



Universiteit
Leiden
The Netherlands

On methods of dealing with facts and opinions in a treatment of the progressive palatalization of Slavic

Kortlandt, F.H.H.

Citation

Kortlandt, F. H. H. (1989). On methods of dealing with facts and opinions in a treatment of the progressive palatalization of Slavic. Retrieved from <https://hdl.handle.net/1887/1902>

Version: Not Applicable (or Unknown)

License: [Leiden University Non-exclusive license](#)

Downloaded from: <https://hdl.handle.net/1887/1902>

Note: To cite this publication please use the final published version (if applicable).

ON METHODS OF DEALING WITH FACTS AND OPINIONS
IN A TREATMENT
OF THE PROGRESSIVE PALATALIZATION OF SLAVIC

FREDERIK KORTLANDT

1. In 1981 Horace Lunt published an odd little book on the progressive palatalization of Slavic. The author discussed the pertinent facts and a number of relevant opinions about them on the basis of the assumption that the progressive palatalization was an early Slavic development. Since several colleagues, including myself, were dissatisfied with his treatment of the problem, I decided to write a short critique, which was published in this journal (1984). The gist of my objections was twofold. My first concern was Lunt's way of handling the counter-evidence to his basic assumption that the progressive palatalization was an early development. As I pointed out, his chronology forced him "to posit an arbitrary phonetic development of the diphthongs and an arbitrary morphological replacement in the imperative, to reformulate the progressive palatalization as a subphonemic development, to dismiss the counter-evidence of petrified forms by questioning the material without offering a solution, and to exclude pronominal forms from consideration because they may have been subject to unknown developments" (1984:217). My second concern was Lunt's way of dealing with the earlier scholarly literature in terms of "prejudice" and "error", or worse, e.g. "Wukasch's ingenious discussion of rule ordering is completely vitiated by his failure to recognize that the 'historical order' of changes he starts with is purely a construct of Shevelov's reasoning (which I consider blatantly faulty as well as internally inconsistent), and therefore the proposed 'reorderings' — Wukas[c]h's own constructs — can make no contribution to linguistic theory" (Lunt 1981:62). It seems to me that this way of discussing one's predecessors does not contribute to a better understanding of the issues involved.

When I circulated my little article among a small group of colleagues before submitting it for publication, I received various reactions. On the one hand, I was advised that Lunt would probably answer my criticism by launching a personal attack or by looking for a way of taking revenge rather than by entering into a scholarly debate. I thought that this should not keep me from publishing my criticism. On the other hand, I was asked why I did not discuss other objectionable aspects of Lunt's work, such as the curious organization of his little book. Since I was rather interested in his methodology of dealing with facts and opinions, I preferred to leave such aspects out of consideration.

It was not to be expected that Lunt would welcome my criticism. He did not. As a matter of fact, he was so enraged that he needed forty pages and more than a hundred footnotes in order to vent his gall in this journal (1987). The result is a curtain of offensive language which largely serves to conceal the substantial issues from the reader's view. It is a perfect exemplification of the second point I was concerned with in my criticism. His reaction will stand as a monument of bad taste and bad manners. In the following I shall limit myself to reviewing what can be gathered from his text with respect to the specific issues mentioned above.

2. Lunt clearly recognizes that his formulation of the progressive palatalization "implies that the older diphthongs **ai* and **ei* no longer ended in **i*, and I suggest that the system had evolved modified diphthongs, **ie* < **ei* and **ae* < **ai*" (1987:270). The only reason for his ad hoc assumption of "modified diphthongs" is that he wants to avoid the straightforward solution that "the palatalization was posterior to the monophthongization of the diphthongs, which yielded long monophthongs of the timbre **ā*, **ē*" (Kortlandt 1984:214).

In his earlier treatment Lunt assumed that **ei* yielded **eē* in early Slavic (1981:19f.), but this possibility apparently looks too much like a monophthongization to him now. He therefore returns to an oral suggestion by his teacher Roman Jakobson and proposes "that the Baltic development suggests a plausible solution: **ai* has remained (cf. Lith. *atlaikas* 'remainder', OCS *отълѣкъ*) but **ei* often has metathesized (cf. Lith. *mielas* 'dear', OCS *миль*). Therefore I now suggest that in early Slavic **ai* > **ae* but **ei* > **ie*" (1987:260). Unfortunately, Lunt is not sufficiently familiar with the Baltic

material. Not only **ei*, but also **ai* yielded *ie* in the East Baltic languages, e.g. Lith. *dieveris* 'brother-in-law', *piemuõ* 'shepherd', Gr. *dāēi*, *poimēn*. Moreover, this is not the result of a metathesis, but of an early monophthongization followed by a much later diphthongization (cf. Stang 1966: 52-68 and Kortlandt 1977: 323-328). The monophthongization was limited to the stressed syllable, as Lunt might have detected if he had written accent marks on *ātlaikas* and *mīelas*. The original distribution was obscured by later developments, such as the retraction of the stress from antevocalic *i* which gave rise to metatony. The Lithuanian diphthongization to *ie* was limited to the Aukštaitian dialects (cf. Zinkevičius 1966:503). The Latvian diphthongization was probably posterior to the elimination of the nasal vowels, which shared the development, e.g. *pieci* 'five'. The available evidence actually points to the 17th century for the Latvian diphthongization, and the raised diphthong *ie* does not appear before the 19th century (cf. McKenzie 1918:156-161). It turns out that the Baltic parallel can only be adduced in support of a monophthongization of **ei* to **ē* and later raising to **ī* in Slavic. Similarly, **au* was monophthongized to **ō* and later raised to **ū*, as is clear from the loanwords which have *uo* in Latvian but *ū* in Lithuanian, e.g. Latv. *bljuõda*, *puõsts*, Lith. *bliũdas*, *pũstas*, Russ. *bljúdo* 'dish', *pustój* 'empty'. These words were apparently borrowed at a time when **ō* had already been diphthongized to *uo* in Lithuanian, but not yet in Latvian, so that Slavic **ō* from **au* was best rendered by **ō* in Latvian and **ū* in Lithuanian (cf. McKenzie 1918:168-171).

When Lunt changed his mind about the development of **ei* in Slavic, he evidently did not calculate the consequences for the vowel system as a whole. Apart from the developments **ei* > **ie* > **ī* and **ai* > **aē* > **ē* he now assumes **ai* > **aē* > **eē* > **ī* after **j* (1987:272) and **ai* > **aē* > **ei* > **ī* after progressively palatalized **k* (1987:284). This requires an additional rule for „not only a fronting but a raising of the original *a* between the /+high/ glide or consonant and /-low/ vowel” (fn. 59). Pure hand-waving, as they would say at MIT.

3. Concerning the other points mentioned above, Lunt only repeats his earlier views and does not offer anything new in his reaction. For plural imperatives such as *řbcēte* 'say' Lunt is "inclined to believe that the *-ě-* in these verbs is a morphological

replacement" (fn. 30). The point is that the only reason to assume a morphological replacement is Lunt's postulate that the progressive palatalization was an early Slavic development. Moreover, this postulate can only be maintained if the progressive palatalization is defined as a subphonemic development. In distinctive terms, the progressively palatalized **k* remains a variant of /*k*/ not only after the first regressive palatalization (Lunt 1981:27f.), but may have remained so until it merged with the outcome of the second regressive palatalization after the monophthongization, as appears from Lunt's table II (1987:284). If Lunt would not insist on "vowel adjustment" after this subphonemic variant of /*k*/, the form *rbčēte* would be regular. However, this possibility is complicated by his a priori assumption that the root vowel of *igo* < **yugom* 'yoke' was delabialized before the "vowel adjustment", a factor which he evidently did not take into account when he drew up his table II.

The root of the problem is that Lunt desperately wants the locative forms *otbci* "father" and *otbcixъ* 'fathers' to be phonetically regular and goes out of his way to accommodate the contrary evidence in order to save his cherished idea. Apart from the imperative *rbčēte* and the Russian gen.sg. form **stbgy* 'path', which "should be **stbzě*" (Lunt 1981:31), he has to explain away such form as *lbzě* 'permitted', which is usually considered to be a petrified dative-locative (cf. Vaillant 1950:55, 1977:84), and inst.sg. masc. *vbšěmbъ* 'all', *sicěmbъ* 'such', gen.-loc. pl. *vbšěxъ*, *sicěxъ*. He hypothesizes that *lbzě* represents a nominative **lbza* which was eliminated from the language and then reintroduced by Cyril and Methodius in its Moravian form **lbza*, written *lbzě* because of "confusion in spelling" (1981:35). This is sheer fantasy. Lunt dismisses the pronominal forms because "pronouns are far more likely to develop peculiar innovations along with haphazard rearrangements of old materials" (1981:36, similarly 1987:280). He ignores Van Wijk's observation that "in der nominalen Flexion sind die Kasus mit *ě*-Vokalismus weniger zahlreich als in der pronominalen" and therefore more liable to analogical replacement (1931:68, similarly but more explicitly 1950:306f.). Lunt's dismissive attitude is at variance with the principle that facts and opinions must be taken seriously.

After dismissing *vbšъ* as evidence because it may have been subject to unknown developments, Lunt announces that the Novgorod birch-bark documents, where we find both *vbx-* instead of *vbš-*

and absence of the second regressive palatalization, support his assumption that the progressive palatalization “was a process completed before the rise of *x” (1987:283). This is indeed a remarkable way of handling the evidence. The best explanation for the distribution of palatalization in the available material has been proposed by Willem Vermeer: “The most striking properties of the North Russian consonant system can be attributed to two otherwise general Common Slavic innovations which, however, reached the Slavic dialects of the Novgorod/Pskov area in the reverse order” (1986:512). His thesis is that the monophthongization of diphthongs reached the area later than the progressive and second regressive palatalization, so that the conditions for the rise of new palatal consonants were different from those we find elsewhere. As a result, the regressive palatalization did not operate at all and the progressive palatalization gave rise to a marginal series of new palatal consonants in stem-final position. After the monophthongization of diphthongs, the new alternation between velars and palatals could easily be eliminated by restoration of the velars, except for the fact that “/č/ occurred in several productive suffixes” and therefore “was much more firmly entrenched in the system” than the palatal alternants of *g* and *x* (Vermeer 1986:510). This theory offers a possibility of accounting for the absence of palatalization in such forms as dial. (Rjazan’) *otĕk* ‘father’ and ORuss. *varjagŭ* ‘Varangian’ in a straightforward way (cf. Shevelov 1964:346-350).

4. As I said above, I will not go into the kaleidoscope of personal insults which constitutes the larger part of Lunt’s article. It may nevertheless be useful that I briefly comment on a number of points where Lunt’s misrepresentation can easily confuse the unsuspecting reader.

“The remarkably different environments of the progressive and the second regressive palatalizations are strong reasons for assuming different chronology until that possibility can be definitely ruled out” (fn. 19). I would hold that “since they produce the same outcome it seems most rational simply to identify them” until we find evidence to the contrary (Vermeer 1986:506). Otherwise it must be explained why two distinct innovations yielded the same outcome.

According to Lunt, I “firmly assert” that the raising of **o* to **u* in acc.sg. masc. and gen.pl. **-om* must be dated to the Balto-Slavic period (fn. 21). He does not inform his readers that in the very sentence he quotes I refer to two articles which I have written on the subject.

According to Lunt, I assume “an intermediate long-vowel system with three front vowels, \bar{i} \bar{e} \bar{e} and, floating unnoticed in the background, the reflex of $*ai$ ” (fn. 25). Since $*ai$ yielded $*\bar{e}$ when $*ei$ yielded $*\bar{e}$, the floating background is limited to Lunt’s creative rhetoric.

“Kortlandt’s formulation of Mono saddles him with a merger of $*ai$ and $*\bar{e}$ ” (fn. 37). This is correct for the position after dentals and labials, where both are reflected as OCS. \bar{e} . It is incorrect for the position after velars and palatals, where the distinction is reflected as \bar{e}/i versus a (cf. Kortlandt 1979:266).

“Mareš chose to specify delabialization; I did not” (p. 266) and “Mareš explicitly posits *phonetic* delabialization” (fn. 42). I quote: “Rounding was not distinctive from the beginning of Slavic (as opposed to Baltic where some contrast between $*\bar{a}$ and $*\bar{o}$ was retained), as Mareš 13 recognized” (Lunt 1981:60f), and: “Wir glauben aber, dass auch hier der phonologische Funktions- und nicht der Detailwert der entscheidende Faktor bei den Artikulationsverhältnissen ist” (Mareš 1969:14). Lunt’s “emphasis added” (1987:265) does not change the basic fact that both authors assume an early Slavic phonological delabialization. The main difference is that Mareš’ formulations are more careful and less equivocal.

“Surely the reference to other languages — a reference Kortlandt did not tell his readers about — is incompatible with the notion of synchronic underlying forms” (fn. 46). Correct. I was commenting on Lunt’s claim that “internal reconstruction alone suffices to establish a stage where every desinence in the OCS twofold declension begins with $*a$ or $*\bar{a}$ ” (1981:16). Lunt confuses internal and comparative reconstruction.

Lunt writes about his failure to cite his predecessors that “if Mareš originated the notion, I am glad to give him credit”, but finds Ebeling “culpable” of the same negligence (fn. 50). He does not consider the possibility that Ebeling may have “originated the notion” independently, or earlier than Mareš.

“A question not even suggested by Kortlandt is the loss of the formant $*j$ ” (fn. 56). As a matter of fact, the loss of $*j$ plays a central role in my account of the historical phonology of Slavic (e.g., 1975:30, 1979:270, 1982:186, cf. now Vermeer 1984:362 and *passim*). Lunt is simply unaware of the problems involved because he disregards the prosodic evidence.

“Kortlandt posits a ‘first palatalization’ change whereby /ke kē/ ‘were rephonemicized as’ /čä čā/ (1979:266), a manoeuvre effected by means of the presupposition of ‘the existence of the archiphonemes’ /ä ā/. Telescoping several articulatory changes into a single move relieves him of the responsibility of establishing a chronological sequence” (fn. 58), and: “Kortlandt in his ‘second palatalization’ again avoids discrete chronological steps” (fn. 60). The rephonemicization of /ke/ as /čä/ is the transfer of the distinctive palatal feature from the vowel to the preceding consonant, which is the essence of a phonemic palatalization. As a result, we now have a phoneme /č/, after which the distinction between /e/ and /a/ is neutralized in the archiphoneme /ä/. There is no “telescoping of several articulatory changes” because there are no articulatory changes. Jakobson’s 1947 theory to which Lunt adheres does not account for the transfer of a distinctive feature from one segment to another, as in the case of a palatalization. Lunt’s theoretical framework prevents him from seeing the facts in their proper perspective.

“Kortlandt’s source” and “the scholar who proposed this” (fn. 67) refer to Torbiörnsson and Rozwadowski, whose names Lunt does not mention. Treating

one's predecessors as non-persons has at times been fashionable in some countries but is at variance with the principle that earlier scholarship must be examined respectfully. Lunt writes about OCS. *rodosъ, narodotsъ, to estъ*: "This is no longer 'Common Slavic' but western (Bulgaro-) Macedonian dialect (or else East Slavic dialect, as in the Dobrilo Gospel of 1164)". Rozwadowski argued that we find this *o* in texts which otherwise preserve the jers unchanged, and that we also find it in Polish, Czech, and East Bulgarian, where *o* cannot have developed from a jer, e.g. OCz. *večeros*: "Nie może to być zatem nic innego jak resztką starego *-o < -os*" (1914:16). Lunt orates about "credibility as a Slavic historical linguist" but does not bother to look.

"*j* at phonetic and morphological levels" (fn. 70): this does not preclude the absence of phonemic /j/.

"The laryngeals, Kortlandt's major original claim of 1975, are not even mentioned in 1979" (fn. 72). The reason is that I tried to make my article accessible to those colleagues who, like Lunt, are not sufficiently familiar with the prehistory of Slavic accentuation and may therefore have difficulty in interpreting the prosodic evidence. Those who are familiar with the subject will have no difficulty in adding tones and quantities.

I reconstruct a high nasal front vowel **ɛN* which yielded South Slavic *-eN* and North Slavic *-ě* in the acc. pl. endings of the *jo-* and *jā-*stems, adding in a footnote: "This conclusion was first drawn by N. van Wijk" (1979:259). Lunt comments: "Quite the contrary" (fn. 73). Van Wijk actually proposed "dass *-ě* und *-e*, beide lautgesetzlich auf eine ältere Gestalt zurückgehen" (1916:461). I designate this "ältere Gestalt" as **ɛN*, which is the outcome of earlier **-jons* and **-jāns* after the loss of **-s* and the delabialization of high rounded vowels (1979:267f.). Similarly, I reconstruct "an unrounded nasal back vowel, which I shall write **aN* and which yielded South Slavic *-y* and North Slavic *-a*" in the participial ending which developed from PIE. **-onts* (1979:260). Van Wijk compared the different developments of **-onts* and **-ons* with those of **-jonts* and **-jons* and concluded: "dann lässt sich aber das Verhältnis nordslav. *bera* : südslav. *běry* auf einen ursprünglich unbedeutenden dialektischen Unterschied in der Färbung des *o* zurückführen" (1925:284). After the loss of **-s*, the ending **-aN* yielded *-a* in the north and *-y* in the south (cf. also Kortlandt 1983:180).

Lunt asserts that my "change 12 yields eight nasals, while change 13 reduces it [sic] to six" (p. 277). Since these are pairs of nasal vowels in complementary distribution, the reader should substitute "four" and "three".

"What is the relationship of *acute-intonation* to a *laryngeal vocalic feature*?" (fn. 79). This is the subject of my *Slavic Accentuation* (1975). For the back quality of OCS. *sъto* 'hundred' beside Lith. *šimtąs* which seems to bother Lunt, I may refer to Trautmann (1923:4), as I did in my article (1979:264, fn. 19).

"This is well after the establishment of complementary distribution of front/back vowels by the action of VA (change 6), and I am puzzled that Kortlandt lists three what look very much like front/back nasal vowel pairs in the systems of change 13 and 14" (p. 277f.). This is all wrong. There is no complementary distribution between front and back vowels at stage 6, which cannot be identified with Lunt's "vowel adjustment" (which I reject, 1984:214f.), and there is nothing like front/back nasal vowel pairs at stages 13 and 14, as will be clear to anyone who cares to have a look at the relevant pages of my article (1979:269f.).

Lunt grossly misrepresents my views in a deliberate attempt to avoid a real discussion. He misinforms his readers about the relevant issues and refuses to face the problems which arise from his own ill-considered assumptions.

5. Thus, it seems to me that Lunt's performance does not warrant the pompous haughtiness of his authoritarian rhetoric. His treatment is at variance with the four principles of scholarly behavior which he conveniently lists in a footnote (1987:261), kindly though rather surprisingly attributing them to the present author. "Adequate knowledge of the material is a prerequisite for any useful scholarly activity": Lunt does not show adequate knowledge (let alone "mastery", fn. 82) of the material in the case of the East Baltic development of **ai* and **ei* into *ie*, the prosodic consequences of the loss of **j*, and the traces of **-os* in Slavic. "The factual data must be carefully analyzed": Lunt introduces ad hoc assumptions in order to dismiss unwelcome counter-evidence in such cases as *rbčěte*, *lbzě*, *vbsěxb*. "Verifiable fact must be kept distinct from hypothetical constructs": Lunt exercises no restraint in mixing fact and fiction when he states that "nothing changes the major fact: all desinences begin with a low back vowel" (1981:16). And last but not least: "Opinions set forth in earlier scholarship must be examined seriously and respectfully". Lunt violates this principle when he deals with Rozwadowski, Torbiörnsson, Ebeling, Shevelov, Birnbaum, Wukasch, or anyone with whom he happens to disagree. He finds it difficult even to restrain his pretensions when assenting to the views of his greatest predecessors: "Meillet and Van Wijk intuitively grasped the morphophonemic relationships seven decades ago, although the specific concepts of phoneme and morphophoneme had not yet emerged" (1987:277, fn. 76). It is unclear how this attitude can be tolerated in a "teaching and research milieu, where investigation takes for granted free and open discussion about data, assumptions, procedures, speculations, theories", and where it is "a matter of course that arguments and conclusions are subject to critical evaluations" (fn. 82).

FREDERIK KORTLANDT
Cobetstraat 24
NL-2313 KC LEIDEN
HOLLAND

REFERENCES

- Kortlandt, Frederik
 1975 *Slavic accentuation A study in relative chronology* (Lisse Peter de Ridder)
 1977 Historical laws of Baltic accentuation, *Baltistica* 13/2, 319-330
 1979 On the history of the Slavic nasal vowels, *Indogermansiche Forschungen* 84, 259-272
 1982 Early dialectal diversity in South Slavic, *Studies in Slavic and General Linguistics* 2 177-192
 1983 On final syllables in Slavic, *Journal of Indo-European Studies* 11, 167-185
 1984 The progressive palatalization of Slavic, *Folia Linguistica Historica* 5/2, 211-219
- Lunt, Horace G
 1981 *The progressive palatalization of Common Slavic* (Skopje Macedonian Academy of Sciences and Arts)
 1987 The progressive palatalization of early Slavic Opinions, facts, methods, *Folia Linguistica Historica* 7/2, 251-290
- Mareš, Frantisek V
 1969 *Diachronische Phonologie des Ur- und Fruhslavischen* (Muncheu Otto Sagner)
- McKenzie, Roderick
 1918 Notes sur l'histoire des diphtongues *ie* et *uo* dans les langues baltiques, *Bulletin de la Societe de Linguistique de Paris* 21 156-174
- Rozwadowski, Jan
 1914 Przyczynki do historycznej foneyki językow słowiańskich, *Rocznik Slawistyczny* 7, 9-21
- Shevelov, George Y
 1964 *A prehistory of Slavic The historical phonology of Common Slavic* (Heidelberg Carl Winter)
- Stang, Christian S
 1966 *Vergleichende Grammatik der baltischen Sprachen* (Oslo Universitetsforlaget)
- Trautmann, Reinhold
 1923 Ein Kapitel aus der Lautlehre der baltisch-slavischen Sprachen, *Slavia* 2, 1-4
- Vaillant, Andre
 1950 *Grammaire comparee des langues slaves I Phonetique* (Lyon IAC)
 1977 *Grammaire comparee des langues slaves 5 La syntaxe* (Paris Klincksieck)
- Vermeer, Willem R
 1984 On clarifying some points of Slavonic accentology The quantity of the thematic vowel in the present tense and related issues, *Folia Linguistica Historica* 5/2, 331-395
 1986 The rise of the North Russian dialect of Common Slavic, *Studies in Slavic and General Linguistics* 8, 503-515

Van Wijk, Nicolaas

- 1916 -e und -ě im Akk.Plur. der jo-Stämme und im Gen.Sing., Nom.Akk.Plur.der jā-Stämme, *Archiv für slavische Philologie* 36, 460-464.
- 1925 Zur Entwicklung der partizipialen Nominativendung -onts in den slavischen Sprachen, *Zeitschrift für slavische Philologie* 1, 279-286.
- 1931 *Geschichte der altkirchenslavischen Sprache 1: Laut- und Formenlehre* (Berlin: Walter de Gruyter).
- 1950 K istorii fonologičeskoj sistemy v obščeslavjanskom jazyke pozdnego perioda, *Slavia* 19, 293-313.

Zinkevičius, Zigmās

- 1966 *Lietuvių, dialektologija: Lyginamoji tarmių, fonetika ir morfologija* (Vilnius: Mintis).