaia Pravda*, November 18, 2000.
- Bogoiavlenskii, D.D. 1985. "Vliianie etnicheskikh protsessov na dinamiku chislennosti narodnosti severa." In *Sotsial'no-ekonomicheskie i demograficheskie problemy zaversheniia perevoda kochevogo naseleniia na osedlyi obraz zhizni*, edited by B.M. Levin, S.N. Batulin, and F.S. Donskoi, 92–93. Apatity: Akademiia nauk SSSR / Murmanskii oblastnoi ispolnitel'nyi komitet.
- Bogoyavlensky, D.D. 1996. "Peoples of Russia's North: Demographic Information." In *Anxious North: Indigenous Peoples in Soviet and Post-Soviet Russia: Selected Documents, Letters, and Articles*, edited by Alexander Pika, Jens Dahl, and Inge Larsen, 81:35–43. IWGIA Document. Copenhagen: International Work Group for Indigenous Affairs.
- . 1997. "Native Peoples of Kamchatka: Epidemiological Transition and Violent Death." *Arctic Anthropology* 34 (1): 57–67.
- Bolotova, Alla. 2014. "Conquering Nature and Engaging with the Environment in the Russian Industrialised North." Rovaniemi: University of Lapland. <https://lauda.ulapland.fi/handle/10024/61790>.
- Bombay, Amy, Kim Matheson, and Hymie Anisman. 2009. "Intergenerational Trauma: Convergence of Multiple Processes among First Nations Peoples in Canada." *Journal of Aboriginal Health* 5 (3): 6–47.

- Bombay, Amy, Kimberly Matheson, and Hymie Anisman. 2014. "The Intergenerational Effects of Indian Residential Schools: Implications for the Concept of Historical Trauma." *Transcultural Psychiatry* 51 (3): 320–38. <https://doi.org/10.1177/1363461513503380>.
- Bourdieu, Pierre. 1971. "Une Interprétation de la théorie de la religion selon Max Weber." *European Journal of Sociology / Archives Européennes de Sociologie* 12 (1): 3–21. <https://doi.org/10.1017/S000397560002174>.
- . 1977. *Outline of a Theory of Practice*. Cambridge Studies in Social Anthropology 16. Cambridge: Cambridge U. P.
- . 1987. "The Biographical Illusion." In *Working Papers and Proceedings of the Center for Psychosocial Studies*, 297–303. Chicago: Center for Psychosocial Studies.
- Bourdieu, Pierre, and Patrick Champagne. 1999. "Outcasts on the Inside." In *The Weight of the World: Social Suffering in Contemporary Society*, edited by Pierre Bourdieu, 421–26. Cambridge: Polity Press.
- Bourdieu, Pierre, and Jean-Claude Passeron. 1990. *Reproduction in Education, Society and Culture*. London, Newbury Park, New Delhi: SAGE Publications.
- Briggs, Charles L. 2007. "Anthropology, Interviewing, and Communicability in Contemporary Society." *Current Anthropology* 48 (4): 551–80. <https://doi.org/10.1086/518300>.
- Brubaker, Rogers. 2002. "Ethnicity without Groups." *European Journal of Sociology / Archives Européennes de Sociologie* 43 (2): 163–89. <https://doi.org/10.1017/S0003975602001066>.
- . 2004. *Ethnicity Without Groups*. Harvard University Press.
- Bruno, Andy. 2016. *The Nature of Soviet Power: An Arctic Environmental History*. Studies in Environment and History. New York, NY: Cambridge University Press.
- Bryleva, Z.I. 1996. "The History of the Kola Peninsular Komi-Izhemets." In *Essays on Indigenous Identity and Rights*, edited by Irja Seurujärvi-Kari and Ulla-Maija Kulonen, 157–69. Helsinki: Helsinki University Press.
- Carr, James. 1970. "Translator's Introduction." In Husserl, Edmund. *The Crisis of the European Sciences and Transcendental Phenomenology: An Introduction to Phenomenology*, xv–xliii. Evanston, Ill: Northwestern University Press.
- Chappell, David A. 1995. "Active Agents versus Passive Victims: Decolonized Historiography or Problematic Paradigm?" *The Contemporary Pacific* 7 (2): 303–26.
- Charmaz, Kathy. 2006. *Constructing Grounded Theory*. London ; Thousand Oaks, Calif: Sage Publications.
- Chatty, Dawn, and Marcus Colchester, eds. 2002. *Conservation and Mobile Indigenous Peoples: Displacement, Forced Settlement, and Sustainable Development*. New York: Berghahn Books.
- "Chislennost." n.d. Pravitel'stvo Murmanskoi Oblasti. Accessed June 9, 2019. https://gov-murman.ru/region/saami/general_info/population/index.php.
- Coffey, Amanda. 2014. "Analysing Documents." In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 367–79. Thousand Oaks, CA: Sage Publications.
- Colburn, Forrest D. 1989. "Introduction." In *Everyday Forms of Peasant Resistance*, edited by Forrest D. Colburn, ix–xv. Armonk, N.Y.: Sharpe.
- Conquest, Robert. 1967. *The Politics of Ideas in the USSR*. London: Bodley Head.
- . 1986. *The Harvest of Sorrow: Soviet Collectivization and the Terror-Famine*. New York: Oxford University Press.
- Corbin, Juliet, and Anselm Strauss. 2014. *Basics of Qualitative Research: Techniques and Procedures for Developing Grounded Theory*. SAGE Publications.
- Cruikshank, Julie. 1998. *The Social Life of Stories: Narrative and Knowledge in the Yukon Territory*. University of Nebraska Press.

- Csonka, Yvon. 1995. *Les Abiarmiut: À l'écart des Inuit Caribous*. Neuchâtel: Éditions V. Attinger.
- Davies, Bronwyn, and Rom Harré. 1990. "Positioning: The Discursive Production of Selves." *Journal for the Theory of Social Behaviour* 20 (1): 43–63. <https://doi.org/10.1111/j.1468-5914.1990.tb00174.x>.
- Davies, James. 2017a. "Political Pills: Psychopharmaceuticals and Neoliberalism as Mutually Supporting." In *The Sedated Society: The Causes and Harms of Our Psychiatric Drug Epidemic*, edited by James Davies, 189–226. Cham: Palgrave Macmillan.
- . 2017b. *The Sedated Society: The Causes and Harms of Our Psychiatric Drug Epidemic*. Cham: Palgrave Macmillan.
- Denzin, Norman K. 1989. *Interpretive Interactionism*. Newbury Park, London, New Delhi: Sage Publications.
- . 2009. *Qualitative Inquiry under Fire: Toward a New Paradigm Dialogue*. Walnut Creek: Left Coast Press.
- . 2014. "Writing and/as Analysis or Performing the World." In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 569–84. Thousand Oaks, CA: Sage Publications.
- Derrida, Jacques. 1981. *Positions*. Chicago: University of Chicago Press.
- Dobrov, V.V., V.P. Toichkina, and A.D. Korchak. 1985. "sotsial'no-ekonomicheskie i demograficheskie problemy vosproizvodstva saamskogo naseleniia." In *Sotsial'no-ekonomicheskie i demograficheskie problemy zaversheniia perevoda kochevogo naseleniia na osedlyi obraz zhizni*, edited by B.M. Levin, S.N. Batulin, and F.S. Donskoi, 97–98. Apatity: Akademiia nauk SSSR / Murmanskii oblastnoi ispolnitel'nyi komitet.
- Doiban, Vladimir A., Michael Pretes, and Alexei V. Sekarev. 1992. "Economic Development in the Kola Region, USSR: An Overview." *Polar Record* 28 (164): 7–16. <https://doi.org/10.1017/S0032247400020209>.
- Downing, Theodore E., and Carmen Garcia-Downing. 2009. "Routine and Dissonant Cultures: A Theory about the Psycho-Socio-Cultural Disruptions of Involuntary Displacement and Ways to Mitigate Them without Inflicting Even More Damage." In *Development and Dispossession: The Crisis of Forced Displacement and Resettlement*, edited by Anthony Oliver-Smith, 225–54. Santa Fe, N.M.: School for Advanced Research Press.
- Dudeck, Stephan. 2013a. *Der Tag des Rentierzüchters: Repräsentation indigener Lebensstile zwischen Taigawohnplatz und Erdölstadt in Westsibirien*. Studies in Social and Cultural Anthropology. Fürstenberg/Havel: BoD – Books on Demand.
- . 2013b. "Oral History of Empires by Elders in the Arctic (ORHELIA): A Common History, a Common Economy, Common Language Roots, and Different Practices among Four Arctic Indigenous Peoples." In *Oral History: Dialogue With Society = Mutvārdu Vēsture: Dialogs Ar Sabiedrību*, edited by Ieva Garda-Rozenberga, 56–76. Riga: Institute of Philosophy and Sociology, University of Latvia. http://issuu.com/lufsi/docs/oral_history/1.
- . 2018. "Reindeer Returning from Combat: War Stories among the Nenets of European Russia." *Arctic Anthropology* 55 (1): 73–90. <https://doi.org/10.3368/aa.55.1.73>.
- Eberle, Thomas S. 2014. "Phenomenology as a Research Method." In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 184–202. Thousand Oaks, CA: Sage Publications.
- Eidlitz Kuoljok, Kerstin. 1985. *The Revolution in the North*. Stockholm: Almqvist & Wiksell.
- Einarsson, Niels, Joan Nymand Larsen, Annika Nilsson, and Oran R. Young. 2004. *Arctic Human Development Report*. Stefansson Arctic Institute, under the auspices of the Icelandic Chairmanship of the Arctic Council 2002-2004. <https://oaarchive.arctic-council.org/handle/11374/51>.

- Eisner, Elliot W. 1997. "The New Frontier in Qualitative Research Methodology." *Qualitative Inquiry* 3 (3): 259–73. <https://doi.org/10.1177/107780049700300301>.
- Engel'gardt, Aleksandr Platonovich. 2009. *Russkii Sever: Putevye zapiski [1897]*. Moskva: OGI.
- Erickson, Frederick. 2012. "Comments on Causality in Qualitative Inquiry." *Qualitative Inquiry* 18 (8): 686–88. <https://doi.org/10.1177/1077800412454834>.
- Eriksen, Thomas Hylland. 2010. *Ethnicity and Nationalism: Anthropological Perspectives*. 3rd ed. Anthropology, Culture, and Society. London ; New York : New York: Pluto Press ; Distributed in the United States of America exclusively by Palgrave Macmillan.
- . 2013. "Norwegian Anthropologists Study Minorities at Home: Political and Academic Agendas." In *Toward Engaged Anthropology*, edited by Sam Beck and Carl A. Maida, 36–54. New York: Berghahn Books.
- Esin, Cigdem, Mastoureh Fathi, and Corinne Squire. 2014. "Narrative Analysis: The Constructionist Approach." In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 203–16. Thousand Oaks, CA: Sage Publications.
- Fabian, Johannes. 1983. *Time and the Other: How Anthropology Makes Its Object*. New York: Columbia University Press.
- Farnell, Brenda, and Laura R. Graham. 2015. "Discourse-Centered Methods." In *Handbook of Methods in Cultural Anthropology*, edited by Harvey Russell Bernard and Clarence C. Gravlee, 2nd ed., 391–438. Lanham: Rowman & Littlefield.
- Fedorov, P.V. 2009. *Severnii vektor v Rossiiskoi istorii. Tsentri i kol'skoe zapoliar'e v XVI-XX Vv*. Murmansk: Murmanskii Gosudarstvennyi Pedagogicheskii Universitet.
- Field, Mark G., and David E. Powell. 1981. "Alcohol Abuse in the Soviet Union." *The Hastings Center Report* 11 (5): 40–44. <https://doi.org/10.2307/3561298>.
- Fincham, Benjamin, Susanne Langer, Jonathan Scourfield, and Michael Shiner, eds. 2011. *Understanding Suicide: A Sociological Autopsy*. Basingstoke: Palgrave Macmillan.
- Fischer, Edward F. 2014. *The Good Life: Aspiration, Dignity, and the Anthropology of Wellbeing*. Stanford: Stanford University Press.
- Fitzpatrick, Sheila. 1993. "How the Mice Buried the Cat: Scenes from the Great Purges of 1937 in the Russian Provinces." *The Russian Review* 52 (3): 299–320.
- Florin, Moritz. 2015. *Kirgistan und die sowjetische Moderne: 1941-1991*. Kultur- und Sozialgeschichte Osteuropas 3. Göttingen: V & R Unipress.
- Fogel-Chance, Nancy. 1993. "Living in Both Worlds: 'Modernity' and 'Tradition' among North Slope Inupiaq Women in Anchorage." *Arctic Anthropology* 30 (1): 94–108.
- Forbes, Bruce C., and Florian Stammler. 2009. "Arctic Climate Change Discourse: The Contrasting Politics of Research Agendas in the West and Russia." *Polar Research* 28 (1): 28–42. <https://doi.org/10.1111/j.1751-8369.2009.00100.x>.
- Forsey, Martin G. 2010. "Ethnography as Participant Listening." *Ethnography* 11 (4): 558–72. <https://doi.org/10.1177/1466138110372587>.
- Forsyth, James. 1992. *A History of the Peoples of Siberia: Russia's North Asian Colony, 1581-1990*. Cambridge: Cambridge University Press.
- Foucault, Michel. 1997. "The Punitive Society." In *Ethics: Subjectivity and Truth*, edited by Paul Rabinow, 23–37. The Essential Works of Foucault, vol. 1. New York: New Press.
- Garfinkel, Harold. 2002. *Ethnomethodology's Program: Working out Durkheim's Aporism*. Edited by Anne Warfield Rawls. Legacies of Social Thought. Lanham, Md: Rowman & Littlefield.
- . 2006. *Seeing Sociologically: The Routine Grounds of Social Action*. Edited by Anne Warfield Rawls. Great Barrington Books. Boulder: Paradigm.

- Gibbs, Graham R. 2014. "Using Software in Qualitative Analysis." In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 277–94. Thousand Oaks, CA: Sage Publications.
- Giddens, Anthony. 1995. *The Constitution of Society: Outline of the Theory of Structuration*. Cambridge: Polity Press.
- Girard, René. 1986. *The Scapegoat*. Baltimore: Johns Hopkins University.
- Glaser, Barney G. 1992. *Basics of Grounded Theory Analysis*. Mill Valley, CA: Sociology Press.
- Goffman, Erving. 1955. "On Face-Work." *Psychiatry* 18 (3): 213–31. <https://doi.org/10.1521/00332747.1955.11023008>.
- Goldner, Fred H., R. Richard Ritti, and Thomas P. Ference. 1977. "The Production of Cynical Knowledge in Organizations." *American Sociological Review* 42 (4): 539–51. <https://doi.org/10.2307/2094553>.
- Golovnev, Andrei, and Gail Osherenko. 1999. *Siberian Survival: The Nenets and Their Story*. Cornell University Press.
- Gramsci, Antonio. 1920. "Discorso agli anarchici." *L'Ordine Nuovo* 1 (43): 1–2.
- Grant, Bruce. 1995. *In the Soviet House of Culture: A Century of Perestroikas*. Princeton, N.J.: Princeton University Press.
- Gray, Patty, Nikolai Vakhtin, and Peter Schweitzer. 2004. "Who Owns Siberian Ethnography? A Critical Assessment of a Re-Internationalized Field." *Sibirica: Journal of Siberian Studies* 3 (2): 194–216. <https://doi.org/10.1080/1361736042000245312>.
- Grenoble, Lenore A. 2003. *Language Policy in the Soviet Union*. Dordrecht: Kluwer Academic Publishers.
- Gubrium, Jaber F., and James A. Holstein. 2014. "Analytic Inspiration in Ethnographic Fieldwork." In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 35–48. Thousand Oaks, CA: Sage Publications.
- Gutsol, N.N., S.N. Vinogradova, and A.G. Samorukova. 2007. *Pereselennye gruppy Kol'skikh saamov*. Apatity: Kol'skii nauchnyi tsentr RAN.
- Habeck, Joachim Otto. 2005. *What It Means to Be a Herdsman: The Practice and Image of Reindeer Husbandry among the Komi of Northern Russia*. Halle Studies in the Anthropology of Eurasia, Vol. 5. Münster: LIT.
- . 2015. "Lukas Allemann: Die Samen der Kola-Halbinsel. Über das Leben einer ethnischen Minderheit in der Sowjetunion. Frankfurt a.M. [Usw.]: Lang, 2010. 130 S., 18 Abb., Ktc., Tab. = Menschen und Strukturen. Historisch-Sozialwissenschaftliche Studien, 18. ISBN: 978-3-631-61201-9." *Jahrbücher Für Geschichte Osteuropas* 63 (1): 155–56.
- Hagemeister, Michael, and Julia Richers. 2016. "Utopien der Revolution." In *Die Russische Revolution 1917*, edited by Heiko Haumann. Köln; Weimar; Wien Böhlau Verlag.
- Halbwachs, Maurice. 1992 [1925]. *On Collective Memory*. Chicago: University Of Chicago Press.
- Halfin, Igal. 2003. *Terror in My Soul: Communist Autobiographies on Trial*. Cambridge (Mass.): Harvard University Press.
- Hamilton, Lawrence C., Rasmus Ole Rasmussen, Nicholas E. Flanders, and Carole L. Seyfrit. 1996. "Outmigration and Gender Balance in Greenland." *Arctic Anthropology* 33 (1): 89–97.
- Hamilton, Lawrence C., and Carole L. Seyfrit. 1993. "Town-Village Contrasts in Alaskan Youth Aspirations." *Arctic* 46 (3): 255–63.
- . 1994. "Coming out of the Country: Community Size and Gender Balance among Alaskan Natives." *Arctic Anthropology* 31 (1): 16–25.
- Hastrup, Kirsten. 2004. "Getting It Right: Knowledge and Evidence in Anthropology." *Anthropological Theory* 4 (4): 455–72. <https://doi.org/10.1177/1463499604047921>.

- Haumann, Heiko. 2006. "Geschichte, Lebenswelt, Sinn: Über die Interpretation von Selbstzeugnissen." In *Anfang und Grenzen des Sinns: Für Emil Angehrn*, edited by Emil Angehrn, Brigitte Hilmer, Georg Lohmann, and Tilo Wesche, 42–54. Weilerswist: Velbrück Wissenschaft.
- Heidegger, Martin. 1971. *Poetry, Language, Thought*. New York: Harper and Row.
- Heininen, Lassi. 2010. "Globalization and Security in the Circumpolar North." In *Globalization and the Circumpolar North*, edited by Lassi Heininen and Chris Southcott, 221–64. Fairbanks: University of Alaska Press.
- Herman, Edward S., and Noam Chomsky. 1988. *Manufacturing Consent: The Political Economy of the Mass Media*. New York: Pantheon Books.
- Herzfeld, Michael. 1997. *Cultural Intimacy: Social Poetics in the Nation-State*. New York: Routledge.
- Himanka, Juha. 2001. "Before and After the Reduction." *Journal of the British Society for Phenomenology* 32 (2): 188–204. <https://doi.org/10.1080/00071773.2001.11007329>.
- . 2010. "The Idea of Phenomenology: Reading Husserliana as Reductions." *Dialogue: Canadian Philosophical Review/Revue Canadienne de Philosophie* 49 (4): 617–640.
- Hobsbawm, Eric John. 1968. *Industry and Empire*. The Pelican Economic History of Britain. Harmondsworth: Penguin.
- . 2000. *Bandits*. New York (NY): New Press.
- Hokkanen, Liisa. 2017. *Experiences of Inclusion and Welfare Services among Finnish Sámi with Disabilities*. Stockholm: Nordic Welfare Centre. <https://nordicwelfare.org/en/publikationer/experiences-of-inclusion-and-welfare-services-among-finnish-sami-with-disabilities/>.
- Holzlehner, Tobias. 2011. "Engineering Socialism: A History of Village Relocations in Chukotka, Russia." In *Engineering Earth, 1957–73*. Dordrecht: Springer. https://doi.org/10.1007/978-90-481-9920-4_108.
- Hønneland, Geir. 2013. *Borderland Russians: Identity, Narrative and International Relations*. Palgrave Macmillan.
- Hønneland, Geir, and Anne-Kristin Jørgensen. 1999. *Integration vs. Autonomy: Civil-Military Relations on the Kola Peninsula*. Aldershot: Ashgate.
- Humphrey, Caroline. 1994. "Remembering an 'Enemy': The Bogd Khaan in Twentieth-Century Mongolia." In *Memory, History, and Opposition under State Socialism*, edited by Rubie S. Watson, 21–44. School of American Research Advanced Seminar Series. Santa Fe, N.M.: School of American Research Press.
- Husserl, Edmund. 1970 [1936]. *The Crisis of the European Sciences and Transcendental Phenomenology: An Introduction to Phenomenology*. Evanston, Ill: Northwestern University Press.
- . 1982 [1931]. *Cartesian Mediations: An Introduction to Phenomenology*. 7th ed. The Hague, Boston, London: Martinus Nijhoff.
- . 1983 [1913]. *Ideas Pertaining to a Pure Phenomenology and to a Phenomenological Philosophy, First Book: General Introduction to a Pure Phenomenology*. The Hague, Boston, London: Martinus Nijhoff.
- Ilic, Melanie. 2016. "From Interview to Life Story: Methodology and Ethics in Oral History." In *The Soviet Past in the Post-Socialist Present: Methodology and Ethics in Russian, Baltic and Central European Oral History and Memory Studies*, edited by Melanie Ilič and Dalia Leinarte. Routledge Approaches to History. New York: Routledge.
- Ingold, Tim. 2000. *The Perception of the Environment: Essays on Livelihood, Dwelling and Skill*. London; New York: Routledge.
- . 2011. *Being Alive: Essays on Movement, Knowledge and Description*. London; New York: Routledge.

- . 2013. *Making: Anthropology, Archaeology, Art and Architecture*. London ; New York: Routledge.
- . 2017. “Anthropology Contra Ethnography.” *HAA: Journal of Ethnographic Theory* 7 (1): 21–26. <https://doi.org/10.14318/hau7.1.005>.
- . 2019. “The North Is Everywhere.” In *Knowing from the Indigenous North: Sámi Approaches to History, Politics and Belonging*, edited by Thomas Hylland Eriksen, Sanna Valkonen, and Jarno Valkonen, 108–19. Abingdon, Oxon; New York, NY: Routledge.
- “Itogi Vserossiiskoi perepisi naseleniia 2010 goda.” 2010a. 4. http://www.gks.ru/free_doc/new_site/perepis2010/croc/Documents/Vol4/pub-04-19.pdf.
- “Itogi Vserossiiskoi perepisi naseleniia 2010 goda.” ———. 2010b. 25. http://www.gks.ru/free_doc/new_site/perepis2010/croc/Documents/Vol4/pub-04-25.pdf.
- Ivanets, N.N., V.E. Pelipas, and I.A. Nikiforov. 1991. “Novaia kontseptsiiia organizatsii narkologicheskoi pomoshchi v SSSR.” *Voprosy Narkologii* 4: 2–6.
- Jääts, Indrek. 2009. “The Komi, Ethnic Stereotypes, and Nationalities Policy in Late Imperial Russia.” *The Russian Review* 68 (2): 199–220.
- Jakobson, Roman. 1960. “Closing Statement: Linguistics and Poetics.” In *Style in Language*, edited by Thomas Albert Sebeok, 350–77. Cambridge (Mass.): MIT Press.
- Jenkins, Timothy. 1994. “Fieldwork and the Perception of Everyday Life.” *Man* 29 (2): 433–55. <https://doi.org/10.2307/2804481>.
- Josephson, Paul R. 2014. *The Conquest of the Russian Arctic*. Cambridge: Harvard University Press.
- Junka-Aikio, Laura. 2016. “Can the Sámi Speak Now? Deconstructive Research Ethos and the Debate on Who Is a Sámi in Finland.” *Cultural Studies* 30 (2): 205–33. <https://doi.org/10.1080/09502386.2014.978803>.
- Kalstad, Iokhan Al’bert. 2009. *Dorogoi nadezhd. Politika Rossiiskogo Gosudarstva i polozhenie saamskogo naroda v Rossii (1864-2003)*. Murmansk: Murmanskoe knizhnoe izdatel’stvo.
- Kaminsky, V.I. 1996. “Medical Care of the Indigenous Peoples in the Lovozero County.” In *Essays on Indigenous Identity and Rights*, edited by Irja Seurujärvi-Kari and Ulla-Maija Kulonen, 149–56. Helsinki: Helsinki University Press.
- Kasatkina, Aleksandra, Zinaida Vasilyeva, and Roman Khandozhko. 2018. “Thrown into Collaboration: An Ethnography of Transcript Authorization.” In *Experimental Collaborations: Ethnography through Fieldwork Devices*, edited by Adolfo Estalella and Tomás Sánchez Criado, 132–53. New York: Berghahn Books.
- Keesing, Roger M. 1989. “Creating the Past: Custom and Identity in the Contemporary Pacific.” *The Contemporary Pacific* 1 (1/2): 19–42.
- Kelle, Udo. 2014. “Theorization from Data.” In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 554–68. Thousand Oaks, CA: Sage Publications.
- Kelly, Catriona, and David Shepherd, eds. 1998. “Programmes for Identity: The ‘New Man’ and the ‘New Woman.’” In *Constructing Russian Culture in the Age of Revolution, 1881-1940*, 256–90. Oxford [England] ; New York: Oxford University Press.
- Kharkhordin, Oleg. 1999. *The Collective and the Individual in Russia: A Study of Practices*. Studies on the History of Society and Culture 32. Berkeley: University of California Press.
- Kheveshi, M.A. 2002. *Tolkoviye slovar’ ideologicheskikh i politicheskikh terminov sovetskogo perioda*. Moskva: Mezhdunarodnye otnosheniia.
- Khlinovskaya Rockhill, Elena. 2010. *Lost To The State: Family Discontinuity, Social Orphanhood and Residential Care in the Russian Far East*. Oxford: Berghahn Books.
- Kiselev, A.A. 2009. *Ocherki etnicheskoi istorii kol’skogo severa*. Murmansk: Murmanskii Gosudarstvennyi Pedagogicheskii Universitet.

- Kohlhoff, Dean. 1995. *When the Wind Was a River: Aleut Evacuation in World War II*. Seattle: University of Washington Press.
- Kol'skaia Entsiklopediia. 2016. 4 vols. Apatity, Murmansk, Sankt-Peterburg: IS, KNTs RAN. http://ke.culture.gov-murman.ru/slovník/?ELEMENT_ID=259244.
- Konakov, N. D., and Lydia T. Black. 1993. "Ecological Adaptation of Komi Resettled Groups." *Arctic Anthropology* 30 (2): 92–102.
- Konstantinov, Yulian. 2005a. "From 'Traditional' to Collectivized Reindeer Herding on the Kola Peninsula: Continuity or Disruption?" *Acta Borealia* 22 (2): 170–88. <https://doi.org/10.1080/08003830500370168>.
- . 2005b. *Reindeer-Herders: Field-Notes from the Kola Peninsula (1994-95)*. Dissertations and Documents in Cultural Anthropology 4. Uppsala: Uppsala univ.
- . 2007. "Reinterpreting the Sovkhoz." *Sibirica* 6 (2): 1–25. <https://doi.org/10.3167/sib.2007.060201>.
- . 2009. "Roadlessness and the Person: Modes of Travel in the Reindeer Herding Part of the Kola Peninsula." *Acta Borealia* 26 (1): 27–49. <https://doi.org/10.1080/08003830902951524>.
- . 2010. "Socioeconomic Life of Climate Change: Extensivity in Reindeer Husbandry in Relation to Synergies between Social and Climate Change (Kola Peninsula)." *Acta Borealia* 27 (1): 44–65. <https://doi.org/10.1080/08003831.2010.486925>.
- . 2015. *Conversations with Power: Soviet and Post-Soviet Developments in the Reindeer Husbandry Part of the Kola Peninsula*. Uppsala Studies in Cultural Anthropology 56. Uppsala: Acta Universitatis Upsaliensis. <http://uu.diva-portal.org/smash/record.jsf?pid=diva2:865695>.
- . 2017. "Tourist Hegemonies of Outside Powers: The Case of Salmon Fishing Safari Camps in Territories of Traditional Land Use (Kola Peninsula)." In *Tourism and Indigeneity in the Arctic*, edited by Arvid Viken and Dieter K. Müller. Tourism and Cultural Change 51. Bristol, UK Blue Ridge Summit, PA, USA: Channel View Publications.
- . 2018. "'Hyper-Gender' Asymmetries: Women's Absence in Illegal Taking from Nature (Poaching) (Notes from Bulgaria and Murmansk Region, NW Russia)." *Polar Geography* 41 (3): 217–33. <https://doi.org/10.1080/1088937X.2018.1491651>.
- Konstantinov, Yulian, Kirill Istomin, Inna Ryzhkova, and Yulia Mitina. 2018. "'Uncontrolled Sovkhoism': Administering Reindeer Husbandry in the Russian Far North (Kola Peninsula)." *Acta Borealia* 0 (0): 1–20. <https://doi.org/10.1080/08003831.2018.1536196>.
- Koskinen-Koivisto, Eerika, and Suzie Thomas. 2017. "Lapland's Dark Heritage: Responses to the Legacy of World War II." In *Heritage in Action: Making the Past in the Present*, edited by Helaine Silverman, Emma Waterton, and Steve Watson, 121–33. Cham: Springer International Publishing. https://doi.org/10.1007/978-3-319-42870-3_9.
- Kotljarchuk, Andrej. 2012. "Kola Sami in the Stalinist Terror: A Quantitative Analysis." *Journal of Northern Studies* 6 (2): 59–82.
- . 2014. "The Nordic Threat: Soviet Ethnic Cleansing on the Kola Peninsula." In *The Sea of Identities: A Century of Baltic and East European Experiences with Nationality, Class, and Gender*, edited by Norbert Götz, 53–83. Huddinge: Södertörn University.
- . 2017. "Propaganda of Hatred in the Great Terror. A Nordic Approach." In *Ethnic and Religious Minorities in Stalin's Soviet Union: New Dimensions of Research*, edited by Andrej Kotljarchuk and Olle Sundström, 89–121. Huddinge: Södertörn University.
- Kotov, O.V. 1987. *Etnicheskoe samosoznanie kol'skikh Komi: doklad na zasedanii prezidiuma Komi nauchnogo tsentra UrO AN SSSR 17 dekabria 1987 g.* Nauchnye doklady, Vypusk 178. Syktyvkar: Komi nauchnyi tsestr UrO AN SSSR.

- Kozlov, A.I., and D.D. Bogoiavlenskii. 2008. "Smertnost' ot vneshnikh prichin i otkloniaiusheesia povedenie." In *Kol'skie saamy v meniaiusbchemsia mire*, edited by A.I. Kozlov, D.V. Lisitsyn, and M.A. Kozlova, 78–85. Moskva: Institut nasledia. <http://www.hse.ru/data/2012/12/17/1300811327/book.pdf>.
- Kozlov, A.I., D.V. Lisitsyn, and M.A. Kozlova, eds. 2008. *Kol'skie saamy v meniaiusbchemsia mire*. Moskva: Institut nasledia. <http://www.hse.ru/data/2012/12/17/1300811327/book.pdf>.
- Krömer, Alicia, and Lukas Allemann. 2016. "Arktische Assimilierungspolitiken: Indigene Kinder in den Internatsschulen des 20. Jahrhunderts." In *Arktis und Subarktis: Geschichte, Kultur, Gesellschaft*, edited by Gertrude Saxinger, Peter Schweitzer, and Stefan Donecker, 103–20. Edition Weltregionen. Wien: New Academic Press.
- Krupnik, Igor, and Mikhail Chlenov. 2007. "The End of 'Eskimo Land': Yupik Relocation in Chukotka, 1958-1959." *Études/Inuit/Studies* 31 (1/2): 59–81.
- Kuchinskii, M.G. 2007. "Ritorika traditsionnosti i realii prirodopol'zovaniia." In *Rasy i narody: Sovremennye etnicheskie i rasovye problemy*, edited by D.A. Funk, 33:58–88. Moskva: Nauka.
- . 2008. *Saami Kol'skogo Uezda v XVI-XVIII vv.: Model' Sotsial'Noi Struktury*. Diedut, 2/2008. Kautokeino: Sámi University College [includes an English summary].
- Kulonen, Ulla-Maija, Irja Seurujärvi-Kari, and Risto Pulkkinen, eds. 2005. *The Saami: A Cultural Encyclopaedia*. Helsinki: Suomalaisen Kirjallisuuden Seura.
- Kuokkanen, Rauna. 2009. "Indigenous Women in Traditional Economies: The Case of Sámi Reindeer Herding." *Signs: Journal of Women in Culture and Society* 34 (3): 499–504. <https://doi.org/10.1086/593382>.
- Kuropjatnik, Marina. 1999. "Expeditions to Sámi Territories. A History of the Studies of the Kola Sami in the 1920s-1930s." *Acta Borealia* 16 (1): 117–124.
- Kuznetsova, Anna. 2014a. "Antropologi issleduiut korennye narody." *Lovozerskaia Pravda*, May 16, 2014.
- . 2014b. "Sobirateľ istorii, ili sud'by v ustnykh rasskazakh." *Lovozerskaia Pravda*, September 12, 2014.
- Lantto, Patrik. 2014. "The Consequences of State Intervention: Forced Relocations and Sámi Rights in Sweden, 1919–2012." *Journal of Ethnology and Folkloristics* 8 (2): 53–73.
- Laptander, Roza. 2014. "Processes of Remembering and Forgetting: Tundra Nenets' Reminiscences of the 1943 Mandalada Rebellions." *Sibirica* 13 (3): 22–44. <https://doi.org/10.3167/sib.2014.130302>.
- . 2017. "Collective and Individual Memories: Narrations about the Transformations in the Nenets Society." *Arctic Anthropology* 54 (1): 22–31. <https://doi.org/10.3368/aa.54.1.22>.
- Lashov, B.V. 1985. "Sotsial'nyi aspekt perevoda kochevogo naseleniia k osedlosti." In *Sotsial'no-ekonomicheskie i demograficheskie problemy zaversheniia perevoda kochevogo naseleniia na osedlyi obraz zhizni*, edited by B.M. Levin, S.N. Batulin, and F.S. Donskoi, 48–50. Apatity: Akademiia nauk SSSR / Murmanskii oblastnoi ispolnitel'nyi komitet.
- Leete, Art. 2018. "Editorial Impressions: Bricolage and the Ethnographic Field." *Journal of Ethnology and Folkloristics* 12 (2): 3–7. <https://doi.org/10.2478/jef-2018-0007>.
- Leinarte, Dalia. 2016. "Silence in Biographical Accounts and Life Stories. The Ethical Aspects of Interpretation." In *The Soviet Past in the Post-Socialist Present: Methodology and Ethics in Russian, Baltic and Central European Oral History and Memory Studies*, edited by Melanie Ilič and Dalia Leinarte, 12–18. Routledge Approaches to History. New York: Routledge.
- Levy, Robert I., and Douglas W. Hollan. 2015. "Person-Centered Interviewing and Observation." In *Handbook of Methods in Cultural Anthropology*, edited by Harvey Russell Bernard and Clarence C. Gravlee, 2nd ed., 313–42. Lanham: Rowman & Littlefield.

- Liarskaia, Elena. 2003. "Severnnye internaty i transformatsiia traditsionnoi kul'tury (na primere nentsev Iamala)." Doctoral thesis, St. Petersburg: European University.
- Liarskaya, Elena. 2004. "Northern Residential Schools in Contemporary Yamal Nenets Culture." *Sibirica: Journal of Siberian Studies* 4 (1): 74–87.
- Lippmann, Walter. 1922. *Public Opinion*. New York: Harcourt, Brace and Company.
- Lokhanov, Aleksandr, ed. 2013. "Ekostrovskii pogost." In *Kol'skii Sever: Entsiklopedicheskii leksikon*. Murmansk: Prosvetitel'skii tsentr "Dobrokhhot." http://lexicon.dobrohot.org/index.php/экостровский_погост.
- Lomawaima, K. Tsianina. 1994. *They Called It Prairie Light: The Story of Chilocco Indian School*. Lincoln (Neb.) [etc.]: University of Nebraska Press.
- Lovozerkaia Pravda. 1988. "V ideologicheskoi komissii," May 7, 1988.
- Lüdtke, Alf. 1995. "Introduction: What Is the History of Everyday Life and Who Are Its Practitioners?" In *The History of Everyday Life: Reconstructing Historical Experiences and Ways of Life*, edited by Alf Lüdtke, 3–40. Princeton, N.J.: Princeton University Press.
- Luk'ianchenko, T.V. 1971. *Material'naia kul'tura saamov (loparei) Kol'skogo poluostrova v kontse XIX-XX v.* Moskva: Nauka.
- Lukin, Karina. 2017. "Leaving Novaia Zemlia: Narrative Strategies of the Resettlement of the Nenets." *Arctic Anthropology* 54 (1): 32–45. <https://doi.org/10.3368/aa.54.1.32>.
- Luzin, Gennady P., Michael Pretes, and Vladimir V. Vasiliev. 1994. "The Kola Peninsula: Geography, History and Resources." *Arctic* 47 (1): 1–15.
- Madison, Bernice Q. 1968. *Social Welfare in the Soviet Union*. Stanford (Calif.): Stanford University Press.
- Mankova, Petia. 2018a. "The Komi of the Kola Peninsula within Ethnographic Descriptions and State Policies." *Nationalities Papers* 46 (1): 34–51. <https://doi.org/10.1080/00905992.2017.1345882>.
- . 2018b. "Homewarding Remoteness: Representations, Agency and Everyday Life in a Tundra Village (NW Russia)," February. <https://munin.uit.no/handle/10037/12107>.
- Marcus, Alan R. 1995. *Relocating Eden: The Image and Politics of Inuit Exile in the Canadian Arctic*. Hanover: University Press of New England.
- Martin, Terry. 1998. "The Origins of Soviet Ethnic Cleansing." *The Journal of Modern History* 70 (4): 813–61. <https://doi.org/10.1086/235168>.
- . 2001. *The Affirmative Action Empire: Nations and Nationalism in the Soviet Union, 1923-1939*. Ithaca (N.Y.) ; London: Cornell University Press.
- Marvasti, Amir B. 2014. "Analysing Observations." In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 354–66. Thousand Oaks, CA: Sage Publications.
- Maxwell, Joseph A., and Margaret Chmiel. 2014. "Generalization in and from Qualitative Analysis." In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 540–53. Thousand Oaks, CA: Sage Publications.
- May, Tim, and Beth Perry. 2014. "Reflexivity and the Practice of Qualitative Research." In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 109–22. Thousand Oaks, CA: Sage Publications.
- Mayring, Philipp. 2002. *Einführung in die qualitative Sozialforschung*. Weinheim, Basel: Beltz.
- Mazzullo, Nuccio. 2017. "And People Asked: 'We Want to Have Lakes to Fish!' And Lakes Were Given. Skolt Sámi Relocation after WWII in Finland." *Arctic Anthropology* 54 (1): 46–60. <https://doi.org/10.3368/aa.54.1.46>.
- Melvin, Neil. 2003. *Soviet Power and the Countryside: Policy Innovation and Institutional Decay*. Houndmills, Basingstoke, Hampshire; New York: Palgrave Macmillan in association with St. Antony's College, Oxford.

- Miles, Matthew B., A. Michael Huberman, and Johnny Saldana. 2013. *Qualitative Data Analysis: A Methods Sourcebook*. Thousand Oaks, CA: SAGE Publications.
- Miles, Matthew B., and Alan M. Huberman. 1984. *Qualitative Data Analysis: A Sourcebook of New Methods*. Beverly Hills (Calif) [etc.]: Sage Publ.
- Minde, Henry. 2003. "Assimilation of the Sami – Implementation and Consequences." *Acta Borealia* 20 (2): 121–46. <https://doi.org/10.1080/08003830310002877>.
- Nachtigal, Reinhard. 2007. *Die Murmanbahn 1915 Bis 1919: Kriegsnotwendigkeit Und Wirtschaftsinteressen*. Remshalden: BAG-Verlag.
- Niethammer, Lutz. 2012. "Fragen - Antworten - Fragen. Methodische Erfahrungen und Erwägungen zur Oral History [1985]." In *Oral History*, edited by Julia Obertreis, 31–71. Stuttgart: Steiner.
- Nietzsche, Friedrich Wilhelm. 2007. *On the Genealogy of Morality*. Cambridge ; New York: Cambridge University Press.
- Obertreis, Julia. 2004. *Tränen des Sozialismus: Wohnen in Leningrad zwischen Alltag und Utopie 1917-1937*. Beiträge zur Geschichte Osteuropas, Bd. 37. Köln: Böhlau.
- . 2015. "Sprechen über das Leben Im Sozialismus. Vom Recht auf Glück und auf Scham." In *Es gilt das gesprochene Wort. Oral History und Zeitgeschichte heute (Festschrift für Dorothee Wierling)*, edited by Knud Andersen, Linde Apel, and Linde Heinsohn, 98–113. Göttingen: Wallstein Verlag.
- Oliver-Smith, Anthony, ed. 2009a. *Development & Dispossession: The Crisis of Forced Displacement and Resettlement*. Santa Fe, N.M.: School for Advanced Research Press.
- . 2009b. "Introduction: Development-Forced Displacement and Resettlement: A Global Human Rights Crisis." In *Development and Dispossession: The Crisis of Forced Displacement and Resettlement*, edited by Anthony Oliver-Smith, 3–23. Santa Fe, N.M.: School for Advanced Research Press.
- Orehova, E.A. 2009. "Kolonizatsiia murmanskogo berega kol'skogo poluostrova vo vtoroi polovine XIX-pervoii treti XX vv." Doctoral thesis, St. Petersburg: St. Petersburg State University. http://kolanord.ru/html_public/col_avtory/OrehovaEA/OrehovaEA_Kolonizac-Murman-berega-Kol_poluost_dissertac_2009/index.html.
- Ortner, Sherry B. 1995. "Resistance and the Problem of Ethnographic Refusal." *Comparative Studies in Society and History* 37 (1): 173–93.
- . 2016. "Dark Anthropology and Its Others: Theory since the Eighties." *HAU: Journal of Ethnographic Theory* 6 (1): 47–73. <https://doi.org/10.14318/hau6.1.004>.
- Overland, Indra, and Mikkel Berg-Nordlie. 2012. *Bridging Divides: Ethno-Political Leadership among the Russian Sami*. Berghahn Books.
- Passerini, Luisa. 1998. "Work Ideology and Consensus under Italian Fascism." In *The Oral History Reader*, edited by Robert Perks and Alistair Thomson, 53–62. London ; New York: Routledge.
- Pelto, Pertti J. 1973. *The Snowmobile Revolution: Technology and Social Change in the Arctic*. Menlo Park, CA: Cummings Publishing Company.
- Pika, Alexander. 1993. "The Spatial-Temporal Dynamic of Violent Death among the Native Peoples of Northern Russia." *Arctic Anthropology* 30 (2): 61–76.
- . 1996. "The Small Peoples of the North: From Primitive Communism to 'Real Socialism.'" In *Anxious North: Indigenous Peoples in Soviet and Post-Soviet Russia: Selected Documents, Letters, and Articles*, edited by Alexander Pika, Jens Dahl, and Inge Larsen, 81:15–33. IWGIA Document. Copenhagen: International Work Group for Indigenous Affairs.
- Pika, Alexander, Jens Dahl, and Inge Larsen, eds. 1996. *Anxious North: Indigenous Peoples in Soviet and Post-Soviet Russia: Selected Documents, Letters, and Articles*. Vol. 81. IWGIA Document. Copenhagen: International Work Group for Indigenous Affairs.

- Plotkin, F.B. 2015. "Novye tendentsii v Rossiiskoi narkologii: Vozvrat k starym silovym metodam prinuditel'nogo lecheniia." *Nezavisimyi psikhiatricheskii zhurnal*, no. 4: 13–24.
- Podgórecki, Adam. 1996. "Sociotechnics: Basic concepts and issues." In *Social Engineering*, edited by Adam Podgórecki, Jon Alexander, and Rob Shields, 23–57. Ottawa: Carleton University Press.
- Portelli, Alessandro. 1991. "The Death of Luigi Trastulli: Memory and the Event." In *The Death of Luigi Trastulli, and Other Stories: Form and Meaning in Oral History*. SUNY Series in Oral and Public History. Albany, N.Y.: State University of New York Press.
- . 1998. "What Makes Oral History Different." In *The Oral History Reader*, edited by Robert Perks and Alistair Thomson, 63–74. London; New York: Routledge.
- Povoroznyuk, Olga, Joachim Otto Habeck, and Virginie Vaté. 2010. "Introduction: On the Definition, Theory, and Practice of Gender Shift in the North of Russia." *Anthropology of East Europe Review* 28 (2): 1–37.
- Prokhorov, A.M., ed. 1978. "Tovarishcheskii Sud." In *Bol'shaia Sovetskaia Entsiklopediia*, 3rd ed. Moskva: Iz-vo "Sovetskaia Entsiklopediia."
- Psthas, George, ed. 1973. *Phenomenological Sociology: Issues and Applications*. A Wiley-Interscience Publication. New York: John Wiley & Sons.
- . 1989. *Phenomenology and Sociology: Theory and Research*. Current Continental Research 215. Washington (D.C.): Center for Advanced Research in Phenomenology Univ. Press of America.
- Raikhel, Eugene A. 2016. *Governing Habits: Treating Alcoholism in the Post-Soviet Clinic*. Ithaca: Cornell University Press.
- Rethmann, Petra. 2001. *Tundra Passages: History and Gender in the Russian Far East*. Post-Communist Cultural Studies Series. University Park, PA: Pennsylvania State University Press.
- Ries, Nancy. 1997. *Russian Talk: Culture and Conversation during Perestroika*. Ithaca, NY: Cornell Univ. Press.
- Riessler, Michael, and Joshua Wilbur. 2017. "Documenting Endangered Oral Histories of the Arctic: A Proposed Symbiosis for Documentary Linguistics and Oral History Research, Illustrated by Saami and Komi Examples." In *Oral History Meets Linguistics*, edited by Erich Kasten, Katja Roller, and Joshua Wilbur, 31–64. Fürstenberg/Havel: Kulturstiftung Sibirien. http://www.siberian-studies.org/publications/PDF/orhili_riessler_wilbur.pdf.
- Riva, Mylene, Pierrick Plusquellec, Robert-Paul Juster, Elhadji A. Laouan-Sidi, Belkacem Abdous, Michel Lucas, Serge Dery, and Eric Dewailly. 2014. "Household Crowding Is Associated with Higher Allostatic Load among the Inuit." *J Epidemiol Community Health* 68 (4): 363–69. <https://doi.org/10.1136/jech-2013-203270>.
- Robbins, Joel. 2013. "Beyond the Suffering Subject: Toward an Anthropology of the Good." *Journal of the Royal Anthropological Institute* 19 (3): 447–462.
- Robinson, Andrew. 2011. "Bakhtin: Dialogism, Polyphony and Heteroglossia." *Ceasefire Magazine*, July 29, 2011. <https://ceasefiremagazine.co.uk/in-theory-bakhtin-1/>.
- Robinson, Michael P., Karim-Aly S. Kassam, and Leif Rantala. 1998. *Sami Potatoes: Living with Reindeer and Perestroika*. Calgary: Bayeux Arts.
- Rochev, R. 1985. "Pozor p'ianitsam." *Lovozerskaia Pravda*, May 28, 1985.
- Rosenthal, Gabriele. 1987. "Wenn alles in Scherben fällt": *Von Leben und Sinnwelt der Kriegsgeneration*. Opladen: Leske + Budrich.
- . 2003. "The Healing Effects of Storytelling: On the Conditions of Curative Storytelling in the Context of Research and Counseling." *Qualitative Inquiry* 9 (6): 915–33. <https://doi.org/10.1177/1077800403254888>.

- Roulston, Kathryn. 2014. "Analysing Interviews." In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 297–312. Thousand Oaks, CA: Sage Publications.
- Rouse, Timothy P., and N. Prabha Unnithan. 1993. "Comparative Ideologies and Alcoholism: The Protestant and Proletarian Ethics." *Social Problems* 40 (2): 213–27.
- Saxinger, Gertrude. 2016. "Lured by Oil and Gas: Labour Mobility, Multi-Locality and Negotiating Normality & Extreme in the Russian Far North." *The Extractive Industries and Society* 3 (1): 50–59. <https://doi.org/10.1016/j.exis.2015.12.002>.
- Scheller, Elisabeth. 2013. "Kola Sami Language Revitalisation – Opportunities and Challenges." In *L'Image Du Sápmi II: Études Comparées / Textes Réunis Par Kajsa Andersson*, edited by Kajsa Andersson, 16:392–421. Humanistica Oerebroensia. Artes et Linguae. Örebro: Örebro University.
- Schrad, Mark Lawrence. 2014. *Vodka Politics: Alcohol, Autocracy, and the Secret History of the Russian State*. Oxford: Oxford University Press.
- Schubert, Jon. 2017. *Working the System: A Political Ethnography of the New Angola*. Ithaca (N.Y.); London: Cornell Univ. Press.
- Schutz, Alfred. 1970. *Collected Papers*. Vol. 1. The Hague: Martinus Nijhoff.
- Schutz, Alfred, and Thomas Luckmann. 1973. *The Structures of the Life-World*. Evanston, Ill: Northwestern University Press.
- Schütz, Alfred, and Thomas Luckmann. 1979. *Strukturen der Lebenswelt*. Vol. 1. Frankfurt a.M: Suhrkamp.
- Scott, James C. 1985. *Weapons of the Weak: Everyday Forms of Peasant Resistance*. New Haven (Conn.); London: Yale University Press
- . 1989. "Everyday Forms of Resistance." In *Everyday Forms of Peasant Resistance*, edited by Forrest D. Colburn, 1–34. Armonk, N.Y: Sharpe.
- . 1998. *Seeing like a State: How Certain Schemes to Improve the Human Condition Have Failed*. New Haven: Yale University Press.
- Scudder, Thayer. 2009. "Resettlement Theory and the Kariba Case: An Anthropology of Resettlement." In *Development and Dispossession: The Crisis of Forced Displacement and Resettlement*, edited by Anthony Oliver-Smith, 25–48. Santa Fe, N.M.: School for Advanced Research Press.
- Sergejeva, Jelena. 2000. "The Eastern Sámi: A Short Account of Their History and Identity." *Acta Borealia* 17 (2): 5–37. <https://doi.org/10.1080/08003830008580509>.
- Shah, Alpa. 2007. "The Dark Side of Indigeneity? Indigenous People, Rights and Development in India." *History Compass* 5 (6): 1806–32. <https://doi.org/10.1111/j.1478-0542.2007.00471.x>.
- Shashkov, V.Ya. 2004. *Spetspereselentsy v istorii Murmanskoi Oblasti*. Murmansk: Maksimum.
- Shavrov, Nikolai Aleksandrovich. 1898. *Kolonizatsiia, ee sovremennoe polozheniie i mery dlia russkogo zaseleniia murmana*. Vol. 4. Sankt-Peterburg: Tipografia I. Gol'dberga.
- Sherstyuk, Tatiana. 2008. "Women and the Tundra: Problems of Family Life among Reindeer Herders in the Kola Peninsula, Northwest Russia." In *Människor i norr: samisk forskning på nya vägar*, edited by Peter Sköld, 269–86. Miscellaneous publications, no. 11. Umeå: Umeå University.
- Shklovsky, Victor. 1965. "Art as Technique." In *Russian Formalist Criticism: Four Essays*, edited by Lee T. Lemon and Marion J. Reis, 3–24. Lincoln, London: University of Nebraska Press.
- Shved, Andrei, and Vitaliia Chiknaeva. 2019. "Trud, distsiplina i baraki: kak byli ustroeny sovetskie profilaktorii, v kotorykh lechili bol'nykh alkogolizmom." *Komanda 29 – sudy, soveti, istorii* (blog). April 11, 2019. <https://team29.org/story/alkoholizminusr/>.
- Sieder, Reinhard. 2012. "Gesellschaft und Person: Geschichte und Biographie. Nachschrift [1999]." In *Oral History*, edited by Julia Obertreis, 187–212. Stuttgart: Steiner.

- Simandan, Dragos. 2018. "Rethinking the Health Consequences of Social Class and Social Mobility." *Social Science & Medicine* 200 (March): 258–61. <https://doi.org/10.1016/j.socscimed.2017.11.037>.
- Skultans, Vieda. 2007. *Empathy and Healing: Essays in Medical and Narrative Anthropology*. New York: Berghahn Books.
- Slavin, Samuil Venediktovič. 1982. *Osvoenie Severa Sovetskogo Sojuza*. 2-oe, dopolnennoe izd ed. Problemy Nauki i Techničeskogo Progressa. Moskva: Izd. "Nauka."
- Slezkine, Yuri. 1994a. *Arctic Mirrors: Russia and the Small Peoples of the North*. Ithaca (N.Y.): Cornell University Press.
- . 1994b. "The USSR as a Communal Apartment, or How a Socialist State Promoted Ethnic Particularism." *Slavic Review* 53 (2): 414–52. <https://doi.org/10.2307/2501300>.
- Smith, David Woodruff. 2018. "Phenomenology." In *The Stanford Encyclopedia of Philosophy*, edited by Edward N. Zalta, Summer 2018. Metaphysics Research Lab, Stanford University. <https://plato.stanford.edu/archives/sum2018/entries/phenomenology/>.
- Sokolova, Z.P. 2017. "Sovetskii magazin: Torgovlia i alkogol' na obskom severe (korennoe naselenie v 1950-1980 gg.)." *Etnograficheskoe Obozrenie*, no. 1: 97–108.
- Ssorin-Chaikov, Nikolai V. 2003. *The Social Life of the State in Subarctic Siberia*. Stanford, CA: Stanford University Press.
- Stammler, Florian. 2005. *Reindeer Nomads Meet the Market: Culture, Property and Globalisation at the End of the Land*. Vol. 6. Halle Studies in the Anthropology of Eurasia. Muenster: Lit Verlag.
- . 2011. "Oil without Conflict? The Anthropology of Industrialisation in Northern Russia." In *Crude Domination: An Anthropology of Oil*, edited by Andrea Behrends, Stephen P. Reyna, and Günther Schlee, 243–69. New York: Berghahn Books.
- Stammler, Florian, Aytalina Ivanova, and Lena Sidorova. 2017. "The Ethnography of Memory in East Siberia: Do Life Histories from the Arctic Coast Matter?" *Arctic Anthropology* 54 (2): 1–23. <https://doi.org/10.3368/aa.54.2.1>.
- Strauss, Claudia. 1992. "Models and Motives." In *Human Motives and Cultural Models*, edited by Claudia Strauss and Roy G. D'Andrade, 1–20. Publications of the Society for Psychological Anthropology. Cambridge: Cambridge University Press.
- Struchkova, Natalia, and Aimar Ventsel. 2015. "Reflections and Thoughts about the Social and Cultural Role of Alcohol." *Folklore: Electronic Journal of Folklore* 61 (August): 177–84. <https://doi.org/10.7592/FEJF2015.61.discussion>.
- Tester, Frank, and Peter Kulchyski. 1994. *Tammarniit (Mistakes): Inuit Relocation in the Eastern Arctic, 1939-63*. Vancouver, B.C.: UBC Press.
- Theodossopoulos, Dimitrios, ed. 2014. *Rethinking Resistance in the 21st Century*. History and Anthropology, Special Issue, 25 (4).
- Thomson, Alistair. 2007. "Four Paradigm Transformations in Oral History." *Oral History Review* 34 (1): 49–70.
- Thornberg, Robert, and Kathy Charmaz. 2014. "Grounded Theory and Theoretical Coding." In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 153–69. Thousand Oaks, CA: Sage Publications.
- Truth and Reconciliation Commission of Canada. 2015. *The Survivors Speak*. http://nctr.ca/assets/reports/Final%20Reports/Survivors_Speak_English_Web.pdf.
- Tuisku, Tuula. 2001. "The Displacement of Nenets Women from Reindeer Herding and the Tundra in the Nenets Autonomous Okrug, Northwestern Russia." *Acta Borealia* 18 (2): 41–60.
- Ulturgasheva, Olga. 2014. "Attaining Khinem: Challenges, Coping Strategies and Resilience among Eveny Adolescents in Northeastern Siberia." *Transcultural Psychiatry* 51 (5): 632–50. <https://doi.org/10.1177/1363461514546246>.

- Vakhtin, Nikolai. 1992. *Native Peoples of the Russian Far North*. London: Minority Rights Group International.
- Valkonen, Jarno, Sanna Valkonen, and Timo Koivurova. 2017. "Groupism and the Politics of Indigeneity: A Case Study on the Sámi Debate in Finland." *Ethnicities* 17 (4): 526–45. <https://doi.org/10.1177/1468796816654175>.
- Van Manen, Max. 2014. *Phenomenology of Practice: Meaning-Giving Methods in Phenomenological Research and Writing*. Developing Qualitative Inquiry, volume 13. Walnut Creek, California: Left Coast Press.
- Vatonena, Liubov'. 1988. "Est' takie problemy." *Lovozerskaia Pravda*, May 11, 1988.
- . 1989a. "Soviet Union: The Problem of the Soviet Saami." *IWGLA (International Work Group for Indigenous Affairs) Newsletter*, no. 59: 89–91.
- . 1989b. "Internat nuzhen? Komu!?" *Lovozerskaia Pravda*, August 15, 1989.
- . 1990. "O sotsial'no-ekonomicheskoi programme." *Lovozerskaia Pravda*, October 13, 1990.
- Verdery, Katherine. 2018. *My Life as a Spy: Investigations in a Secret Police File*. Durham: Duke University Press.
- Vinokurova, Lilia. 2010. "Yakutia's Men Today: Widowing Wives and Longing for Life?" *Anthropology of East Europe Review* 28 (2): 140–63.
- Vitebsky, Piers. 2010. "From Materfamilias to Dinner-Lady: The Administrative Destruction of the Reindeer Herder's Family Life." *Anthropology of East Europe Review* 28 (2): 38–50.
- Vladimirova, Vladislava. 2006. *Just Labor: Labor Ethic in a Post-Soviet Reindeer Herding Community*. Uppsala: Acta Universitatis Upsaliensis.
- . 2009. "Having the Cake and Eating It : Transformation of Reindeer Ownership in NW Russia." In *Post-Soviet Transformations: Politics of Ethnicity and Resource Use in Russia*, edited by Hugh Beach, Dmitri Funk, and Lennard Sillanpää, 217–44. Acta Universitatis Upsaliensis. <http://urn.kb.se/resolve?urn=urn:nbn:se:uu:diva-208535>.
- . 2011. "'We Are Reindeer People, We Come from Reindeer.' Reindeer Herding in Representations of the Sami in Russia." *Acta Borealia* 28 (1): 89–113. <https://doi.org/10.1080/08003831.2011.575661>.
- . 2014a. "Contested Political Representation of the Sami in the Kola Peninsula, Northwest Russia." In *ReMindings: Co-Constituting Indigenous, Academic, Artistic Knowledges*, edited by Johan Gärdebo, May-Britt Öhman, and Hiroshi Maruyama, 215–28. Uppsala Multiethnic Papers 55. Uppsala: The Hugo Valentin Centre, Uppsala University.
- . 2014b. "'It Is Not Our Reindeer but Our Politicians That Are Wild:' Contests over Reindeer and Categories in the Kola Peninsula, Northwestern Russia." *Arctic Anthropology* 51 (1): 24–40.
- . 2017a. "Nature Conservation in Russia: The Case of Indigenous Sámi Rights in the Kola Peninsula." In *Indigenous Rights in Modern Landscapes: Nordic Conservation Regimes in Global Context*, edited by Lars Elenius, Camilla Sandström, and Christina Allard, 94–110. London ; New York: Routledge.
- . 2017b. "Reindeer Herding as Heritage in the Kola Peninsula, Northwestern Russia." In *Archaeologies of "Us" and "Them" Debating History, Heritage and Indigeneity*, edited by Charlotta Hillerdal, Anna Karlström, and Carl-Gösta Ojala, 272–93.
- . 2017c. "Sport and Folklore Festivals of the North as Sites of Indigenous Cultural Revitalization in Russia." In *Tourism and Indigeneity in the Arctic*, edited by Arvid Viken and Dieter K. Müller. Tourism and Cultural Change 51. Bristol, UK Blue Ridge Summit, PA, USA: Channel View Publications.

- . 2017d. “Producers’ Cooperation within or against Cooperative Agricultural Institutions? The Case of Reindeer Husbandry in Post-Soviet Russia.” *Journal of Rural Studies* 53 (July): 247–58. <https://doi.org/10.1016/j.rurstud.2017.02.004>.
- Vladimirova, Vladislava, and J. Otto Habeck. 2018. “Introduction: Feminist Approaches and the Study of Gender in Arctic Social Sciences.” *Polar Geography* 41 (3): 145–63. <https://doi.org/10.1080/1088937X.2018.1496368>.
- Vladimirova, Vladislava, and Yulian Konstantinov. 2002. *Ambiguous Transition: Agrarian Reforms, Management, and Coping Practices in Murmansk Region Reindeer Herding*. Vol. 35. Max Planck Institute For Social Anthropology Working Papers. Halle/Saale.
- Volkov, N. N. 1996. *The Russian Sami: historical-ethnographic essays*. Diedut 1996, nr. 2. Kautokejno: Nordic Sami Institut.
- “Vsesoiuznaia perepis’ naseleniia 1926 goda.” n.d. Accessed April 24, 2018. http://www.demoscope.ru/weekly/ssp/rus_nac_26.php.
- “Vsesoiuznaia perepis’ naseleniia 1939 goda.” n.d. Accessed April 24, 2018. http://www.demoscope.ru/weekly/ssp/rus_nac_39.php.
- “Vsesoiuznaia perepis’ naseleniia 1959 goda.” n.d. Accessed April 24, 2018. http://www.demoscope.ru/weekly/ssp/rus_nac_59.php.
- “Vsesoiuznaia perepis’ naseleniia 1979 goda.” n.d. Accessed April 24, 2018. http://www.demoscope.ru/weekly/ssp/rus_nac_79.php.
- “Vsesoiuznaia perepis’ naseleniia 1989 goda.” n.d. Accessed April 24, 2018. http://www.demoscope.ru/weekly/ssp/rus_nac_89.php.
- Wenzel, George W. 1991. *Animal Rights, Human Rights: Ecology, Economy and Ideology in the Canadian Arctic*. Toronto: University of Toronto Press.
- Wet, Chris de. 2009. “Does Development Displace Ethics? The Challenge of Forced Resettlement.” In *Development and Dispossession: The Crisis of Forced Displacement and Resettlement*, edited by Anthony Oliver-Smith, 77–98. Santa Fe, N.M.: School for Advanced Research Press.
- Wheelerburg, Robert P., and Natalia Gutsol. 2008. “Babinski and Ekostrovski: Saami Pogosty on the Western Kola Peninsula, Russia from 1880 to 1940.” *Arctic Anthropology* 45 (1): 79–96. <https://doi.org/10.3368/aa.45.1.79>.
- . 2010. “Traditional Saami Reindeer Herding Village Resource Territories on the Western Kola Peninsula, Russia.” *Polar Record* 46 (03): 222–232. <https://doi.org/10.1017/S0032247409990106>.
- Willig, Carla. 2014a. “Discourses and Discourse Analysis.” In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 341–53. Thousand Oaks, CA: Sage Publications.
- . 2014b. “Interpretation and Analysis.” In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 136–50. Thousand Oaks, CA: Sage Publications.
- Winter, Rainer. 2014. “Cultural Studies.” In *The SAGE Handbook of Qualitative Data Analysis*, edited by Uwe Flick, 247–61. Thousand Oaks, CA: Sage Publications.
- Wolcott, Harry F. 1994. *Transforming Qualitative Data: Description, Analysis, and Interpretation*. 2nd print. Thousand Oaks (Calif.) [etc.]: Sage Publ.
- Woodman, Jo, and Alicia Kroemer. 2018. “There Are Hundreds of Thousands of Indigenous Children in Residential Schools around the World Today.” *Intercontinental Cry*, September 29, 2018. <https://intercontinentalcry.org/there-are-hundreds-of-thousands-of-indigenous-children-in-residential-schools-around-the-world-today/>.
- Young, T. Kue, and Peter Bjerregaard, eds. 2008. *Health Transitions in Arctic Populations*. Toronto: University of Toronto Press.
- Yurchak, Alexei. 2006. *Everything Was Forever, Until It Was No More: The Last Soviet Generation*. Princeton, NJ: Princeton University Press.

Interviews

The toponyms refer to the place where the interview was conducted.

- AA. 2014. Female, born in the early 1930s, Lovozero.
AD. 2013a. Female, born in the early 1960s, Murmansk.
———. 2013b. Female, born in the early 1960s, Murmansk.
AE. 2013. Female, born in the mid-1930s, Lovozero.
AI. 2013. Female, born in the early 1930s, Lovozero.
AM. 2015. Male, born in the early 1960s, Lovozero.
AP. 2014. Male, born in the early 1950s, Lovozero.
AS. 2014. Female, born in the mid-1940s, Revda.
EK. 2014. Female, born in the early 1940s, Lovozero.
EP. 2015. Female, born in the early 1950s, Lovozero.
GP. 2015. Female, born in the early 1960s, place anonymised.
LA. 2013. Female, born in the mid-1950s, Lovozero.
LA, EP. 2015. Female, born in the mid-1950s and female, born in the early 1950s, Lovozero.
LI. 2014. Female, born in the mid-1940s, Lovozero.
LP. 2013. Female, born in the early 1960s, Lovozero.
MA. 2013. Female, born in the early 1930s, Lovozero.
NA. 2013. Female, born in the early 1960s, Lovozero.
NE. 2008. Female, born in the late 1930s, Murmansk.
———. 2013a. Female, born in the late 1930s, Murmansk.
———. 2013b. Female, born in the late 1930s, Murmansk.
NN. 2013. Female, born in the mid-1930s, Uмба.
SF. 2013. Male, born in the late 1960s, Lovozero.
SG. 2013. Female, born in the mid-1960s, Lovozero.
SI. 2014. Male, born in the late 1940s, Lovozero.
VA. 2013. Female, born in the late 1950s, Lovozero.
VIa. 2013. Female, born in the late 1930s, Murmansk.
ZP. 2013. Female, born in the early 1930s, Verkhnetulomsk.

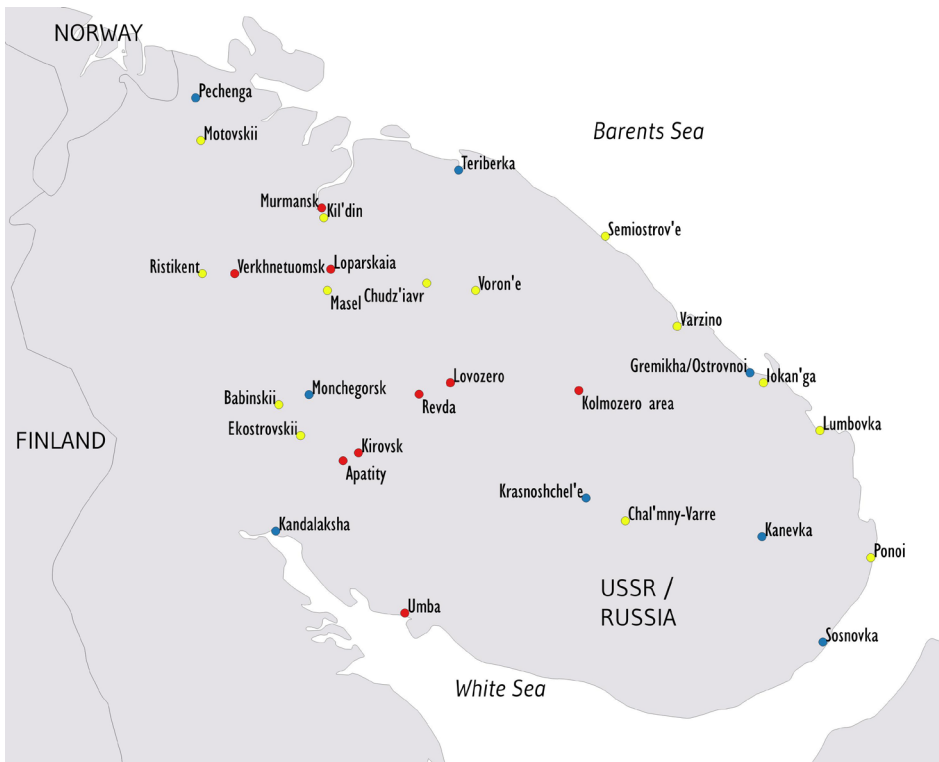
Archival documents

All cited documents are archived at the Murmansk Regional State Archive, Kirovsk branch. The location in the archive is indicated as shown in this example: f.242 op.1 d.1 l.7-8 means collection (*fond*) 242, list (*opis*) 1, file (*delo*) 1, sheets (*listy*) 7 and 8.

- f.146 op.5 d.3 l.116. 1965. "Protokol no. 1 zasedaniia Iсполnitel'nogo Komiteta Lovozerskogo Raionnogo Soveta Deputatov Trudiashchikhsia." State Archive of the Murmansk Region, Kirovsk.
f.146 op.5 d.16 l.74. 1968. "Protokol no. 25 zasedaniia Iсполnitel'nogo Komiteta Lovozerskogo Raionnogo Soveta Deputatov Trudiashchikhsia." State Archive of the Murmansk Region, Kirovsk.
f.146 op.5 d.16 l.96. 1968. "Vypiska iz protokola no. 2 sobraniia upolnomochennykh chlenov

- kolkhoza „Tundra” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.141 l.179, 181. 1972. “Protokol no. 30 zasedaniia Ispolnitel'nogo Komiteta Lovozerskogo Raionnogo Soveta Deputatov Trudiashchikhsia.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.156 l.80-83. 1972. “O khode bor'by s pi'anstvom.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.156 l.125-153. 1972. “Spravka po podgotovke voprosa na sessii oblastnogo Soveta „O zadachakh mestnykh Sovetov Deputatov Trudiashchikhsia po dal'neishemu uluchsheniuiu vospitaniia molodezhi v svete trebovaniu XXIV s'ezda KPSS” po Lovozerskomu Raionu.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.188 l.48-50. 1973. “O sostoianii zdavookhraneniia sredi korennoho naseleniia.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.192 l.6-7. 1973. “Reshenie zasedaniia Ispolnitel'nogo Komiteta Lovozerskogo Raionnogo Soveta Deputatov Trudiashchikhsia „Ob aktivizatsii raboty Sovetskikh i administrativnykh organov raiona po bor'be s litsami, ukoniavshchimisia ot obshchestvenno-poleznogo truda i vedushchimi paraziticheskii, antiobshchestvennyi obraz zhizni.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.192 l.98. 1974. “Spravka o sostoianii i merakh po ukrepleniuiu sotsialisticheskoi zakonnosti, usileniia bor'by s prestupnost'iu i khishchenii na predpriiatiakh i v organizatsiakh Lovozerskogo Raiona.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.193 l.1-3. 1973. “Protokol no. 1 zasedaniia komissii po delam nesovershennoletnikh pri lovozerskom Raiispolkome.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.194 l.126-132. 1974. “Spisok uchashchikhsia v Lovozerskoi shkole-internat na 1974-75 uchebnyi god, s ukazaniem natsional'nosti.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.210 l.202. 1974. “Reshenie iz protokola no. 6 zasedaniia ispolnitel'nogo komiteta lovozerskogo raionnogo Soveta Deputatov Trudiashchikhsia.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.215 l.6-7, 24-28, 53-54. 1978. “Otchet o rabote komissii po delam nesovershennoletnikh pri lovozerskom Raiispolkome 1974-1978.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.224 l.1-2. 1975. “Protokol no. 1 zasedaniia komissii po delam nesovershennoletnikh pri lovozerskom Raiispolkome.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.224 l.4-8. 1975. “Protokol no. 2 zasedaniia komissii po delam nesovershennoletnikh pri lovozerskom Raiispolkome.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.226 l.63-66. 1976. “O rabote raionnogo otdeleniia vnutrennykh del Raiispolkoma.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.226 l.96, 99. 1976. “Protokoly, plany i otchety postoiannoii komissii po zdavookhraneniuiu i sotsial'nomu obespecheniiu.” State Archive of the Murmansk Region, Kirovsk.
- f.146 op.5 d.468 l.103-104. 1982. “O rabote tov. suda v s/kh ‘Tundra.’” State Archive of the Murmansk Region, Kirovsk.
- f.242 op.1 d.1 l.7-8. 1935. “Protokol no. 3 obshchego sobraniia zhitelei pog. Kil'din.” State Archive of the Murmansk Region, Kirovsk.
- f.302 op.1 d.134 l.4. 1966. “Protokol no. 8 zasedaniia varzinskogo Sel'skogo Soveta Deputatov Trudiashchikhsia.” State Archive of the Murmansk Region, Kirovsk.
- f.302 op.1 d.145. 1967. “Protokoly zasedanii varzinskogo Sel'skogo Soveta Deputatov Trudiashchikhsia.” State Archive of the Murmansk Region, Kirovsk.
- f.352 op.1 d.60 l.4. 1978. “Plan uchebno-vospitatel'noi raboty za 77-78 uch. god.” State Archive of the Murmansk Region, Kirovsk.

Appendix 1: Map of fieldwork places, selected contemporary and former settlements



Map 2: Selected settlements in the Murmansk Region, including fieldwork sites (red), other contemporary settlements (blue), and closed-down settlements (yellow).

Appendix 2: List of interview topics

The following common list of the ORHELIA project topics served as a rough guideline during my oral history fieldwork. This list is neither exhaustive nor was it compulsory to broach all topics listed here in every conversation. Having such a list was crucial for the comparative dimension of the project; at the same time it left enough space for an individual approach to each person and field site.

Alcohol

Ancestors & kinship, descendants

Belief system & practices (spirituality, shamanism, conversion, baptism, Laestadianism, communism, atheism, antireligious issues, orthodox religion, repression of indigenous religion, syadei (seita), offering practices and sacrifices, repression of orthodox religion, underground religious practices, the orthodox church, the protestant church, new protestant movements, missionary work, traveling for pilgrimage or to sacred places, previous beliefs, punishment by the supernatural, sacred sites)

Biography, personal story

Borders, restricted/closed areas, military zones, connections with abroad

Change of political structure, Revolution in Yamal, new Russia, Soviet times

Cinema

Clothing (headwear, karvalakki, female hats according to marital status, new clothing materials, Sámegákti (Sámi dress))

Collectivisation, expropriation, merging of economic units

Cooperatives, collective farms, state farms, herding cooperatives, kolkhoz, sovkhos, state directives in local economy, subsidies

Customs, such as marriage, divorce, caring for widows, inheritance, courting, cruelty of relatives, inheritance, living place after marriage, marriage by love or arrangement, searching/travelling for brides, intermarriage

Education, boarding school, nomadic school, adult education, methods to adapt children to school life personal school recollections, teachers' education methods, alphabetisation

Employment, choice/change of profession, new professions

Engineered culture, day of the reindeer herder, state celebrations, communist folklore, revitalisation, disco, festival/holiday, indoctrination, agitation, ideological inducement, libraries and books in the tundra and small villages, permanent cultural facilities/ houses of culture, Saamy/Sami (official and endonym)

Fishing, fresh water fishing, fishing techniques, obligatory fishing, equipment, poaching

Food, food in the boarding school

Fuel, firewood, kerosene, blubber, petrol, diesel

Health, disease, doctors, medical services in the tundra, permanent facilities for medical services

Herding system and techniques, herding/husbandry control, reindeer pastures, degradation, lichen, land use, grazing pattern, forest, reindeer herding, slaughter, theft of property, migration routes

Historical stories (everything beyond personal), location/place: settlement, images of place, history and stories, legends related to place, loss of settlements, legends

Hunting, fur hunting, trapping, poaching

Industrialisation of fishing, of meat production, of tundra, industrial workers, meeting the first geologists, industrial workers oil and gas, commercial development of deposits, pollution

Internal leadership, luottamusmies (village ombudsman), sobbar (village meeting)

Language change, laws, speaking one's native language at school

Law, state laws, community laws

Leisure, tourism, traditional feasts

Market economy, liberalisation, market forces in local economy, obshchiny (kin-based communities), free market economy, patrons, mafia, privatisation, selling of reindeer herders products, trade and post routes shops and delivery of consumer staples, supply (imported goods) food supply, trading posts, velvet antlers, tourism, withdrawal of educational/cultural/medical services

Military, army service

Music and dance (leudd, katrilli) singing: leudd, joik, yarabts, khynabts

Natural hazards, icing over, coastal erosion, flooding)

New economic activities induced by the state: e.g. livestock breeding, forestry, tourism, chicken and other poultry farming, fur farms, hay making, potatoes

Persecution, prison, prison camps, state repression of enemies of the state/the people

Political agency, empowerment, disempowerment, collectivisation, *glasnost* and *perestroika*

Poverty, starvation

Response to state measures grassroots/bottom-up/local activities or low-key resistance against army service, collectivisation, expropriation, resettlement and sedentarisation, schooling, support for the enemies of the state, support for resisting people or refugees

Sedentarisation, separation of habitation and reindeer herding, housing, sedentary life in settlements

Settlement dynamics, relocation, deportation as punishment by the state, escape, evacuation, individual, family or settlement resettlement by the state or other circumstances, reestablishment of settlements, *ukrupnenie* (enlargement), village construction

State institutions, representatives/agencies/services: municipality, police, hospital, etc.

Technological innovations: snowmobiles, cars, cassette recorders etc., chainsaws, DVD and video, generators, stoves, mobile phones, computers, GPS, players, rubber boots radio, telephone, television, fencing, reuse of materials and objects of extractive industries, electricity, running water, internet, veterinarian services (in the tundra)

Traditional handicraft, beadwork, bark, fish skin, others

Transport, reindeer, snowmobile, tank, airplane, helicopter, ATV, boat, dog sledging, infrastructure, roads, railroad, ports, public transport, maintained winter roads, motorboats and ships, tanks/*vezdekhod*, road construction, special tundra vehicles

Uprisings, violent resistance

Violent death, suicide, murder, poisoning by found things

War, atrocities, economy during the war, fathers, brothers, grandfathers leaving to war, fighting in the war, gender roles during war, official commemoration of the war, personal war experiences, reindeer in warfare, reindeer troops at war, war patriotism, working for the front