BALTISTICA XXXIX (2) 2004 171–177

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BALTO-SLAVIC ACCENTUATION: TELLING NEWS FROM NOISE

The recently appeared Festschrift for Jens Elmegård Rasmussen contains a short article by me called "Acute vs. circumflex: Some notes on PIE and post-PIE prosodic phonology" (Jasanoff 2004). My discussion of this topic seems to have irritated Frederik Kortlandt, whose reply appeared in the last issue of *Baltistica* (Kortlandt 2004). To go by what Kortland says, my outdated and ill-informed views, though partly redeemed here and there by an occasional (unacknowledged) overlap with his own positions, ignore the fact that "our knowledge of Slavic accentuation has increased dramatically" since the early 1980's. He concludes that my "distorted picture of Balto-Slavic accentuation is a result of insufficient knowledge of the data and the scholarly literature. Some news travels slowly, alas."

K does not say explicitly what news he is talking about, but a look at his bibliography provides a clue. Out of the sixteen works that he cites with publication dates of 1970 or later, twelve are authored by him. Evidently K believes that it is no longer possible to say anything meaningful about Balto-Slavic accentuation – not even in the context of a nine-page Indo-European-focused overview that also discusses Indo-Iranian, Greek, and Germanic – without building on his own results, his "news." Here he is simply mistaken.

K's "news travels slowly" motif is a rhetorical ploy, designed to save himself the trouble of having to deal with a critic – in this case, me – who is quite familiar with his views and rejects them. The prosodic phonology of PIE and its daughter languages is a highly contested area, in part because terms like "accent" and "tone" are used in imprecise and inconsistent ways, in part because the written record is often uninformative about prosodic distinctions, and in part because the facts are genuinely difficult to sort out. My article, insofar as it dealt with the issues that exercise K, was consciously motivated by a desire to cut through the tangle of secondary hypotheses and "laws" that clutter the ground in the field of Balto-Slavic accentology, and to see how much we could learn by methodically exploiting the relatively few facts that we really *know.* Three such facts served as my point of departure: 1) **-VHV-* sequences remained distinct from other sources of long vowels within the PIE period; 2) such sequences yielded trimoric long vowels in final syllables in Germanic; and 3) Germanic trimoric vowels in final syllables correspond to Balto-Slavic circumflex vowels in final syllables. From these observations I drew three conclusions: 1) Balto-Slavic

must also once have had trimoric vowels; 2) the circumflex : acute contrast in Balto-Slavic must reflect an earlier hyperlong / trimoric : long / bimoric contrast; and 3) the acute vowels of Balto-Slavic must therefore have been the shorter or "checked" (i.e., clipped, presumably glottalized) versions of their "unchecked" circumflex counterparts. This was my explanation – economical if nothing else – of the rise of the prosodic feature that we know as "acuteness."

K was not impressed by any of this[1]. He agrees that **-VHV-* sequences remained distinct from other sources of long vowels in PIE, but rejects my claim that such sequences yielded trimoric vowels in Germanic. The evidence for trimoric long vowels in Germanic, he says, "is fully explained in terms of segmental features and well-motivated analogical developments." He offers no justification for this remarkable assertion, other than to repeat his often-stated belief that the PIE gen. pl. ended in **-om*, with a short vowel. Readers wanting to know more about K's views on the Germanic *Auslautsgesetze* might consult B o u t k a n (1995), which appears to reflect many of his ideas. Not many outside K's circle will be, or have been, persuaded.

The point on which K and I *do* agree, of course, is that the Balto-Slavic acute was a kind of *stød* or broken tone. K's suggestion that I somehow owe this idea to him is incorrect. Our positions were arrived at via different routes and for different reasons. For me, acuteness arose through a shift of phonological markedness: overlong nuclei (marked) became long nuclei of the normal type (circumflex, unmarked), while long nuclei that were *not* overlong (unmarked) became long nuclei of the "checked" type (acute, marked). For K, who refuses to reckon with overlong vowels in pre-Balto-Slavic, the broken tone of an acute vowel arose by *direct contact with a contiguous glottalic stop or laryngeal.* This is the nub of his theory, and it is an extremely difficult position to maintain. The "glottalic theory" (cf. G a m k r e l i d z e, I v a n o v 1972, 16), which asserts that the traditionally reconstructed PIE voiced stops (**b*, **d*, **g*, etc.) were in fact "glottalized" or ejective (**p'*, **t'*, **k'*, etc.), excited a brief flurry of interest in the 1970's and 80's, but was never generally adopted and has now mostly been discarded. A glottalic-type system, to judge from petrified lexical facts like the bar against two voiced (< glottalic?) stops in the same PIE root, may well have existed in *pre*-PIE. But no IE branch – not even (*pace* K) Germanic and Armenian – escaped the change of the alleged PIE glottalic stops to voiced stops. To assert, as K does, that glottalic stops were responsible for intonational effects within the post-PIE history of Balto-Slavic is *ipso facto* to maintain that the change of **p'*, **t'*, **k'* to **b*, **d*, **g was an independent change in every IE tradition.* This too is news – bad news.

[1] If indeed he took the trouble to read it carefully; his account of my views is so inaccurate that there is room for doubt on this score. Note that, contrary to what K says, I emphatically do not "stick to the neogrammarian assumption of tonal distinctions."

To make the laryngeal part of his theory work, K is obliged to posit an elaborate, almost wholly ad hoc choreography of rules relating to laryngeal loss and retention. That there were three laryngeal consonants in PIE is not in doubt, nor is the fact that one or more of them may have been phonetically accompanied by glottal constriction. But in every other branch of the family outside Anatolian the laryngeals were lost very early, vocalizing in some environments and disappearing, with or without associated side effects, in others. There were no segmental laryngeals in the later phases of languages like Greek, Italic, or Germanic – only long vowels, circumflex vowels, trimoric vowels, *a*-colored vowels, etc. This does not, of course, logically imply that the laryngeals *had* to disappear early in Balto-Slavic as well. But since Baltic and Slavic are fundamentally "normal" branches of the family in which laryngeals were eventually lost as thoroughly as they were everywhere else, there are good common-sense reasons to be skeptical of K's claim that segmental laryngeal reflexes survived in Slavic up to the time of the (very late) monophthongization of **ai* to *ě* and **au* to *u.* To see something of K's *modus operandi* in matters involving laryngeals, we will focus on a specific example.

In the long list of categories which K accuses me of having "missed," he mentions the circumflex in the 2, 3 sg. form of the sigmatic aorist, which he illustrates with SCr. *dâ* 'gave' (vs. 1 sg. *dȁh).* The segmental history of these forms is reasonably well known. The PIE starting point was a root aorist 1 sg. **déh₃-m* (> **dóh₃-m*), 2 sg. **déh₃-s* (> **dóh₃-s*), etc. (cf. Ved. *ádām*, *ádās*, etc.) – forms which, according to the *communis opinio* that K and I share, would have received an acute in Balto-Slavic. In Slavic, where the morphology of the *s*-aorist was extremely productive, the 1 sg. was remade to **dās(o)m*, still with acute; and the 1 pl. was remade to **das(o)mo(s)*, the 2 pl. to **dāste*, the 3 pl. to **dāsint*, etc. SCr. *dȁh*, *dȁsmo*, *dȁste*, *dȁše*, confirm the correctness of these reconstructions, including the acutes. The problem is to explain the intonation of the 2, 3 sg., for which we might have expected **dȁ* (< sigmatized **dȃs(s)*, **dȃst*), but where the actual form is *dâ*, with circumflex.

It is not our job to solve this problem here, but it is worth thinking about how we might *try* to find a solution. An ordinarily well-informed Balto-Slavicist / Indo-Europeanist would immediately think of two potential ways to explain the circumflex in *dâ*, one phonological, the other morphological. The phonological approach would be to try to exploit the most obvious and suggestive difference between pre-Slavic **dās(s)*, **dāst* and the rest of the aorist paradigm – the fact that they are monosyllabic. Monosyllables, typologically prone to lengthening as well as to normal end-of-word effects, are prosodically "special" in many languages – a fact implicitly acknowledged by K when he accepts Wackernagel's outmoded explanation of the lengthened grade in the PIE *s*-aorist (cf., e.g., Kortlandt 1988, 300–301). It is interesting to note

that a rule of "monosyllabic circumflexion" (i.e., de-marking of acutes in monosyllables) is proposed for Balto-Slavic by Rasmussen (1992, 188 ff.), in a discussion that also deals elegantly with some of the other forms that K considers "news." While Rasmussen has nothing to say about SCr. *dâ* in particular, it is clear that his rule – straightforwardly interpretable as a variety of monosyllabic lengthening and hence de- "checking" – would directly explain the circumflex.

A morphological approach to the problem of *dâ* would start by noting that the oldest *s*-aorists in Slavic – those associated with primary thematic presents of the type OCS *vedǫ* 'lead' (aor. *vědъ*), *vezǫ* 'convey' (aor. *vezoxъ* for **věxъ*), and *rekǫ* 'say' (aor. *rěxъ*) – suppletively employ their historical *imperfects* in the 2, 3 sg. (*vede*, *veze*, *reče*) in place of the expected true aorist forms (**vě*, **vě*, **rě*). This substitution, presumably motivated by the greater transparency of the imperfect, had the effect of giving the Slavic *s*-aorist a non-acute stem variant in the 2, 3 sg., while leaving the rest of the paradigm, which was based on an inherited lengthened grade (**u̯ḗdh-s-*, **u̯ḗĝh-s-*, **rḗk-s-*, etc.), acute (*pace* K)[2]. The pattern was actually more widespread: a similar acute : circumflex alternation occurred in the productive *i*-verbs, where the 2, 3 sg. had **-î* < impf. **-eі̯es*, **-eі̯et* (cf. PSlav. **nosî* 'brought'), while the other forms, with historical **-ī-s-*, had acute **-í-* (**nosíxъ*). Conditions were thus ripe for a proportional analogy: inf. **nosíti* : 1 sg. aor. **nosíxъ* : 2, 3 sg. aor. **nosî* :: inf. **dắti* : 1 sg. aor. **dắxъ* : 2, 3 sg. aor. *X*, where *X* was solved as **dâ.*

On the face of it, then, the circumflex of SCr. *dâ* would not seem to pose an insoluble problem. Let us now see what K has to say about it. K is evidently not bothered by the fact that pre-Slavic **dās(o)m*, **dās(s)*, **dāst*, etc. was a sigmatized root aorist; he treats it exactly as if it were an inherited *s*-aorist with a sigmatic paradigm going back to PIE. This allows him to set up a paradigm with the same eccentric ablaut pattern that he assumes for the *s*-aorist in general – lengthened grade in the 2, 3 sg. only (!) and full grade elsewhere. The Slavic acute forms (**dắxъ*, SCr. *dàh*, etc.) thus go back, according to K, to preforms in **dóh₃-s-*, with *full grade*, while the circumflex 2, 3 sg. (**dâ*, SCr. *dâ*) goes back to *lengthened-grade *dṓh₃-s-s*, **dṓh₃-s-t.* In the latter forms a special sound change deleted the laryngeal after the long vowel, giving **dṓs(s)*, **dṓst.* These, K says, regularly received circumflex intonation.

It is not easy to know where to begin listing the improbabilities of this scenario. Even if K's views on the ablaut of the *s*-aorist were well-motivated, it would be hard

[2] Circumflexed forms like those cited by K (SCr. 1 sg. *dònijeh* 'I brought', *ùmrijeh* 'I died', etc.) are, *when based on genuinely old lengthened-grade s-aorists,* due to intonational leveling of the well-attested Baltic and Slavic type. But the caveat is important: note that the pre-Slavic aorist **mer-s-* 'die', as the replacement of a PIE middle root aorist (3 sg. **mér-to*), probably never had a lengthened grade at all.

to believe that the post-IE sigmatization of 3 sg. aor. **déh₃-t* [dóh₃t] could have been attended by an actual change from *e*-grade to *ē*-grade – especially since a 3 sg. **dḗh₃-s-t* would, by Eichner's Law, have been realized as [dḗh₃-s-t], not [dóh₃-s-t]. And in any case, K's idea that only the 2 sg. and 3 sg. of the *s*-aorist had lengthened grade is directly contradicted by the facts of Indo-Iranian, Tocharian, and Slavic itself. The Vedic forms *stoṣam* 'I will praise' and *jeṣam* 'I would win', which constitute K's only evidence for the supposed priority of *guṇa* over *vr̥ddhi* vocalism outside the 2, 3 sg., are not, as he maintains, injunctives (so first Kortlandt 1987), but a subjunctive and a precative (i.e., optative), respectively[3]. Nor is there any independently convincing evidence for K's final step – the claimed sound change by which **dṓh₃-s-*, but not **dóh₃-s-* lost its laryngeal, thus allowing the acute of SCr. *dȁh*, *dȁsmo*, *dȁste*, *dȁše*, etc. to be linked to the presence of a laryngeal and the circumflex of SCr. *dâ* to the absence of one.

"Explanations" like this recall the work of the late American cartoonist Rube Goldberg, who was famed for his drawings of amusingly complicated devices to perform simple tasks. But K is a serious scholar, and he is not trying to be funny. That being the case, it is astonishing to see how elaborate a structure he is prepared to build on the *dȁh* : *dâ* contrast, using it to a) press his tendentious revision of the paradigm of the PIE *s*-aorist; b) support his view that such a "revised" *s*-aorist, with all its apophonic peculiarities intact, was substituted for the inherited root aorist of the root **deh₃-*; and c) justify a virtually untestable sound law as a *deus ex machina* to make the forms come out right in the end. The reason for all the apparatus, of course, is that K's real agenda is to bolster a conception of Balto-Slavic accentuation whose central tenet – that all acutes go back directly to glottalic consonants or laryngeals – is only defensible as part of an intricate fabric of special assumptions. When a circumflex, contrary to expectation, appears in a Slavic form *despite* the presence of a laryngeal, a rule must be found to delete the laryngeal precociously (*dâ* < **dṓst* < **dṓh₃-s-t*). And it then follows that if *dâ* goes back to a lengthened grade, *dȁh* must go back to a full grade.

All of this is interesting for the light it sheds on K's methodology, but it is not "news." Nor, for that matter, is there anything especially newsworthy about the other forms that K faults me for not having discussed. The non-acute lengthened-grade vowel of former root nouns like Lith. *gėlà* 'pain' and SCr. *čâr* 'magic' is better explained by a rule of monosyllabic circumflexion *à la* Rasmussen (see above), or by paradigmatic leveling between the nom. sg. (with lengthened grade) and the oblique

[3] *stoṣam*, despite its irregular secondary ending, is synonymous with and clearly inseparable from the subjunctives *stoṣāni*, *stoṣat*, and *stoṣāma*. On the important form *jeṣam* see Hoffmann (1967, 254) and Jasanoff (2003, 187).

cases (without it), than by K's principle that lengthened-grade vowels were circumflex by nature. The circumflex in the second syllable of remade nom. sg. forms like Latv. *âbuõls* 'apple' and SCr. *žȅrāv* 'crane' is probably analogical to the circumflex of root nouns and nom. sg. forms of the type Lith. *duktẽ* 'daughter' and *akmuõ* 'stone'. *Pace* K, there is no reason to reject the standard view that Lith. *srẽbė* 'sipped' owes its circumflex to the influence of the present *srebiù* and other inherited forms with a short vowel; the expected acute appears in the type Lith. *gė́rė* 'drank' (<*$g^{u}ērh_3$-), which K unconvincingly explains by direct reference to the laryngeal (K o r t l a n d t 1988, 300). The metatony observable in future forms of the type *duõs* 'will give' and *kalbẽs* 'will speak' – forms which K seems to confuse with *s*-aorists – is trivially explainable by the normal phonological processes of Lithuanian. And finally, the circumflex of the productive Baltic feminine suffix **-ē-* (cf. Lith. *žvaigždẽ* 'star', etc.) follows directly from its origin as a contraction product (<*-*iį̄ā*-); K's far-fetched attempt to extract the circumflex from nom. sg. forms of the type *arklìdė* 'stable' < **-dhēh$_1$(s)* has nothing to recommend it.

It must be emphasized, of course, that since I take the standard position that acutes go back to both tautosyllabic **-VH-* sequences and *(contra* K) old long vowels, most of what K finds fault with in my "system" is generic and has nothing to do with my particular views. One of his few more specific objections is to my characterization of the Lithuanian dat. sg. in *-ui* and instr. pl. in *-ais* as "secondarily circumflex": if these endings had originally been acute, he says (echoing S t a n g 1966, 67), they would have attracted the accent in accordance with Saussure's Law, like the 1, 2 sg. endings *-aũ* and *-aĩ* (cf. *sakaũ*, *-aĩ* 'I/ you say', 3 p. *sãko*). In my view, however, the "acute" properties of the latter two endings are analogical, typologically comparable to the acute of the athematic 1 sg. in *-mì* < **-mai*, which is modelled on thematic *-ù* < **-uo*. K also objects to my attempt to take both the nom. pl. in *-aĩ* and *-ì* < *-í(e)* from the PIE nom. pl. masc. in **-oi*; his solution, suggested by an obsolete theory of J. Schmidt, is to take *-aĩ* from **-oi* and *-í(e)* from the supposed neuter plural ending **-eh$_2$-i* (cf. Lat. *hae-c*). Quite apart from the obvious entity-multiplying inefficiency of this idea, no parallel cases are known to me where a nom.-acc. neuter form is selectively reinterpreted as an animate (masculine or feminine) nominative or an animate accusative, but not both. If a form like nom. pl. masc. *pilnì* 'full' were really an old neuter, its acc. pl. would probably have been **pilnì*, not *pìlnus.*

What should the non-specialist reader take away from all this detail? The object of my article, to the extent it had anything to say about Balto-Slavic at all, was to clarify the nature of the connection between the Balto-Slavic acute: circumflex distinction and the bimoric : trimoric distinction in Germanic. The conclusion I reached, as I have said, was that the "northern" post-PIE dialect area had both long and

hyperlong vowels, the latter mainly derived from original *-*VHV*- sequences in final syllables. Germanic retained this distinction, at least in final syllables; Balto-Slavic rephonologized it into an opposition between shorter, or "checked" (= acute) longs, realized with a *stød* or similar glottal feature, and normal-length (circumflex) longs, realized without it. It is not hard to see why K should object to this scenario: if true, it would destroy the basis for his own theory of the Balto-Slavic acute, which links the *stød* of the acute directly to the presence of a following glottalic stop or laryngeal.

So far, so good: a normal scholarly disagreement. But K crosses the bounds of good judgment in trumpeting his own opinions as "news". They are not that. They are his well-known litany of examples and personal interpretations, most of them unconvincing and some sharply at odds with long-established results of IE scholarship. Not news, alas, but noise.

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