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THE PREHISTORIC MULTILINGUAL SPEAKER: What can we know about the multilingualism of Proto-Uralic speakers?

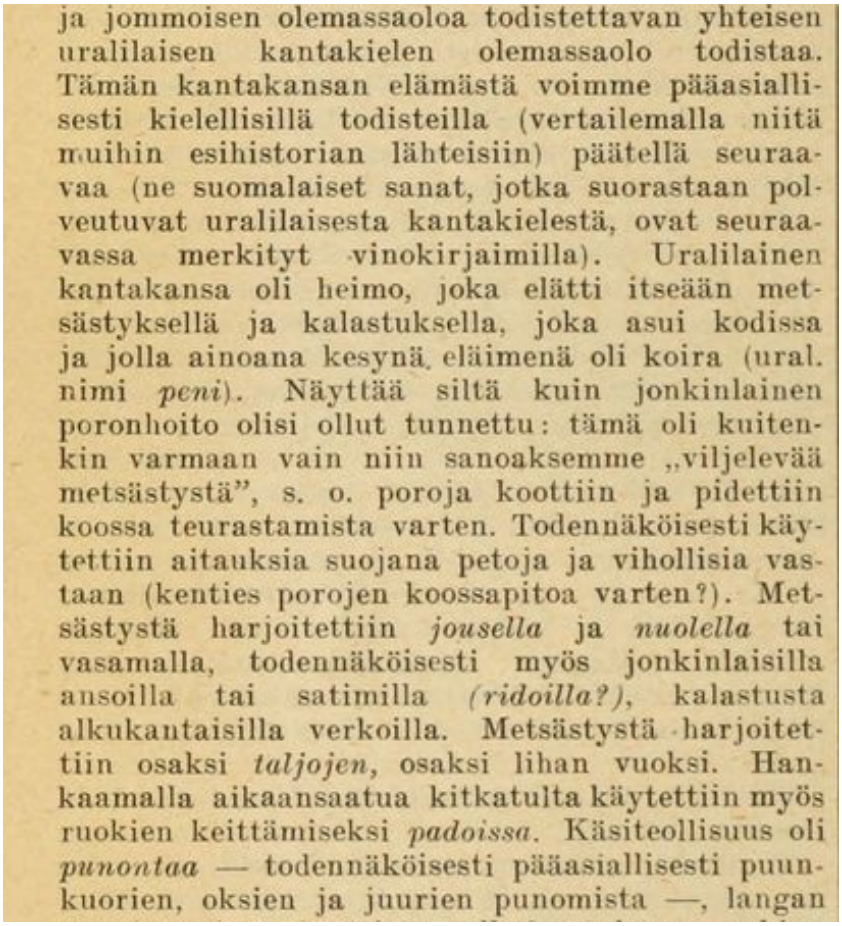
Talk at the 7th Budapest Uralic Workshop (http://www.nytud.hu/buw7/), February 3, 2014

(I have to start with an apology. As it so often happens, I overestimated my time and strength, moreover, I was severely distracted during the last two weeks by the 2nd International Winter School of Finno-Ugric Studies which this time took place in Szeged, a wonderful and very intensive event. The topic I had promised to talk about turned out to be too wide, too interesting and too little explored, and so I stand before you like the tailor mouse in the Finnish folk tale, who has promised to sew a new suit for the cat, then just the jacket, then just the vest, and finally has to admit that she has cut up and spoilt the material and there will only be enough cloth for a little purse. Please bear with me while I wade through the introduction into what would be an immensely exciting topic of much more extensive studies, never reaching the really interesting things which I wrote about in the abstract which you may have seen in the conference programme.)

Monolingual proto-speakers: ideology or method?

> *Proto-Uralic was the language spoken in the vast area between the Baltic Sea and the Ural Mountains (and perhaps beyond) during the Mesolithic age. Nothing indicates that there would have been other linguistic groups in the area and no relicts of other languages have been found. (Sammallahti 1988: 480)*

In the tradition of historical Uralic studies, very little attention has been paid to multilingualism and language contacts – with the obvious exception of loanwords, an issue to which I will come back a little later. Basically, the speaker communities of reconstructed proto-languages have been seen, as in classical autonomous linguistics in general, as monolingual and homogeneous, living or even migrating in an ethnolinguistic vacuum at least in the earliest reconstructed phases – as there seems to be no uncontroversial evidence of very early contacts. This idea has persisted during more than a hundred years and is only now slowly giving way to a new interest in very early language contact situations or even multilingualism.



ja jommoisen olemassaoloa todistettavan yhteisen uralilaisen kantakielen olemassaolo todistaa. Tämän kantakansan elämästä voimme pääasiallisesti kielellisillä todisteilla (vertailemalla niitä muihin esihistorian lähteisiin) päätellä seuraavaa (ne suomalaiset sanat, jotka suorastaan polveutuvat uralilaisesta kantakielestä, ovat seuraavassa merkityt vinokirjaimilla). Uralilainen kantakansa oli heimo, joka elätti itseään metsästyksellä ja kalastuksella, joka asui kodissa ja jolla ainoana kesynä eläimenä oli koira (ural. nimi *peni*). Näyttää siltä kuin jonkinlainen poronhoito olisi ollut tunnettu: tämä oli kuitenkin varmaan vain niin sanoaksemme „viljelevää metsästystä", s. o. poroja koottiin ja pidettiin koossa teurastamista varten. Todennäköisesti käytettiin aitauksia suojana petoja ja vihollisia vastaan (kenties porojen koossapitoa varten?). Metsästystä harjoitettiin *jousella* ja *nuolella* tai vasamalla, todennäköisesti myös jonkinlaisilla ansoilla tai satimilla *(ridoilla?)*, kalastusta alkukantaisilla verkoilla. Metsästystä harjoitettiin osaksi *taljojen*, osaksi lihan vuoksi. Hankaamalla aikaansaatua kitkatulta käytettiin myös ruokien keittämiseksi *padoissa*. Käsiteollisuus oli *punontaa* — todennäköisesti pääasiallisesti puunkuorien, oksien ja juurien punomista —, langan

(In the Finnish encyclopedia *Tietosanakirja,* published between 1909–1922[1], the entry for "Uralic peoples" *(uralilaiset kansat)* describes the reconstructible form of living of the Proto-Uralic speakers as can be deduced from the vocabulary: they lived from hunting and fishing, lived in tents *(kota)* and only knew one domesticated animal, the dog *(peni).* "It seems that some kind of reindeer husbandry was known; however, this seems to have been more like 'breeding for hunting', that is, reindeer were gathered and kept together for slaughter. Probably, fences were used as protection from predators and enemies (perhaps to keep the reindeer together?). Hunting was practised with bow *(jousi)* and arrows *(nuoli)* or blunt arrows *(vasama),* probably also with traps *(rita)* of some kind, primitive nets were used for fishing. Hunting was practised partly for animal skins *(talja),* partly for meat. Fire was lit by way of friction and it was used for cooking the food in pots *(pata).* The handicrafts consisted of plaiting *(puno-)* – probably mainly of tree bark, twigs and roots –, ...")

Linguistic evidence, thus, is used to draw conclusions about the everyday life and culture of the speakers of this reconstructed language, as in this example or as in the famous work of Benveniste (1969) – but the question whether there were other languages present in this life and culture simply does not arise. There have been only two exceptions to this. Firstly, the borrowing of vocabulary, especially cultural lexicon, has been investigated in connection with the issues of real or purported "cultural superiority" or "prestige", or in terms of what could be called the "prestige determinism hypothesis" (cf. Laakso 2013): borrowing of cultural vocabulary mainly takes place from a superior culture to an inferior one, which means that the feasibility of language contact

[1] Available online at http://runeberg.org/tieto/10/0140.html .

hypotheses has been evaluated in the light of what is known and what is believed about the cultural advancedness and prestige relationships of the populations at issue. And secondly, hypotheses of complete language shifts by whole speaker populations have been posed, in order to explain why racially "different" peoples can be linguistically related, as in the case of the Sámi (the "Proto-Lapp hypothesis", constructively reevaluated by Aikio 2004) or the Samoyed peoples in comparison to the Finns.

The latter question was dealt with by E. N. Setälä almost a hundred years ago (1915). (Setälä, perhaps the most politically influential Finnish linguist of all times, deserves to be mentioned also because in Finland, the celebration of the 150th anniversary of his birth is currently being prepared.) Note that Setälä sees language shift and borrowing as aspects of basically the same phenomenon, regarding language shift as a special case of "borrowing": all contact-induced change is "borrowing", and there is no fundamental distinction between borrowing, language acquisition and language shift, as everything leads to the same "normal state", which – if we can read between the lines – seems to mean monolingualism.

> Die übertragung einer sprache auf ein individuum ist ja nichts anderes als entlehnung im weitesten sinn des wortes: das kind entlehnt die sprache seiner nächsten umgebung, seiner eltern, seiner wärterinnen, seiner spielkameraden, jeder verbreitet seinerseits die sprache weiter, indem er anderen eindrücke vermittelt und selbst eindrücke empfängt. Auch besteht darin kein grundsätzlicher unterschied, ob die sprachform, welche andere verdrängt und auf neue individuen und individuengruppen übergeht, wesentlich der sprachform oder den sprachformen, die sie vernichtet, *gleich* oder von ihnen wesentlich *verschieden* ist. Wenn eine *gemeinsprache* wie etwa die griechische κοινή die anderen griechischen sprachformen verdrängt oder die französische gemeinsprache die patois vernichtet und an ihre stelle tritt, so ist das eigentlich prinzipiell dasselbe, wie wenn das vulgärlateinische die ursprünglichen volkssprachen verdrängte. (Setälä 1915: 9)

Moreover, even Setälä, while comparing large-scale language shifts (which he calls the "borrowing of a language") with more restricted borrowing, only briefly refers to bilingualism as a transitory stage (Der ersetzung der eigenen sprache durch eine fremde ist sicher immer eine allgemeine zweisprachigkeit vorangegangen; Setälä 1915: 12). His idea of bilingualism doesn't seem to reach as far as the reconstructed proto-language stages.

It may be that the traditional, almost unquestioned idea of monolingual and homogeneous proto-speaker communities reflects an unconscious projection of Romantic Nationalist ideologies on national history. (This, by the way, is the problem which Director Kenesei also referred to in the Hungarian context: people want to see their prehistory in terms of fixed entities, eternal nations...) What researchers in the golden age of Uralic linguistic fieldwork, in the years before World War I, saw, was the beginning or, in some cases, already a fairly advanced process of linguistic assimilation. They researched and observed languages which were threatened by the dominant language, usually Russian: the Russian colonisation, with its arrogant civil servants, greedy vodka-selling merchants and Orthodox priests, and modernisation, with its technical and industrial innovations, with its books and papers and schools (in the majority language), would put an end to the traditional way of living and the traditional

transmission of the language. In the best case, there were still remote villages in which monolingual speakers of the traditional language variety lived and maintained their traditional culture, untouched by the majority language, but sooner or later these would also be affected by the same development which the classic Uralic field linguists could witness with their own eyes: "The songs are not learned any more and not even listened to with pleasure, because people do not understand their language" (a statement quoted in the title of Widmer 1998).

At least partly true is the answer which I got from an old Khanty man: 'The Khanty have given up their old gods, they have stopped worshipping them, sacrificing to them, but they haven't been able to placate the Russian gods, this is why the Khanty people must die out.' The Khanty must die, because through the influence of the Russian culture they have become unable to accept this culture; they have lost their strength, their energy; the acknowledgment of their powerlessness, their imminent extinction has made them indifferent and apathetic, and 'God will not help him who will not help himself'. (K. F. Karjalainen, translation JL)

The classic authors of Finno-Ugric studies were inspired by Romantic Nationalism and its idea of the monolingual language-based identity as the very soul of ethnicity or nationhood. The multilingualism which they saw in their fieldwork among the Finno-Ugric minorities was a recent development (in contrast to the idealised past in which the whole community only used their own ethnolect), a consequence of colonisation, and quite obviously it was a transitory stage leading to ethnic assimilation and language loss. (Especially, as the ideas of language revitalisation and reversing language shift by way of socio-political processes would take more than 50 years to emerge.) Against this background, it is easy to understand why the pre-colonial past of the Uralic peoples in their view could only be a world of monolingual ethnic communities: only in a monolingual community could a language be maintained throughout millennia.

Furthermore, all language contacts investigated in the classical Finno-Ugric studies were either obviously recent – as in the case of Russian – or in any case related to the supposed migrations of the Uralic peoples from the primeval home. Contacts between Finnic (or Finnic-Sámi) and Indo-European (especially Germanic and Baltic), traditionally the most intensively studied area of Uralic contact linguistics, were first seen in the light of migration hypotheses, as something that took place in connection with the westward spread of Uralic. Moreover, these contacts had taken place with languages which were investigated in their own right: Indo-European (and partly Turkic) historical linguistics could offer points of comparison. Earlier reconstructed proto-languages, in contrast, did not have such obvious and well-known contact partners; besides, the issue of early contacts could not yet be completely separated from questions of Ural-Altaic or Indo-Uralic or Indo-Yukaghir relatedness which remained within the scope of legitimate historical Uralic studies even into the second half of the 20th century. This way of thinking is related to the idea which has been evident in the tradition of Finno-Ugric historical linguistics practically up to our days: it was thought that language, left to its own devices, is basically a very conservative thing, and change is always due to language contact – it is language contacts that caused each branching of the classical family tree.

A further factor contributing to the monolingual bias in historical linguistics was its strong connection with ethnic archaeology. The ethnolinguistic assumption, that is, the

national-romantic idea that each nation has its own language, identity, and culture, was already early on coupled with archaeologists' ideas of "cultures" or even "ethnogenesis". Languages were seen as organic parts of ethnic identities or even racially determinable groups, except for the rare occasions in which a complete language is "borrowed", that is, a racially "different" group takes over the language of another.

In the "ethnic archaeology", often connected especially with the name of the German archaeologist Gustav Kossinna and practiced especially in the young nation-states of Eastern Europe and in contexts in which political borders were changed or challenged (Ligi 1994: 112–114), features of material culture were connected with ethnic groups or "races". As the Estonian archaeologist Priit Ligi (1994: 115)[2] has pointed out, the autonomist identification "culture = race = language" became a self-supporting "interdisciplinary" system in which ideas from one discipline were used to support hypotheses in another discipline, and which typically favoured the ideas of continuity and large primeval homes, which in linguistic terms mean stable and conservative, autonomous proto-languages spread over large geographic areas. Ligi's example is the idea of the 5000 years of ethnolinguistic continuity in Estonia, a hypothesis or an ideological construction which in the post-WWII Sovietised Estonia served as an important building-block for national identity and self-esteem.

There were, thus, historical and ideological reasons to why the idea of multilingualism did not surface in the classical reconstruction of Finno-Ugric or Uralic protolanguages. In the 19th century, the heyday of evolution, family trees and holistic nationalism, historical linguistics played the leading role, which meant interpreting the prehistory of speaker communities in terms of migrations (the family tree was understood as a map of migrations, its branches as arrows showing the spread of the populations from the primeval home). Moreover, the greatest and politically most influential Uralic-speaking nation, the Hungarians, had an irrefutably migratory history: they had arrived from somewhere to their "promised land". In Uralic studies, this meant what Juha Janhunen (2001) has called the "conventional paradigm": reconstructing westward migrations from a primeval home somewhere in the East. In the 20th century, in contrast, the interdisciplinary paradigm was strongly influenced by ethnic archaeology – in Eastern European nation-states as well as, paradoxically enough, in the Soviet Union (where most Uralic peoples lived). Where linguists see migrations, archaeologists typically see continuities of settlement; this, in turn, led to the idea which for nations such as the Estonians was very attractive: our "nation" has "always been there".

However, it may be that the methodological reasons were even more powerful. The comparative method in historical linguistics presupposes language as a system which is complete and homogeneous, covering basically the whole range of human communicative and expressive needs. (This is plausible – we know that monolingualism, although curable, is possible.) This view on language means that multilingualism must be seen as a set of parallel monolingualisms, and the comparative method is basically restricted to one such system at a time. When reconstructing the sounds, the words or the structures of a hypothetical proto-language, what we do relates to the hypothetical sound system, the hypothetical lexicon or the hypothetical grammar of just one such

[2] Thanks to Santeri Junttila (p.c.) for drawing my attention to Ligi's paper; it appeared, in Estonian, in the literary-cultural journal *Looming* shortly before the author's untimely death and has probably not received all the attention it deserves.

system. Further systems, other languages, are something that we only resort to when all else fails.

For this reason, language contacts in traditional Uralic linguistics have typically been invoked only when there is unambiguous substance to support the hypothesis of a language contact, that is – in practice, almost exclusively – in loanword studies. Even here, it is traditionally held that an internal explanation is always to be preferred – for instance, in the case of competing etymologies, the internal one is considered better, other things being equal. In the area of morphosyntax, Honti ("Syntaktische Konstruktionen fremden Ursprungs: Eine skeptische Stellungnahme", 2007) has a few years ago critically analysed some purportedly contact-induced innovations in diverse Uralic languages; his conclusion seems to be that proponents of contact explanations do not have sufficient knowledge of the possible internal origins of these innovations but, instead, jump to conclusions whenever they see coincidental similarities form an areal pattern.

In the end of his paper, instead of a clearly formulated conclusion, Honti briefly states: "Außerdem hat Nelde (1992: 241) mit Recht festgestellt, dass Sprachkontaktforschung noch keine Methodologie hat." (Honti 2007: 66.) It is not quite clear what he means by this "lack of methodology", but presumably he wants to refer to the fact that historical linguistics is based on the idea of "normal transmission" in the terminology of Thomason & Kaufman (1988); the system is by default transmitted as a whole, and the burden of proof lies on those who want to claim that instead of transmitting the complete system, the speakers have chosen to replace parts of it with material from other sources.

Now Mailhammer (2013: 11) claims that this view is biased. What he calls the *Internal Development Bias* means that in traditional internal historical linguistics (which Honti's approach, among many others, represents) "normal transmission is equated with transmission in a quarantined community", that is, "normal" has been understood to mean "monolingual". However, in a speaker community where other languages and multilingualism are present, innovations can, in principle, equally well be based on material from other languages. (In fact, as I have tried to point out in a conference paper (Laakso 2013), departing from the idea that "humans are default infocopiers", as Henrich & Gil-White (2001: 175) put it, it is *in principle* possible that in a certain situation, certain foreign-based innovations might even be more probable and more plausible than inherited ones, if their sources are more salient, more accessible and more attractive.) Not surprisingly, this common-sense idea was formulated already by E. N. Setälä (1915: 7):

> Die sprache pflanzt sich ja nur so fort, dass sie sich im gesellschaftlichen leben vom menschen auf den menschen überträgt. Und damit verschwindet in wirklichkeit der prinzipielle unterschied zwischen verwandtschaft und entlehnung.

On the other hand, however, languages as separate systems do exist for multilingual speakers: multilingual exposure in childhood normally leads to multilingualism, *not* to the evolution of a mixed language, and multilingual speakers, even if their everyday language use is full of code-switching and code-mixing, are also able to keep the codes apart and stick to just one code, for instance, when speaking to monolinguals. I will come back to these questions in a moment.

From monolingual proto-speakers to prehistoric multilingualism

As already mentioned, the neglect of language contacts in historical Uralic studies – with the exception of loanword research as part of the research into the history of cultural contacts – stems from the traditions of interdisciplinary "ethnicising". Within this tradition, the geographic continuities identified by archaeologists were taken as evidence of stable and geographically wide primeval homes. In Uralic studies, this idea was perhaps most beautifully expressed in the thought cherished by many Finnish scholars in the late 20th century: that Proto-Uralic was spoken in a wide area of the forest zone of Northern Eurasia, and that the area of Proto-Finno-Ugric might have reached from the Volga region (or from the Urals, or even from Siberia) to the present-day Baltic countries and/or Fennoscandia. According to this theory, a Uralic (practically: Proto-Finno-Ugric) language form would have spread westward, together with the Neolithic so-called Combed Ware culture, perhaps already some 4000 to 6000 years ago. As Janhunen (2001: 32–35) points out, the "wide homeland" theory, which dominated in the Uralic studies at least in Finland and Estonia until the 1970s, 1980s or partly even later, is both linguistically and archaeologically very thinly founded – but, in the lack of unambiguous counterevidence, it has been accepted for instance by Sammallahti (1988) in his classical study quoted at the beginning of this talk.

At least since the turn of the millennium, the theory of large primeval homes has been giving way to new views, and one of the most important factors behind this paradigm shift seems to be substratum studies. As for substratum, there was an intermediate stage in research history in which so-called "revolutionary" views attracted attention especially outside academia: the theories of Kalevi Wiik, a versatile Finnish phonetician, phonologist and linguist (see especially Wiik 2002), and, to some extent at least, his congenial colleagues such as János Pusztay in Hungary and Ago Künnap in Estonia. The idea was basically to reduce almost all language change into language contacts, involving substratum effects resulting from large-scale language shifts. For instance, to put it crudely, "Germanic is Indo-European spoken by earlier speakers of Uralic with a Uralic accent", that is, the essential Germanic sound changes can be explained as substratum effects based on Uralic phonology. The idea of multilingualism was also involved, although in practice only marginally; it was thought that both Uralic and Indo-European originally spread across Europe as lingue franche of periglacial big-game hunters and of agriculturalists, respectively. The use of the term *lingua franca* seems to imply that these languages were used alongside the original language or language(s) of the populations at issue for some time. However, the circumstances and the process itself of language shift were never described in more detail nor supported with sociolinguistic or ethnographic data or parallels. Obviously, Wiik and his colleagues were not interested in multilingualism either; they, as well, saw it as only an inevitably transitory stage.

The ideas of Kalevi Wiik and his associates, also known as "the contact theory", have been harshly but justly criticised on various grounds (see, e.g., Janhunen 2001: 35-37; Kallio 2002, Palviainen 2003, Lindstedt 2004; the debate in Finnish media and scholarly circles has been comprehensively analysed by Tirkkonen 2012) and never won the acceptance of mainstream Uralic linguists. As many critics pointed out, Wiik's theories were both internally controversial and incompatible with what is generally known about the prehistory of the languages of Europe. From the point of view of this talk, the most important point is that the "big-game-hunter lingua franca" covering practically the whole Northern Europe is not a very plausible hypothesis. The pre-agricultural indigenous populations of the forest zones of Northern Eurasia and North America do

not seem to have known such immensely wide-spread lingue franche, nor are there any traces of such lingue franche replacing the diversity of small ethnolects (which largely persisted until the European colonisation). In addition to its many other flaws, the "contact theory" thus suffers from the same problem as the "wide homeland hypothesis": it is empirically unfounded.

Recent studies on Uralic and other substrata in Northeastern Europe (see, for instance, Carpelan & al. (eds.) 2001, Nuorluoto (ed.) 2006, Saarikivi 2006, Grünthal & Kallio (eds.) 2012) have made it clear that instead of just a few big building blocks, representing the proto-forms of today's language families spread over large areas, we should assume a high extent of prehistoric linguistic diversity, most of which has disappeared beyond record (see e.g. Aikio 2012: 101) although traces of it may have survived in toponyms or in etymologically obscure or irregular substrate words. This diversity has included both diverse Uralic varieties and numerous now extinct languages of other, probably unknown origin. The spread of Finno-Ugric language varieties into their present-day areas especially in the Northwest and North may be a process much more recent than the Finnish and Estonian mainstream of the late 20th century assumed (in addition to the articles in Grünthal & Kallio (eds.) 2012, see especially Kallio 2006). Instead of resembling today's nation-states or the latter-day spread zones of expansive populations such as the Nenets in the Eurasian Far North, the reconstructions are starting to look more and more like New Guinea, with many coexistent small hunter-gatherer populations speaking each their own ethnolect or many such ethnolects.

This diversity may have existed for millennia until it was for the most part covered by the spread of Uralic, Indo-European and Turkic languages. And it is highly probable that this diversity involved patterns of bi- or multilingualism, applied in trade relations or exogamy, as with some Siberian peoples almost until our days. (For instance, close contacts – obviously involving interethnic marriages – between speakers of Enets and Nganasan or Forest Enets and Tundra Nenets used to be frequent (Wagner-Nagy 2011: 9).) The question is now: what can we know about this prehistoric language diversity? Are the methods of historical linguistics so intimately connected with the monolingual bias, the idea of one language as a complete, autonomous system, that they are of no use for reconstructing diversity? And if it is so, what can we do?

Questioning "languageness" – and its diachronic implications

In recent decades, many linguists have begun to question the idea of "languageness" (Garner 2004), languages as fixed and clearly delimitable entities. In (post)modern urban or postcolonial contexts, there is a diversity of multilingual language use patterns intertwining with intralingual variation ("dialects" or closely related varieties which often challenge the concept of "language"), a reality which defies the ideology behind Western language policies: to see languages as numerable and distinguishable entities. As is widely known by linguists who work with closely related and/or internally diverse language varieties, for instance in the Finnic language area, not even speakers themselves always agree on the denominations and classifications of their languages as promoted by language activists or even as acknowledged in official language policies: Võro and Seto in Southern Estonia, officially dialects or "regional varieties" of Estonian, and Meänkieli in Sweden or Kven in Norway, officially recognised as minority languages but linguistically also classifiable as dialects of Finnish, are good examples of cases in which speakers themselves and local people may disagree and have diverse, diffuse or

conflicting opinions (see, e.g., Arola & al. [forthcoming], Koreinik 2011). Today's minorities also typically live amidst a diversity – or "superdiversity" (Vertovec 2007, Blommaert & Rampton 2011) – of both minority and majority language varieties, both standard and colloquial, and sometimes it is claimed that for them, minority and majority varieties, for instance Russian and Karelian may melt together into a mixed code, a "way of speaking" which for its speakers can "be approached from the same viewpoint as stylistic and/or register variation in monolingual situations" (Sarhimaa 1999: 306–307).

Some of the linguists who work with the multilingualism of today's minorities have ended up condemning the whole idea of language-as-a-system as "epistemic violence" (Makoni & Pennycook 2007: 16): "languages" are just learnèd constructions which were created and forced upon the diverse and heterogeneous speaker communities by nationalist, colonialist, Christian, Eurocentric regimes. Moreover, these constructions are still maintained by high-modern language policies (language testing, language planning and standardising, language education policies etc.) in order to control the superdiversity which can be perceived as threatening the still-strong nation-state ideology and its basis, the ethnolinguistic assumption (Blommaert &al. 2012). In this alternative view, the focus is – in the interests of emancipation and empowerment of speakers – transferred from language as a system to the individual speakers and their agency, their choices and actions as "languagers" who "language", or "polylanguage", that is, "language users employ whatever linguistic features are at their disposal with the intention of achieving their communicative aims" (Jørgensen 2008: 169-170), or in the words of Garner (2004: 212), multilingual code-switchers are not speaking "different languages" but "simply communicating in patterns that [are] familiar to them". This criticism of the ethnolinguistic assumption is connected to the important insight that people often have multiple and overlapping identities, that many multilingual language users cannot unambiguously define their "mother tongue" (which makes the concept of "native speaker" very problematic) nor identify themselves as members of one social group only (Rampton 1990: 107; Ansaldo 2010: 617).

If we assume that languages as institutionalised entities are a relatively recent development, brought forth by Western civilisation, nationalism and colonialism, we may be tempted to project the ideas of diffuse diversity into the prehistoric past. The attractive conclusion is that languages in prehistorical times were not distinct, contrasting units which their speakers perceived as strictly separate codes: there was a free lateral transmission of linguistic resources between what we from our perspective, distorted by Western nation-state ideologies, might want to call "languages". This hypothesis plays a prominent role in R. M. W. Dixon's (1997) "punctuated equilibrium" model: the idea is that in the "equilibrium" phase, in regions such as pre-colonial Australia, with the speakers of diverse languages sharing a fairly similar culture and coexisting in relatively peaceful terms, there were no major conflicts and especially no hierarchical relationships between the speaker communities. Thus, instead of languages spreading and ousting each other there was something like a free lateral transmission or unconstrained diffusion of substance and structures between languages. Only in the "punctuation" phase did something trigger a diversification of languages and a geographic spread which resulted in diachronic developments traditionally illustrated with the family tree model, that is, the split of languages into daughter languages.

Similarly, Ansaldo (2010: 621) assumes that of the two types of language ecologies described in the following figure, "Ecology A" has been "more common, more frequent and unmarked", as "Ecology B" in its extreme form "is the product of Western European nation states of the last 200 years and does not account for earlier human settlements in general, whether hunter gatherers or farming communities".

ECOLOGY A	ECOLOGY B
Speakers know more than 1 variety	Speakers are monolingual
Society allows mixing	Society supports purism
Language is transmitted informally	Language is transmitted through institutions
Language use is creative	Language use is normative

(Ansaldo 2010: 622)

While in Dixon's model, language change – that is, the change of language-as-a-system – as described and reconstructed in traditional historical linguistics is something that only applies for certain (exceptional?) phases in the history of mankind, Ansaldo takes a step further. Paradoxically, he arrives at a conclusion which is very similar to the ideas of many traditional historical linguists: "there is good reason to believe that language change almost always implies language contact" (l.cit.), that is, languages left to their own devices tend to be very conservative, while in a contact situation, "language creation" takes place and new varieties come into being by way of creative combination of language resources.

Dixon's punctuated equilibrium model has the fundamental problem – which he himself admits – that it is impossible to verify empirically, because the equilibrium stage can never be directly observed. The presence of Western linguists already implies that the paradise is lost, the conditions for equilibrium do not exist any more. Ansaldo, together with many others, joins the chorus of those who see language contact, grammatical diffusion and mixing (or even "language creation as active differentiation from neighbouring groups") as an important or even the primary driving force of language change and thus, more or less directly, attack the traditional language tree model (see e.g. Campbell 2006: 18–19). In this view, languages do not come into being by way of splitting (proto-languages giving rise to daughter languages, as in the case of (Vulgar) Latin and today's Romance languages) and continuous change (languages gradually morphing into something almost completely different: Ancient Greek into Modern Greek, or Proto-Hungarian into Modern Hungarian) but by way of mixture, merger and socially conditioned "language creation". This alternative view completely forgets the reality of internal diversification which in some parts of the world is still taking place without any visible external impetus (see e.g. the detailed account of diversification in Northern Vanuatu by François 2011).

I will not deal with the problems of these approaches in more detail but merely refer to expert criticism as presented, for instance, by Helimski (2001: 195: "The lengthy reflections on how non-cognate languages could have been transformed into a language

family due to prolonged and intensive contacts are a poor substitute for examples that are lacking.") or Campbell (2006: 18–21: "Because the methods [= of historical-comparative linguistics] have been successful in so many cases, we do not abandon them just because the extant evidence in some specific instance is insufficient, just as we do not conclude that a vehicle can never take us anywhere just because on one occasion the petrol ran out."). The supposed merger or mixture developments are typical of certain special situations only and cannot be used as a general model of language contact and change. Above all, it must be noted that Ansaldo's material is not representative of premodern conditions. The example cases he gives are Sri Lanka Malay, the Peranakan or Baba community in Singapore, and the Macanese community descending from Portuguese, Asian-Portuguese and Southern Chinese populations. All of these are connected to processes of European (and, partly, Chinese) colonisation, involving stratified and technologically advanced societies, interethnic hierarchies and institutionalised power relations. Processes of this type have given rise to other famous "mixed marriage languages" as well, such as Michif or Copper Island Aleut, and as Croft (2000: 214–217) notes, these are extreme cases resulting from a fundamental reorganisation of social structures. Thus, this kind of "mixing" or "merger" is probably fundamentally different from what we can assume for the hunter-gatherer populations in prehistoric Northern Eurasia.

Moreover, even in contexts where multilingualism and language alternation are considered normal, speakers themselves have ideas and opinions about languages, and they ascribe certain features and functions to certain languages, or codes, or whatever we want to call them. To use Jørgensen's (2008: 166-167) example, for Turkish-Danish bilingual immigrants in Denmark, switching from Danish to Turkish in

jeg har ikke nogen saks, hvor er saksen, makas ver
[Danish:] I don't have scissors, where are the scissors, [Turkish:] give me the scissors

signalises friendliness and solidarity, while switching the order of languages as in

makasım, makas nerede, giv mig en saks

might give the utterance the opposite function: signalising authority and distance. So, although the speakers in a situation where the so-called polylingual norm prevails may "freely" switch between Danish and Turkish, elements stemming from these two languages may still have different functional values which can be consciously used in communication. Although rules and regularities of code-switching, code alternation or polylingual communication are difficult to determine (as we know from the long tradition of code-switching research and the numerous theoretical frameworks suggested for its description), it is obvious that code-switching has communicative functions – which, again, means that even for multilingual speakers and speech communities, languages or codes as distinct entities do exist.

Furthermore, it seems that discussions in which the concept of languageness (Garner 2004) or language-as-a-system is questioned sometimes confuse two things. The coexistence of closely related varieties or the internal variation in any language, which Rudolf de Cillia (2010) has called "internal multilingualism" *(innere Mehrsprachigkeit)* – even monolinguals normally master different registers or varieties of their mother

tongue, sometimes even deeply different dialects – is a phenomenon of a different level than the coexistence of unrelated or distantly related languages. In the former case, for instance between Võro and Standard Estonian, or Meänkieli and Standard Finnish, not only typological and structural similarities but identification in large parts of grammar and lexicon, i.e. mutual intelligibility, makes true hybridisation, etymological nativisation (adaptation of loanwords to the model of true cognates, as in the case of Sámi and Finnic, cf. Aikio 2007) and transmission of large parts of structure and substance possible. Speakers realise this and may apply, for instance, shared patterns of word formation to create "possible words", such as Finnish *työttää* 'to work', based on the model of Estonian *töötama. Työttää* does not appear in modern Finnish dictionaries, but it has been attested with Finnish-Estonian bilinguals, and like its Estonian equivalent, it is, in principle, a valid causative derivative from the noun *työ* 'work' (it is just that in Finnish, its use seems to be blocked by the competing derivative *työskennellä* 'to work, to be at work, to be employed'). What I mean is that *työttää* for bilingual Finnish-Estonian speakers is not just a word borrowed from Estonian, it is a potential Finnish word which is now realised with the sisterly support of Estonian. This kind of hybrid words with so-to-say legitimate dual affiliation do not necessarily appear in cases where the contacting languages are not closely related.

What can we know about Proto-Uralic multilingualism?

It seems that we have come back to where we started. The methods of comparative historical linguistics seem to be the only reliable way of getting information of what may have happened in the linguistic world and language use of prehistoric hunter-gatherers in Northern Eurasia. And these methods are essentially based on an idealised monolingual system. The comparative method does not give us any means to reconstruct linguistic diversity which has been lost, be it dialectal divergence or the presence of languages which have disappeared beyond record.

However, we have good reason to assume that the world we are attempting to reconstruct was multilingual and characterised by both convergence and divergence of small ethnolects, some of them probably conspicuously related and hybridisable, some unrelated and mutually unintelligible. (But even if all these ethnolects in the region were related, they might get diversified with time; as François (2011: 235) puts it, "where linguistic diversity does not exist, it can be invented.") It seems probable that a major part of this multilingualism was acquired in early childhood in families and small communities which were multilingual due to interethnic marriage patterns, but the possibility of interethnic lingue franche, pidgins or trade jargons which were learnt by adults (and used in certain institutionalised contexts?) cannot be ruled out either.

And if the languages, Uralic and other, were mostly acquired in early childhood and mastered on the notorious "native-speaker level", there will be no such typically simplificatory changes which we could identify – or distinguish from the generally simplifying effect of our reconstruction method. Künnap (1999) attempted to interpret the reconstruction of Proto-Uralic by Janhunen (1982) as reflecting the "deficient" character of the proto-language, as it would fit in with his (or Kalevi Wiik's) idea of the proto-language as something like a pidgin used (originally?) in interethnic communication. Sadly enough (cf. Laakso 1999), it's not that easy: the simplicity of the reconstruction is simply due to the reconstruction method and the fact that time

consumes information, making reconstructions the more simple and the vocabularies the more deficient, the bigger the time depth.

> 'The poverty of the reconstructed Proto-Uralic might, in my opinion, clearly indicate that the result of the reconstruction is not a living, complete language which has existed at some time but merely a deficient auxiliary language, a *lingua franca.* I consider Juha Janhunen's reconstruction of Proto-Uralic (1981, 1982) relatively good; it reflects a minimalist grammatical and lexical material which can be reconstructed for the hypothetical Proto-Uralic by very rigorous criteria.' (Künnap 1999: 422; translation JL)

Furthermore, not only is there still very little serious in-depth research of processes of language change in bi- or multilingual communities, as Ringe and Eska (2013: 64–65) point out. It also seems that in language contact situations, to put it crudely, almost anything can happen or not happen. For instance, there are contact situations in which words are easily borrowed even between unrelated and typologically different languages, and there are languages or speaker communities which resist lexical borrowing and are only susceptible to structural convergence or not even that (for examples see e.g. Foley 1997: 385–386, Thomason 2001: 236).

> Montana Salish is an exception. With fewer than sixty fluent speakers remaining, the language is seriously endangered, and will die soon if the current preservation efforts fail to produce a new generation of speakers. But in spite of a hundred and fifty years of increasingly intense contact with, and pressure from, English, Montana Salish has borrowed almost nothing from English – only a handful of words, and no structure at all. The Montana Salish people have acculturated to English-language culture to a great extent, but instead of borrowing English words for new objects, they invent new words out of Salish morphemes; we saw one example in Chapter 1. Nor has Montana Salish undergone extensive attrition. It has lost some lexical domains as a result of culture change, as noted above; but these losses are not obviously more extensive than losses of lexical domains in fully viable languages such as English. (Thomason 2001: 236)

In fact, it might even be that hunter-gatherer populations are typically characterised by such conservativity; Bowern & al. (2011) conducted a statistical comparison of loanword rates in a sample of languages of hunter-gatherer populations on three continents and found no support for the claims for "equilibrium" or "free lateral transmission"; despite high variation in demographic, social and ecological conditions, loan rates were generally low, and it seems that local factors are more crucial than general sociocultural factors or types of livelihood. So, contrary to Ansaldo's (2010: 621) claims that linguistic purism and monolingual norms are a product of modern Western nation-state ideologies, the "tendency to keep languages distinct", which Bowern & al. (2011: 5) have noticed especially in Amazonia (and, interestingly, in connection with exogamous marriage patterns!), may well have prevailed also in prehistorical Northern Eurasia.

One promising avenue of research might be opened by substratum studies in etymology and toponym research. Here, Uralicists such as Saarikivi (2006) or Aikio (2004, 2012, etc.) have developed methodological tools for detecting traces of extinct Uralic or non-

Uralic languages in Fennoscandia and Northern Russia. In the best case, the material accumulating from toponyms or possibly substrate-origin vocabulary could perhaps be systematically analysed to reveal features of extinct languages, in the same way as some Indo-Europeanists have tried to identify or even partly reconstruct Palaeo-European substrate languages (Schrijver 2001). A parallel avenue of research is opened by the research into possible internal loans between early Uralic varieties, some of which may, of course, be extinct. A more rigorous etymological analysis might reveal part of phonological irregularities as resulting from internal borrowing (see, in particular, Gábor Bereczki's work on Mari-Permic contacts; e.g. Bereczki 2005 with further references) or etymological nativisation of loanwords (Ringe & Eska 2013: 61), a phenomenon which is already relatively well known and investigated in the framework of Finnic-Sámi contacts (Aikio 2007) but would deserve similar examinations in other possible intra-Uralic contacts as well. And finally, of course, the early contacts with Indo-European – or, possibly, so-called "Altaic" – languages will probably need reappraisals in the future as well, connected as they are with the location and dating of the proto-languages, an issue in which there is, obviously, a paradigm shift taking place.

This, of course, would only mean the first step: identifying the presence of other languages, of which loanwords, of course, would provide the most uncontroversial evidence. The following step could be drawing conclusions from the quantity and quality of the identified loanwords: do they reflect a close contact in which bilingualism was frequent, or more superficial contacts with just a few bilingual key individuals? Interestingly, in the history of Uralic studies even in the case of well-known language contacts there are either no attempts at describing a scenario of "what happened really" (surprisingly enough, this seems to be the case with the Finnish-Swedish language contacts), or there are deep differences of opinion: Klára Sándor (2011: 244, 274) claims that a considerable part, probably even the majority of the pre-conquest Hungarians were bilingual in Turkic, while Klára Agyagási (2013; my translation) emphasises that in the early contacts with Turkic "the Finno-Ugric- and Ugric-based code of the Hungarian language was not damaged (!), whereby the presence of bilingualism in certain parts of the society during the contacts cannot be excluded". And this, although in principle all serious linguists agree that there is a substantial number of Turkic loanwords in Hungarian and that the Turkic languages and cultures had a significant influence on the pre-conquest Hungarians. Again, keeping in mind the cases in which even an intensive and well-documented language contact with omnipresent bilingualism does not lead to a massive influx of loanwords, this shouldn't surprise us. In fact, the mere quantity of loanwords might be a less good indicator of the presence of bilingualism.

Another possibility would be to attempt a qualitative and semantic analysis of possible loanwords – once they are identified or reevaluated. In the abstract to this talk (in which I hoped to be able to deal with numerous interesting issues for which I now don't have any time), I mentioned the possible lexical reflections of cultural traits which might indicate systematic lexical borrowing connected to certain intercultural practices, for instance, bilingual parenting in interethnic marriages. If the observation of Kulonen (1999) really holds true and the Finno-Ugric words for women are typically loanwords from Indo-European more often than the corresponding words for men, this might indicate a systematic pattern of exogamy. (Unless, of course, the greater borrowability of words for women can be explained by the fact that the words especially for young, marriageable women, girls, dolls, chicks, whatever they are called in various languages, simply form a typical lexical attraction centre.)

Lexical evidence, however, has its restrictions. Words are replaced, or their meanings change together with the cultural practices they refer to. In fact, many contact-linguistic studies point out that contact influences are more reliably reflected in morphosyntax, because it is less salient and more prone to unconscious change: "divergence typically affects word forms, whereas convergence mostly takes place between linguistic structures" (François 2011: 176), or, as Bowern & al. (2011: 3) note, in the Vaupès region of the Amazonas, among peoples practising regular exogamy, "cultural attitudes condemning language mixing impede lexical borrowing and code-switching, but do not appear to be a significant obstacle to grammatical diffusion." Words or phonological features are probably more readily connected to ethnic or regional identity, and they may be easier to adopt – and easier to lose – than morphosyntactic features.

Detecting traces of earlier multilingualism might basically be more possible or more uncontroversial in morphosyntax. Here, however, there are three almost insuperable obstacles. First of all, changes in morphosyntax typically have many competing explanations, both internal and external, or they may involve multiple causation. Secondly, for this reason, if the donor language of a morphosyntactic innovation is vanished beyond record, we most probably have no means of identifying this innovation as a contact-induced one. Thirdly, the reconstruction of Uralic morphosyntax has still too many white spots, or rather, the reconstructions presented in literature are probably both too speculative and too generalising that irregularities or typological anomalies which might indicate contact-induced changes could be spotted. Still, I believe that the possible traces of multilingualism are like the proverbial salmon in Finnish: so valuable that it is worth while to fish for them even at the risk of not catching any.

Instead of a proper conclusion, I would like to end this talk with a historical paradox. Language was once used as the main building block for reconstructions of ethnolinguistic processes based on the ethnolinguistic assumption, i.e. operating with language-based but "interdisciplinary" ethnocultural entities and, in larger time-depth, connecting them into genetically related complexes, language families. Now paradoxically, giving up the ethnolinguistic "macroculture" assumption (and admitting that there can be many languages and patterns of multilingualism within one "culture") means, in fact, that language becomes the carrier and criterion of diversity. Autonomist historical linguistics was used for lumping, sociolinguistically sensitive ("connectionist") historical linguistics could be used for splitting?

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