Reflections on Linguistic Fieldwork Within Moribund Speech Communities

Réflexions sur le travail de terrain linguistique avec des communautés linguistiquement moribondes

Keelelised välitööd väljasurevates keelekogukondades

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The more normal situation is for every exchange of words to take place in a context which allows the verbal activity to be associated with other kinds of activity and other kinds of message.

(Leach, 1982, p 150)

“Being there” and “being here”

- 1 Private and reflexive accounts on “being in the field” and “coming back from the field” appear to be largely neglected in accounts of linguistic fieldwork. Although “being there” is the prerequisite for subsequent research, “being there” usually tends not to be directly verbalised and, if this topic appears at all, then usually as condensed front matter in grammatical sketches or grammars.¹ This standard procedure must strike cultural/social anthropologists (hereafter subsumed under the general label anthropologist/anthropology) as odd, for at least three reasons. First, if any discipline relies as heavily on fieldwork as documentary fieldworks, it is most certainly anthropology. This means that field linguistics and especially documentary linguistics shares the concept of “being there” with anthropology in joy and sorrow. However, whereas epistemological questions have been theorized in anthropology at least since Bronislaw Malinowski, documentary linguistics seems to have avoided such discussions quite successfully. Second, after the reflexive Geertzian-Cliffordian turn in anthropology, “being there” and the complex interaction between fieldwork, self-

reflection, interpretation and the claim that “anthropologists are authors” (Geertz, 1988), the idea that data is simply out there waiting to be documented should have been finally proven wrong. Or course, also linguists are authors, yet different.² The most obvious instances of authorship are belletristic accounts on “being there”; as my expertise lies mainly in Northern Eurasia, it is not difficult to identify such texts because this genre used to be very prominent among early Finnish scholars e.g., Donner (1915), Ramstedt (1944), Lagercrantz (1950) and Lehtisalo (1959) are among the central texts.³ However, these belletristic accounts of “being there” neither allow any sophisticated look at the practicalities of fieldwork nor do they show how data was indeed “gathered”. Although these texts report about “being there”, they do not allow a discussion similar to the objectives of e.g., Geertz (1988). Therefore, this thread will not be followed. Instead, a Geertz influenced reflexive yet partly impressionistic and incomplete account on “being there” is offered which draws from personal experiences and challenges. Although I am obviously too young to offer any ground-breaking wisdom accumulated over several decades of active work, I am nevertheless convinced that 10 years of work dominated by moribund languages including 16 months of fieldwork in situ may offer something to report – if not for the researcher, then perhaps for students who are unsure whether to conduct fieldwork themselves. Due to developments about which I am not fully aware myself, most of my fieldwork has indeed been conducted on moribund languages, often with the last speakers of a given language and within the remains of these speech communities. This perspective is somehow more extreme and most certainly not congruent with the general premises of linguistic fieldwork.⁴ As already mentioned, I will reflect foremost on my fieldwork experience(s) and already here I want to apologize for the exorbitant high occurrence of first person pronouns which is unusual in linguistic writing. Although the central argument to be made below is by no means unique or unexpected, it is not often explicitly verbalised: field sites are unique and working with speakers of moribund languages in different countries may mean very different things and imply highly diverging challenges – what works in field site X does not necessarily work in field site Y and vice versa.⁵ These contextual challenges, although being meaningful and to some degree decisive do not require alternative approaches; although the context constrains which kind of data, how much data and how quickly data can be gathered (and indeed, variation is quite drastic), the basic analytical concepts such as phonemes, morphemes, constituent structure, parts of speech and the like remain the same, at least initially.⁶

- 2 The main bulk of experiences on which I will reflect comes from my work on two moribund Uralic languages, Forest Enets (Samoyedic) and Ume Saami (Saami) which have played a prominent role in my career as a (field) linguist so far. These experiences will be contrasted, at least episodically, with experiences from fieldwork and/or consultant work on other languages. Although my fieldwork on the Taimyr Peninsula initially focused on the moribund language Forest Enets, during a later phase I started to work with Dolgan (Turkic) and the Taimyr variety of Tundra Nenets (Samoyedic); even though the latter are comparatively safe for one generation or two, they need to be classified as severely endangered nevertheless.⁷ Another moribund language on which I could collect sizable data is Tundra Yukaghir (isolate), though in contrast to the aforementioned languages, this data was collected during several meetings with a native speaker in an asylum centre in Eastern Finland and not in the field. Although I won't be able to go into detail concerning this episode, due to the accompanying circumstances this unusual instance of consultant work has altered my perspective on

fieldwork quite drastically. For the sake of completeness, intensive consultant work with a native speaker of Tuvín (Turkic) in Helsinki needs to be mentioned on which I will elaborate a little more.

“Being there” and finding consultants

- 3 Some preliminary remarks concerning central concerns in linguistic fieldwork need to be sketched already now although these will be approached in more detail later. Linguistic fieldwork requires face to face interaction and mostly elicitation in a language shared by both researcher and potential consultant⁸ – participant observation does not reveal linguistic structure, at least not in the beginning. Usually, non-native linguists cannot enter the field with practical language skills and need to acquire them during work. Occasionally two languages need to be learned – one from scratch and knowledge of the other needs to be improved. Although finding a consultant is a general problem for the professional stranger, finding a linguistic consultant in a moribund speech community is even more challenging because there simply are not too many potential consultants left. Concerning Forest Enets and Ume Saami, work had to rely entirely on elder speakers, often from the grand-parental generation who have not been able to speak and/or use their languages on a regular basis. In principle, the same would be valid for Tundra Yukaghir, but in my case, the speaker looked for a linguist on her own and simply walked into my office in Helsinki in March 2013. However, also she had not used the language on a regular basis for many years. In contrast, during my long-term fieldwork on the Taimyr Peninsula among the Forest Enetses, I became acquainted with many Nenets and Dolgan activists, culture workers, teachers, radio and TV reporters. Of course, due to the fact that both languages are in a better state and are spoken by more individuals, finding a consultant was much easier. Furthermore, I was no longer the unknown foreigner in town but had earned a reputation as somebody who was genuinely interested in the indigenous people.⁹ However, even here I ended up working mainly with elder people, again often in the grand-parental generation and quite often, with elder women.¹⁰ In Swedish Lapland among the Ume Saami, four out of five consultants were elder men (all +70) who had preserved language skills due to their engagement in reindeer herding. The female speaker, roughly 80 years of age, was also engaged in reindeer herding which explained her language skills.
- 4 In contrast, fieldwork at home does not allow any “choosing” of consultants, because speakers reside outside their speech community from which they are isolated. Instead of choosing, it is justified to say that it was actually me who was chosen. As already mentioned, the Tundra Yukaghir speaker suddenly appeared in my office and the Tuvín speaker approached me during a conference.¹¹ However, here the similarities take an abrupt stop. The Tuvín consultant, a woman of roughly my age is the only speaker among my consultants who uses the language on a daily basis; further, she is literate in her own language, occasionally still writing in Tuvín for local journals at home and later enrolled for a PhD degree in anthropology investigating the interaction between Tuvíns and horses (e.g., Peemot 2017). Further, in contrast to all other languages, working on Tuvín does not presuppose direct face to face contact. Such work is even possible via Internet, often on very short notice e.g., just with a simple skype call and/or by e-mail.

“Being there” – fieldwork at home and fieldwork away

- 5 In some recent approaches to linguistic fieldwork (e.g., Dixon 2010, pp 314-315), fieldwork “at home” aka consultant work is not considered even with the type of fieldwork “being there”. From an anthropological, but also from a linguistic perspective, this distinction is justified and should be maintained (see also the survey in Chelliah & de Reuse 2011). Nevertheless, I side with the perspective that linguistic fieldwork at home is possible, even though this is work on idiolects spoken outside the speech community and therefore less representative.¹² Another frequently suggested requirement of linguistic fieldwork is that after the initial trips, fieldwork should be conducted monolingually as much as possible.¹³ Again, in the two major field sites of mine, this was simply not applicable although elsewhere this might be possible, even among speakers of moribund languages. The Forest Enets and Ume Saami speech communities have ceased to exist and the remaining speakers live their everyday life in a different language in a rural/urban environment; there simply was never a chance to conduct fieldwork in other languages than Russian or Swedish. To be sure, monolingual fieldwork in the Tuva Republic or within the Tundra Nenets or Dolgan dominated areas of the Taimyr Peninsula appear to be theoretically possible, but not within moribund speech communities such as Forest Enets, Ume Saami and quite likely neither among the Tundra Yukaghir. Work with my Tuvin consultant comes very close to the idealised scenario of consultant/fieldwork because I have the chance to work with a completely fluent and linguistically creative consultant of an endangered, yet vital language. In comparison to the majority of fieldwork and/or consultant work settings of mine elsewhere, this situation is therefore markedly different.

On “unlearning” and “learning”

- 6 Whereas I have consciously chosen Forest Enets as the first language in the field, all other languages on which I started to work on later crossed my paths by chance (Dolgan, Taimyr Nenets, Ume Saami, Tundra Yukaghir, Tuvin, Meithei). Due to personal preferences, learning the language “under investigation” is a central aim; although learning these languages is a humble sign of appreciation and should not require any further explanation, practical language skills pay off during transcription of language materials. After all, something one understands can be transcribed quicker than something one does not understand. However, before “learning” a language, I was quite often forced to “unlearn”¹⁴ another language first and in this section I would like to elaborate on this process in some detail.¹⁵ This process of “unlearning” and “learning” interacts with another concept which I subsume as “minority within a minority”. Their mutual interdependence will, hopefully, become clear in a moment.
- 7 In 2005, while spending several months in Helsinki as a visiting PhD student in order to prepare myself for the initial field trip (02/2006) and the subsequent long-field trip to the Taimyr Peninsula (10/2006-05/2007), I had the unique chance to acquire practical and additional theoretical skills in Tundra Nenets. The motivation for learning Tundra Nenets was two-fold. First, Soviet ethnography had presented a picture that Forest Enetses were under considerable Tundra Nenets cultural pressure and therefore mostly bilingual. Second, Tundra Nenets as the largest Samoyedic language was much better known in Samoyedic linguistics; Forest Enets, the language of the minority within the

minority most definitely not. As described elsewhere (Siegl, 2010a: pp 221), the acquired skills in Tundra Nenets did not pay off; instead, I had to unlearn one language, Tundra Nenets, in order to learn a new one, Forest Enets, in a relatively short period. Several years later in 2011 when I assisted Lotta Jalava to gather data for her dissertation project among the urban Tundra Nenetses in Dudinka (Jalava, 2015), I had a chance to work with Tundra Nenets again. Although the theoretical skills were still there, the practical skills were no longer. In the end, I looked at the structure of Tundra Nenets from an Enets perspective.

- 8 Work with the Ume Saamis resulted in a similar *déjà-vu* experience. During my two first encounters with Ume Saami speakers and rememberers in 2011, I could rely on some limited skills in North Saami and other shorter experiences with other Saami languages (South, Pite and Inari Saami); nevertheless, work was conducted entirely in Swedish. The rudimentary skills in North Saami did most certainly speed up the process of getting into the basics of Ume Saami grammatical structure. During my post-doc period at the University of Helsinki (2012-2015), I had the chance to learn some practical North Saami. In 2015 when returning to the Ume Saamis, another minority within a minority, initial fieldwork revealed rather painfully, that I started to run danger to approach the structure of Ume Saami too much from a North Saami perspective. Again, another instance of “unlearning” had to begin. By the end of the project, active North Saami skills had practically faded.
- 9 As for the work with the two Turkic languages, some superficial parallels exist, yet neither the “unlearning” metaphor nor the “minority within a minority” concept really apply. Sometimes in spring 2008, while spending a weekend with some members from the Dolgan intelligentsia in Dudinka, I mentioned that I did understand some bits and pieces of their language due to the fact that I had attended courses in Turkish as a MA student. During this occasion, a member of the Dolgan community frankly asked me whether I would be interested in learning some Dolgan: “You already know one Turkic language, learning another one is easy” to which she added “that Turkic languages are very easy, Nenets is so complicated”.¹⁶ Honestly speaking, any practical skills in Turkish were anyway long gone in 2008, but work on basic vocabulary and sentence structure went much quicker than in comparison to Enets. Furthermore, I was already much more experienced in fieldwork too.
- 10 For Tuvín, the “unlearning” process does not apply either. Whereas for the Taimyrian languages suitable learning materials for adults are missing, Tuvín would have such materials, but due to the way how quickly work started, there was no chance to get to know the language in advance. Instead, work on Tuvín started from a Dolgan based perspective. Instead of “unlearning” Dolgan to learn Tuvín, work could be paraphrased as close to five months of “contrastive language learning” – quite often, I deliberately re-used questionnaires which I had once compiled for Dolgan in order to get similar data from Tuvín.
- 11 This leaves Tundra Yukaghir as the only language for which “unlearning” simply could not apply.¹⁷ Everything started the usual way from lexical work in order to acquire some understanding of phonetics/phonology which continued with simple predication and ended in complex predication. As the elder lady had not spoken the language for a longer period and turned out to be bilingual in Chukchi and Tundra Yukaghir with clear preference for Chukchi, she tried to reclaim the language of her childhood and teenage years by working on the language. This was indeed already unusual, but due to

the unusual setting of our work which predominantly took place on an irregular basis in an asylum centre in the middle of nowhere in Eastern Finland, working on Tundra Yukaghir was an unusual experience in more than just one concern.¹⁸

How to become a field linguist?

- ¹² Although linguistic fieldwork and especially documentary linguistics has certainly become more visible due to the rising awareness of language endangerment in the last 20 years or so,¹⁹ the role of fieldwork is clearly marginal in the discipline of linguistics as such; it is still perfectly possible to become a linguist without ever engaging in fieldwork, more so if one is a native speaker of a large, often Indo-European language with freely available corpora and/or good intuitions about grammaticality of clauses of the famous “Colourless green ideas sleep furiously” type. In this respect, the question “how” and perhaps even “why to become a field linguist” is a justified question. In contrast, in anthropology where fieldwork remains the “standard initiation rite”, the opposite prototypically applies — fieldwork is part of the game. Nevertheless, even though fieldwork is the rule and not the exception, two influential anthropologists Claude Lévi-Strauss and Ruth Benedict defied this rule — both conducted only little fieldwork, yet were deemed worth of an own chapter in Geertz (1988, pp 37; pp 108). Ironically, even though Claude Lévi-Strauss conducted very little fieldwork,²⁰ his impressionistic fieldwork underlying *Tristes Tropiques* had quite some impact, both within the discipline and on European structuralism in general. Clearly, the role of fieldwork may indeed be overrated once in a while.
- ¹³ Returning to the question of this section “how and why to become a field linguistics” allows for a very simple and profane answer on my behalf: by chance. Due to the fact that I was acquainted with researchers who already had conducted fieldwork in Western Siberia, I became curious. I ended up writing a proposal with an Estonian colleague who had already conducted linguistic fieldwork in Western Siberia and indeed we received substantial funding (see Siegl, 2010a).

Why moribund languages are not the same — experiences from Siberia and Swedish Lapland

- ¹⁴ In the following, I offer a dense, yet condensed overview of my fieldwork in Siberia and Swedish Lapland. Although fieldwork has a dedicated Boasian “salvage ethnography” moment, both circumstances and practicalities of fieldwork will show that the only uniting feature of both field sites is the fact that both languages are moribund. I will continue to use the ethnographic I, albeit now from a dedicated linguistic perspective.

Potential Forest Enets - Ume Saami parallels — introductory remarks

- ¹⁵ Initially it appears that a number of superficial parallels are attested. Geographically, both languages belong to the transitional taiga to tundra area in the Eurasian North; for

both people, reindeer herding relying on relatively small herds (a constraint due to the natural surroundings within the taiga belt of Northern Eurasia) has played and continues to play an essential role even though the majority of speakers/rememberers of either language are no longer actively engaged in reindeer husbandry. As a symbol of ethnic self-identification, reindeer husbandry continues to be very powerful. Quite unsurprisingly, active engagement in reindeer husbandry is reflected in language skills; speakers/rememberers with good language skills have had intimate contact with reindeer herding or come from families with strong traditions in reindeer husbandry. Whereas all of my male Forest Enets consultants had already retired from reindeer herding when I came to Potapovo, both major Ume Saami consultants, albeit of age are still actively participating in reindeer herding. Another uniting feature is their status as a minority within a minority. In almost every concern, their close and more numerous linguistic relatives Tundra Nenets and North Saami are better known;²¹ Forest Enetses and Ume Saamis have stood in the shadow of their more numerous linguistic relatives for a comparatively long period.²² Another uniting fact which applies to both languages is their status as oral languages. Although Forest Enets has made the transition to a written language in the late 1990s, this development had no impact on language sociology (Siegl & Rießler, 2015). Ume Saami has an approved orthography since April 2016, but without any promoters of literacy and almost no published materials in this orthography (to which a general lack of written resources needs to be added), the recently created orthography remains a symbolic act for the time being.

- 16 However, when entering the juridical level, similarities and dissimilarities tend to mix. First, although both Enetses and Saamis are officially recognized indigenous peoples within the states they reside, their official linguistic status is unsettled. The linguistic differences between Forest Enets and Tundra Enets suggest the existence of two different languages, though officially the Enetses are considered as one people speaking one language. The Ume Saami case is not too different. Whereas the Saamis are recognized as one of several indigenous people of Sweden, the fact that Saamis in Sweden speak several different languages, quite often even unintelligible to each other, is frequently overlooked. Although, at least linguistically, Ume Saami should have been classified as an independent Saami language already earlier, which for a number of reasons has not happened until recently (see Siegl, 2017a), the decisive step for the recognition of Ume Saami as an independent language has been attributed to the approval of the Ume Saami orthography in April 2016.
- 17 Last but not least, another argument needs to be brought forward. Both, the Forest Enets and the Ume Saami community have ceased to exist as a speech community for quite some years already. This means that linguistic identity based on active and/or passive language skills constitutes only a marginal means of self-identification; therefore, this resource is available only for a few Forest Enetses and Ume Saamis but not the majority of Forest Enetses/Ume Saamis. Although language can continue to have a symbolic value (e.g., by learning words and phrases to strengthen ethnic identity), anthropological and linguistic perspectives do part at this point. Whereas the knowledge of some words and phrases are surely of importance for ethnic identification, documentary linguistics focuses on language as a system and requires speakers who at least understand the grammatical system.

Dissimilar similarities

- 18 To be sure, there are more superficial similarities which after closer inspection turn out to separate both languages from each other. In the following, I will deconstruct several arguments and pave the way towards the main point of this contribution – although both languages are moribund, they refute any meaningful equation beyond the fact that both are “moribund”.

Indeed moribund?

- 19 From the outsider’s linguistic perspective, both languages need to be classified as moribund. Whereas this fact was clearly understood by all Forest Enetses with whom I have worked and at least by the speakers/remembers of Ume Saami from Arvidsjaur, the majority of ethnic Forest Enetses and presumably Ume Saamis in more southern parts have a different perspective and a different opinion. Discourse on “revitalisation” is vivid and indeed some attempts have been initiated, but to date there are no obvious linguistic results.
- 20 Since 2011 some language nests have been opened on the Taimyr Peninsula (Siegl, 2013b, pp 24) including one in Potapovo for Forest Enets, but first-hand information is absent and its impact cannot be evaluated. As for Ume Saami, there are occasional language courses targeted at teenagers and adults, a process which may have started already in the late 1980s.²³ In May 2016, I was invited to a “teacher and apprentice” meeting in Arvidsjaur to give a presentation about the scarce linguistic materials available on Ume Saami.²⁴ Both, feedback by the conveyor and the majority of participants makes me believe, that this course and prior attempts of revitalisation improved symbolic skills (mastering of phrases), yet not conversational competence. Ironically yet unintentionally, my presentation was followed by a statement of the organisers who informed the participants that this was the last meeting of this course which had lasted for about a year (on a monthly basis). Their decision was framed by relying on a number of arguments such as the absence of language materials (including teaching materials), speakers who could serve as “teachers” and unsecured further funding. Although Ume Saami activists appear frequently in the Saami media reporting about revitalisation progress or complaining that current effort is insufficient, the debate appears to be programmatic at best. Whether revitalisation efforts among the Forest Enetses and the Ume Saamis are indeed capable of creating new speakers, new contexts for language use and to reverse language shift remains to be seen; in my eyes, language shift has progressed to a point where reversal has become unlikely.

On language shift

- 21 Although both speech communities have undergone almost complete language shift, the path underlying this development qualifies as another dissimilar similarity. The situation for the Forest Enetses has been covered elsewhere in more detail (2013) and the following overview is highly condensed and simplified. As for the Forest Enetses, the decisive period seems to be the years after the Second World War; apart from prolonged education in Potapovo’s boarding school, new marriage patterns, enforced settlement in Potapovo and the multilingual background of the village triggered rather

abrupt language shift to Russian. Even Enetses which were born after the 1950s did no longer acquire the language in their families anymore. With the death of the speakers from the parental generation (late 1980s until the early years of the new millennium) with whom communication had to take place in Forest Enets, the language fell out of everyday usage quickly. In comparison to Potapovo, the picture which evolved for the area around Arvidsjaur, suggests a longer process.²⁵ As for education, boarding schools of the *nomadskola* type were unknown in Arvidsjaur and Ume Saami children attended regular village schools as Swedish children.²⁶ Second, due to the fact that the area of Arvidsjaur was already partly colonized since the early 18th century, ethnic intermarriages and bilingualism had arrived to this area already in the 19th century. Further, due to several epidemic diseases among reindeer and several harsh winters during which Ume Saamis lost many animals and occasionally entire herds, many reindeer owners gave up reindeer husbandry in the early 20th century. In particular, the so called “deep snow winter” in 1937 seemed to have had a very devastating effect. With the abandonment of reindeer husbandry, the remaining semi-nomadic Ume Saamis made the transition to a Swedish-dominated life which seems to have accelerated language shift.²⁷ This means that bilingualism and language shift was already rather advanced and Swedish language, even in Ume Saami marriages started to become usual already in the 1920s and 1930s. Whereas it appears that language transfer in and around Potapovo was interrupted by a number of significant developments in a very short period in the 1950s, transition in Arvidsjaur was a long-lasting process, sometimes initiated in the 19th century. After the 1920s, language transfer became rare and ceased in the 1940s. According to my central consultants in Arvidsjaur, Ume Saami continued to be used mainly by men as the major working language in reindeer herding in which the language resisted Swedish until the 1980s. Elsewhere, the language fell into oblivion in the 1950s and 1960s. The last speakers who according to my consultants would have had reasonably good and active skills were reported to have died around the turn of the millennium.

(Under)documentation

- 22 The degree of (under)documentation of both languages has been addressed in detail in Siegl (2013, 2017a, b) and only superficial remarks will be made here. For Forest Enets, the majority of materials were collected in the second half of the 20th century and even today, new materials can be collected. In comparison, a substantial amount of Ume Saami materials was collected in the first half of the 20th century. The first recordings were made in 1943 and recordings continued to play a very decisive role in documentation during the second half of the 20th century. However, here the parallels end. Whereas some primary Forest Enets materials and linguistic studies were already published in the 20th century, except for Wolfgang Schlacher’s publications (both primary materials and analysis based thereon) and the short yet tremendously influential account on Ume Saami by Knut Bergsland and Gustav Hasselbrink, Ume Saami was ignored.

Similarities and dissimilar similarities summarized

- 23 Having elaborated on the premises and starting points from which fieldwork has taken off, I hope to have been able to visualise that seeming parallels on the surface are not

informative without a deeper understanding of their background. Although both languages qualify as moribund, this result derives from different socio-historical developments. Lest one deliberately intends to compare apples with pears, most of the potential comparisons remain rather unsuccessful and potential results irrelevant.

Fieldwork on the Taimyr Peninsula²⁸

- ²⁴ Due to a number of political and bureaucratic reasons (mentioned in Siegl 2010a), fieldwork on the Taimyr Peninsula was planned as long-term fieldwork right from the beginning. Although fieldwork was predominantly linguistic, the long-term perspective offered chances to make further sociolinguistic and anthropological observations. As mentioned already in the beginning, fieldwork began on Forest Enets for which dedicated funding was received. Later, Dolgan and Tundra Nenets followed.

In the field

- ²⁵ The first visit to the area, then with an Estonian colleague, took place in the beginning of February 2006 and lasted for two weeks. The central aims were to become acquainted with the area, to get in touch with potential speakers and if possible to gather some data; further, we had to contact local authorities regarding further research permits. Due to extraordinarily cold weather an anticipated trip to Potapovo could not be realized, and we were stranded in Dudinka. However, the pilot trip turned out to be successful and the anticipated long field trip later that year became possible. I returned to Dudinka in late October 2006 to continue work on Forest Enets, now both in Dudinka and Potapovo, and stayed in the area until the end of April 2007. Due to changes in the local administration and the problematic relations between Estonia and the Russian Federation, further attempts to enter the area failed for reasons we could not influence. Only in the spring of 2008 the next trip became possible and I could spend almost three months in Dudinka and Potapovo again (05/2008-07/2008). During the end of this trip, the first sessions with a speaker of Dolgan took place. The last field trip to the area to date took place in fall 2011 (from early August to early October); although intended as trip to the Taimyr Peninsula, this trip stranded in Dudinka, again; all attempts to leave for any of the villages around failed — tickets to any destination, either by boat, helicopter or aircraft were sold out for months ahead and so all work had to be conducted in the district capital. Furthermore, for two other reasons, the trip was fairly unusual. First, I was accompanied by Lotta Jalava, then a PhD student in Helsinki whom I assisted in collecting data on Tundra Nenets. This gave me the chance to get to know the local variety of Tundra Nenets better; although I had done some initial attempts already during the long trip 2006-2007, this was accidental and unsystematic. Second, my original aim of this trip was to continue with my own research on Forest Enets and Dolgan to which Tundra Nenets had to be added and I ended up working on three languages — on quite a number of days all of them in a row. Although I was fortunate having had some company in the field which resulted in roughly 3 months of joint fieldwork (2006, 2011), I have spent most of the time (around 10 months) alone in the field.
- ²⁶ For the sake of completeness, consultant work elsewhere needs to be mentioned. In the end of 2007, a central Forest Enets consultant visited me in Tartu for 10 days in order to

compensate that I could not come to Dudinka because the necessary research permits could not be obtained. In December 2013, I met another Forest Enets consultant of mine in Helsinki who attended a conference at the university. As she was accompanied by a central Dolgan consultant of mine, I was fortunate enough that I got a chance to clarify a number of unsolved puzzles for not only one but, but two languages!

Work-flow

- 27 As already mentioned above, Forest Enets fell out of daily use for the rest of the remaining speakers around the turn of the millennium when their parents died. Although I identified 41 potential speakers²⁹ back in 2007, all of whom in the generation +50 then, only about 10 seemed to have preserved very good languages skills suitable for the work I intended to conduct.³⁰ Four semi-speakers with varying linguistic skills belonged to the extended circle of consultants to whom additional tasks could be assigned. Whereas in the village, I could work with both male and female speakers, female speakers dominated in Dudinka. Nevertheless, a certain kind of gender balance in the corpus may be postulated. Because the language had only recently fallen out of daily use for most of my central consultants, linguistic work such as collecting clausal examples and working on paradigms did not contain insurmountable challenges.³¹ Nevertheless, many consultants had lived in a more Russian dominated environments and eventually challenges did emerge. For example, the production of monologues was initially problematic but this changed over time for the better. Dialogues could not be staged at all because the current generation had hardly ever spoken in Enets to each other and all attempts failed. Also, attempts to elaborate on more complex syntactic questions for which one has to rely on elicitation turned out to become too problematic (e.g., relativisation of NP, causative formation). Although I had singled out two consultants whose language skills and linguistic creativity were clearly on a very high level, there was not much to be gained. Fortunately, some gaps could be closed with examples which appeared in spontaneous narratives later, yet not all of them. During the two long trips (2006-2007; 2008) transcription meant transcribing narratives from speakers representing the current generation whom I had recorded, predominantly in Potapovo. During the trip in 2011, transcription of these materials needed to be continued although a number of donated materials featuring the parental generation could be transcribed as well. Even though transcription with assistance from consultants turned out to be less problematic than I had anticipated, problems appeared elsewhere. Most of my consultants have not had contacts with personal computers; only two had access to computers but let others do the typing. Further, most of the consultants were illiterate in their L1 whose existence was mainly seen as a symbol, not as an invitation for personal engagement. Although I had hoped that after initial transcription as teamwork, I could outsource preliminary transcription to native assistants, this turned out to be illusory.³² Although from the point of view of language learning, transcription of spontaneous Forest Enets narratives (by now exceeding 7 hours) has certainly sharpened my understanding and positively affected my active command of the language, this task is one of the most time-consuming and quite often a very frustrating experience. One assistant may not be enough to understand what has been said; due to the fact that the speech community is small and the number of potential assistant limited, a second opinion was often hard to get.

“Entering the field”

- 28 In the remainder of this section dedicated to experiences from my fieldwork on the Taimyr Peninsula, I want to elaborate on the concept of “entering the field”. First, although the concept “entering the field” suggests a singular experience, this is indeed a repetitive experience and I assume that I had to enter the field at least three times. The first time in February 2006, I entered the field (= Dudinka) with my Estonian colleague who, as both the more experienced fieldworker and due to his better skills in Russian, took over many formal tasks in order to guarantee that I could enter the field alone later that year. Due to the fact that we had quite a number of formal official meetings, the first meetings with Forest Enetses appeared very formal too – this was confirmed some years later privately over a cup of tea. When returning in late fall, I witnessed a number of surprises and had to enter the field again; quite a number of persons my Estonian colleague and I had met earlier that year were no longer working in the same positions and another round of formal meetings became necessary – this time alone. Several weeks later after having worked with a number of Enetses residing in Dudinka, it was time to fly to Potapovo. I arrived there during the polar night which was far from being a positive head start for “entering the field” – -40°C outside, nobody on the streets, no contacts apart from some names in a notebook. So, the typical pilgrimage to the village administration, the house of culture and local library began in order to find somebody who would be willing to show me where potential speakers live. As the helicopter I arrived with brought social allowances, getting to know somebody had to be delayed for some additional days until the village would sober up again. In the end, it took about two weeks to get started.³³ In March 2007, after having returned to Dudinka, another “entering the field” took place. My local contact person approached me and suggested that I should talk about my fieldwork in front of the native intelligentsia of Dudinka. She organised this meeting on very short notice and indeed, quite a number of Forest Enetses, Tundra Nenetses and Dolgans appeared. After the formal part, many of them remained at the location and over several cups of tea I had to answer the typical question why a German living in Estonia was interested in indigenous people which most of the Russian tend to call “our local Chukchis.” The ice was broken... Soon, the local Tundra Nenets family club asked me out for fishing and barbecue on the Yenisei on a bright and sunny day in early April 2007 and several of the Nenetses I got to know there became consultants in 2011.
- 29 I want to conclude this section with a short note on the circumstances of my work; although I have been working with the languages of indigenous people whose former way of life is vividly represented in my field data, I have not (yet) have “lived” among them in the anthropological sense. Fieldwork in Dudinka in an urban environment cannot live up to such expectations and I used to live in one of the hotels in town. During day time I visited consultants or worked in the local cultural centres of town, in the evening I analysed what could be done during the day and prepared meetings for the next day(s); in Potapovo, I lived in a large log house which was subdivided into four apartments; the apartment I could live in was otherwise functioning as a kind of social housing and it had quite some reputation. Therefore, almost all work happened in my consultants’ homes or in one occasion, in the “hospital”. A trip to the tundra remained out of reach because almost nobody lived there anymore besides the members of the last reindeer brigade. Although several members of the brigade were ethnic Forest

Enetses, only two of them, actually siblings, were reported to have active skills. Whereas the male speaker was out in the tundra, his elder sister spent most of the winter 2007 in the village. With only one speaker residing in the tundra and all remaining speakers being permanent residents of Potapovo, there was no obvious linguistic benefit spending some days in the tundra. By the time of my second visit to Potapovo in the summer of 2008, also the last Forest Enets speaking herder had permanently moved to the village; his sister had died rather surprisingly some weeks after I had left Potapovo in March 2007. Whereas linguistic fieldwork living among Tundra Nenetses and Dolgans is in principle possible and ranking high on my agenda, this simply was no longer possible among the Enetses. Apart from the fact that the speech community had dissolved, so had their traditional way of living. The remaining speakers lived in a rural Russian dominated village or a small Russian town which happened to be situated on their traditional homeland in the Yeniseian North.

Fieldwork in Arvidsjaur

- 30 Ume Saami, as all languages after Forest Enets belongs to the group of languages on which work began “by accident”. Everything started on my way back from Arjeplog in May 2011 after an intensive week of work with two speakers of Pite Saami together with Joshua Wilbur. Before returning to Estonia where I was living then, I had the chance to stay for a couple of days in Arvidsjaur. What I priorly knew about Arvidsjaur was not much, mostly that the village is located on border between Ume and Pite Saami, and that Ume Saami was once spoken and also documented there. Further, it was unclear whether there still were any speakers alive.

In the field

- 31 After having arrived to Arvidsjaur, my host took me for a longer walk to show me the village and the famous Saami church village *lappstaden* (Manker & Hvarfner, 1969). I asked him whether he knew any Saamis and a couple of minutes later we sat in the kitchen of a man of roughly 80 years of age. When I asked him which Saami language he speaks he answered Ume Saami but added immediately that he was more of a rememberer than an active speaker. During the conversation, he mentioned that there were four persons with skills in Ume Saami left in Arvidsjaur (including himself) and one of them was supposed to speak the language as the preferred means of communication. The person he spoke his language to was his wife, a North Saami who had learned the language in the marriage. He referred to this group including him as the “old ones” because they were all around 75-80 years of age. The fourth person whom he called the youngster, because he was “only 65 years old”, was not in the village. For the next day, we arranged a meeting with the speaker who mentioned that he would have an hour or two the next day. Symbolically, he wanted to meet us in *lappstaden* where he owned a traditional cottage. The next day, I arrived with pen, paper, a recorder and my backpack because I had to get to the airport immediately after the session would end. And so, on one of the first spring days in Arvidsjaur in May 2011, I gathered two hours of Ume Saami.
- 32 In late November 2011, I had a chance to return to Arvidsjaur for additional fieldwork. Whereas the rememberer I met first was willing to help as much as he could (he is a

widower living alone with quite some free time), the fluent speaker was hesitating. We tried to persuade him to continue with the kind of work we did in his cottage half a year earlier, but he thought that there was no use to continue – the language was gone and I came too late. Nevertheless, he agreed to show up the next morning, spent some two hours with us, but was never seen again. So we tried to get in contact with the youngster who, again, was away. I still had about a week left in Arvidsjaur and decided that the rememberer and I should try to transcribe some legacy data (which I had obtained in the meanwhile). This was in fact the final resort as all other chances to get further data had dissolved. The next day, we started an attempt to transcribe and translate a conversation between two Ume Saamis recorded in the late 1980s. The rememberer knew both men personally and anticipated that his passive skills should be sufficient. Indeed, this worked out surprisingly well, and we managed to transcribe and translate this long conversation (about 30 minutes long) and two additional short recordings from 1943. These recordings were not among the materials which I had obtained, but were provided by the rememberer himself. Some years earlier, he had organised a copy of Björn Collinder's recordings in Arvidsjaur in 1943 (see Siegl, [accepted for publication] for further background) from the archives of the Institute for Language and Folklore in Uppsala because he knew that his father was among Collinder's consultants; soon after the recording, his father had died. Although I suggested that we should try to transcribe his father's speech, this was not feasible due to the bad sound quality of the recording. Instead, we managed to transcribe fractions of two narratives told by the father of the youngster, who, as a matter of fact was still away. In the second week of December, I left Arvidsjaur unsure whether I would have a chance to continue. I was painfully aware of the urgency due to the miserable state of knowledge of Ume Saami (see Siegl, 2017a), but I could not foresee whether I would have a chance to continue. In 2015, a project proposal for a two-year salvation project was granted and in late fall 2015, I could return to Arvidsjaur. In the meanwhile, Joshua Wilbur had published his monograph on the structure of Pite Saami (Wilbur, 2015) and the circumstances of his fieldwork which I had followed over the years made me believe that I could encounter a similar situation among the Ume Saamis. Wilbur had begun his fieldwork with a handful of Pite Saami speakers, but kept on discovering more and more speakers and rememberers with whom he eventually could work. However, in the Ume Saami case this turned out to be an illusion: there were indeed only the four persons I had already heard about in 2011. Further, I did not understand the reference to Arvidsjaur correctly then. Whereas I assumed that this would refer to the village, indeed the whole county was meant.³⁴ Further, the Ume Saami speaker and his Ume Saami speaking North Saami wife were suffering from dementia which left only the rememberer (then 85) and the youngster now almost 70 years old available. At least, the youngster was in town and I finally got to meet him in person. Due to the fact that one of the original aims of the project, recording contemporary Ume Saami, was completely out of reach, I had to concentrate entirely on the second aim of the project – transcription of old sound recordings. Fortunately, the archives in Uppsala contained enough of them, especially from the 1950s and 1960s which featured close kin and former colleagues of the rememberer. In the next 18 months, I came regularly to Arvidsjaur for short-term fieldwork (roughly 7-10 days) which resulted in about three months of fieldwork altogether. As my family situation had changed in the meanwhile, long-term fieldwork simply was no longer possible.³⁵

Work-flow

- 33 When I had realised that there was no more chance to accumulate any new data in Arvidsjaur (and as a matter of fact neither in Ammarnäs), the whole project had to be re-organised. Although reorganisation would have allowed different options, e.g., focusing on written materials,³⁶ I deliberately decided to discard this option. Analysis of written materials does not necessarily presuppose the assistance of native speakers or rememberers of Ume Saami. As for transcription of archived sound recordings, such assistance is mandatory and at hands were the last possible moments to do so. The challenge was that neither of us three was a competent L1 or L2 speaker of Ume Saami. Further, whereas I was used to learning a language (and unlearn another one) via elicitation, this was no longer possible. This meant that I was forced to decode Ume Saami via South and North Saami skills, relying to a much larger degree on my consultants and their passive language skills. Besides, both assistants had hearing aids which had their own influence on the transcription process. Once over a cup of coffee and reindeer meat we joked that the three of us were the “rough equivalent” of one former Ume Saami speaker. The rememberer contributed with his astonishingly profound passive lexicon and his creativity to guess what an unknown word would mean by relying on the context of the narrative/recording. The youngster showed clear preference for syntactic nuances and tended to hear things better than the rememberer. I was responsible for looking up words in Schlachter’s dictionary (Schlachter, 1958) or in one of the Lule Saami dictionaries which the rememberer owned or to offer alternative interpretations on constructions based on my dwindling active knowledge of North and South Saami.³⁷ Due to the fact that both consultants/assistants were no longer the youngest, we could work for about 4 hours in the morning.³⁸ On good days, we would transcribe and translate 8-10 minutes on paper, on other days, perhaps only 6 minutes — this depended on the quality and the content of the recording. Afterwards, we sat for another 30-45 minutes to listen to the rememberer’s comments. Most of the incidents and/or stories were general oral knowledge when he was a young man and occasionally he came up with additional nuances which he then retold in the local dialect of Swedish (often resulting in a recording). Otherwise, we talked about recent events, local gossip, news from reindeer breeding and very occasionally, on Saami politics. Whereas linguistic work with the recordings brought us into the ethnographic past which was still witnessed by the rememberer (e.g., milking reindeer which stopped in the late 1930s) yet only known as hearsay by the youngster, coffee table talk focused on the recent past. Summing up the work-flow, transcription work followed a relatively stable and predictable routine. When I arrived in Arvidsjaur, we sat down and listened to a number of narratives which I had extracted from archived recordings in order to check whether both could understand the language (the quality of recordings varied quite a bit), had heard the story before and found it interesting and worthwhile to transcribe and translate. After this procedure, it happened more than once that a local map was brought to the table to identify the places which had occurred in the narratives (a step often intended to offer further background information for me but sometimes even for themselves); given that a narrative contained names of local Ume Saamis, another 10 minutes passed during which kinship relations between narrators, protagonists and themselves were reconstructed (mostly, three generations ago, everybody seemed to be related to

everybody anyway).³⁹ After these steps had been tackled, we started transcribing and translating. During my visits to Arvidsjaur we managed to transcribe almost four hours of narratives, mainly from Arvidsjaur and Arjeplog county. This choice was conscious, because both men have spent most if not all of their lifetime in these areas where the narratives were anchored. Further, most of the narratives which were chosen for transcription and translation featured closed kin or incidents at places they had seen themselves or stories which they had heard themselves. Occasionally I tried to present narratives from other areas from the south, but these turned out to be “culturally” problematic and were discarded — whereas the dialectal differences would have been surmountable, the stories themselves were the problem; due to unknown narrators, unknown protagonists, unknown place names and unknown incidents, they remained “untranslatable” for them.⁴⁰

- 34 Finally a note on elicitation is in order; after roughly a year when I could see that regular work had revitalised their knowledge of the language which resulted in a quicker and smoother work-flow, I tried to smuggle some disguised forms of elicitation into our work, mostly targeting morphology because umlaut patterns lacked certain crucial forms in order to derive full paradigms; all attempts failed and I refrained from further attempts. Whereas elicitation was still possible among the last speakers of Forest Enets, this was no longer in Arvidsjaur.

“Entering the field”

- 35 So far, the Ume Saami account has focused on Arvidsjaur for reasons which I sketch below. Recapitulating “entering the field” in Arvidsjaur, this process should probably be broken down into multiple instances of entering the field too. The first time in May 2011 was extremely easy and in less than 48 hours I was able to get some linguistic work started. However, when returning half a year later, the last speaker had no interest in any similar work; further, all attempts to get to know the youngster failed. Although the youngster joined efforts in 2015, it became obvious that this was not his decision in the beginning. Instead, the rememberer exerted his influence by relying on his status as one of the oldest Ume Saami within the community and the fact that he and the youngster were relatives. Apart from the two old men, other contacts with Ume Saamis were infrequent and mostly professional such as several meetings with the former Saami representative of Arvidsjaur county (herself an ethnic Ume Saami from the neighbouring county Malå). Although I could meet several younger Ume Saamis either when they dropped in at the rememberer’s place when we worked there or at two more official occasions, all conversations remained on a very shallow level. Whereas I got to know the two elder men to some degree, the ethnic community remained close.⁴¹
- 36 This brings me to Ammarnäs, another village with a former sizeable Ume Saami community which I visited twice (November 2015; May 2016) looking for potential speakers. Although I could meet two elder individuals (both around 80 years of age) with some passive skills in Ume Saami, the interpersonal level remained rather frosty and it became obvious that it was rather unlikely to gather any new materials there; additionally, the holdings of the Institute for Language and Folklore in Uppsala did not contain significant materials from the Ammarnäs area which made work similar to that in Arvidsjaur more difficult. In May 2016 I returned for an attempt to transcribe some

stretches of two simple narratives recorded in the village in the 1970s. As both speakers did not appear to have good personal contacts, I had to meet both speakers separately. Unfortunately, it became quickly evident that this task proved to be too challenging. Furthermore, although the narratives were recorded around Ammarnäs, neither of the consultants could identify the speakers before I revealed their names. Due to the constraint that fieldwork could only be conducted as short-term fieldwork, Ammarnäs turned out to be too much of a risk instead, I decided to focus on Arvidsjaur for which the chemistry between my consultants and me was good and I could rely on more recordings. Although having visited Ammarnäs twice, I would not claim that I had “been there” in the Geertzian sense.

Major differences between both field sites

- 37 Although I have contrasted some significant differences between both field sites already above, I return to this topic for a more thorough comparison. Fieldwork among the Forest Enetses could be conducted with competent speakers⁴² with additional support from several semi-speakers. Whereas fieldwork from 2006-2008 focused on the language of the current generation, the perspective could be expanded during the field trip in 2011 during which donated materials and/or materials obtained from local archives have played a more prominent role. Nevertheless, data collection among the current generation of speakers continued. From a linguistic perspective, fieldwork followed the standard documentary and functional typological procedures which resulted in Siegl (2013). Leaving the linguistic work aside and focusing on the question of what the Forest Enetses told, a clear preference for “life stories” is observable.⁴³ Occasionally, topics of historical and/or ethnographic relevance could be recorded, though quite often in Russian and only less so in Forest Enets. Clearly, certain topics appeared to be language dependent for which the sociolinguistic history should be made responsible. The time frame covered in narratives (regardless of language choice) coincides with the second half of the 20th century and the lifetime of the speakers. Donated materials, often recorded from speakers from the parental generation in the late Soviet period contained either traditional narratives, fragments of legends and occasionally historic accounts; the first two have fallen out of use in the meanwhile and therefore could not be documented but, at least, were passively still known. As for the content of narratives which I intended to record, the idea of the documentation project was to offer the Forest Enets community the central role what the emerging corpus should contain. This decision was made consciously in order to minimize the uneven power relations as much as possible and resulted in the accommodation that only “interesting stories” should be told and recorded. In this respect, “interesting stories” was understood both locally and globally because a central concern of my Forest Enets consultants was that via my mediation, the world should learn something about them. Again, this metaphor was consciously chosen by the Forest Enetses themselves — after all, the fact that their language was on the verge of extinction was known and well understood by everybody. Returning once more to the (documentary) linguistic perspective, a short note on the corpus is in order. First, due to the fact that the language has lost its function as the primary means of communication as well as the traditional context of usage, these gaps are visible in the corpus. Whereas from a corpus — linguistic perspective, every corpus of an endangered language will remain unbalanced and small, the Forest Enets corpus will remain even more unbalanced.

Speakers were no longer capable of using the language in all the contexts in which the language had been used earlier. Genres such as child language, baby talk, teenage gossiping but even differences in register could not be gathered. What could be documented coincides with the use of the language around the turn of the millennium. Whereas grammatically, the language showed only mild signs of restructuring in comparison to the language of their parents who preferred Forest Enets over Russian, the contextual use of language use had changed significantly.

- 38 Turning to the results of fieldwork among Ume Saamis, the picture looks markedly different. First, own materials gathered in 2011 are quantitatively irrelevant and indeed new materials could hardly be collected during the project period 06/2015-05/2017. As the description above has shown, the project became completely dependent on the holdings of the Institute for Language and Folklore in Uppsala. In retrospective, it appears to be a fortunate coincidence that any data could indeed be produced. The last two available rememberers in Arvidsjaur could and enjoyed working together⁴⁴ and proved to have enough linguistic skills to assist in transcription and translation. Further, they found the task and most importantly, the stories interesting. However, when looking at the emerging corpus of Ume Saami, meaningful sampling was out of reach; the corpus is severely biased due to the materials which were deemed transcribeable. First and foremost, the speaker had to be known for at least one of the consultants; second, the narrative had to be anchored in the county of Arvidsjaur or in the neighbouring county of Arjeplog. Third, due to the fact that both consultants depend on hearing aids, recordings needed to be in reasonably good quality; fourth, the diction of the speaker needed to be good. Speakers with high speech tempo proved to be too difficult to follow and so were discarded. This means that the corpus which currently consists of approximately 4 hours of transcribed narratives is a conglomerate of numerous conscious decisions and the outcome of what two Ume Saami men and I could cope with. Therefore most of the narratives represent the subdialect around Arvidsjaur; the creation of a corpus representative for Ume Saami was simply out of reach. This means that the gaps in this corpus are even more drastic in comparison to Forest Enets which indeed is representative of (contemporary) Forest Enets. Due to the fact that elicitation in 2011 yielded little output which, additionally, hardly contains any complex structures and further elicitation failed at all, a potential description of Ume Saami will become a philological endeavour. In this concern, one should better speak of text linguistics, although with a clear focus on spoken language. Although an important body of written archival Ume Saami data does exist which has recently been employed for the first time by Larsson (Larsson, 2012), most of these materials are syntactically irrelevant, because they were collected during a period where language was reduced to words and morphemes (Siegl, 2017a). This means that albeit its shortcomings and constraints, this preliminary corpus is currently the largest and so far most balanced collection of Ume Saami, even though it is heavily based on the variety spoken around Arvidsjaur and still requires much work before it can be made available.

⁴⁵ Finally, when looking at the content of the Ume Saami corpus regardless of all constraints underlying its compilation, the corpus is less biased. Most of the narratives transcribed were collected in the 1950s and 1960s by Israel Ruong and Tryggve Sköld. Although both researchers worked around the same time in the same area and shared quite a number of consultants, the content and the style of their recordings differ. Ruong's recordings are mainly interviews where Ruong asked Ume Saami acquaintances in Ume Saami about ethnographic and occasionally historical

events – the interviews are informal and were collected during several trips in the 1950s. In contrast, Sköld worked mainly in Swedish and recorded almost 10 hours of speech (most of his recordings are in Ume Saami) during his last trip to Arvidsjaur in 1963 (Siegl, 2017b). Although Sköld's recordings appear more formal, most of them contain narratives in the classical sense. In Ruong's recordings, more than once it turned out to be problematic to cut out a stretch of speech for transcription because both a dedicated beginning and a dedicated end did not exist; a story started somewhere in the dialogue and before it faded, it intermingled with the beginning of a new one. As for the topics of transcribed narratives, life stories are attested, though these play a minor role. Narratives with either a folkloristic or an ethnographic-historical perspective dominate.

Personal reflections

- 39 The experiences from the Taimyr Peninsula and Sábmme are most certainly not unique; Evans' (2001) experiences verbalise quite a number of observations for which I would find ample parallels in my work of the last 10 years. As a *leitmotiv* for this section I would like to quote Evans: “[o]bviously, doing fieldwork in such situations [among the last speakers, FS] is a race against time, and tracking down good speakers before it is too late involved a great deal of detective work” (Evans, 2001: pp 251). In the following I will suggest that the metaphor “detective work” should not be reserved for the linguist alone.

Helping others finding their place

- 40 On one occasion in Arvidsjaur, I told my two consultants how the archive work in Uppsala looked like in order to identify relevant materials for our work. Saami holdings at the Institute for Language and Folklore in Uppsala are catalogued by county and not by language. As for sound recordings, Saami from Arvidsjaur or Malå county is unproblematic because such recordings contain Ume Saami (and often quite some Swedish too, the content is not always retrievable due to missing meta-information). However, for the neighbouring county Arjeplog where four different Saami languages were documented (Ume, Pite, Lule and occasionally even North Saami), the only chance to identify the language was to listen to sound recordings and I must have spent five working days in the archives just doing so. After having listened to my story over a cup of coffee and dried reindeer meat, the youngster concluded that this sounded like a detective story and not like the kind of archive work a layman would imagine. He however continued that our joint effort would most certainly also qualify as detective work, because after all, we often had to track down the meaning of an Ume Saami word unknown to us in a variety of resources, often starting in the unpublished word collections of Axel Calleberg and occasionally ending with the standard North Saami dictionaries when I was back in Tromsø. However, not always did this kind of detective work yield any results. Again, even though quite some detective work was needed in the Forest Enets case, the Ume Saami project clearly required much more of it. Therefore, this section will focus more on experiences from the Ume Saami project, simply because there is more to tell. I want to start with the outcome of another short conversation over coffee and dried reindeer meat. As mentioned above, a decisive factor why the Ume Saami project could eventually produce transcripts at all was the

fortunate coincidence that most of the recordings from Arvidsjaur county featured the voices of close kin, or Ume Saamis with whom at least the older consultant had begun working in reindeer husbandry in the late 1940s. Quite frequently, those stories contained names and made references to places which had fallen into oblivion and suddenly re-appeared on the scene and came back to the rememberer's memory. Although this "resurrection" of forgotten knowledge is most certainly a positive outcome, bringing elder people back into the forgotten and partly neglected past is dangerous business. And so, after we had tackled another longer and very personal story and needed a break, I asked my consultants whether these verbal visits to the past had resulted in any unusual feelings, emotions or dreams. I knew that this was a very personal question and I did not anticipate any straight answer. Quite surprisingly, the rememberer answered immediately and frankly mentioned that he did dream about his dead wife who occasionally spoke Ume Saami with him. He claimed that such dreams tended to happen when I was in town, otherwise such instances were rare. He clearly attributed these dreams to our work because transcription made him think more about the language than otherwise. The youngster mentioned that he had not experienced anything worthwhile and our coffee break continued with other things to talk about. However, several days later the youngster surprised us quite a bit. Although we knew that he had attended the boarding school in the neighbouring village Arjeplog, he had never told about this episode of his life.⁴⁶ The reasons of neglect were not too difficult to anticipate, because the *nomadskola* in Arjeplog had a bad reputation, and he was the only Forrest Saami from the neighbouring county who, for unknown reasons, had attended it. Almost out of blue sky, he opened up and told what had happened to him there. Although I had heard and occasionally recorded similar stories in Siberia, the degree of humiliation (which he framed unconsciously by relying on the minority within minority metaphor) was quite shocking for both me and the rememberer who had visited a regular Swedish school in Arvidsjaur county. After the youngster had cooled down, a different man sat behind the table and one could see how a burden which he had carried along for more than half a century had fallen off. Finally, and almost *en passant* he added that this opening could have been a direct outcome of the linguistic work he could contribute to and the question I had asked some days ago. Whereas in the beginning he felt that he had to contribute to our work because the rememberer wanted to have a further helper around, this perspective changed. Coincidentally, the youngster's close kin, his father but especially his father's sisters were among the narrators I singled out for potential transcriptions because they were extraordinary narrators and the recordings were of very good sound quality; further, I anticipated that individual idiolectal features would be known because both men had been in frequent contact with them. During our work, the youngster felt that he really knew something and that his help was appreciated and most important, relevant.⁴⁷

Finding one's place...

- 41 For practical reasons, I will stick to the detective metaphor for a little while; fieldwork needs funding and due to the fact that work on endangered languages is most certainly not top priority for national funding agencies, one needs to be a good detective to find relevant funding programs. Further, whereas an anthropologist applying for a tenured position most certainly benefits from fieldwork experience, this is most certainly not the case for a linguist. Although international initiatives such as the former DOBES

program or ELDP have attempted to raise and in the case of the latter, continue to rise awareness for documentary linguistics, discussions with colleagues within and outside of fieldwork based linguistics makes me believe that this has not been achieved – the fieldwork conducting linguist is still and most probably remains a marginal phenomenon in the discipline of linguistics.⁴⁸ As long as linguistics focusing on indigenous languages is judged by the same principles which apply to “nation state language” linguistics, a competition on even grounds remains unthinkable. Even in empirical linguistics which is increasingly dominated by big-data and statistics, a linguist who is working on a small language with a corpus of several ten thousand tokens, often alone and therefore responsible for the analysis on all linguistics levels from phonology to pragmatics simply will never be able to compete.⁴⁹ Further, without a secure position, it is impossible to recruit, train and supervise students who eventually could assist in data management and analysis. The tenor of this argument is obvious and this thread will not be continued. Instead, two practical problems will be touched upon. Although these are more of a personal nature, I assume that the arguments are applicable beyond the personal horizon in which they are framed. The current climate in research based on non-tenured project research within two or three-year funding cycles is by definition hostile to work on more than one understudied language, especially when working alone. During the Ume Saami project, continuation of work on other languages on which I had gathered materials simply had to stop. Most likely, this situation will continue in the immediate future. This scenario where prolonged work on one language has immediate negative implications for the others and “unlearning” starts to interfere – one simply loses intimate contact with the data one has once collected. At this point, the dividing line between enthusiasm and frustration becomes very thin.⁵⁰ Another personal argument, perhaps too personal for the linguistic audience, are the psychological challenges of fieldwork. Again, this argument hardly comes as a surprise to anthropologists; Bronislaw Malinowski, the godfather of anthropological long-time fieldwork and the dispute which arose after the posthumous publications of his strictly private field diaries are well-known in anthropology. In addition to the competition in the race for funding and tenure and the overall race against time, work on moribund languages without obvious non-academic implications is perhaps nothing else than a kind of academic “terminal care” – one simply has to get used to the fact that one has to work with elderly people whose language is on the way towards extinction. One may enter the field with all kinds of ideas what one would like to do, but the sociolinguistic reality imposes restrictions and quite likely, very soon. Further, given the fact that the moribund languages on which I have been working are spoken by elder people, one simply cannot assume that one can work 6-8 hours a day with speakers/rememberers; consultants have their own life, consultants get tired, especially when they have not used their language for a while and quite often, there are not that many consultants available so that one could work 6-8 hours a day. Of course, the circumstances of this kind of fieldwork I have been engaged with are not among the prototypical instances of fieldwork in the textbook sense, but after all which field sites are as described in textbooks in the long run? Nevertheless, when fieldwork is conducted in urgent circumstances with only a handful of speakers/rememberers around, one loses track that not indeed all languages are moribund. I still vividly remember how perplexed I felt when sometimes during my stay in Dudinka in 2011 three young Dolgan men asked me whether they could try this kind of work I was doing with “the old ladies”. Another and rather similar episode

happened some days later. Two Nenets teenagers from the boarding school in Dudinka were sent as consultants by their teacher who herself was a consultant some days earlier. Indeed, all of them spoke their native languages, though not everybody was interested in this kind of work in the long run. In this concern, working with my Tuvin consultant opened a new page — by chance I had met a fluent speaker of an endangered language who was literate in both L1 and L2 (I doubt that any of the five young adults in Dudinka was literate in their L1). Further, she was not trained as a Tuvin philologist and therefore not concerned with normative grammar — a combination indeed unusual for many professional users of Siberian minority languages. Clearly, not all endangered languages are moribund but due to the settings of my fieldwork, this perspective appears markedly unusual...

Conclusions

- 42 Instead of summarizing ongoing work based on what could be collected on the Taimyr Peninsula and in Arvidsjaur, I will conclude by taking up a more general perspective again. After all, the topic of the workshop was dedicated to linguistic and ethnographic fieldwork in general. Although running danger of carrying coals to Newcastle, it goes without saying that many challenges which linguistic fieldwork imposes on the discipline of linguistics have been on the agenda in anthropology for many decades. Clearly, anthropology is better off in this concern. First and foremost, fieldwork is much more than just collecting data. Unfortunately, the personal dimension of fieldwork e.g., entering the field, getting acquainted, building and maintaining a sphere of mutual respect and understanding which are the necessary prerequisites before any linguistic data can be gathered, analysed and eventually published are generally overlooked and, as I would say, often simply taken for granted by those who have never engaged in fieldwork.⁵¹ It is quite obvious that here the discipline of linguistics still needs to learn from the experience of anthropology, even though fieldwork won't become a similar topic of theoretical and epistemological concern in linguistics as in anthropology. Neither will it become a dominant tool for linguistic data collection, although at least its nature should be understood much better.
- 43 Turning to another problem of general nature is the time-consuming analysis of fieldwork materials. This problem is, again, neither a unique linguistic or anthropological problem, but a general problem of a discipline which cannot gather data in comfortable arm chairs relying on introspection. This problem unites *a priori* all disciplines relying on fieldwork, even though e.g., geological fieldwork's potential overlapping with linguistics or anthropology appears to be rather limited. Knowing that primary data cannot be turned into "machine-readable" data overnight, for precisely the same reasons as mentioned in the prior section, one wonders why digital humanities should make any difference. Digital humanities is interested in the data and the format of it, but only to lesser degree who the data has produced and how it ended up in a "machine-readable format". Yet precisely here, reflexivity becomes an issue and the Geertzian perspective which reduced the anthropologist to become an author has altered the rules. Although extreme reflexivity is problematic and due to the role of "descriptive tools" in linguistics eventually quite unlikely to achieve a similar role, reflexivity needs to be accommodated in fieldwork based linguistics too: data is not just out there waiting to be documented. This begs for another important question for

which I have to refrain from offering any answers — given that digital humanities is all about quantitative data, how relevant is data deriving from fieldwork, for example gathered in moribund speech communities, which is, *a priori*, qualitative? If linguistics as one of the disciplines in digital humanities were the study of human language and not as far too often, the study of certain privileged languages spoken in industrial and post-industrial First World Nation States for which data tends to be around and needs not to be gathered, analysed and assembled first, the chance that fieldwork based linguistics would occupy a more prominent role would be likely. In this context, one should not forget Edward Sapir's programmatic statement that all languages matter for the study of language: "When it comes to linguistic form, Plato walks with the Macedonian swineherd, Confucius with the head-hunting savage of Assam" (Sapir, 1921: pp 234). Almost a century after its first publication, Sapir's statement is still essentially correct and bewilderingly contemporary. The fact that this statement came from a fieldwork conducting linguist is hardly surprising.

- 44 I wish to conclude this essay with a short superficial comment on current linguistic practicalities. Linguists who predominantly subscribe to the assumption that linguistics is a science relying exclusively on native-speaker intuitions and abstract formal models of syntax in order to arrive to a generalization about language, usually based on the analysis of one particular language tend to look at the kind of linguistics discussed in this essay as "descriptive", "theory-low" or "theory-free". Of course, the "descriptive tools" required in fieldwork based linguistics on underdocumented language cannot be "theory-neutral", because even the postulation of concepts such as syllables, words, parts of speech and the like is theoretic — their degree of theoretical sophistication might differ indeed, but they are not Platonic *a priori* concepts. Much of this dispute between formal and functional approaches to grammar have resulted in a theory labelled as Basic Linguistic Theory (BLT) to which many fieldworkers subscribe and it was in this context in which Gil's concept of "unlearning" was originally framed (Gil, 2001). Fieldwork on languages which are radically different from the languages standard theorizing is relying on (predominantly still European languages) requires escaping Eurocentrism and "unlearning". In the meanwhile, a number of Basic Linguistic Theories (Dryer, 2006; Dixon, 2009) have appeared and perhaps this should be taken as a sign that BLT has been accepted as a theory of its own — it is debated and starts to differentiate. A particular crucial moment in theoretical discussions of this type (e.g., Gil, 2001) which today is framed within BLT are centred about the relationship between theory and description and how both should ideally feed each other and not be considered insurmountable theoretical borders. To be honest, this problem as such is not one which is restricted to fieldwork based linguistics and typology but surfaces in other functional theories of language. Although Systemic Functional Linguistics (SFL) is not known to exercise influence on theorising linguistic fieldwork, it offers some insights how theory and application need not be surmountable obstacles as such. "[...] SFL tends to neutralise the boundary between (theoretical) linguistics and applied linguistics. It has been called an 'applied' theory, and its evolution has tended to be driven by the ongoing experience of its use and by its constant extension to new areas of enquiry and of action [...]. Every context of application brings with it new demands on the theory; and the lessons gained from facing up to these demands feed back into the theory and enrich it." (Halliday, 2009: pp 61). At this point, one of Geertz's observations from the introduction reappears on the stage — although Ruth Benedict and Claude Lévi-Strauss did not conduct much

fieldwork, their writings had quite some influence and most certainly benefited from their otherwise limited fieldwork. M.A.K. Halliday, too, started his early career as a fieldworker on Chinese dialects before becoming the foundation father of a linguistic theory called Systemic Functional Linguistics (SFL). Apparently, fieldwork does matter.⁵² And in the end, fieldwork among speakers of moribund languages is fieldwork too, yet, to some degree, different.

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NOTES

1. Front matter is used in the sense of Mosel (2006). There are of course some exceptions such as Dixon (1984) or the collection of articles edited by Newman & Ratliff (2001), but clearly, reflections about fieldwork do not necessarily belong to the standards of linguistic fieldworks which the recent handbook by Chelliah & de Reuse (2011) exemplifies. Although it offers detailed advice on how to do fieldwork and what to expect, reflexive moments are scarce.
2. For the sake of clarity, I wish to emphasize that although I recognize the importance of self-reflection as propagated by e.g., Geertz (1988), I am siding with Silverman whose historiographic approach has verbalized a number of central problems with the "ethnography as texapproach" to which I, as a linguist, subscribe: "Ethnography becomes the core of the discipline, and it becomes equated with writing. There is no separation between fieldwork and write-up or between ethnographic data and the theory; writing is theory, and the ethnographic experience and its representation in writing are of a piece. (This narrowing of the scope of anthropology to ethnography is one reason for the general antipathy of archaeologists, biological anthropologists and even linguistics to this approach.) Ethnographic authority is deeply questioned; in effect, the whole body of literature of cultural anthropology becomes suspect. Critique and reformation of the discipline are goals, but they translate into experimentation in writing" (Silverman, 2005: pp 324)
3. Most likely, the famous travel logs of M.A. Castrén are responsible which served as a kind of standard to follow.
4. Fieldwork of the kind as reflected here is clearly extraordinary and due to its peculiar status; it should not be forgotten that there are different kinds of linguistic fieldworks as practised by e.g., dialectologists or sociolinguists. Although fieldwork is

primarily a tool for data gathering, this equation is too simple. The speaker behind the gathered utterance, his social roles and his linguistic creativity cannot be separated from the data, even though this is frequently still assumed. In this concern, quite a bit of linguistic fieldwork and the data gathered is still approached from a positivist perspective.

5. Evans (2001) account on work under similar circumstances is very informative.

6. The interplay between general concepts and language dependent categories is an entirely different matter on which I do not touch upon as it is not directly related to the scope of this contribution. No fieldworker can afford to exclude any linguistic category such as e.g., glottalisation, tone or adjectives a priori when starting to work on an unknown language. However, during analysis, the linguistic profile of a language will show whether these categories exist in a given language and if so, how they should be approached and described.

7. Fieldwork on Enets and its background have been covered in Siegl (2010a, 2013a). The socio-linguistic situation of Dolgan and Taimyr Tundra Nenets were addressed to some degree in Siegl (2013b).

8. Of course, fieldwork even without a shared language is possible and practised, but this is rather unusual; in the field sites from which my experiences derive, such an approach would be rather unlikely because the speech communities do no longer exist and the speakers live a rural life as the majority of the people.

9. Intensive work on Taimyr Nenets and Dolgan started shortly after the defence of my PhD dissertation on Forest Enets in 2011 during which I was accompanied by a PhD student from Helsinki. Although the fact that I had defended a *doktorskaja dissertacija* on a Taimyrian language and returned with a student was most definitely not an obstacle, most members from the indigenous community in the district capital Dudinka were more impressed that I had spent about five months in a village and mostly during the winter. Many indigenous people residing in the district capital do not like to visit these villages themselves and their discourse concerning rural social problems hardly differs from the local Russian discourse.

10. Whereas I could work with both elder women and elder men among the Forest Enetses, all central Dolgan consultants were female though three young men agreed for a test run once but quickly gave up as they considered their language skills insufficient. Nenets consultants were female, including two teenagers, except for one male cultural worker from the neighbouring Ust-Yenisei district who visited Dudinka and decided to tell me several stories.

11. The Tuvin speaker, residing in Helsinki, attended a local conference in Helsinki on minority studies because she had heard that somebody from the panel was working on a small Turkic language. During a coffee break at the conference she was introduced to me by the lecturer of North Saami who had organized the event. We exchanged addresses, met separately for another cup of coffee some days later to get to know each other. Some weeks later, we started meeting on a weekly basis for almost 4 months until I had to move from Finland.

12. Of course, such work may result in awkward scenarios once a while, especially concerning lexical semantics or the lexicon in general for which field methods courses within university departments are perfect examples. Already from one field methods class with a speaker of Manipuri (Sino-Tibetan), then living and studying in Estonia,

numerous examples could be mentioned because both the language family and the environment was unknown for me as the instructor and the participating students. On the other side, work with my Tuvin consultant turned out less-problematic because the postsocialist rural Siberian context was shared; further, as my consultant was intimately connected to horse breeding in her kin, discussions about horse breeding and reindeer breeding and related problems opened quite some doors.

13. This point is somehow avoided by e.g., Dixon (2010: pp 325). Given the degree of language endangerment on a global level and its anticipated development, I doubt that chances for monolingual fieldwork will eventually increase. A similar statement can be found in the opener of Evans (2001). As a kind of Kantian imperative, Dixon's statement is of course valid. The otherwise comprehensive handbook by Chelliah & de Reuse has nothing special to say about fieldwork within moribund speech communities except some very superficial lines (Chelliah & de Reuse 2011: pp 62-63).

14. The term "unlearning" was consciously borrowed from Gil (2001), though the argumentation differs from his.

15. At this moment it appears, that this might be another topic excluded from linguistic writing.

16. Several weeks later this lady told me that while being a student at the Institute of the People of the North in Leningrad, she had attended classes in Tundra Nenets for a year, and she considered the language very complex and difficult. As this lady has basic skills in linguistics, this statement contains indeed a grain of truth. This should not be understood as indigenous discourse on linguistic complexity for which the Nganasans are notoriously well-known (Siegl 2013b).

17. In contrast to Tuvin where I started from scratch, I had read a bit about Yukaghir already earlier and started my work with some initial theoretical background knowledge.

18. The asylum centre operated within an estate of a former prison; most of the former prison structures were still vivid and partly not even completely defunct. In the end, the application for asylum was unsuccessful and I lost contact after her returning to Russia.

19. The second chapter in Chelliah & de Reuse (2011) offers a comprehensive overview.

20. The criticism uttered by Leach (1991: pp 21-22) is much more straightforward in comparison to the one uttered by Geertz (1988). However, after having consulted Parkin (2005), Lévi-Strauss' rudimentary fieldwork does not appear to be as unusual when comparing it with other Francophone anthropologists of his generation.

21. The following anecdote demonstrates this point at ease. In 2009, I was invited to submit a manuscript to a popular scientific volume "The Story of Storage" focusing on what humans store and why (Siegl, 2010b). In this paper I relied extensively on my field experiences among the Forest Enetses and how to store an endangered language. During the final proof-reading round by a professional proof-reader, all instances of Enets were "corrected" to Nenets throughout the text and I ended up having published a paper on a people with whom I had not yet worked with. Later, this volume was re-published online and the editors added the following clarification on the volume's homepage (translation from Swedish is mine): "A factual mistake got smuggled into this chapter. The people who Florian Siegl has studied are called Enetses (without initial n). They are related to the Nenetses (with initial n). We apologize for

this mistake.” (<http://www.kb.se/aktuellt/Butik-och-Publikationer/mediehistoriskt-arkiv2/Kapitlen-i-The-Story-of-Storage/> accessed 24/10/2017)

22. Eventually, Ume Saamis should be classified as a double minority within a minority. A peculiarity of ethnographic research on the Saamis of Sweden is the old yet still used subdivision into Mountain and Forest Saamis. The North and South Saami and the Ume Saami enclave in Ammarnäs belongs to the Mountain Saamis. The majority of Ume Saamis belongs to the Forest Saami group together with the Pite and Lule Saamis, yet the Ume Saamis are outnumbered by the other members of this group (see Siegl, 2017a for the historical background).

23. All attempts to get into contact with Ume Saamis who have attended such courses or with the conveyors of these courses have failed and I have to rely on what my two consultants from Arvidsjaur have heard about these courses. The elder man to whom I later will refer to as the rememberer had once participated in such a course, but could no longer recall when; further, only a fraction of course materials had survived in his private archive. As the variety of Ume Saami propagated in this course was based on the southern dialect with which speakers/rememberers from Arvidsjaur had little if no direct contact, this might explain decreased interest.

24. This particular course was organised by the Language Center of the Saami Parliament of Sweden.

25. The following overview is not representative for the whole Ume Saami area, but only for the area of Arvidsjaur.

26. Forest Saamis used to be semi-nomadic (Manker, 1968) and had more interaction with the local Swedish communities. To some degree, earlier missionary schools in the late 19th and early 20th century separated (Ume) Saami children around Arvidsjaur from Swedish children and taught them separately, but their impact is hard to anticipate.

27. Furthermore, the influence of the 1918 flu pandemic (Swedish: *spanska sjukan*) should not be underestimated. Although the disease killed more Pite Saamis in Arjeplog than Ume Saamis in Arvidsjaur, the disease seemed to have killed extraordinary many Saamis which diminished the number of speakers in these fragile speech communities significantly.

28. This topic has been covered in detail elsewhere (Siegl, 2007, 2010a, 2013a).

29. This label refers to individuals who entered the local boarding school in Potapovo without knowing any Russian; several individuals in this group were bilingual Forest Enest/Tundra Nenets speakers; for one individual, I assume skills in Russian already prior to boarding school.

30. Not all of them wanted to work with me and I ended up working with relatives of my consultants in Dudinka.

31. Lexicographic work was not required due to the existence of two reasonably concise recent bilingual dictionaries.

32. As a matter of fact, this situation has not changed later, regardless of language.

33. Several details from this period are discussed in Siegl (2010a).

34. Joshua Wilbur later confirmed that there were no Ume Saami speakers left in Arjeplog county.

35. I have been to Arvidsjaur in October and November 2015; in 2016 in January, March, May, April, December and in 2017 in February, April and May. Further, I made two visits

to another Ume Saami village, Ammarnäs, in November 2015 and May 2016 (altogether eight days). The trip in August 2016 marked yet another unusual experience and resulted in the longest trip to Arvidsjaur so far because I could take my family along.

36. A conservative guess suggests that there are more than 10 000 pages of written materials in Ume Saami from word collections to inflectional paradigms stored in the archives of the Institute for Language and Folklore in Uppsala.

37. As mentioned above, by the end of the project I had completely unlearned North Saami.

38. The mandatory *ficka* (Swedish: coffee break) after roughly two hours of work is excluded.

39. I recall that once the identification of some kinship relations led to a 20-minute-long phone call discussing these matters with an elder Ume Saami lady from Arvidsjaur county who had moved to Umeå more than 30 years ago. When I met this lady in Umeå in December 2016 in person, we were already well acquainted even though we had not met earlier.

40. Although this is most certainly not the kind of “stalking with stories” to which Basso (1996) has referred to, the importance of landscape and place names which serve as the anchor for many stories is clearly observable. Similar to Basso, I had to learn both the place names and the stories attached to them. This does not mean that landscape would not play an important role in the Forest Enets narratives though relatively speaking, landscape occupies a more important role in Ume Saami narratives.

41. Initially I assumed that this might have been due to the short-term nature of my fieldwork, but this is at best a partial yet not the only explanation.

42. This was a conscious decision which I referred to explicitly in Siegl (2013a: pp 65).

43. See Siegl (2010a: pp 229-230) for background information why and how this genre began to dominate.

44. Against the experiences of Evans (2001) among the last speakers of a number of languages, this situation appears to be unusual.

45. This means that at least syntactically, Ume Saami needs no longer to be equated with the idiolect of Lars Sjulsson from Malå (see Larsson, 2012).

46. As a matter of fact, he was the only Ume Saami from Arvidsjaur in this generation who was sent to this school in the neighbouring. All other Ume Saamis from Arvidsjaur county went to regular Swedish schools.

47. During a later trip I learned in the village that after this unusual coffee table incident, he started to tell about his years in the boarding school openly to others too. This means that this was not an isolated event.

48. Of course, the role and importance of fieldwork most certainly shows variation, but on the general level, fieldwork is marginal.

49. The fact that a central goal of documentary linguistics is the compilation of a grammar, which requires understanding of all levels of grammar, presupposes a rather holistic approach to language. This is a central challenges “in a world where specialists—and linguists are no exception—know more and more about less and less” [...] (Mosel, 2006: pp 63).

50. Most of the data on Ume Saami I have been working with has been freely available for at least 30 years. It is needless to say that work with these materials with then fully

competent speakers could have produced qualitatively much more results with much less effort. However, due to the fact that neither grammaticography nor synchronic linguistics were deemed important in Saami studies in that period, this simply was not done (see Siegl, 2017a). Parallels exist elsewhere in the study of endangered Uralic languages (Siegl, 2016).

51. It is still surprising that quite many grammars published in prestigious grammar series hardly ever classify, whether an example comes from elicitation, monologues, dialogues, sound recordings, or video recordings and whether the speaker is female, male, young, old, etc. At best, some information is retrievable from the so called “front matter”. Regardless whether one classifies linguistics as science or humanities, this neglect of basic scientific adequacy is odd.

52. Coincidentally, Halliday had a distinctive influence on Dixon becoming a fieldworker (Dixon, 1984: 4-5).

ABSTRACTS

Whereas linguists conducting fieldwork tend to be a minority among linguists, linguists conducting fieldwork among the last speakers of an underresearched/underdocumented or even undocumented language are a minority within a minority. Although a number of fieldwork manuals have been published in recent decades, the perspective of fieldwork in extreme sociolinguistic situations such as among the last speakers of a language is usually underrepresented in the literature. It is precisely this perspective which will be presented by shedding some light on personal experiences and challenges from ongoing work on two moribund languages Forest Enets (Samoyedic, Uralic) and Ume Saami (Saami, Uralic). These impressions are contrasted with personal experiences from fieldwork and consultant work on other languages such as Dolgan and Tuvin (both Turkic), Taimyr Tundra Nenets (Samoyedic, Uralic) as well as Meithei (Sino-Tibetan) and Tundra Yukaghir (isolate). A central claim of this article is to show that work with extremely endangered languages does not allow any meaningful equation; field sites are unique and working with speakers of moribund languages in different countries may mean very different things and imply highly diverging challenges—what works in field site X does not necessarily work in field site Y and vice versa.

Alors que les linguistes de terrain ont tendance à être une minorité dans leur discipline, ceux qui font leurs terrains parmi les derniers locuteurs de langues sous-étudiées, sous-documentées, voire non documentées sont encore une minorité de la minorité. Même si un certain nombre de manuels de terrain ont été publiés dans les dernières décennies, la perspective du terrain dans des situations sociolinguistiques extrêmes comme c'est le cas parmi les derniers locuteurs d'une langue est en général sous-représenté dans la littérature existante. C'est cette perspective qui fait l'objet du présent article à partir d'une expérience personnelle d'un travail sur le terrain, ainsi que de consultations sur des langues aussi diverses que le dolgane et le touvinien (toutes deux turques), le nénése de la toundra parlé dans le Tajmyr (une langue samoyède, ouraliennne) ainsi que le meithei (langue sino-tibétaine) et le youkaghir de la toundra (isolat). L'un des principaux objectifs de cet article est de montrer que la notion de langues en danger extrême ne recouvre jamais les mêmes paramètres, chaque site est unique, et travailler avec les derniers locuteurs

d'une langue dans des pays différents peut signifier des activités fort différentes et présenter des défis fort divers – ce qui fonctionne sur un site x peut ne pas fonctionner sur un site y et vice-versa.

Kui välitöid harrastavad keeleteadlased on keeleteadlaste hulgas vähemus, need kes teevad välitöid viimaste inimeste juures, kes kõnelevad alauuritid, aladokumenteeritud või üldse mitte dokumenteeritud keelt on veel vähemuse vähemus. Kuigi viimaste aastakümnete jooksul on ilmunud mõned välitöö õpikud, ekstreemsed olukorrad, mis tekivad töötades viimaste keelekõnelejatega, käsitletakse harva olemasolevas kirjanduses. Selles on jutt käesolevad artiklis isikliku kogemuse põhjal, nii välitöös kui ka konsultandiks olemisest erinevate keelte jaoks – dolgaani ja tuva keeled (mõlemad türki keelkonnast), Taimõri tundraneenetsi keel (samojeedi, uurali keel) kui ka meithei (sino-tibeti keel) ja tundrajukagiiri keel (isoleeritud keel). Sele artikli üks peamistest eesmärkidest on näidata, kuidas ekstreemselt ohustatute keelte mõiste kunagi ei kata samasuguseid reaalsusi: iga tööväli on unikaalne, ja viiaste keelekõnelejatega töötamine võib tähendada väga erinevaid asju erinevatel riikidel ning esitada väga erinevaid väljakutseid – see, mis toimib ühel alal ei pruugi toimida mujal.

INDEX

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