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WHAT IS THE ORIGIN OF THE BALTO-SLAVIC ACUTE?

In a recent article, Miguel Villanueva Svensson (henceforth MVS) distinguishes “two major approaches in modern scholarship” (2020, 647):

- (1) “What may be termed the traditional theory (going back, in essence, to Saussure 1894), identifies the Balto-Slavic acute with (post-)Indo-European length (of whatever origin).”
- (2) “The second approach focuses on the widespread characterization of the acute as glottalization. [...] This is Kortlandt’s theory”.

According to MVS (l.c.), “Kortlandt’s theory [...] starts from extraordinarily marked premises at the Indo-European level (a specific version of the glottalic theory; an idiosyncratic theory of the origin of the PIE lengthened grade) and is characterized by a highly tendentious analysis of the Baltic and Slavic facts.” In fact, none of these allegations can be substantiated.

According to the glottalic theory, the traditional unaspirated voiced stops of the Indo-European protolanguage were actually glottalized (for the evidence see Kortlandt 2021). This is not “a

specific version of the glottalic theory” but simply the universally agreed essence of the glottalic theory.

My explanation of the PIE lengthened grade is not “an idiosyncratic theory” but the theory put forward by Jakob Wackernagel in the 19th century (1896, 66–68). It is in agreement with Saussure’s view that “à part deux ou trois cas spéciaux (...) l’alternance *e – ē* n’est pas indo-européenne” (1894, 428). This theory is reductionist in the sense that the number of possible reconstructions is kept to a minimum. It is thereby superior to later theories that allow students the freedom to posit any variety of prehistoric formations with lengthened grade vowels for which there is simply no evidence (cf. Kortlandt 2012).

The allegation that my analysis of the Baltic and Slavic facts is “highly tendentious” is supported by a reference to Kapović (2019) without mentioning my reply to this cascade of personal insults and offensive remarks (see Kortlandt 2020). As I pointed out in my reaction, Kapović disregards the chronological aspects of linguistic developments, disregards the linguistic

system in which developments take place, and multiplies the input criteria of his rules in order to arrive at the correct output. My chronology is the result of an integration of the developments established during the past 150 years of historical linguistic research into a coherent chronological sequence. This is not the place to go into a detailed account of the Slavic data, for which I refer to my article. It must be regretted that Kapović's way of presenting the evidence misinforms his readers and obscures the issues and that MVS uncritically mentions the article as a useful contribution.

MVS states that “it is hard to imagine how nom.sg. *ǵʰuēr ‘wild animal’ could have ended up as Bl.-Sl. *žuẽʀis or *žueʀis (Lith. žvėris AP 3)” (Villanueva Svensson 2020, 648). I agree, and this is because there is no such thing as spontaneous glottalization, which is unattested anywhere in the world. This is precisely why the “traditional” theory is mistaken. In my view, we have to reconstruct PIE *ǵʰueʀ- (cf. Kortlandt 2012, 251), with shortening in Latin *ferus* < *fēros in accordance with Dybo's shortening

rule (cf. Dybo 1961 and Kortlandt 2007, 28). Thus, the argument MVS adduces disproves his own theory.

Both MVS (2011) and Jasanoff (2017) still maintain that the Balto-Slavic reflex of the PIE lengthened grade was acute. Referring to these publications, MVS does not mention my reaction to them (see Kortlandt 2012 and 2018, respectively, and Pronk 2012). Here again, there is no reason to repeat the arguments put forward in the discussion. Suffice it to say that they are largely based on mistaken data and misrepresentation of the evidence. There is simply no evidence for an acute as the phonetic reflex of a lengthened grade vowel in Balto-Slavic.

The real topic of MVS's article is Osthoff's law and the role it played in the development of the Balto-Slavic prosodic system. He concludes that “Osthoff's law does not go back to Balto-Slavic” (Villanueva Svensson 2020, 660). Elsewhere I have argued that there is no evidence for Osthoff's law anywhere in the ancient Indo-European languages (Kortlandt 2014, 220, fn.1). It appears that MVS and Jasanoff never understood the ultimate consequences of the laryngeal theory.

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