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Opponent's review for habilitation thesis

### **The development of the IE clusters *obstruent + t/s(d<sup>h</sup>)***

of PhDr. Ondřej Šefčík, Ph.D.

The habilitation thesis of Ondřej Šefčík (Š.), investigates a particularly complicated problem of phonological development in the Indo-European (IE) languages, namely the changes of clusters consisting of a stop or sibilant and a following dental stop or sibilant (these latter being the most frequent obstruents in IE suffixes and endings). The development of these clusters is especially complicated because obstruents – in contrast to sonorants – show phonological contrasts in laryngeal articulation (e.g., voicing and aspiration etc, “modal classes” in Š.'s terminology) and thus conflicts between the specifications of adjacent obstruents can arise, in addition to the rather diverse place contrasts between stops (“local series”). Clusters of such sounds therefore are of special interest for the understanding of the phonology or PIE and its daughter languages, and they are also very relevant for the classification and phylogeny of IE since their developments can be used as branch-defining features shedding light on prehistoric relations between branches. This thesis is possibly the first complete investigation of this particular problem considering all branches of Indo-European. It therefore is broader in scope than the studies by Görtzen (1998) and Hill (2003) who only investigated the dental + dental clusters (the most intricate part); however, it is less deep within the range of this narrower focus. The main quality of the thesis is its comprehensive character and more general perspective, compared to studies of the individual branches or more special cases.

Generally the thesis is a solid scholarly work which shows good command of the data and methods of comparative and historical linguistics, especially Indo-European. Within Indo-European, all major branches are treated, and Š. shows that he is well-acquainted with their synchronic grammar and historical development, including the most recent literature. The general methodology of historical linguistics is followed and combined with a structural perspective, aiming at uniting similar sounds and environments as much as possible. This is an innovative viewpoint and can lead to less commonly





propagated views, especially of “spirantization trajectories” in cases where both the input and the output of the change show a stop and not a fricative. In some cases this may explain the data better than other solutions. E.g., the Slavic outcomes of palatalized *\*kt* (merging with *\*tj*) are possibly easier to understand from *\*xtʲ > \*çt* than from a cluster with preserved stop. It may also very well be the case that the fricativization of the first stop in such clusters in Germanic was much earlier than the sound shift (Rask/Grimm’s Law), as argued on pp. 221f.

One particular such hypothesis appears problematic to me, namely the “spirantization model” of Bartholomae’s Law (pp. 49f.), according to which the “voiced aspirates” were originally fricatives (“spirants”) and that the development of D<sup>h</sup>T clusters in Indo-Iranian went through a stage of two-fricative-clusters later undergoing fortition. Such a high amount of fortition even in the rather weak antecorsonantal position would be unusual and therefore require special reasons, but no strong arguments are given for it. That the model allows to avoid voiced aspirated sibilants may be considered an advantage but does not suffice to make the presupposed fortition plausible. This is also valid for the spirantization model preferred by Š. for the development of original palatalovelars in Indo-Iranian: It requires rather too many fortitions from sibilant to affricate or stop. The argument that the difference between *\*t+t > \*tst > \*tt* and *\*k+t > čt [tʃt] > ʃt* is harder to understand if we assume an affricate stage is only apparently valid, since an affricate stage on the path from *\*k* to *ʃ* is needed anyway, even if we assume an unconditioned development to *\*ś* already in Proto-Indo-Iranian. So if we accept *\*tst* as a PIE stage, there must have been a stage in which former *\*k̂t* was represented as [tʃt] (or [tct]) and the further development of *\*tst (> tt)* vs. *\*[tʃt] (> \*ʃt > ʃt)* was not parallel in Indo-Aryan. If *\*tst* were rejected (as Š. later does, at least as a possibility) the argument would become invalid anyway. Therefore I cannot follow in the conclusion (p. 246) that the “spirantization trajectory” is better for all branches other than Anatolian and Tocharian.

An interesting observation is that *\*ǵʰ + T > \*dʰ* yielded Vedic *ḍḍh* (with lengthening) but *\*k̂/ǵ + \*dʰ* (rare) always turned into *ḍḍh* (however, most of the latter examples might be analogical) but also here I would argue that this would rather favour the assumption of affricates in a prestage of Indo-Aryan.

There is also a number of less important problematic points or mistakes. In the following remarks I will mainly concentrate on Indo-Iranian, where my expertise is strongest:

P. 26f. The claim that *\*dʰ + dʰ* always yielded *ḍḍh* is not completely supported by the examples since Š. himself rightly added “(-ddh-)” after *indhvám*. After a further consonant, simple *dh* and *ḍḍh* do not contrast in Vedic, so the base form may still have had *ddh*.

P. 28f. Only one example of *ʃ+dʰ* is given, showing OIA *ḍḍh*, as in the overview table; however, in the summary texts „two variants, either as *ḍḍh* or as *oḍḍh*” are mentioned (which is correct). An example of the second case should be added (e.g., *\*riʃ-dhvam > riḍhvam* TB; *\*dʀ-ʃ-dhvam > dʀḍhvam* B).





P. 32 "YAv. *bəṛəyda-*" does not exist (unfortunately). We only find OAV. and YAv. *bəṛəxδa-* (as correctly given by all the references cited by Š.) which can only be derived from *\*bərx-θa-* < *\*bərg+tha-*, not from *\*bərg-da-*.

The important example of OAV. *dugəd(a)r-*, YAv. *duyδ(a)r-* 'daughter' is missing. Although this might be argued to be correct since the original cluster was *\*gh<sub>2</sub>-t*, not *\*g<sup>h</sup>-t*, and the rest of Iranian probably shows *\*xt*, the inclusion of the same word as an example in other languages would require some comment why it is not given for Avestan.

P. 34 OAV. *važdra-* is mentioned but YAv. *važdri-* is missing.

P. 36 OAV. *urūraost* is not an example of BL; OAV. *bazda-* and YAv. *basta-* are missing.

P. 42 derivation of Pr. *ḍogū* 'milk' from *\*dug<sup>h</sup>-ta-* is erroneous or would have required more argumentation.

P.42ff. False suggestion of a difference in outcome between *ǰ+s* and *T +s* by omission of the diacritic dot over *c* in Nur. words with the former cluster: cf. Ktv. *dačyu*, A. *daču*; KA. *kūc* (not *kūc̣*).

P. 47f. It is in fact not clear whether Indic really had „aspiration only on the right plosive“. In spite of the traditional spelling, there are good arguments for assuming breathy voice for the whole cluster which was however only heard at its end. So the traditional spelling only marks the phonetic cue but not the underlying specification, <ddh> being a geminate just as <tt>.

P. 66 The use of „i“ for an unclear approximant with different actual effects (even if explained in footnote 66) is disturbing. A more abstract symbol would be better.

P.69f. That one would have to assume an affricate also for *t+s* clusters by the „affricate trajectory“ has never been proposed before (as far as I remember) and lacks sufficient support: The traditional affricate theory claims that the affricates arose only before a following stop where they can be motivated. No such motivation exists for the position before a sibilant.

P. 87(ff.), 100, 105 The lack of a RUKI change of *\*s* after *k* in Lithuanian is surprising but all cited examples show the synchronically highly productive future suffix which is not ideal to demonstrate the regular outcome of a rather ancient sound law. Are there really no cases of older word formation showing the same phenomenon? Words like *áukštas* < *\*h<sub>2</sub>awg-s-to-* seem to be counterexamples, and the discussion of the literature as well as the formulations "fully operational", "probably due to analogy" and "analogical restorations of the older non-palatal sibilant" (p. 105) appear to point to such a solution but it remains a bit unclear if Š. really follows this.

P. 102 Balto-Finnic (BF) *\*h* does not continue "palatal sibilants": Uralic *\*š* is a non-palatal retroflex distinct from palatal *\*ś* or *\*č*. So the reflection of Baltic *\*š/ž* as *\*ś* > *\*h* would only show that the contact between BF and Baltic happened when either the Baltic sibilants had already become depalatalized, or when BF *\*ś* had already become depalatalized so that retroflex *\*š* was most similar to the Baltic postalveolar sibilants. However, the latter is problematic since these loans are also found in Saamic where original *\*č* was never depalatalized. So, perhaps, the Baltic loanwords were borrowed before the deaffrication of *\*č* in BF, so that the only postalveolar sibilant in BF would have been *\*š*.





P. 109f. The alleged cases for \*pt > st are all quite dubious. For an alternative etymology of the first example (\*stryb), cf. Gippert, Neues zu „Slavisch st aus älterem pl?“, in Peter Anreiter et al. (eds.), *Namen, Sprachen und Kulturen. Imena, Jeziki in Kulture. Festschrift für Heinz Dieter Pohl zum 60. Geburtstag*, 239-256. Vienna. Without this example, the other cases are not probable enough, and this “minor” development should rather be rejected (and even a fricative trajectory would imply an improbable change of \*ϕ > s).

P. 187ff. The possibility that also Latin went through a “spirantization trajectory” (as probably implied by caes such as *quīntus*) should have been discussed here (and not only shortly mentioned on p. 178).

P. 223ff. There is no discussion of the question of a fortis/lenis contrast in Hittite clusters, but this might be relevant here, cf. the recent contribution of Yates 2019 (IEL 7, 241-307). That the clusters in question represent a neutralization context cannot be taken for granted, as this is under discussion for Anatolian.

P. 234 The old derivation of Toch. B *laks* ‘fish’ from \**loks-* ‘salmon’ remains problematic.

P. 235 Toch. B *wästarye* is listed under tʰt/tʰs without a comment but seems to show a different development, as noted on p. 238.

The language and style of the thesis are generally appropriate, and considering its content, it is not difficult to read. The topic, the research questions and the proposed solutions are clearly formulated, and alternatives are critically discussed. According to my non-native judgment, there are some incorrect uses of the English definite article and other peculiarities, so before publication, checking by a native speaker is recommended.

There are but rather few clear typos or misprints, I have noted the following:

- Görtzen is sometimes wrongly cited as “Götzen”
- P. 20 “átapsīt” > átāpsīt
- P. 220 “secondary plosive” must stand for *secondary spirant*
- P. 232 \**pstém* must stand for \**pstén*
- P. 235 \**nätsw-* is mistakenly placed in the explanation of *orotstse*
- P. 242 the Albanian outcome of *Tt\** is given as “c” but it must be s

In sum, the thesis fulfills the standard requirements for a habilitation in the field of Indo-European Linguistics, although there would certainly be some room for improvement, as is usual for such complicated topics.

Vienna, June 2021