

# ETYMOLOGICAL NOTES ON ALEUT (III)\*

## With methodological notes on (Eskimo-Aleut) historical linguistics

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### Abstract

*The goal of this contribution is twofold: on the one hand, to review two relatively recent contributions in the field of Eskimo-Aleut historical linguistics in which it is proposed that Eskimo-Aleut languages are related genealogically to Wakashan (Holst 2004) and/or Nostratic (Krougly-Enke 2008). These contributions can be characterized by saying that their authors have taken little care to be diligent and responsible in the application of the comparative method, and that their familiarity with the languages involved is insufficient. Eskimo-Aleut languages belong to a very exclusive group of language families that have been (and still are) used, sometimes compulsively, in the business of so-called “long-range comparisons”. Those carrying out such studies are very often unaware of the most basic facts regarding the philological and linguistic traditions of those languages, as a result of what mountains of very low quality works with almost no-relevancy for the specialist grow every year to the desperation of the scientific community, whose attitude toward them ranges from the most profound indifference to the toughest (and most explicit) critical tone. Since Basque also belongs to this group of “compare-with-everything-you-come-across” languages, it is my intention to provide the Basque readership with a sort of “pedagogical case” to show that little known languages, far from underrepresented in the field, already have a very long tradition in historical and comparative linguistics, i.e. nobody can approach them without previous acquaintance with the materials.*

*Studies dealing with the methodological inappropriateness of the Moscow School’s Nostratic hypothesis or the incorrectness of many of the proposed new taxonomic Amerindian subfamilies (several of them involving the aforementioned Wakashan languages), that is to say, the frameworks on which Krougly-Enke and Holst work, respectively, are plenty (i.a. Campbell 1997: 260-329, Campbell & Poser 2008: 234-96), therefore there is no reason to insist once more on the very same point. This is the reason why I will not discuss per se Eskimo-Aleut–Wakashan or Eskimo-Aleut–Nostratic. On the contrary, I will focus attention upon very concrete aspects of Krougly-Enke and Holst’s proposals, i.e. when they work on “less ambitious” problems, for example, dealing with the minutiae of internal facts or analyzing certain words from the sole perspective of Eskimo-Aleut ma-*

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*materials (in other words, those cases in which even they do not invoke the ad hoc help of Nostratic stuff). I will try to explain why some of their proposals are wrong, demonstrate where the problem lies, and fix it if possible. In doing so, I will propose new etymologies in an attempt at showing how we may proceed. The main difference between this and handbook examples lies in the reality of what we are doing: this is a pure etymological exercise from beginning to end. I will try to throw a bit of light on a couple of problematic questions regarding Aleut historical phonology, demonstrating how much work should be done at the lowest level of the Eskimo-Aleut pyramid; it is technically impossible to reach the peak of the pyramid without having completed the base. As far as Aleut is regarded, I will mainly profit not only from the use of the traditional philological analysis of Aleut (and, eventually, of Eskimo) materials, but also of diachronic typology, bringing into discussion what in my opinion seems useful, and in some cases I think decisive, parallels.*

*It is worth noting that this paper makes up yet another part of a series of exploratory works dealing with etymological aspects of the reconstruction of Proto-Eskimo-Aleut, with special emphasis on Aleut (vid. i.a. Alonso de la Fuente 2006/2007, 2008a, 2008b, 2010a), whose main goal is to become the solid basis for an etymological dictionary of the Aleut language, currently in progress.*

## 1. Introduction

If we, researchers of minority (family) languages, whether they are isolated (Basque, Ainu, Gilyak, Zuni, &c.) or they are not (Eskimo-Aleut, Yeniseian,<sup>1</sup> Wakashan, Pama-Nyungan, &c.), put ourselves for a moment in the skin of an outsider and, from this perspective, we attempt to read and truly understand certain papers ascribed to the “long-range comparison” trend dealing with (pre-)historical facts regarding the languages we are working on, we would probably think “such a easy stuff, I can also do it! where can I find one of these languages?”. However, when we, researchers, read the very same papers, we soon realize that their authors treat every aspect of the philological analysis of the materials rather lightly, sometimes to the point of serious irresponsibility. Although these authors can commit the same mistakes when approaching language families that are supposedly are better known (e.g. Uralic, Semitic, Indo-European), the first impression is that doing this with minority languages is even legitimate, maybe based on a reasoning process like the following: «since they are “minority” languages, maybe the tradition is also “minor”, thus we can propose everything we want, because nothing has been done in this respect before, we are pioneers and as pioneers we are free of being condemned as the authors of serious misunderstandings». Of course, this hypothetical statement has been never explicitly articulated like that, to the best of my knowledge. At any rate, if there is a grain of truth behind it, and I am pretty sure that in some cases it is so, then we, re-

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<sup>1</sup> On the isolate nature of Ket, the only surviving language of the Yeniseian family, Tamboltsev (2008: 81) comments that “[...] the new language family —Yeniseyan (sic!)— has been invented. Nevertheless, it is not a solution of the problem.” Regrettably, Tamboltsev does not elaborate this statement further, so we cannot know what he means by “invented”. In any case, I think it is very unfortunate, and on the basis of Krejnovič, Starostin, Vajda, Werner or Georg’s works, I will consider here that Yeniseian to be actually a well-established language family and not just an “invention”.

searchers of those languages, should not keep quiet, because the outsider will think that those languages, just because they are labeled as minority languages, actually have no tradition, i.e. no previous philological studies, and therefore it is naturally unnecessary to be acquainted with something that does not exist. Basque is a perfect example (*vid. i.a.* Trask 1992): long-rangers, even when wishing to solve a “minor” problem, fail to grasp the most elemental principles of the philological discipline and mess everything up. Many authors consider it enough to quote the outdated and long superseded Azkue trilingual dictionary (1905-6), without even bothering to know what real, or potential advancements have been achieved in the field by other researchers, especially for example K. Mitxelena, L. Trask, or J. Lakarra.

All this holds true also for Eskimo-Aleut historical linguistics, a typical case in point of an underestimated research field, despite the fact that the specialist in this discipline has every tool for its correct practice, i.e. comparative grammars and dictionaries, dialectal atlases, historical approaches to individual languages, &c. It goes without saying that there is no direct relation between having access to these tools and the correct practice of the discipline. More than just a good library is needed to produce well-argued papers on etymology. Be that as it may, for the time being one can conclude that authors of “long-range comparisons” not only lack any familiarity with Malkiel’s basic concept of “*l’histoire du problème*”, but they are also unaware of what has been done most recently; they just do what they think is best for their own interests: to demonstrate extremely speculative genealogical links between languages of the most varied origins. Unfortunately the recent publication of an introductory handbook to Eskimo-Aleut linguistics (Holst 2004) as well as one attempt at linking these languages to the highly speculative Nostratic macrophilum (in its Moscow school’s formulation) demonstrate even more clearly how feeble the approach toward an understanding of the (pre-)history of these languages by certain researchers is and how necessary serious and profound studies on different aspects of the Proto-Eskimo-Aleut reconstruction are. This is especially true of Holst, whose work is conceived as an introductory study to the discipline.

## 2. Generalia. Re-addressing Proto-Eskimo-Aleut common lexicon

2.1. More than two decades ago, Bergsland noted that there is a great deal of common lexicon shared by both Eskimo and Aleut. In his own words, “[...] the difference between Eskimo and Aleut is not much greater than the difference between Baltic Finnish and Saami” (1986: 130-1). Since this fact is well-known, almost evident at first sight, it is entirely understandable that Holst had prepared the Swadesh’s 100-word list with Eskimo(-Aleut) materials for his introduction to the linguistics of these languages (2004: 172-6 [= historical Eskimo-Aleut languages], 209-11 [= Proto-Eskimo]). However, the way in which Holst has undertaken the task is most regrettable. To begin with, Holst has not adapted the classic Swadesh’s 100-word list to the requirements of the Eskimo-Aleut cultural environment, as it is now customary (for a recent illustrative example, *vid. i.a.* Janhunen, Peltomaa, Sandman, and Dongzhou 2008: 116-27). Therefore, many words seem to be absent from the list, or badly represented, not in fact because Eskimo-Aleut languages lack the given word, but because they lack the exact match for the cultural concept the word de-

finer. For example, unless the researcher specifies the kind of ‘bark’ (of a tree, fish skin, scale, etc.) he or she wants to find a match for, it is technically impossible to select one, given the importance, and then the specialization, of this element in the material culture of the Eskimos. A similar critique can be made about ‘stone’: it is true that a generic word for stone does not exist in Eskimo and Aleut languages, but it is very hard to believe that there are no words meaning ‘a k. of stone’, ‘rock’, etc. Thus, lexical specialization accounts for such “lack”. The same holds true for [41] ‘horn’, [67] ‘road (path)’, [68] ‘root’, [74] ‘sit’, or [73] ‘seed’ (in the following paragraphs I will supply the item number between brackets according to Holst’s numeration). Had Holst taken into consideration this “little” detail, he would have found many more items for his Swadesh’s 100-word table. For example, Holst seems to claim that there is no common Eskimo word for [3] ‘bark’ (p. 209), but there is actually at least one: PE *\*/amirar/* ‘bark or fish skin’ (CED 23b: CAY & GRI *amirraq*, probably Sir *aminraX* ‘skin prepared for use on hull of skin boat, newly covered boat’), a transparent derivative of *\*/amir/* ‘skin’. Instead, Holst quotes one of the Inuit cognates associated to this item, namely “Siglitun” (= western dialect of Western Canadian Inuit) *amirraq* ‘bark, velvet on caribou antlers’, with Proto-Yupik-Sirenik *\*/qaltə/* ‘bark or scale’ (CED 280a: AAY *qalta*, CAY *qətta* ‘bark, peeling’), and Eastern Aleut *ukalax* ‘bark (of a tree)’ (AD 428a). Even if it is accepted that this equation is based solely on the distribution of synchronic meanings (“Siglitun” and historical continuations of Proto-Yupik-Sirenik mean all only ‘bark’, in opposition to the semantic variety observed among historical continuations of PE *\*/amirar/*), it is obvious that diachronic issues must be taken into account when making cultural inferences, especially when those are actually the result of applying (inappropriately!) linguistic tools as is the case here with the Swadesh’s list.

There is another conceptual problem in Holst’s presentation of the materials in relation to his definition of P[roto-]E[skimo] and P[roto-]E[skimo-]A[leut]. One wonders why the second of Holst’s Swadesh’s word lists is restricted to PE and does not cover the entire PEA area. A possible explanation is that Holst wants to note reconstructions and CED only provides PE reconstructions with the occasional quoting of Aleut cognates (mainly after Bergsland’s work). But Holst pretends to use Swadesh’s word list to show the degree of relationship existing between Eskimo-Aleut languages. If one takes into consideration the many problems the Swadesh’s word list poses in this respect, it is better not to consider what would happen in the event of restricting materials! The result is, obviously, an extremely distorted view of PEA as far as its lexicon is regarded. The very question of when to postulate a reconstruction and to what stage it must be ascribed is actually a serious conceptual problem for the outsider when dealing with the *res Esquimoaleutica*. It must be stated that the tradition of labeling a reconstructed form as “PEA” only if its historical continuations are attested in both Eskimo and Aleut is a severe misunderstanding of the comparative method.<sup>2</sup> Most Eskimologists accept today that Eskimo and Aleut

<sup>2</sup> The following abbreviations will be used throughout this paper: A(A) = Atkan (Aleut), AAY = Alutiiq Alaskan Yupik, AD = Bergsland (2001<sup>2</sup>), AG = Bergsland (1997), Au = Attuan (Aleut), CAY = Central Alaskan Yupik, CED = Fortescue, Jacobson, and Kaplan (1994), CSY = Central Siberian Yupik, Ea = Eastern (Aleut) dialects, ECI = Eastern Canadian Inuit, GRI = (West) Greenlandic, I = (Inupiaq-)

could be the remnants of a much larger prehistoric continuum, and that the “split” may reflect the loss of intermediate forms (Krauss 1980: 7-8, Woodbury 1984: 62, Bergsland 1986: 131, Berge 2010). Similarly, in the field of Indo-European comparative linguistics, nobody would refrain from labelling “Proto-Indo-European” those words attested in every historical branch, but not in Anatolian and/or Tocharian. This false impression on the part of Holst may be due to the influence of Uralic linguistics, a field where specialists (at least some of them) keep operating in similar ways: the label “Proto-Uralic” means that the material upon which the reconstruction is based represents both Finno-Ugric and Samoyedic, otherwise the reconstruction must be labeled Proto-Finno-Ugric or Proto-Samoyedic. In addition, historical forms may have undergone many different kinds of changes, so very often Aleut cognates just do not reflect anymore the very same primary meaning of the Eskimo pair, and *vice versa* (see *infra* for the discussion of other problems and examples). On many occasions it may be the case that we only have Yupik, or Inuit (but not both at the same time) cognates for one Aleut form. Thus, if we assume only the PE level, with no place for Aleut, we may generate the impression that a given concept is the result of Yupik or Inuit innovation, when the reality can be wholly the opposite (see *infra* [10] ‘bone’).

On the other hand, sticking to the Swadesh’s word list can actually have a very negative effect from a pedagogical viewpoint, as many other “common” lexemes potentially of much more relevance may be overlooked. For example, Holst argues that there is no common PEA term for ‘nose’. Strictly speaking, that is true. However, the Aleut word *any.-usi-x̣* ‘nose’ does not stand alone: it is related to an entire net of PEA derivatives involving words like CAY *anərnəq* ‘soul, ghost, life’ < PE \*/anər-nəq/ ← PEA verb \*/anər(-)/ ‘life, spirit; to breathe (out)’ (CED 28a) > PA \*/ánər(-)/ > \*/any.(-)/ → *any.-i-x̣* ‘ghost, spirit, voice’, *any.-* ‘to breathe’ → *any.-usi-x̣* ‘nose’ (AD 73b-76b). So, it is true that there is no common word for ‘nose’, but the concept of ‘life, ghost, spirit’ seems to be present in the deepest roots of Eskimo-Aleut people and its etymological aspect of it raises many interesting questions that otherwise remain untreated: keeping in mind that we are discussing about the appropriateness of an introductory handbook, one cannot afford to pay such a price. It goes without saying that this is of course one of the many inherent problems of the Swadesh’s word list: including or excluding—it depends on the case—one derivative after semantic specialization in one language may make an etymology affecting the entire language family vanish. Holst writes an introductory handbook, and I do not know whether such lost chances for beginners can be compensated in any other way. The point here is that any etymo-

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Inuit, NAI = North Alaskan Inuit, NSY = Naukanski Siberian Yupik, PA = Proto-Aleut, PE = Proto-Eskimo, PEA = Proto-Eskimo-Aleut, PIE = Proto-Indo-European, Sir = Sireniki, SPI = Seward Peninsula Inuit, Y = Yupik.

Though an official writing system was designed for Aleut in 1972 by Bergsland (see AD xvi-xxiv), this paper will use the orthographical conventions in Bergsland (1986: 66-7), the main differences being that the aspirated consonants are written with capital letters instead of combinations of h + C, e.g. <W> and not <hw>; the uvular and velar fricatives are written <x̣>, <ɣ̣>, and <ɣ̣.̣>, instead of <x̣̄>, <g̣>, and <ḡ̣> respectively; the voiced dental fricative is written <ð̣> instead of <ḍ> (conventionally this sound is written <ṛ̌> or even <ð̣̄>, but for the sake of clarity we will adopt the same orthography as in Aleut); the palatal stop is written <č̣> instead of <cḥ>; and the voiced velar nasal is written <ŋ̣> instead of <ng̣>.

logical exercise must be done taking into account all the possible perspectives, and not force the materials into molds that may offer a rather untrue picture of the whole. This is especially relevant here, for as mentioned Holst pretends to use the Swadesh's word list to show the degree of relationship existing between Eskimo-Aleut languages. Even from a quantitative point of view, Holst's decision about leaving out 'breath' to include 'nose' is unfair, and if his intention is to show the degree of relationship of these languages, the best way is to go to phonology and morphology. Regrettably, Holst does not touch upon these aspects of the PEA reconstruction (only phonology, but very superficially and always from the (Proto-)Eskimo viewpoint).

Be that as it may, in the following list I will provide new etymologies for some of the "blanks" in Holst's list or simply make a few comments on certain claims by Holst about the reliability of materials for a given lexical item in the Swadesh's word list.

- [10] 'bone': generic words for 'bone' are attested independently in Yupik, Inuit and Aleut, but according to Holst there is not even one word which could be traced back to PEA times. This situation is awkward to say the least. Bergsland proposed that A *quðγ-(u)ma-* 'shinbone, tibia' (AD 330b) may be related to PI *\*/qu(C)ək/* 'narrow bone in hind flipper of seal or walrus' (CED 312b: GRI *quuik* 'id'). The specialization of both terms should not be an impediment to postulate a PEA *\*/quðəγ/* 'a k. of bone' (via PA *\*/qúðəγ/* > *\*/quðγ-/* to the historical forms) and assume that it was lost or replaced in Yupik by a more convenient term. It must be noted that (C) in the Inuit reconstruction stands for the "velar dropping", i.e. the implicational emptied space left by one of the lenited velar consonants in intervocalic position, namely *\*/r η γ/* (see general statement in CED xvii/a), usually preserved in the Yupik branch as well as in Aleut. However, PEA *\*/ð/* is also dropped in Inuit when the word has sequences of the sort full vowel (i.e. *\*/a i u/*) + reduced vowel *\*/ə/* (CED xvi/b).
- [22] 'earth': the case of the word for 'earth' is probably the most surprising of Holst's claims. One cannot help thinking how it is possible that Holst missed one of the most classic examples of PEA etymology, namely *\*/nuna/* 'land, earth, place' > PE *\*/nuna/* (CED 240a: CAY & GRI *nuna* 'id') and PA *\*/tana(-r)/* 'land, island' > A+E+Au *tana-x̣* (AD 388b), already noted in the pioneering studies by Marsh and Swadesh after identifying systematic instances with the sound correspondence PE *\*/n-/* : (P)A *\*/t-/*, and later refined by Bergsland (1986: 78-81). A possible explanation may lie in the fact that Holst considered that 'earth' and 'land' (this is usually the first meaning given in Eskimo and Aleut dictionaries) actually belong to different semantic entities.
- [26] 'fat (grease)': although it is true that no word with a "general" meaning for 'fat' can be postulated for PEA, other concepts of similar (or superior cultural) value are perfectly available, e.g. PEA *\*/əγə-/* 'render oil from blubber' > PE *\*/əγə-/* 'id.' (CED 99a: CAY *əγə-*, GRI *iyi-*, Sir *əγυə-* 'id.') and PA *\*/əγə-γna-/* > A *iyiγna-x̣* 'fermented seal blubber' (AD 178b), with second-



ary epenthetic vowel /i/ after adding the postbase *-yna(-)*. Moreover, many EA words contains ‘fat’ as an important qualifier, e.g. ‘remove fat layer’, ‘fat around neck of sea lion bull’, ‘fat (tallow)’, ‘fat floating on surface’, &c. Of course, here we are here dealing once again with a case of semantic specialization. Knowing, as Holst surely knows, how important fat is not only in Eskimo-Aleut societies, but rather in many cultures throughout the Eurasian zone, it is rather naïve to expect to find a general word for it.

- [27] ‘feather’: also well-known among Eskimologists, the relation between PEA *\*/mələ/* ‘fur, feather, body hair’  $\rightarrow$  PE *\*/məl-qur/* ‘id.’ (CED 197b s.v. CAY *məłqur*, Sir *məłqəX*, GRI *miqquq* ‘id.’) and A *imli-x* ‘id’ (Bergsland 1986: 93, AD 198a), with an initial epenthetic vowel /i/ to avoid merging with *hla-x* ‘child, boy’ < PE(A) *\*/məly/* ‘to suck’ (on the latter see below §3.1.). As I have proposed elsewhere (Alonso de la Fuente 2006/2007: 328-31), the Aleut material could be enlarged by adding Slyunin’s <millegdax> = /*(h)mil-i-γda-x/* ‘a kind of bird’, lit. ‘something (concrete) related to feathers’, with the base *mil-* as the result of competing results in PA stage, i.e. *imli-* ← *\*/mli-/* < *\*/mələ-/* and *\*/mil-/* < PA *\*/mələ-/* (on this see below §3.4), the alternative accentual position having been triggered maybe by the different derivational contexts to which these words belong. Be that as it may, it is obvious that there was a word for ‘feather’ in PEA times.
- [53] ‘meat (flesh)’: although at first sight it seems that there is no common word for ‘meat, flesh’ at PEA level (CED’s authors and Bergsland also thought the same), one cannot fail to notice that PE *\*/kəməy/* ‘flesh’ (CED 168b: CAY *kəmak*, GRI *kimik*) could be, after all, related to Aleut *iji-yu-x* ‘flesh, body’ or *ij-alu-ŋin* ‘body’ (AD 207b) by means of the initial *\*CəN-*collapse in Aleut, partially described in Bergsland (1986: 79-80). This process basically describes the collapse of certain consonant clusters after the dropping of unstressed *\*/ə/*, e.g. PEA *\*/nəŋə/* ‘stretch or lower rope’ > PE *\*/nəŋə-/* ‘to stretch, descend (on rope)’ (CED 227b: CAY *nəŋə-*, GRI *niŋi-t-* ‘lower on rope’) and A *ŋi-* ‘net for fishing; to fish’, related to *ŋi(t)-* in *ŋiɖusi-x* ‘rope with hook hanging under corner of anteroom’ (AD 286b) from PA *\*/(ŋ)ŋə/* < *\*/(n)ŋə/* < *\*/nəŋə-/*. Thus, if a similar evolution is assumed, then PEA *\*/kəmə-/* > PA *\*/kəmə-/* > *\*/kŋə/* > *\*/(ŋ)ŋə/* > A *ŋi-* → *i-ŋi-*, with the very same epenthetic vowel /i/ invoked in at least another two instances to avoid confusion with other competing words (see [27] and Alonso de la Fuente 2010a: §3.4), in this case *ŋi-* ‘net for fishing; to fish’.
- [54] ‘moon’: another surprising claim by Holst is the absence of words to name ‘moon’ and ‘star’, even though both celestial bodies play (“played” would be perhaps more appropriate) crucial roles in Eskimo-Aleut daily life. Furthermore, as is well-known in cultural anthropology, words meaning ‘moon’ are strongly (and somewhat logically) related to those meaning ‘star’ (and this in its turn to ‘day’ or even ‘light’), and both are related to the concept of ‘movement’ and/or ‘position’.<sup>3</sup> This semantic connection

<sup>3</sup> This also holds true for Indo-European words, *vid. i.a.* Buck (1949: §§1.53-4, §14.41).

and the fact that stars and the moon are essential points of reference for Eskimo hunters during their journeys (see in general the wonderful books by MacDonald 1998 and Thorpe, Hakongak, and Eyegetok 2001) make the semantic connection even natural. Thus, Proto-Yupik \*/aɣyaq/ 'star' (CED 9a: CAY *aɣyaq*) is a clear derivative of PE \*/aɣ(ə)-/ 'go (over or pass)' (pace CED 7a: CAY *aɣə-*, GRI *aa-* 'id'), Proto-Yupik \*/iraluq/ 'moon' continues in Sireniski as *iraləqətaaq* 'star' (CED 144a), whereas PE \*/umɬur/ 'day' (CED 370b: Sir *umtəX*, GRI *ultuq* 'id.') continues in PI \*/uɬluriaq/ 'star' (*ibid.*, cf. SPI *uɬluriaq*, GRI *ulturiaq*, with retention of the PI cluster in the old orthography <uvdloriaq>). Last, but not least, PEA \*/nuɣə-/ 'to rise, appear' > PE \*/nuɣə-/ 'id' (CED 236b: CAY *nuɣə-*, GRI *nui-* 'id.') and Aleut *tuyida-x* 'moon' (AD 402b). The fact that literally there is no PEA etymology showing 'star' or 'moon' as the only and uncontested meaning—it is legitimate to assume that in PEA times those concepts were expressed by either metaphoric constructions or, most unlikely, words that were in the course of the years—seems to be a trick of the comparative method due to its limitations especially when dealing with (abstract & material) culture. JHH should inform the reader about these subtle questions.

- [65] 'rain': given the particular weather conditions under which Eskimo-Aleut societies have been living for the last millennia, one cannot sincerely expect to find an ancient word to mean just 'rain'. In addition, as a meteorological phenomenon, it is more pertinent to the Aleutian Islands and Southern Alaska than to Northern Canada and Eastern Siberia. This explains for example the diversified semantics of PY-S words like \*/nəpyuk/ 'wet weather → rain' > CAY *ivzuk* (dialectal), CSY *nəpsuk* 'wet weather, rain', Sir *yəpɣəX* 'rain' (CED 229b), with specialization from East to West. Nevertheless, 'bad weather' in general is a fairly common concept among Eskimo-Aleut peoples that has transpired even in mythological and religious beliefs as an essential part of the shamanic nature of these societies, according to which the maintenance of equilibrium between nature and humans is as vital as food and air. In this context, the most important word is by far A *sla-x* 'weather, wind' (AD 367a-b) < PEA \*/čəla/ 'weather(s) spirit' > PE \*/cila/ 'weather(s) spirit; air, atmosphere, outside, the world' > AAY *ła*, CAY *cila*, GRI *sila* 'weather, outside, the world, atmosphere, air; awareness, intelligence' (CED 78a), for it is the origin of the name of *Sila*, the spirit of weather and the sea, a central character in Eskimo mythology (one can just skip through popular, but informed, books like Rink (1997[1875]) or Hall (1975) to realize about this). *Sila*'s humor depends heavily on human's behavior and she can act benignly as well as in terribly. That is why the very same PE word \*/cila/ appears in derivatives with meanings like \*/cila-kiɣ-/ 'be good weather' (*ibid.* 78a) and \*/cila-ɬuy/ 'rain or bad weather' (*ibid.* 78b), both of them with PE pedigree.

[80] 'star': see under [54] 'moon'.

[88] 'tongue': Although it is true that the most common etymological picture of words meaning 'language' is usually rather depressing, e.g. almost every



Indo-European language has its own term making the task of reconstructing a unique word for PIE stages very difficult (see Buck 1949: 230 §4.26 TONGUE), in the case of Eskimo-Aleut such a situation is not that bad. CED's editors inform us that PY-S \*/ulu(q)/ 'tongue' > AAY *uluq*, CSY *ulu*, Sir *ula* 'tongue, language' (CED 367b) could be tentatively segmented as \*/u-lu(r)/, with the postbase \*/+lu(r)/ and this makes more or less obvious a connection between the PY-S word and PI \*/uqaq(-)/ 'tongue; speak, say' > NAI, ECI, GRI *uqaq* 'id.' (CED 377a). Here we have a dead-end, since there is no postbase \*/+qaq/ (*vel sim.*) to account for the corresponding segment in the PI form. If we travel further, to the Aleutian Islands, we find Aleut *ulux* 'meat, flesh' (AD 436b). Bergsland comments, as precautious as he always was, "*perhaps cf. Y ulu tongue*". The reasons for such an attitude are plenty: it is true that *ulux* can be segmented *u-lu-x*, that the resulting postbase /+lu-/ may be the same as in *aya(-)lu-x* 'tooth' (AD 21a), i.e. another body part (this was noted already by Bergsland 1986: 105), and even that the resulting Aleut base \*/u-/ (now we *have* to posit an asterisked form!) is identical to PY-S \*/u-/ in \*/u-lu(q)/ and perhaps to PI in \*/u(-)qaq/. However, reconstructing PEA \*/u-/ 'tongue; speak' would leave us with many unanswered questions: what about the semantic change in Aleut? What about the exact segmentation in PY-S and PI? Bergsland, talking precisely about the same etymological puzzle, concluded: "[...] to extend this analysis [...] leaving only a vowel as the "root", would be to leave the ground of empirical reasoning" (*ibid.*). Nevertheless, the problem of this etymology lies in what we do not know (we lack enough information about the nature and origin of many fossilized postbases), and not in the total impossibility of the etymology. In this sense, Holst's exclusion of a PEA word for 'tongue' without previous discussion is perhaps excessive.

- [100] 'yellow': in many languages around the world chromatic terminology is the result of derivate processes. As for 'yellow', it does not take a genius to see the relation between this color and urine. In fact, in both Eskimo and Aleut the word for 'yellow' has come to be independently via verbal roots meaning 'urinate', e.g. PE \*/qurə-/ 'urinate' (CED 319a-b: CAY *quXə-*, GRI *qui-* 'id.') >→ PI \*/quqcuq-/ 'be yellowish' (*ibid.*, cf. GRI *qursuk*). The derivation in Aleut is a bit more complicated, i.e. *čin-yalu-x* 'yellow' ← *či(n)ki-* 'to urinate' (AD 144b), specially given Bergsland's orthography that for the former postulates an internal /ŋ/ instead of the cluster /nŋ/. Since Bergsland's orthography distinguishes both sequences by means of a simple apostrophe, namely <ng> vs <n'g>, respectively, and that *čin-yalu-x* 'yellow' is a *hapax legomenon*, one could assume that this is a misprint or an erratum by Bergsland. Curiously enough, there are two (rather unproductive) postbases *-alu-* (AD 483-4) and *-yalu-* (AD 505), both meaning 'related to X' (*vel sim.*), so they are of no help for both could be equally plausible (for further discussion, although with a couple of factual mistakes and a systematic misprint of <gà> instead of the correct <ĝ>, see Alonso de la Fuente 2008c: 80-81). If after all Bergsland's is accepted as genuine, *čin-alu-x* could be related to *čin-la-x* 'fever', *čin-li-x* 'hot, heat' (*ibid.*, of unknown etymology according to Bergsland).

In sum, only the superficial knowledge of the languages (and cultures as well) exhibited and accounted for in previous lines can explain the many mistakes, of different kinds, that Holst made when approaching etymological matters related to the Eskimo-Aleut languages. The main error in Holst's conception is not to underline the importance of the unity between Es-

kimo and Aleut. Although the solidity and deep understanding of such unity is still in its earliest stage, to remain silent about this is very irresponsible. The “fallacy” becomes aggravated if one takes into consideration that (1) Holst’s book is addressed to neophytes, beginners in the field, and (2) he aims at proposing / demonstrating the genealogical unity of Eskimo and Wakashan languages ignoring Eskimo-Aleut elemental issues. Even if in the case of (2) it is not strictly necessary to have at one’s disposition a full reconstruction of Eskimo-Aleut, it goes without saying that it would be of considerable help. Therefore, it is understanding that a recommendation about the necessity of a full reconstruction of Eskimo-Aleut would be more appropriate in an introductory handbook than to throw oneself into such speculative adventures as ‘Eskimo-Wakashan’ or the like. To finish, this latter critique does not concern the inappropriate choices of contents, but rather the fact that Holst “chose” those contents because unfortunately he appears not to be in a position to afford anything else: “long-range comparisons” are much easier than, for example, presenting / solving the riddle about the PEA verbal system and its evolution into Eskimo and Aleut.

**2.2.** Krougly-Enke (2008) tries to link the Eskimo-Aleut name of animals with their supposed Nostratic cognates. One immediately finds three problems with Krougly-Enke’s proposals. The first of them is that he really believes that the genealogical relation between Eskimo-Aleut and the rest of Nostratic languages is already demonstrated beyond any reasonable doubt. The second bias is that he accepts uncritically everything that is said in the main sources, unless these go against his etymological beliefs about the Nostratic pedigree of a specific word. This means that for most of time he just copies *verbatim* what these sources (mainly CED and AD, there is no reference to individual works like e.g. dictionaries, grammars, anthologies, or specialized articles) say about the required material for a Nostratic etymology, with no philological discussion of any kind.<sup>4</sup> As the German linguist Stefan Georg put it: it lacks the compiler’s voice. This uncritical attitude spreads also to the quotation of Non-Eskimo-Aleut materials, as we will see immediately. The third problem, perhaps the most serious, is that Krougly-Enke believes that the reconstruction of Proto-Eskimo-Aleut is a matter of somehow adapting CED’s Proto-Eskimo forms with Bergsland’s Aleut dictionary *lemmata*, regardless of sound correspondences, dialects, potential loanwords, etc. Even though many Proto-Eskimo-Aleut forms are proposed for the first time, thus deserving a bit of attention, most of them are obscure, dubious or plainly wrong. Since a discussion of every proposed etymology is impossible, I will just go in details regarding the very first item of Krougly-Enke’s paper, in which each point mentioned above can be easily observed. Later, I will take one additional case that could be of some potential value only after the careful and necessary philological study of the materials. On this occasion, Krougly-Enke may stay as a “discoverer” of new etymological paths.

**2.2.1.** Krougly-Enke [hereafter KE] relates his “PA” \*/al(a)-/ ‘whale (generic term)’, deduced after (also his) \*/al-āda-/ ‘harbor porpoise’ and \*/ala-may/ ‘hump-

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<sup>4</sup> The uncritical reproduction *verbatim* of materials from consolidated and well-researched (etymological) dictionaries seems to be fairly solid evidence of careless, if not negligent, methodology. For a very recent, illustrative, and celebrated example, see Georg (2008: 248) and Vovin (2009: 106 “mechanical reproduction”).

back whale' (AD 48a), to different Eurasian words meaning basically 'deer, roe; wild animal', all of them sharing the notion that they are terrestrial, quadrupeds, and mammals (Krougly-Enke 2008: 259-60). The quote of materials takes up almost a half of page, therefore, since, as commented above, all the materials have been already reproduced somewhere else, and there is no need to do it here because the present paper concentrates only on Eskimo-Aleut, then we encourage the reader to check out the corresponding sources by him/herself. Still, KE feels that it is not enough and comments that "[w]e are prone to separate these items from P[roto-]Al[eut] \**alya-* 'animal, mammal'." to add more "potential" cognates, e.g. Proto-Indo-European \*/Hól-k-is/ 'elk', but quoted in its Pokornian pre-laryngealist formulation, i.e. \**elk'-* - \**alk'-*. Of course, there is nothing wrong with that. However, it would be a good idea to explain why this way of quoting and not the (generally accepted) other was chosen. In other words, philological discussion is at order in every stage of the etymological exercise —that includes even details concerning the way of writing down linguistic (abstract) reconstructions!— and KE seems to avoid it whenever possible. Since the striking difference in meanings of all forms involved in this Nostratic etymology, ranging from 'whale, sea mammals' to 'deer, roe; wild animal', is self-evident, KE produces the following explanation: "[t]he basic Aleut variant would underwent [*sic!*] a further semantic specification > 'the animal par excellence', i.e. 'whale'." I think that such a comparison in which the researcher quotes materials *ad libitum* without any degree of critical assessment, does not deserve further qualifications, for it qualifies itself.

Having presented KE's proposal, let us comment upon several aspects of it. To begin with, Krougly-Enke's PA reconstructions are, to say the least, surprising. There is absolutely no need to reconstruct the base \*/ala-/ because it actually exists, i.e. *ala-x* 'whale'. The same holds true for derivatives, given the transparency with which they can be analyzed: the dubitative /a/ noted by KE between brackets is spurious, since *alaada-x* = *ala-* + *-aada-*, not \*\**al-* + *-aada-*. As for KE's \*/ala-may/, this deserves several remarks. To begin with, the most correct citation of the material would be *alamax*, pl. *ala-may-i-s* → analogical sg. *ala-may-i-x*. The origin of the variant lies in the generalization of vowel-stems over consonant-stems on the basis of forms like the pl. or the reflexive 3<sup>rd</sup> person, as in *anax* 'club', with the variant *anay-i-x* analogically created after pl. A *anay-i-s* or reflexive 3<sup>rd</sup> person (possessive) *anay-i-in* 'his own club' (see AG 50). This generalization reduces considerably the nominal morphology complexity and, naturally, has been documented in the "younger" generation of Aleut native speakers, in Bergsland's opinion those born after 1920. This piece of information is very important because the variant *alamayix* cannot be used as a proof of the originality of final \*/ɣ/. In fact, there is neither internal nor external evidence to posit that the final consonant of the postbase /-max/ is voiced, i.e. /-may/, but rather the opposite: the pl. form demonstrates that the original feature of the velar fricative is voiceless, lenited intervocally. Using internal reconstruction in this case to argue the contrary would be just a classic example of methodological abuse and a very good proof of ignorance with respect to Eskimo-Aleut phonological matters. In Aleut word-final position can be occupied only by *x*, *ɣ*, *m*, *n*, *ŋ*, and *y* (AG 29-30), i.e. there is no theoretical way to see contrast between voiced : voiceless pairs of final consonants. This fact holds true even in Proto-Eskimo-Aleut stages. Of course,

typology tells us that voiced segments tend to be voiceless in the final position, so we can automatically assume that the underlying form contains a voiced consonant. However, in this case, more than ever, such a typological statement without further inquiries is a gratuitous generalization, for in Eskimo-Aleut historical phonology it is well known that final \*/-ɣ/ and \*/-r/ are usually the lenited counterparts of \*/-k/ and \*/-q/ (this was noted already by Swadesh in his classic series of papers, cf. 1952: 168). But I do not think that this is the implication we should deduce or understand from KE's reconstruction. Thus, readers must wonder: (1) what the intention of KE is when reconstructing PA \*/-ɣ/, if even in PEA this sound would be realized [-k], occasionally [-x], and (2) where the evidence is to prove that actually we should see here a case of /-ɣ/ and not one of \*/-k/. I do not know the answer to these questions. The Aleut postbase /-max : -may-/ 'big, real' goes back to PEA \*/-mak : -may-/ 'id.' and it continues in PE \*/-vak : -vay-/ - \*/-Cpak : -Cpay-/ 'id.' (AD 529, CED 431b: the *lemma* under CED gives the etymology of Aleut *alamax* 'humpback whale!'). Then, the most correct reconstruction (~ underlying form) would seem to be PA \*/ala-mak/. In sum, there is no need for asterisked forms... as forms are historically attested.<sup>5</sup>

Another serious question in KE's proposal is the fact that he does not mention Bergsland's previous attempt at etymology, by a long way more convincing than KE's. The Norwegian linguist proposed a connection between *ala-x* 'whale' and the verbal base *ala-* 'need' (AD 48a), the economic importance of the animal being obviously the leitmotiv of the relationship.<sup>6</sup> A rather straightforward parallel can be found in the case of Aleut *qa-x* 'fish' vs. *qa-lix* 'to eat' vs. PEA \*/nəqə/ 'food; fish, meat' (AD 289a-b, CED 230a), in which it is obvious that the meaning 'to eat' is secondary, consolidated only after the (Proto-)Aleut community — somewhat naively explained— had considered that fish and eating are consubstantial (this makes KE's semantic change not that far-fetched after all). Again, KE shows carelessness in not discussing previous research in the field. This proposal is totally valid, and so was logically included in CED (15b s.v. P[roto-]Y[upik] *ala-* 'desire, need'), upon which we can reconstruct PEA \*/ala-/ 'desire'. Once the origin of the word can be safely stated, making clear that the meaning of the Aleut root *ala-* is actually 'need', metaphorically altered to express the most important need of the Aleut people, i.e. 'whale (in gen.)', it is again obvious that no relation can be established between this and the rest of the Eurasian words alluding to 'deer, elk', etc., nor with Aleut *abya-* 'animal, mammal'.<sup>7</sup> Although I am afraid I will repeat myself, I would like to underline that as for the etymology of Aleut *ala-x* 'whale', KE's crucial mistake was to ignore what his predecessors had done. Interestingly enough, Bergsland's original proposal can be found in the very same references that KE constantly use in his own work.

<sup>5</sup> For some of these words, and other potentially related, a proposal of (internal) etymology was recently offered by the present author (Alonso de la Fuente 2008b: 109-12).

<sup>6</sup> It is not necessary to resort to exotic languages to observe such basic (semantic) relationship between one and another concept, *vid. i.a.* Polish *żyć* 'to live' → *żyto* 'corn > rye'.

<sup>7</sup> KE rejects this etymology arguing that "[t]he linkage to the homonymous verb [...] is fortuitous and folk-etymology" (Krougly-Enke 2008: 282). This is plainly wrong.

Last but not least, an apparent “minor” issue deals with the particular semantics of the words KE quotes as support for his proposal. KE and/or his sources systematically mistake ‘deer’ for ‘reindeer’. Such a mistake is not made in the sources consulted by KE, e.g. Nikolaeva’s historical dictionary of Yukaghir (2006: 173 nr. 566 \**ilwe* ‘domestic reindeer’), or Fortescue’s comparative dictionary of Chukchee-Kamchadal (2005: 340 s.v. \**əlwa* ‘wild reindeer’) include the correct translation of the very same words quoted in KE’s materials. The difference between these two animals is notorious, and there is nothing trivial in making it tacit by using the appropriate denomination (in fact, neither Nikolaeva’s nor Fortescue’s *indices verborum* list ‘deer’, as generic term, but only specialized ones). If the name of both animals were interchangeable, maintaining the existence of the two terms ‘deer’ and ‘reindeer’ would be a nonsense, against the most basic principles of lexical economy. This comment, far from being a gratuitous destructive critique, only seeks to underline the fact that semantics is as important in the etymological exercise as sound correspondence or correct morphological segmentations.

**2.2.2.** The amount of etymologies proposed in KE’s paper is considerable, making it almost impossible from a statistical point of view that all the etymologies are equally wrong or do not generate any kind of interest or curiosity. It goes without saying that this does not mean they have to be correct in the end, but simply that at least we have a path to follow (this is already something, compared to what we have sometimes). Leaving aside startling comparisons such as PE \*/nanur/ ‘polar bear’ (CED 213b) : Aleut *tany.aax* ‘bear (in gen.)’ (AD 391b; see Krougly-Enke 2008: 265), which are undoubtedly wrong, some could hide the germs of new, good etymologies. Since there is no space to comment at length on all the proposals I have found more or less reasonable, I have decided to choose one of significant relevance. Once more, KE links in a rather unreliable way Aleut *siimlu-x* (E), *siimblu-x* (E+A), *siivlu-x* (Au) ‘pigeon guillemot’ (AD 361a) with several dozens words spread over Eurasia expressing the general meaning of ‘a k. of bird’ (Krougly-Enke 2008: 279-80). According to Bergsland, there is no Eskimo etymology for this word, and he quotes the dialectal Siberian Yupik word /sipəlaaq.aq/, orth. <sipe-laaghaq> ‘guillemot’ (Badten, Kaneshiro, Oovi & Jacobson 1987<sup>2</sup>: 213).<sup>8</sup> It is not clear if he intended to mean that there is a genealogical connection or rather a potential case of borrowing.<sup>9</sup> What is the opinion of CED’s editors? They link Aleut words (their notation of pre-aspiration is incorrect: CED’s <siimlu-> would correspond to Bergsland’s \*\*<siihmlu->) with Proto-Inuit \*/cirvaq/ ‘guillemot’ > SPI *sirvaq* ‘id.’, NAI *sir-*

<sup>8</sup> KE fails to render the correct phonetic shape of the word, with automatic stress on the second syllable and /-γ.-/ instead of his /-γ-/. Krauss (1975) offers an account of both the history and the process of the orthographic design for CSY SLI, as well as the most important and detailed description of its phonology.

<sup>9</sup> KE adds PE \*/cipala-ra-/, based on “CSY, SPI *sipəlaaraq* ‘id.’”. It was after having checked again and again all sources available to me that I figured out where the problem was here: according to KE’s abbreviation list, SPI stands, as in CED, for “Seward Peninsula Inuit”. The phonology of the word, as KE has written it down, seemed to me from the very beginning very suspicious, for the languages of the Inuit branch merged PE \*/ə/ & /i/. KE confused “SPI” with “SLI” in CSY SLI, i.e. “Central Siberian Yupik from Saint Lawrence Island”. Moreover, Bergsland only quotes one word, not two, thus there is no basis for a PE reconstruction.



*vaq* ‘sea pigeon’, GRI *sirfaq* ‘id’. Then, they add CSY <sipe-laaghaq> and propose PY \*/cip(ə)lar/ tentatively via PE \*/civraq/ with metathesis in Inuit (CED 86a). In the meantime, KE proposes that PI \*/cirvaq/ is related to Eastern Aleut *ciida-x* ‘a k. of land bird’ (AD 136a: *hapax legomenon*), for which Bergsland had no etymological solution. He goes on and claims that PI and Aleut go back to PEA \*/cir-/ ‘a k. of small bird’, of course after cutting off every possible phoneme regardless of known bases & postbases segmentations to fit it better to Eurasian words.<sup>10</sup> KE may have not noticed CED’s editor’s comment on Aleut *siimblu-x*, otherwise it is incomprehensible why KE has kept two different etymologies without letting the readers know that they actually take part in one and the same etymological proposal.

Be that as it may, the essential point in this puzzle is that KE could be right after all in claiming that there is a PE root \*/cir-/, since Aleut does not show a trace of the PE(A) postbase \*/-rar/, while Inuit has erased (reduced) the internal sequence \*/-pəl-/, otherwise necessary to include the CSY SLI word and Aleut materials. My proposal is the following: given the total lack of correspondence between CSY SLI and Inuit (there is a legitimate doubt in the appropriateness of CSY /p/ and Inuit \*/v/, of course, but this is the end of the story, because the rest of the word, from that perspective, is obscure at best), it seems to me that CSY SLI is actually the last remnant of a Yupik proto-form that was borrowed by speakers of Proto-Aleut, for Aleut may be reflecting the original CSY sequence \*/-pəl-/ (the Yupik branch retained PEA \*/ə/ against the merging of it and \*/i/ in PI). As is well known, PEA \*/-m-/ & /-p/ merged in PA \*/-m-/ and \*/ə/ vocalized in stressed syllables, and the very same stress geminated the following consonant that was solved over the years by dissimilation and rising of pre-aspiration (for further details see Alonso de la Fuente 2010a). Thus, Aleut *siimblu-x* & variants might go back to PA \*/sipəlu-r/. The similarity between bases cannot be fortuitous. The direction of the borrowing cannot be, or would be rather unlikely, the opposite, for CSY SLI stress on the second syllable is in this context automatic, i.e. we can explain why it is there (Krauss 1975: 54), while for Proto-Aleut there is no way to predict the original proto-accent, if not after internal and external reconstruction. In addition, PE \*/c-/ yields regularly CSY /s-/, thus, the direction late PY → PA allows us to speculate still on the possible common origin of Inuit and Yupik (CSY) words. In fact, strictly speaking one could argue that CSY SLI and Aleut words may be genealogically related, for sound correspondences, at least those concerning the base-stem, seem to be regular. However, I see no way to account for the following “postbases”.<sup>11</sup> I can only argue that Proto-Aleut speakers just naturalized the word afterwards at their convenience, transforming and/or interpreting the CSY segment /-(l)aaraq/ as /-(l)u-r/. Thus, there is no way to assure the genealogical link between CSY SLI and Aleut. By the same token, the link between

<sup>10</sup> I fail to see how this word could be a derivate of *ciida-x* ‘young spring (of bird, animal), pet’ or Atkan *ciidyu-x* ‘baby, infant’. Even though these two words could actually be related somehow, the problem is that there is no way of segmenting them in morphologically smaller constituents as base vs. postbase.

<sup>11</sup> Both are of unknown origin, cf. however the PE postbase \*/-rar/ ‘young, small’ (CED 423b), not that far-fetched taking into account that the guillemot is a small bird. As for the Aleut postbase /+lu-/, so far it also has an undetermined meaning, see AD (526-7).



CSY and Inuit must be considered inconclusive at best. The following chart will make my point clearer:

PEA ??	PEA ??	PEA ??
PE ??	PE ??	
PI */cirvaq/ > */sirvaq/	PY */ci(pól).../ */si(pól).../	→ PA */sipó(-)lu-r/ > */sipillu-r/ > */simihlu-r/ >
SPI <i>sirvaq</i> GRI <i>sirfaq</i>	CSY SLI /sipólaaγ.aq/	<i>siimblu-χ</i>

All in all, Aleut *siimblu-χ* as well as PI \*/cirvaq/ still lack a solid PEA etymology, but in exchange we have an interesting case of an ancient loanword, likely dating back to PA, preserving some of early PY prosodic features that initially we can recover only via internal reconstruction. I change CED’s editor’s PI metathesis for a (more?) reasonable metathesis in Aleut accounting for the initial long vowel. KE’s assumption about the existence of a PE root \*/cir-/ was not hilarious either, for the most logical segmentation of the Inuit word is \*/cir-vaq/. Unfortunately, we do not know the meaning of \*/cir-/ and (the postbase?) \*/-vaq/ (which for sure has nothing to do with the aforementioned \*/-vak/ > SPI +(a)pak, GRI +(r)paat). As for Aleut *ciðiða-χ* ‘a k. of land bird’, we face the very same problem: it is true that sound correspondences are regular, and that even medial *-i-* could be interpreted as the epenthetic vowel that systematically appears between consonant stems and postbases beginning with a consonant, but this leaves us still with a totally unknown postbase \*\**-ða-*.<sup>12</sup> Nevertheless, one wonders whether this word may be related to PE \*/ciri-/ ‘be in a good mood, jolly’ > AAY *siri-* ‘id’, ECI *siri-* ‘make traditional cry of thanks on receiving sth.’ (CED 85a s.v. *ciri-* ‘be eager or powerful?’). It is a well known fact that some bird-names (but not the generic word for bird!) are created according to onomatopoeic principles or on words alluding to singing, crying, screaming, etc. In addition, sound correspondences match perfectly in this case. Regrettably, since we lack a more detailed description of the bird named by Eastern Aleuts with *ciðiða-χ*, this proposal will remain pure speculation.

Thus, KE’s final decision to propose two different etyma for PI \*/cirvaq/ and Aleut *siimblu-χ* & variants, against the indifference of Bergsland or the opinion of CED’s editors, has been the beginning of an interesting etymological digression whose

<sup>12</sup> Yes, it is true, we agree with the attentive reader: *ciðiða-χ* reminds one of the aforementioned *tuγiða-χ* ‘moon’ (see §2.1. [54]), for which even Bergsland recognized a tentative segmentation \**tuγi-ða-χ*. However, there is a decisive difference between one case and another: for the latter we have PE \*/*nuγə-* ‘id’ (CED 236b: CAY *nuγə-*, GRI *nui-* ‘id.’), while for the latter PI \*/*cir-*. Thus, we still have the irresolvable problem regarding the origin of medial *-i-* and the necessity of recognizing ignorance about the nature, meaning, and origins of the postbase \*\**+ða-* (then theoretically shared by two words meaning ‘moon’ and ‘a k. of small bird’).

conclusion includes attractive evidence for the predictive power of the model we have elsewhere presented for the reconstruction of PA. The scenario I have just set up, inspired in KE's stubbornness, is much more economical and reliable than all the previous attempts. Although KE's may not be right at the end, CED's editor may not be either. As someone once said: where many think the same, no one thinks too much.

### 3. *Miscellanea paedagogica*

**3.1. *Semantic parallels at the service of historical and comparative linguistics.*** In his rather anodyne book review of Carl Buck's *monumentum ære perennius*, namely *A Dictionary of Selected Synonyms in the Principal Indo-European Languages. A Contribution to the History of Ideas*, published in 1949, Sturtevant comments that "[e]very etymologist finds his chief difficulty in handling word meaning. When he thinks of an etymology that involves any considerable change of meaning he tries to think of a parallel among the languages he happens to know." (1950: 330). This is today as true as it was more than a half of century ago, and this is so because semantic parallels still convey the same feeling of validity and assurance in etymological issues as much as it was then. In what follows I will try to show that semantic parallels are sometimes the last piece in solving a puzzle showing satisfactorily the image we were waiting for, which implicitly means that until this last piece is added, the puzzle is incomplete and the image-blurred.

**3.1.1.** In an excellent exercise of etymological guessing, Bergsland (1986: 93-4) proposed that Aleut *hla-x* 'son, child' (AD 252b) had to be the historical continuation of a PEA form of the shape *\*/mələ/* (*vel sim.*), unless there is an alternative explanation for the well-known case of epenthetic initial vowel in *imli-x* 'single hair of head' (AD 198a) to avoid the merging of these two words ( $\leftarrow$  *\*/mli-/* < PA *\*/mələ-/* < PEA *\*/mələ/* 'id.', otherwise PA *\*/mlə-/* > *\*hla-x*). Therefore, it is fair to say that Bergsland already knew half of this etymology, but he lacked a good typological parallel to make it more likely, i.e. what neither Bergsland nor CED's authors apparently ever decided is the concrete PEA base from which Aleut *hla-x* is historically derived, even though they already had a very important clue about how the PEA initial form should look like. The solution, in fact, seems to be fairly simple once one takes into account general tendencies in semantic evolution. Thus, in analogy to Spanish *hijo* ['ixo] 'son, boy' < Lat. *FĪLIUS* < Proto-Romance *\*/dʰi-ljo-/* or Polish *dziecię* 'son, boy' < Common Slavic *\*/dětę/*, both from Proto-Indo-European *\*/dʰej-/* 'to suck' (materials and general discussion already in Pokorny 1959.I: 242 and Fasmer 1964.I: 516), Aleut *hla-x* could be by the same token related to PEA *\*/məluγ-/* 'to suck (breast)' (CED 197b: CAY *məluγ-*, Sir *mələγ-*, GRI *miluγ-* 'id.'). As CED's authors note, this PE form can be contaminated with *\*/muləγ-/* 'nipple or tip' (CED 202b: CAY *əmulək*, Sir *mula*, GRI *muli(k)* 'id.'). The temporal distance between Proto-Romance and Spanish required to complete the semantic change is almost identical to that between Aleut and Proto-Eskimo-Aleut. It is rather obvious that, since both forms can be traced back to PEA times, one has to assume that PE *\*/məluγ-/* comes from PEA *\*/mələγ-/* (and not from, say, PEA *\*\*/muluγ-/*, because such reconstruction would go against the Aleut testimony), otherwise the result of contamination after PEA *\*/muləγ-/* (> A *huli-x* 'leaf', cf. AD 435b) would have been identi-

cal, i.e. \*/mələŋ-/ → \*/muləŋ-/ , leading to an unacceptable merging of terms. Thus, vowel /u/ affected the second syllable.

**3.1.2.** The Aleut word *ču-uda-lix* ‘to decorate’ and other potential related forms, i.a. *ču-γδι-xsxita-* ‘to mark or aim’ (AD 149b) could lastly be related to PE \*/mətu/ ‘slush ice in fishing hole’ (CED 199a). On the one hand, the phonetic shape of the PEA word can only be \*/məču/ ‘slush ice removed from fishing hole’, showing the sound correspondence PEA \*/č/ > PE \*/t/ vs. PA (\*)/č/ as initially described by Bergsland (i.a. 1986: 70-72) and later (typologically) specified in Alonso de la Fuente (2008a: 97-101). On the other hand, it can be shocking to try to relate meanings as disparately distant as ‘to mark’ and ‘slush ice’. However, the proposal becomes a bit more reasonable after inserting the last element into our etymological equation: PE \*/mətləŋ/ ‘knife for carving’. The postbase \*/-ləŋ/ ‘one having, provided with’ enable us to recover safely the root \*/mət-/ ‘to carve’ (< PEA \*/məč-/), from where the lit. meaning ‘one provided with the capacity to carve’ = ‘knife for carving’. Unfortunately, I cannot provide an explanation to the final vowel (suffix?) \*/-u/ in both Aleut and Eskimo materials, apart from commenting that its function seems to be to nominalize the base to which it is attached (standing against Ulving’s (1987) suffixes /-a/ and /-i/ for sg. and pl. verbal acts respectively?). Be that as it may, PEA \*/məč-/ ‘to cut’ → PEA \*/məč-u/ ‘knife’, later in PA reanalyzed as a verb base, so \*/məčú-/ > \*/mčú-/ > (\*/hču-/? >) Atkan *ču-uda-* ‘to decorate’. From the general perspective that offers this preliminary explanation, it should not be a problem to propose different semantic parallels. One is especially significant: English *graphic* adj. ‘1. vivid; 2. of painting, drawing, etc.’, from Classical Greek *γράφειν*, -ω ‘to carve; I carve’ (see Buck 1949: 1283 §18.51). The very range of meanings exhibited alone by the English word *notch* also serves as a good parallel instance. As is obvious, cultural environments generated different semantic routes for Eskimo and Aleut languages: \*‘to cut’ > (1) \*‘knife’, (2) ‘to cut ice in the fishing hole’ > ‘slush ice in the fishing hole’, (3) ‘to carve’ > ‘(to) mark, decorate’. It goes without saying that these routes may look different or could have developed in alternative fashions. However, everything falls within the realm of the most likely known facts.

**3.2.** “Coronal syndrome” in Aleut? Contributions in the monograph edited by Paradis and Prunet (1991) as well as single papers like Steriade (1995) document extensively the generalization that [coronal] is the unmarked, default choice for the oral place node. Although no statistical evidence is available, phonologists have the impression that coronal is the most commonly chose epenthetic or otherwise dummy oral consonant. Thus, one can find across the linguistic map of the world that coronals are more susceptible to Place assimilation than noncoronals, or more likely to be transparent to transconsonantal vowel-echo rules than labials or velars. Whatever the reason accounting for such a curious behavior is,<sup>13</sup> the implication for historical and

<sup>13</sup> Phonologists have sought to explain the suite of properties comprising the coronal syndrome by underspecification of the Place node (see Kenstowicz 1994: 516-21 for a basic statement). On the side of disclaimers, Blevins (2004: 125-9) argues however that there is nothing special about coronal place. Although I agree completely in that there is actually nothing special about this coronal place, it is “just another place” to use her words, Blevins’ argumentation forgets to account for the most important fact: why, after all, those phonemes mostly fulfill the function of epenthetic, prothetic, etc. and not others. That is why I still consider the term “coronal syndrome” valid, at least from the descriptive viewpoint.

comparative studies, usually armed with arguments of descriptive rather than explicative nature, is inescapable. As a matter of fact, it is well known among specialists in different fields that many cases traditionally labeled as “irregularities” or “idiosyncrasies” can be, notwithstanding, inserted in a not-so-irregular-and-idiosyncratic explicative, natural and convincing framework. This is exactly what I intend to demonstrate in the following discussion involving Aleut materials and current (theoretical) issues in the area of phonology.

**3.2.1.** Eastern Aleut *(n)ix* ‘baidar’ (variant *ix* also attested in Atkan), pl. *niy.in* (AD 283b) belongs to a series of nouns showing the correspondence Eastern /n-/ vs. Atkan & Attuan /Ø-/. Bergsland was very well aware of those items (*i.a.* AD xxviii/b-xxix/a), though always qualified them as “idiosyncratic”, without elaborating on them any further. As far as I am able to say, all words displaying this alternation are the following:

Eastern	Atkan	Attuan	Gloss	AD
<i>niy.-ila-x̄ - (n)iy.-ala-x̄</i>	<i>iy.-ila-x̄</i>	<i>iy.-ila-x̄</i>	‘baidar’	283b
<i>nixsu-x̄</i>	<i>ixsu-x̄</i>	—	‘temporary shelter’	283b-284a
<i>nuy.a-asi-x̄</i>	<i>uxa-asi-x̄</i>	<i>uy.a-asi-x̄</i>	‘oar’	284b-285a
<i>na-</i>	(Old) <i>ŋa-</i>	<i>a-</i>	‘south’	280a

For the last case there is a convincing etymology proposed by M. Fortescue, according to which the original stem is /ŋa-/ < PA \*/əŋá-/ , somehow related to the very productive PE base \*/uŋa-/ ‘area beyond (partition)’ and demonstrative-like root \*/iŋ-/ ‘over’ (CED 461) > *iŋa-* ~ *uŋa-* in Inuit *uŋalliq* ‘furthest south’, CAY *uŋalaq* ‘south’, CSY *iŋan* ‘side wall’, etc. (see Fortescue 1988: 23-4, CED 374b-375a). Although Fortescue did not explain the origin of /n/ in the Eastern form *na-*, I think that it can be assumed with a good degree of confidence that this is an Westernism (i.e. a salient feature brought from Atkan+Attuan-like dialects), with the extension of the initial /n-/ as in the rest of instances in the table. This, however, does not account for the origin of the very same phoneme /n-/ in Eastern Aleut dialects, i.e. we can notice the generalization of this feature, but we have still to explain what its origin is. Thus, where does Eastern /n-/ come from? Ohala & Ohala (1991) demonstrated some years ago on the basis of Hindi materials that nasal phonemes can function as epenthetic segments. This fact may shed a bit of light on the Aleut case if we assume for a moment that Eastern /n-/ could be a case of prothetic consonant. This moment, unfortunately, must be brief: “epenthesis” is not the same than as “prothesis”, and there is no reliable phonetic context to set up a credible scenario. Thus, there is no way to apply such explanation to the Aleut data. There is however a second option much more attractive and, what is more, one capable of being checked out in Aleut materials. Let us for a moment concentrate on very famous cases like English *orange* vs. Spanish *naranja*, in which the Spanish *n-* dropped in English after the metanalysis of sequences with the article, i.e. *\*a norange* > *an orange* (the same as in Shakespeare’s *King Lear* form *my nuncle* ‘my uncle’ < *\*mine noncle* < *\*mine on-*

*cle*), or Swedish *ni* ‘you’ (plural, formal) < Old Swedish *i*, but after verbs that ended in *-n* ‘plural agreement’, the sequence *-n + i* was reinterpreted as together, e.g. *veten i > veten ni > vet ni* ‘you know’ (Campbell 1999: 103). Could we apply this analysis to the Aleut material? In order to check whether Eastern Aleut /n-/ is actually the result of a metanalysis, we must take into account textual *corpora*. We are extremely lucky because Knut Bergsland and Moses Dirks worked out an enormous amount of textual materials and got them published in an excellent volume (Bergsland & Dirks 1990). If one takes a look at the passages in which the word (*n*)*ix* appears, it is really obvious and legitimate to conclude that identical process has taken place.

Jochelson's Passage & Page reference	Aleut original text	English translation (after Bergsland & Dirks)
10: 35 [pp. 118-9]	<i>Ixsix tanaay.alix, ay.uun ulaam hačan iqitxadaayim, ayayaan qanjučxilix, čŋayuxtaan niy.iin sitxan ašsix aqadaayim, ulaan ukux awa.</i>	«Upon getting home and removing his load at the entrance of his house, he had his wife enter the house and then after putting his quiver under his baidar he went into the house.»
10: 86 [pp. 124-5]	[č]iilukum, niḵ ukuxtaakakuun ukuxtađaxtalkay.iin, adan uyalix, ilaan ay.alix, sitxa ukukum, ukayam ilan sam qičxuy.in tayuxtaa iijun axtakux ukuxtaqaliiryim, ašsix ŋaan čimy.aasalix, ŋaan tunukux awa.	«While he was returning, he saw a baidar which he had never seen before, went over to it and looked under it, and when he saw that there was bird bound with a harpoon line in the stern, he sprinkled it with mummy oil saying to it, [...]»
35: 63 [pp. 274-7]	[...] <i>Tiŋ akuunusada, akuunusada, niy.iin akuya, maayuniŋ akuya,</i> ” iistayalikuḵ, sakan uusakan, anum qiičii ŋaan uusaqadaayim, tataam ŋaan tunukuu awa. [...]	«Take me ashore, my baidar is ashore there, my belongings are ashore there,” he said, but [the boy] took him seaward to a tidal whirlpool and said to him again, [...]»
75: 10 [pp. 486-7]	<i>Ayaa, uknan, iy.im aqanaa, saayaay.ay.ilakan aqanaa.</i>	«Look, a baidar coming out there, coming without slowness.»

In three instances (*n*)*ix* is preceded by a word ending in nasal. Thus, we can speculate that at least in the case of (*n*)*ix*, by extension also of (*n*)*ixsu-x* ‘temporary shelter’ (since (*n*)*ix-su-x* lit. ‘resembling a baidar’) and (*n*)*uy.a-asi-x* ‘oar’ for all these words are related one way or another,<sup>14</sup> the origin of Eastern /n-/ may be hidden be-

<sup>14</sup> Neither Bergsland, nor anyone I am aware of, has ever related (*n*)*uy.a-asi-x* etymologically to (*n*)*ix*, since there is no direct link between these words enabling us to connect them as one being a derivate of the other (there is actually one derivate with the very same meaning: *niy.iy.i-lix* ‘to row’ < \**niy.-ay.i-*). However, the semantic connection is more than obvious and some kind of influence through this route is likely to have happened.

hind metanalysis, as many of the verbal forms which usually precede those words end in /n/.<sup>15</sup> This resembles quite closely what happened in the instance involving the Swedish personal pronoun *ni* ‘I’. The alternating nature of the initial /n/ in this word may be due to the fact that sequences of words ending in nasal + (n)ix (& derivatives) are actually not that frequent, at least not as much as in the Swedish or English parallel examples, in which the regularity of the context has led to a total reanalysis of the forms involved. Since there seems to be no better explanation, i.e. individual etymology, for Aleut (n)ix ‘baidar’ so far,<sup>16</sup> this solution at least proposes a likely scenario to account for this and another words (for which, unfortunately, we lack such extensive textual material to work with).

**3.2.2.** Another curious pair is the one composed by the two homophones (δ)uxta- ‘fishhook’ (AD 161a) and (δ)uxta- ‘guest; to visit’ (AD 161b). While for the former neither Bergsland nor CED’s author proposed any etymological solution, the latter has been related timidly to CAY *uqtaq* ‘hookless fish lure’ (*ibid.*, CED 389b). However, Bergsland already pointed out that this could be an Aleut borrowing in CAY, as *uqtaq* is an isolated word within the entire Eskimo branch. Therefore, it seems that the etymology of both words can only be accounted for from an internal Aleut perspective. As happened in the case under study in §3.2.1., one and another share the alternating presence of initial /δ-/ , interestingly enough another coronal consonant. Is the explanation for these cases related also to epenthetic, prothetic, or the like according to the “Coronal Syndrome” phenomenology? It is understanding that in this case a more traditional question plays the main role, for I think that although “Coronal Syndrome” could account for the “alternating” nature of the consonant, the very same heart of the question lies in the particular evolution of the original late PA coda resulting from the PEA word-structure of both words. The following chart shows clearly what I have in mind:

- [1] (δ)uxta- ‘fishhook’ < \*/(t)δu-xta-/ ‘having an angle’ (vel sim.) < PA \*/təδú-xta-/ < PEA \*/təru/ ‘sth. at an angle’ (CED 344b) or maybe \*/nəCur/ ‘be curved’ (CED 232a);
- [2] (δ)uxta- ‘guest; to visit’ < \*/(n)δu-xta-/ ‘having a visit’ (vel sim.) < PA \*/nəδú(δ)-xta-/ < PEA \*/nəCur-/ ‘id.’ > PI \*/niur-ru-/ ‘id.’ (CED 236a).

<sup>15</sup> This explanations also applies to Eastern Aleut *niqa-x*, a variant of (Atkan) Aleut *iyya-x*, (Eastern) *iqā-x*, *niqā-x*, and (Attuan) *iyy.a-x*, *iyy.a-x* ‘single-hatch baidara’ (AD 210b). See the new etymology in Alonso de la Fuente (2010b §3.4).

<sup>16</sup> I cannot avoid the temptation to share the following (speculative) idea: Aleut (n)i-x ‘baidar’ < \*/n(η)δ-r/ < PA \*/təηδ-r/ ←< PEA \*/təηə-/ ‘to fly (up); to sail (a boat)’ >> PE \*/təηə-t-/ ‘blow away (wind) (v.t.)’ > AAY, CAY *təηə-* ‘to fly; take off’, GRI *tiji-* ‘to take off’ & \*/təηmi(C)ar/ ‘bird’ > AAY *təηmiaq* ‘goose’, GRI *timmiaq* ‘bird, birdskin coat’ & \*/təηə-lrar-/ ‘sail’ > CAY *təηalrar-* ‘id’, GRI *tijirlaat* ‘id’ (CED 341b). See the semantic parallel in Spanish *vela* ‘sail’ → *velero* ‘boat’. This proposal would imply that /n/ is original and was lost by virtue of the very same metanalysis we are discussing about, a rather unlikely scenario. Note that Marsh and Swadesh (1951: 214) already proposed an Aleut cognate for PE \*/təηə-t-/ &c., namely their <tiiγlar> ‘eagle’ (AD 398b s.v. *tixla-x*), which of course has not evaded KE’s nets, for one can find it in his paper (2008: 280-1; see p. 274 for \*/təηmi(C)ar/ ‘bird’, but without traces of \*/təηə-t-/), of course after “Nostratisation”.



In both cases the unspecified PE consonant /-C-/, impossible to account for on the basis of exclusively Inuit data, must be \*/-r-/.<sup>17</sup> This scenario fulfills all the requirements: as is well known, in later stages of its evolution, PE consonants \*/r γ η/ were dropped between single vowels (in Eskimology this process is commonly labeled “velar dropping”, see complete, but resumed, account in i.a. CED xvii), while in Aleut the fate of PEA \*/r/ can be summed in the regular sound change PEA \*/r/ > (P)A \*(\*)/δ/ (cf. Bergsland 1986: 75-8). Thus, it is crucial for our explanation to see here two very different levels of reconstruction: PE (“velar dropping”) and PEA (Aleut having branched off before “velar dropping” would have affected it). In sum, the word-structure phonetics of both PEA words \*/təru/ & \*/nəCur-/ underwent the very same sound changes in their evolution into PA, i.e. the loss of /ə/ in unstressed syllable and the merger of PEA \*/t/ and \*/n/, with its subsequent loss, leaving the PEA internal consonant \*/-r-/ (> PA \*/δ/) as the syllable-initial of the (P)A resulting forms. These similar paths of evolution explain the word homophony in Aleut dialects. The problem comes now: although Aleut phonotactics allows all the consonants to be found in syllable-initial position (Bergsland 1997: 22-3), the only phonemes showing an alternating presence in such position are coronals. Likewise, it cannot be fortuitous that the exception to this phonotactic rule in Eastern dialects are the phonemes /hδ-/ and /hŋ-/ (pre-aspirated /δ/ and /ŋ/). Thus, the only reasonable conclusion is that there must be a kind of timid tendency to restrict syllable-initial segments to [-coronals]. Of course, this statement needs more philological supporting data and further discussion.

## Conclusions

**4.1.** In this paper I have tried to offer a different image of how a review should look like with the aim of helping and guiding not the people to be criticized, but all those potential readers who occasionally might be interested in knowing what was wrong, what is going in the right direction, and what is in need of order to get if not correct at least interesting achievements about these “academic” works. It is my wish that this is of some use, for example among Basque philologists taking their first steps in the field, who from the very beginning will find studies along the same lines of Holst and Krougly-Enke’s writings.<sup>18</sup> Of course, all this does not mean that there should be no more tough reviews. Although I completely agree with authors like A. Vovin or S. Georg on that persuasive, demolishing, no-room-for-compassion reviews are in order from time to time, I hope the reader of this paper will appreciate that I approach the question from a very different angle, positioning myself nearer to the side of other researchers like E. Helimsky or R. Austerlitz, who preferred more gentle judgments and relaxed critiques, however never denying persuasion by that, and always trying to contribute something of value —original research— apart from the obvious critical content. In that way one can avoid a reasoning like the following: “if

<sup>17</sup> For the function of the (Proto-)Aleut postbase /+xta-/ ‘agentive nouns (vel sim.)’, see AG (108-9).

<sup>18</sup> In general, I believe that Lakarra (2003) offers the best account on the matter, showing, explaining, and demonstrating why all these considerations are crucial in the exercise of historical and comparative linguistics within the philological context (see also 1996, 1999, 2003: 235-47).

I just do not read E[tymological]D[ictionary of the]A[ltaic]L[anguages] (see Starostin, Dybo and Mudrak 2003), *ergo* I do not need e.g. Vovin's long review (2005, 2009, see also 2001)". However, it must be said once for all that such reviews as Vovin's or Georg's are a compulsory requirement of any scientific field: they establish with exactness what amateurism and what professionalism are, making it very clear that someone unaware of basic facts about languages and historical & comparative linguistics (yes, both are necessary) should not approach the etymological exercise, as someone with no M.D. qualification should not attend sick people. However, extremes do not make balance, and I think that it is just as important to have this sort of convincing reviews, as what I have intended to present in this paper—probably not so much of a review after all. In sum, I am not appealing to the classic dichotomy between destructive vs. constructive critiques, but rather to one between destructive-but-necessary vs. truly-constructive critiques.

**4.2.** In previous paragraphs I have tried to demonstrate that several recent contributions in the field of Eskimo-Aleut historical and comparative linguistics do not fulfill the most basic requirements to be considered professional, academic, or serious and of significance to the discipline. Consciously or unconsciously, they keep alive a line of research if not of reasoning according to which in some philological traditions, especially those regarding minority & isolated languages, no previous training seems to be necessary to proceed to etymological analysis. If the researcher has at hand a couple of dictionaries of the given languages, whichever dialect or historical period they cover, then the work can be done. On the whole contrary to this, I claim that no results will be obtained before having grasped in a really deep sense the simplest and most complex linguistic structures of the languages as well as, to the best of our capacities, dealing with textual materials. Intimate knowledge of the traditional philology (and surroundings!) and the highest of the respects to the work of our predecessors is likewise of extreme importance.<sup>19</sup> That is why I have tried to offer a critique of very precise passages in Holst and Krougly-Enke's studies. Those passages reflect the kind of reasoning we should avoid the most. On the other hand, I have proposed new etymologies and paths of research in an attempt to respond to these authors' methodological inadequacies. I cannot hide the fact that I did so also with the intention of showing the reader alternative and more appropriate ways of working in this field. Thus, general and rather common trends in semantics (this includes the necessary adaptation of Swadesh's 100-words list according to the culture and language we are dealing with), and phonology shed a bit of light on a few etymological puzzles that now, we could humbly say, may be considered to be solved. Treatment of textual material turned out to be also very useful, as it was crucial in understanding a phenomenon that was labeled as idiosyncratic and left aside. More than ever one feels the necessity to endorse Loporcaro's (2007: 322) seemingly irrelevant, self-evident, even trivial point: historical linguistics must be done by (trained) historical linguists.

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<sup>19</sup> Especially in the case of Basque or Aleut philology. For those who work in these fields it is most delightful to know that such high-class specialists as Luis Michelena (Koldo Mitxelena [1915-1987]) or Knut Bergsland (1914-1998), respectively, both much more than simply keen historical and comparative linguists, but rather extraordinary philologists. In sum, not on the whole good ground for almost nobody to fight on.

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