The Kortlandt Effect

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by

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Abstract

It has been observed that pre-PIE *d sometimes turns into PIE *h_1, also referred to as the Kortlandt effect, but much is still unclear about the occurrence and nature of this change. In this thesis, I provide an elaborate discussion aimed at establishing the conditions and a phonetic explanation for the development. All words that have thus far been proposed as instances of the *d > *h_1 change will be investigated more closely, leading to the conclusion that the Kortlandt effect is a type of debuccalisation due to dental dissimilation when *d is followed by a consonant. Typological parallels for this type of change, as well as evidence from IE daughter languages, enable us to identify it as a shift from pre-glottalised voiceless stop to glottal stop.

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Content

1 Introduction

A few decades ago, Kortlandt (1983) noticed an apparent tendency of pre-PIE *d to turn into PIE *h_1 in certain words, now sometimes referred to as the Kortlandt effect. It can be found in, for example, *$d\acute{k}_{\eth}tom$ > *$h_1\acute{k}_{\eth}tom$ '100'. After having been underexposed for a while, the topic has recently been picked up on again by several scholars. The existence of the development is getting more and more acknowledged and in the examples adduced thus far, the change seems to have occurred when the *d was followed by another consonant and possibly in a context of dental dissimilation. However, its exact conditions and phonetic implications are as of yet unclear and require more research. In this thesis, I will aim to provide more clarity on the overall nature of the sound change *d > *h_1.

Several works have been written about this phenomenon already. Kortlandt (1983: 98) was the first to reconstruct a development *d > *h_1. He originally proposed this sound change in order to explain the shape of numerals in Greek that were otherwise problematic when comparing them to their cognates in other Indo-European languages. An example is the word for '100'; reflexes like Lat. *centum* and Skt. *śata* suggest a PIE form *$\acute{k}_{\eth}tom$, but this cannot account for the initial vowel found in Greek ἑκατόν. Due to a semantic connection with *$de\acute{k}_{\eth}$ '10', '100' was already often traced back to *$d\acute{k}_{\eth}tom$, but the exact development of its initial *d has not always been clear, as it is not found in any of the daughter languages and therefore must have been dropped at a (pre-)PIE stage already. Kortlandt argues that we should indeed reconstruct *$d\acute{k}_{\eth}t\acute{o}m$ and assumes a development into *$h_1\acute{k}_{\eth}tom$ very early on, explaining the initial vowel in Greek[1] and the lack thereof in other Indo-European languages. Another form that can be explained this way is Gr. πεντήκοντα '50', with η reflecting a long vowel in PIE in a position where neither the known ablaut patterns nor individual lengthening sound laws of PIE would predict one to be. A reconstruction *$penk^wed\acute{k}omt$ had already been proposed, but thus far it had not been explained how the disappearance of *d caused the preceding *e to be lengthened. By assuming *d became *h_1, this problem would be solved. Phonetically, Kortlandt argues this development of *d could be explained in the light of the Glottalic Theory. In his view, *d > *h_1 was a debuccalisation development from a (pre-)glottalised stop to a simple glottal stop when in contact with another consonant, a stop in this case.

In later years, other putative examples of a development of pre-PIE *d > PIE *h_1 have been proposed. For instance, Lubotsky (1994) derives Vedic *ávidhat* 'he allotted' from a root *$h_1\underset{\frown}{u}id^h$-, going back to earlier *$d\underset{\frown}{u}id^h$-. He describes the development as a change of *d to *h_1 before *$\underset{\frown}{u}$ due to dissimilation with *d^h in the following syllable. A later idea by Kortlandt (2010: 41) is aimed at reconciling the Anatolian instrumental ending *-t with the more widespread ending *-h_1 we find in other IE languages. He suggests to reconstruct *-t as the original form, which after the split of Anatolian became *-d and changed into *-h_1 word finally. Lubotsky (2013) also adds Vedic *vār* 'water' to the list, which would belong to the Vedic paradigm of *udán-* 'water'. In order to regularly derive *vār* and *udán-* from the same root, he derives it from *$\underset{\frown}{u}eh_1r$- < *$\underset{\frown}{u}edr$-. He therefore assumes an additional environment where the Kortlandt effect occurs, namely when *d is followed by *r. Garnier (2014) provides an elaborate investigation of the development *d > *h_1, showing with numerous examples that it is indeed a rather widespread phenomenon, and introduces the term *Kortlandt effect*. According to him, the Kortlandt effect could provide more clarity in the following domains:

[1] The aspiration of the Greek ἑ, Kortlandt assumes, must somehow have been taken over from ἕν 'one'. I will come back to this problem later.

1. Allomorphy, already in PIE times, causing such alternations as *h_2edV ~ *h_2eh_1C to yield Lat. *ad*, Goth. *at* on one side and Ved. *ā* on the other.
2. Unmotivated etymologies, such as that of Gr. οὐθαρ and Skt. *ūdhar* 'udder' that never received a proper explanation, but now might be derived from *$ud\text{-}d^hh_1\text{-}r̥$.
3. Nominal morphology, for example to explain the long vowel of Gr. δῆρις 'battle' and Skt. *dāri* 'to split', if they can be traced back to **de-dr(H)-i-*.
4. Previously impossible etymologies, such as a connection between the roots **terd-* and *$terh_1$- 'to pierce'.

He describes the development and conditioning as *-*VdC*- > *-*Vh_1C*-. In a preliminary effort to date the Kortlandt effect, he illustrates how it interacts with a number of other sound laws: that of Saussure,[2] which states that in sequences **#HRo-* and **-oRHC-* the laryngeal disappears; that of Hackstein,[3] which states that in a sequence **CH.CC* the laryngeal disappears; and lastly, a laryngeal-semivowel metathesis of the type **CRH-i/u-to-* > **CRi/uH-to-*. In the first two cases, the Kortlandt effect predates the other development, yielding the laryngeal required for either of the two sound laws. In the latter case, concerning the metathesis, Garnier posits the Kortlandt effect as the later development of the two. Most recently, Ligorio (2019b) added a few more etymologies to the list of instances of the Kortlandt effect, but also provided a very detailed overview of the conditions under which the Kortlandt effect has so far been seen to occur. He describes the change as *dC > *$ʔC$,[4] where *C is one of the following:

1. *$ḱ$, as seen in *$dḱm̥tom$ '100'.
2. *$u̯$ + the following syllable starts with a dental, as seen in *$du̯idḱm̥ti$ 'twenty'.
3. *r, as seen in *$u̯odr̥$ 'water'.
4. An obstruent, likely *t, as seen in *meh_1t- 'to measure'.
5. *s, as seen in *h_1oh_1s 'mouth'.

What we can take from this list are partly still rather specific conditions that are not all immediately reconcilable as one coherent phonetic environment, but it is striking that the majority of these sounds are dental or close to that. This is a pattern that needs to be investigated more elaborately, along with the conditions that do not seem to match it, such as *$dḱ$ suggested in the above.

[2] First described by Saussure (1905: 511ff.), for a more elaborate discussion see also Nussbaum (1997).
[3] For a more elaborate discussion see Hackstein (2002).
[4] Next to his additions to the existing theory of the Kortlandt effect as *d > *h_1, Ligorio (2019a) proposes a whole new view by suggesting that the development occurs not only with **d*, but also with **g*. The main part of this proposal is based on the word *$(s)neh_1$- 'turn', which in his view could go back to an earlier form **(s)neg-* and in that way could be the basis of several words for 'snake', e.g. PGm. **snakk-* and Skt. *nāga*. He posits this development for a few other words as well, but states that this change is clearly more common with **d*. In the end, he does not yet conclude anything about the status or conditions of this sound change in general, but his research provides a good starting point in identifying a new aspect of the Kortlandt effect. This possibility will have to be investigated more in the future, but that is for now beyond the scope of this research.

2 Structure of this research

For further research, two initial questions arise. First of all, it needs to be examined whether the conditions specified in previous works always give rise to the *d > *h_1 change or whether we find exceptions. Second, we would like to know if the change can be observed in environments other than the five mentioned here and how these relate to each other.
The main problem that still obscures the picture lies in the fact that research on this topic has never been a systematic analysis of all the evidence. There is no combined work containing and discussing all the potential instances of the Kortlandt effect, as most articles only cover a few such etymologies. As a result, we do not have a clear overview of the number of cases where it occurs and of all the conditions that have been proposed thus far. I have therefore collected the etymologies that have been connected to the Kortlandt effect in previous works. Not all of these have been treated very extensively, so it is not always clear which we can rely on as certain instances. The first part of this research will therefore be dedicated to discussing each etymology to determine its reliability and to deal with potential counterexamples related to it. I will end each discussion with a schematic description of the sound law and its conditions as proposed by that specific etymology. At this stage, I will not yet draw any conclusions on the types of sounds involved in the individual phonetic environments. This in order to make sure that the final analysis is as objective as possible, so that no potential outcome can be overlooked by prematurely drawing conclusions when only part of the evidence has been observed. Having identified the reliable cases, further analysis of the phonetic environments will lead to a more coherent conditioning of the Kortlandt effect.
With an established conditioning, it still needs to be explained how a shift from *d to *h_1 makes sense phonetically. For the mechanics behind a sound change of this type, it is useful to first investigate how the Kortlandt effect could fit into typological patterns of consonant changes; specifically, of alternations between dental stops and glottalic consonants. With these patterns in mind, the exact nature of the Kortlandt effect deserves more attention. The phonetics of *h_1 remain to this day somewhat disputed. This discussion has not been very explicit within the topic of the Kortlandt effect, so I will review the relevant evidence to determine the most plausible phonetic realisation of *h_1. The question of the realisation of *d is at least equally controversial. Suggestions regarding *d in the light of the Kortlandt effect have been made before, but most of them immediately presupposed the reader's acceptance of the Glottalic Theory. With information on typological tendencies of this type of change, I will give an account of whether and how the Kortlandt effect fits into different interpretations of the PIE stop system. By considering scenarios with different versions of the stop system, I hope to provide a clear view of which interpretation the Kortlandt effect independently favours.
These conclusions combined should create a more complete picture of the sound change.

The structure of this research is as follows. **Chapter 3** will be focussed on analysing the data, of which **3.1** contains the discussion of the list of etymologies and **3.2** the treatment of the phonetic environment. **Chapter 4** will consist of several sections on phonetics, starting with **4.1** on typological parallels for the change. In **4.2**, *h_1 will be discussed and in **4.3**, the phonetic realisation of *d in different PIE stop systems. I will conclude in **Chapter 5**.

3 Instances of the Kortlandt effect

3.1 Etymologies

From previous works on the Kortlandt effect, I have collected the etymologies that potentially underwent the change. I will discuss each of them here to determine the probability of that scenario and the phonetic characteristics that might have been of influence. The etymologies that will be discussed here are the following:

1. Skt. *dāśvāṃs-* 'devout, pious'
2. Gr. ἑκατόν '100' and other decades
3. Skt. *ávidhat* 'he allotted'
4. PIE *$h_1\underset{\cup}{u}id^hh_1e\underset{\cup}{u}$- 'widow'
5. PSl. *$v\text{ъ}tor\text{ъ}j\text{ь}$ 'secondary', Skt. *vitarám* 'farther', OHG *widar* 'against, toward'
6. Ved. *vār* 'water'
7. Gr. δήκατο 'they received'
8. Ved. *ā* 'to, from, at'
9. Skt. *ūdhar* 'udder'
10. Gr. δῆρις 'battle' and Skt. *dāri* 'to split'
11. OHG *gizāmi* and Goth. *gatemiba* 'appropiate'
12. Gr. τρητός 'pierced'
13. PIE *$t\underset{\circ}{m}d$- ~ *$t\underset{\circ}{m}h_1$- 'to slice, cut'
14. PIE *$(s)p\underset{\circ}{n}d$- ~ *$(s)p\underset{\circ}{n}h_1$- 'to stretch, pull'
15. Ved. *véna* 'to spy on'
16. PIE *med- ~ *meh_1- 'to measure'
17. Gr. φιτρός 'wooden ball, tree trunk'
18. PIE *h_2ed- ~ *h_2eh_1- 'to dry up'
19. PIE *$ǵ^hed$- 'to gape'
20. Lat. *sponte* 'will, volition'
21. Gr. κῦριος 'powerful, authoritative'
22. PIE *h_1ed- 'to eat' ~ *h_1oh_1s 'mouth'
23. PIE *$meld$- 'to soften' ~ *$melh_1$- 'to mill, grind'
24. PIE *sed- 'to sit' ~ *h_1eh_1s- 'to sit'
25. Instrumental *-h_1

1. Skt. *dāśvāṃs-* 'devout, pious'

As previously mentioned, Klingenschmitt (1982: 129) was the first to notice an unexpected development in Skt. *dāśvāṃs-* 'devout, pious'. It was originally a PIE perfect participle to the root *$de\acute{k}$- 'to take, perceive' and from a morphological point of view, we would expect perfect participles to show reduplication. In this case, it should have a preform *$de\text{-}d\acute{k}\text{-}\underset{\cup}{u}os$-. In Sanskrit, however, we synchronically find a root with a seeming lengthened grade, *dāś-*, even though the root is elsewhere reflected as *daś-* with short *a*. In the reduplication syllable one would normally not expect to find a PIE long vowel that could otherwise explain the long *ā* in Sanskrit. If we indeed start from a preform *$ded\acute{k}\underset{\cup}{u}os$-, the **e* should have yielded a short *a*, not a long one, and the second PIE **d* was not supposed to be lost in Sanskrit. Klingenschmitt's own explanation that this would be due to dissimilation with compensatory lengthening has been rejected by Lubotsky (1994: 3). He instead describes that the second **d* in *$ded\acute{k}\underset{\cup}{u}os$- could have developed into *h_1 because of the

following **ḱ*, leaving the form ***deh₁ḱu̯os-*. After this, the laryngeal dropped as normally and caused compensatory lengthening on the preceding vowel, which then ended up as *ā* in Sanskrit.
Alternatively, some verb forms with unexpected long vowels have been explained as Narten roots, meaning they belong to a separate class of verbs that always have lengthened grade in the singular active and full grade in the other forms. Theoretically, it would be possible to attempt the same here. However, it must be noted that the long *ā* is an exception within the paradigm of *daś-*, so we would have to be dealing with an isolated remnant of a Narten formation. This is also hypothetically possible, but the perfect participle is not the place where one would expect to find a lengthened grade according to the pattern of Narten roots.[5] We would thus have to assume that the in terms of Narten roots expected lengthened grade from other forms spread analogically to the perfect participle, eventually leaving only this irregular secondary formation *dāśvāṃs-* as a petrified remnant of the original ablaut process. This seems rather ad hoc. Additionally, it is more logical that *dāśvāṃs-* as a perfect participle indeed goes back to a reduplicated formation ***dedḱu̯os-*,[6] so that the *ā* in question here goes back to the vowel in the reduplication syllable. A lengthened grade in a Narten root is expected on the root itself, so it cannot be the cause of the lengthened grade in this situation.
Accepting **d* > **h₁* thus seems like the most plausible solution for the long vowel, as it does not yield problematic scenarios in PIE and is logical from a grammatical, semantic and phonological point of view. For *dāśvāṃs-*, the only conditioning factor that still seems to be accepted is the following **ḱ*, making the sound change as follows:

Pre-PIE **d* > PIE **h₁* / **_ḱ*

The effect of dissimilation as suggested by Klingenschmitt seems to be left out of the picture in the more recent views, even though dissimilation is said to play a role in other instances of **d* > **h₁*, for which see for example the following section. As the aim here is to provide a collection as complete as possible of the conditions in which the Kortlandt effect occurs, I will for now keep the possible effect of dissimilation among the options as well. This would give the following conditioning:

Pre-PIE **d* > PIE **h₁* / **d..._(ḱ)*

As a final remark, it must be noted that the initial **d* specifically does not change into **h₁*, so we might also have to include a counterenvironment:

Pre-PIE **d* > PIE **d* / **_...d*

2. Gr. ἑκατόν ‘100’ and other decades
Reconstructing the PIE word for ‘100’ as the traditional **ḱm̥tom*, which e.g. Lat. *centum*, Skt. *śata* and Goth. *hund* seem to reflect, cannot account for Greek ἑκατόν, so most scholars agree that some sort of additional element needs to be reconstructed in front of the word. The nature of this element, however, is disputed and its explanations so far had been problematic. Most notably, reconstructing **sem-* or **sm̥-* ‘one’ in front of it has been suggested, but as Kortlandt (1983: 97) already mentions, this faces several difficulties due to which it cannot very easily have resulted in

[5] See e.g. Kümmel (1998: 191).
[6] Lubotsky (1994: 3).

ἑ. For instance, when reconstructing **sem-*, the nasal would somehow have to be lost in Greek, and with **sm̥-*, the vocalic **m̥* would have developed into **a* rather than **e*. He also does not think the indeclinability and syntactic behaviour of ἑκατόν are reconcilable with the idea that such a composition as 'one hundred' existed On top of that, this element would be mysteriously absent in all the other Indo-European reflexes of the word for '100', so it would be a very Greek-based reconstruction.

A more attractive alternative, and the only way to directly leave Greek with an **e* that most other IE languages do not have, would be to reconstruct **h₁* in front the original form, so **h₁ḱm̥tom*. Formally, this is the most straightforward solution, but this does not directly match the older idea of a connection between '100', as the tenth decade, to the word for '10', **deḱm̥*. This is where the Kortlandt effect provides an answer. The initial **h₁* can be traced back to **d*, so that we reconstruct earlier **dḱm̥tom*. Something that does remain problematic is the aspiration of ἑκατόν, which cannot be directly explained from **h₁*. Kortlandt believes it must somehow have been taken over from ἕν 'one', but this seems to contradict one of his arguments against reconstructing **s(e)m-* here. He rejects the idea of tracing ἑκατόν back to a composition denoting 'one hundred', but at the same time, assuming that the aspiration came from ἕν suggests that this type of connection did exist. Even if this adoption of aspiration from ἕν happened at a Greek stage and therefore does not suddenly make adding **s(e)m-* more likely, it does imply that there was in fact a connotation of the type 'one hundred' with ἑκατόν. In my opinion, it seems the aspiration might be taken over from some of the decades. The forms ἑξήκοντα '60' and ἑβδομήκοντα '70' regularly have initial aspiration from PIE **s-* and might have influenced words in the same associated set in this way, which is not an unusual thing to occur in numerals.[7] We actually have evidence that aspirated variants of ὀγδοήκοντα '80' and ἐνενήκοντα '90' exist,[8] most likely influenced by the preceding decades that contained etymological initial aspiration. It would not be a very large step to assume this effect also wore off on the word for 'hundred' and therefore left ἑκατόν with an aspirated vowel. In that case, one could ask why the analogical aspiration became the standard only in '100' and remained exceptional in '80' and '90'.

Even with the source of the aspiration of ἑκατόν being disputed, **dḱm̥tom* seems the most plausible origin.[9] Kortlandt (1983: 98) also reconstructs this additional **d*, which he believes to have contained a glottalic feature, in the decades 20 through 90:

εἴκοσι	< **du̯idḱm̥ti*	'20'
τριάκοντα	< **tridḱomt*	'30'
τετταράκοντα	< **kʷetu̯rdḱomt*	'40'
πεντήκοντα	< **penkʷedḱomt*	'50'
ἑξήκοντα	< **su̯eksdḱomt*	'60'
ἑβδομήκοντα	< **septm̥dḱomt*	'70'
ὀγδοήκοντα	< **h₃ektodḱomt*	'80'
ἐνενήκοντα	< **neu̯n̥dḱomt*	'90'

[7] See e.g. Luján Martínez (1999: 200), Beekes (2011: 240).

[8] Beekes (2010: 423).

[9] Alternatively, parallels of this type of root (**dḱ-*) exist in other words with so-called "thorn"-clusters (see Kloekhorst 2014), which in the individual branches became simplified, suggesting this might also have caused the disappearance of **d* here, but, Kloekhorst (2014: 65) has already shown that this is not the case with **dḱm̥tom*.

In some cases, Kortlandt has to assume a few additional developments, such as the occurrence of internal o in ἐβδομήκοντα and ὀγδοήκοντα. However, as his article already contains an adequate discussion of how this works with many of the individual decades, I will not repeat all of it here, but merely discuss a few disputable elements.

Most notably, **dṷidḱm̥ti* '20' is not entirely identical to the other decades, as it contains two **d*'s and is derived from a somewhat different construction than the rest. We have the loss of two **d*'s to account for in εἴκοσι and other IE reflexes of this word. Kortlandt assumes this first **d* must have become **h₁* due to dissimilation, presumably with the **d* in the following syllable, so **dṷidḱm̥ti* > **h₁ṷidḱm̥ti*. A similar scenario could be posited for the second **d*, by dissimilation due to the following **t*, giving **dṷidḱm̥ti* > **dṷih₁ḱm̥ti*. However, they could not both have occurred simultaneously, as the second **d* cannot have been in the process of dissimilating to **h₁* while at the same time being a condition for the initial **d* to dissimilate and it is illogical to assume that one of the two initially resisted the sound change and only later underwent it while the conditioning was already there. We must then assume, as Kortlandt already shows, that the initial **d* first dissimilated to **h₁*. Lubotsky (1994: 3) notes that we then have to assume a later **dḱ* > **ḱ* for the second **d*, which I believe must be **dḱ* > **h₁ḱ* due to the long vowel in e.g. Lat. *vīgintī*. This relative chronology does make one wonder if these two developments can be part of the same "Kortlandt effect", seeing as one would expect a sound law to be able to operate in all applicable instances at the same time and not have an internal chronological order. We might therefore have to treat these two developments of **d* as different sound changes.

In his very interesting article on the connection between Proto-Uralic nasals and PIE glottalic consonants, Kroonen (2019: 113) suggests an alternative scenario for the word for '20', in a large part to account for Skt. *viṃśatí-* '20' with its unexpected nasal. Based on his theory that PIE glottalic stops might have developed from PIU preglottalised nasals, he recontructs the following scenario: Pre-PIE **ˀnṷi-ˀnḱm̥ti-* > dissimilation of the second glottal stop to **ˀnṷi-nḱm̥ti-* > dissimilation of the first nasal to **h₁ṷi-nḱm̥ti-*, which would be able to account directly for Skt. *viṃśatí-*. One problem with this scenario is that most other IE languages do not retain a nasal here, but rather a (long) vowel (e.g. Gr. εἴκοσι, Lat. *vīgintī*). Kroonen acknowledges this issue and discusses for several such IE reflexes how they could be derived from a form with nasal, showing that his scenario is theoretically possible also in those instances. However, this way we would need to account for a lost nasal in almost all the IE branches by assuming all kinds of individual developments, mainly in favour of one form in Sanskrit. To me, it then seems more economical to view the Sanskrit form as the exception and explain it differently.[10] Moreover, we have Skt. *dāśvāṃs-* < PIE **dedḱuos-*, as discussed earlier, which has a similar structure as **dṷidḱm̥ti* in terms of **d*. Within Kroonen's reconstruction, we would therefore have to assume a similar development, with dissimilation of the second glottal stop and the first nasal, and expect to find a reflex of an original nasal in Sanskrit *dāśvāṃs-*, but we do not. Admittedly, **dedḱṷos-* is a reduplicated formation and it could be argued that reduplication arose only after **ˀn* had already become **d*, so that the nasal was already gone at the time that **dedḱṷos-* arose. However, the stage of PIE that Sanskrit eventually developed from obviously already had reduplication, so if Kroonen derives a Sanskrit word directly from PIE **h₁ṷinḱm̥ti-*, he implicitly assumes that this **n* must still have been present at a time where reduplication was also already existent. This makes the lack of additional nasal in *dāśvāṃs-* a problem. While still accepting the possibility of PIU **ˀn* > PIE **d*, I think this specific theory regarding **dṷidḱm̥ti* must be rejected.

[10] E.g. Thurneysen (1883: 312) regards the nasal in *viṃśatí-* as a secondary development by analogy with **saptāśati* < **septm̥ḱm̥ti* and **navāśati* < **neṷnḱm̥ti*.

Another notable point is Kortlandt's explanation for the word for '60', ἑξήκοντα, from **sy̯eḱsdḱomt*. While all along the point of his article has been to show that **d* turns into **h₁* under certain circumstances by loss of its buccal features and retention of its glottalic feature in the laryngeal, he suggests that specifically in the word for '60' the glottalic feature is lost and the buccal part remains as **t*, so almost completely the opposite. He seems to do this mainly to account for the Indo-Iranian suffix *-ti-* found in some of the decades (e.g. *ṣaṣṭí* '60'), but does not explain how it would be justified to suddenly assume a different development of **d* for only one word. To my mind, there is no reason to assume that **d* > **h₁* did not also simply occur in **sy̯eḱsdḱomt*. This would not immediately yield η, but, as Kortlandt already suggests, this long vowel can easily have been taken over from πεντήκοντα '50'.
All in all, the Kortlandt effect provides the best way to account for several vowel developments in the decades and '100'. What remains is to identify the relevant conditions. First of all, **dy̯idḱm̥ti* '20' underwent two changes with respect to the **d*'s in the following environments:

Pre-PIE **d* > PIE **h₁* / **_y̯...d*
Pre-PIE **d* > PIE **h₁* / **_ḱ*

The rest of the forms discussed in this section all have in common that the **d* was followed by **ḱ* and the next syllable contained a **t*, so independently this pattern can then be described as follows:

Pre-PIE **d* > PIE **h₁* / **_ḱ...t*

3. Skt. *ávidhat* 'he allotted'
In Skt. *ávidhat* 'he allotted', the vowel indicating the augment is written as if it were short, but Lubotsky (1994: 1) has shown that its metric behaviour strongly suggests that it is long; the short spelling could very well be due to - not infrequently found - inconsistencies in the Vedic text.
This brings us to the question of what caused this length. As Lubotsky (1994: 1) discusses, the form *ávidhat* belongs to the root *vidh-* 'to allot', so synchronically, it is not immediately evident why the augment *a-* < **h₁e-* should be long here. However, he argues that other verb forms with long augments usually indicate a root starting in a laryngeal, so this is the most plausible scenario for *vidh-* as well. Next to that, it has already been shown by earlier scholars[11] that the root *vidh-* is a compound of the preverb *vi-* 'apart' plus the root *dhā-* 'to put'. This implies that *vi-*, too, started with a laryngeal. Admittedly, since this already explains the long augment, we could stop here and just reconstruct a PIE element **Hy̯i-* 'apart' combined with the well known verb **dʰeh₁-* 'to put' without bringing the Kortlandt effect into the picture at all. However, Lubotsky points out the striking fact that this **Hy̯i-* element is hardly found outside of Indo-Iranian and that when it is found, the next syllable always contains a dental, cf. OHG *widar* 'against' (Lubotsky 1994: 2). On top of that, several non-Indo-Iranian languages do have a more frequent element **dy̯i-* with approximately the same meaning, cf. Lat. *dis-* (Lubotsky 1994: 2). He puts forward the idea that there might have been an alternation between **Hy̯i-* and **dy̯i-*, which the daughter languages later eliminated: Indo-Iranian in favour of the former, other languages in favour of the latter. This requires an explanation for the correspondence between **H* and **d*, which we now know is perfectly possible in view of the Kortlandt effect. He thus assumes that original **dy̯i-* turned into **h₁y̯i-*, but as both forms have reflexes in daughter languages, even next to each other, there must

[11] Thieme (1949: 36f.) and Hoffmann (1975: 238ff.)

have been some sort of phonetic distribution, meaning the change must have been conditioned. The root we reconstruct for PIE is *$d\underset{\wedge}{u}id^h$-, so we see that *$d$ is directly followed by *$\underset{\wedge}{u}$, and by d^h in the following syllable. We can conclude that the Kortlandt effect occurred here under these conditions:

Pre-PIE *d > PIE *h_1 / *$\_\underset{\wedge}{u}...d^h$

4. PIE *$h_1\underset{\wedge}{u}id^hh_1e\underset{\wedge}{u}$- 'widow'
Another etymology treated by Lubotsky (1994: 3-4) is that of the word *widow*. Based on only the Germanic material, which goes back to PGm. *$widuw\bar{o}$ 'widow', it would be most straightforward to reconstruct a PIE form starting in *$\underset{\wedge}{u}id^h$-. However, as explained most extensively by Beekes (1992), *widow* is cognate with the Greek word ἠίθεος 'unmarried young man', of which the initial vowel is explained through *e with metrical lengthening and eventually goes back to *h_1. We must therefore reconstruct *$h_1\underset{\wedge}{u}id^h$-. With the semantics of a widow being in a way separated from her husband, this comes suspiciously close to the *$d\underset{\wedge}{u}i$-d^heh_1- 'to put apart' construction discussed in the previous section. For these reasons, Lubotsky assumes that *vidh* and *widow* both derive from this same construction, so that (the predecessor of) *widow* can also be added to the evidence of the Kortlandt effect. Since it derives from the same construction as *vidh*, the conditions for *d > *h_1 are also the same and do not need to be repeated here.

5. PSl. *$vъtorъjь$ 'second(ary)', Skt. *vitarám* 'farther', OHG *widar* 'against, toward'
From the word *$vъtorъjь$ itself, formally reconstructable as *$\underset{\wedge}{u}itoro$, nothing immediately suggests it must have had a preform starting with *h_1-, let alone *d-. There are, however, a few things that make reconstructing initial *h_1- < *d- more attractive.
One reason, as Lubotsky (1994: 2) points out, is that *$vъtorъjь$ contains the same *$\underset{\wedge}{u}i$- element as seen in the previous etymologies, which has already been shown to go back to *$h_1\underset{\wedge}{u}i$- < *$d\underset{\wedge}{u}i$- 'apart', with dissimilation due to the following *t. It might not be immediately evident how a word for 'secondary' would be derived from a prefix meaning 'apart', but this becomes less of a leap knowing that *$d\underset{\wedge}{u}i$- has been connected to PIE *$d\underset{\wedge}{u}oh_1$ 'two', with the idea that the meaning 'apart' goes back to something like 'in two'. These seem to me phonologically and semantically plausible reasons to reconstruct *$d\underset{\wedge}{u}itoro$.
An additional reason to reconstruct initial *d is a synchronic one. If we look at related numeral forms of the Proto-Slavic ordinal *$vъtorъjь$, we find that they all start with *d. See for example the cardinal *$dъva$ and the collective *$dъvojь$. Considering that *$vъtorъjь$ is the only one in the set without the initial *d, it is plausible that it lost this *d at some point before Proto-Slavic instead of having been derived from a completely different root than its semantic relatives. That leaves us with the question why *$vъtorъjь$ was the only one to end up without *d. Apart from being the only form without initial *d, *$vъtorъjь$ also happens to be the only form where *t follows in the next syllable, so this strongly suggests a connection.
Cognate and usually considered the same construction are Skt. *vitarám* 'farther' (Lubotsky 1994: 2), although it must be with a suffix *-*tero*-, as *o would have given Skt. *ā*, and OHG *widar* 'against, toward' with suffix *-*tro*- (Kroonen 2013: 590). On the basis of these three forms, the conditions for the change here can be described as:

Pre-PIE *d > PIE *h_1 / *$\_(\underset{\wedge}{u})...t$

6. Ved. *vār* ‘water’

For some time, there was no known nom. acc. sg. in the Vedic paradigm of *udan-* ‘water’ and different suggestions had been made about which form would most plausibly fill this gap in the paradigm. That question has now been answered and I will not be repeating the entire discussion here,[12] but it is relevant because the form that Lubotsky (2013) demonstrated to be the suitable candidate is *vār*. Watkins (1987: 402) connects *vār* (and CLuw. *u̯a-a-ar*) to OIr. *fír* ‘milk’ and points out that it is scanned as disyllabic in the texts, which cannot be explained by merely reconstructing a long vowel in PIE, so that we have to reconstruct PIE **u̯eh₁r-*.

At first glance, it seems hard to reconcile *vār* and *udan-*, so it would seem as if the paradigm of ‘water’ were suppletive. However, reconstructing an original separate paradigm of **u̯eh₁r-* is problematic. First of all, we find no evidence whatsoever of the oblique cases that we would expect in this hypothetical paradigm, so these would have to have been lost altogether for some reason. Second, the root **u̯eh₁-* is not known anywhere outside of this situation, so the reconstruction would be based purely on phonology. The fact that **u̯eh₁r-* otherwise seems to be a completely separate form makes it probable that it somehow must actually have fit into the paradigm of ‘water’ next to the *ud(a)n-* forms. Lubotsky (2013: 162) assumes that **u̯eh₁r-* was derived from **u̯edr-*, so that it can be part of the same paradigm as **uden-* without suppletion and with the expected alternation of an *r/n*-stem. All the elements that synchronically make it seem irregular, go back to regular changes.

Since every other attempt to explain *vār* has been problematic and involving the Kortlandt effect solves all of these problems very easily by creating a regular paradigm without introducing any unexpected sound changes, it is safe to say that *vār* is one of the certain instances of the Kortlandt effect. Finally, we are left with the question of conditioning to explain why only **u̯edr-* underwent the change, while the rest of the paradigm remained untouched. Interestingly, there is one thing that phonetically distinguishes **u̯edr-* from all the other forms in the paradigm, namely that it is the only form in which the **d* is followed by **r*. The data therefore suggest the following conditioning:

Pre-PIE **d* > PIE **h₁ / *_r*

7. Gr. δήκατο ‘they received’

The long vowel in δήκατο ‘they received’ had previously been explained by assuming this verb was one of the Narten presents. This is slightly problematic, because in Narten presents, we would expect to find a lengthened grade only in the singular forms. Garnier (2014: 3) now rejects this explanation and presents an alternative solution. To explain the Greek form δήκατο ‘they received’ and Vedic reflexes of the same verb, such as *dāśati* ‘they gave’, he reconstructs an old present **de-doḱ-ti*, **de-dḱ-n̥ti*. This latter form can explain the long vowel in both the Greek and the Vedic word, by assuming it developed into **deh₁ḱn̥to* through the Kortlandt effect.

The connection between the Greek and Vedic verb is not new and works well both semantically and phonologically. Introducing the Kortlandt effect into the picture therefore does not raise additional questions on the plausibility of the etymology. It is attractive because it allows us to derive these Greek and Vedic forms from their original paradigm by regular sound change, which seems preferable to categorising them under the Narten presents with an unexpected long vowel when there is a more regular alternative. It can therefore be concluded that the forms δήκατο and

[12] See Lubotsky (2013) for a discussion.

dāśati are best explained through the Kortlandt effect. Considering they go back to **dédḱn̥ti*, the following conditioning is suggested:

Pre-PIE **d* > PIE **h₁* / **d..._(ḱ)*

8. Ved. *ā* 'to, from, at'

Garnier (2014: 5) describes how Ved. *ā* 'to' seemed to have no relatives in other languages, so that its reconstruction would have to be based purely on this form and it would not be evident that it could be traced back to PIE at all. However, he argues that it is actually not such an isolated form, but that it should be connected to Lat. *ad* and Goth. *at* 'to' with application of the Kortlandt effect. Let us take a look at the pros and cons of this idea.

The etymology of Lat. *ad* and Goth. *at* has already been established as **h₂ed* 'to' and cognates from several branches have been identified.[13] This in itself did not give rise to problematic situations. On the other side, there is Ved. *ā*, also with a directional meaning, that seemed to be an isolated formation. Phonologically, *ā* should go back to either PIE **HVH* or **HV̄*, but as this is not a position where we would expect to find a long vowel,[14] it has to be the former. That leaves us with a rather abstract PIE form with no other known descendents and no way to identify the involved laryngeals and vowels. This is not ideal. If we look further for a root that could be reconciled with **HVH*, however, we quickly arrive at **h₂ed*, as Garnier suggests, for semantic, syntactic and phonological reasons. Applying the Kortlandt effect gives us **h₂eh₁-* and this works perfectly fine to explain Ved. *ā*. The Vedic preposition must therefore be cognate with the Latin and Gothic prepositions. This is supported by the fact that we find a lot of verbs with e.g. Lat. *ad-* and Ved. *ā-* that have similar meanings, such as Lat. *adfu-* 'to be present' and Ved. *ābhū-* 'to be nearby' (< **h₂ed-bʰuH* 'to be nearby'), Lat' *adsideō* 'to stay seated' and Ved. *āsad-* 'to be seated near to' (< **h₂ed-sed-* 'to be seated near to'). Numerous such verbs are discussed by Garnier, so I will not repeat all of them here.

A complication with the reconstruction of **h₂eh₁-* < **h₂ed-* is that since reflexes of both variants are found in daughter languages, there must originally have been a distribution and therefore a conditioning environment, but **h₂ed(-)* alone does not provide us with much that could have caused the change. Garnier solved this by suggesting the instances with **h₁* go back to the longer constructions connected to a verb, where **h₂ed-* was originally followed by a form that started with a consonant, which then caused the **d* to be lenited and become **h₁*. These coexisted with forms where **d* was retained, in constructions where the following word started with a vowel. Such alternating constructions could have been different kinds of related forms. Garnier mentions the possibility of an alternation between **h₂eh₁-tóm* and **h₂éd-im* 'to him', and a lot of verbal paradigms could also have contained this alternation, such as an augmented **h₂ed-e-gʷem-t* 'he has arrived' next to **h₂éh₁-gʷem-e-t* 'he arrived'. This original conditioned distribution disappeared later on, when daughter languages generalised one of the variants, but not all the same one. Indo-Iranian was the only one to retain **h₂eh₁-*, while the other languages in which we still find a reflex of this preposition derive it from **h₂ed-*.

Unfortunately, the individual verbal forms cannot be used separately as data to help identify a phonetic environment, because while some of them might really have undergone the Kortlandt effect, quite a few will merely have ended up with the same reflex due to analogy with those forms.

[13] Beekes (2010: 24)

[14] Beekes (2011: 143, 176)

Garnier himself described the pattern as *d > *h_1 / *_C, but we can actually not really draw any certain conclusions on the phonetic environment from the data we have.

9. Ved. *ūdhar* 'udder'

In a discussion of several cognates meaning 'udder', such as Ved. *ūdhar*, Gr. οὐθαρ and Lat. *über*, Garnier (2014: 8) rejects the previously reconstructed PIE form *$h_1(o)uHd^h$-$r̥$. Instead, he suggests to derive them from a form of the action noun *ud-$d^h h_1$-$r̥$, with *ud-$d^h eh_1$ 'to extract milk' as the underlying verbal root. This verbal root is known from verbs with the same meaning in a few daughter languages (Skt. *dhaya-*, Rus. *vý-doit'*) and according to Garnier serves as a good basis for the word for 'udder'. The first *d would have developed into *h_1 by the Kortlandt effect and caused the long initial *ū*.

Garnier's main reason to reject *$h_1(o)uHd^h r̥$ and to reconstruct *$udd^h h_1 r̥$ instead is to have an underlying verbal root for the nominal paradigm of 'udder'. To me, it does not seem necessary to have a verbal root underlying this nominal form; more nominal paradigms without underlying verbal roots are known to exist. This, of course, is not immediately a reason to reject derivation from *$udd^h h_1 r̥$. Semantically, the connection works, but when looking at the phonological reflexes, *$udd^h h_1 r̥$ causes trouble. The first reconstructed stage is still fine: the introduction of *d does not really make a difference, as the attested reflexes would be descendant from a stage where it had already turned into *h_1 and therefore come down to more or less the same thing as *$h_1(o)uHd^h r̥$. The difficulties arise when trying to explain the Latin *b*. From *$h_1(o)uHd^h r̥$, the *b* would regularly arise from *d^h due to the following *r. In *$udd^h h_1 r̥$, *d^h and *r are not adjacent, so this does not happen.[15] On top of that, the construction *$udd^h h_1 r̥$ cannot account for the apparent full grade in some reflexes. For example Gr. οὐθαρ must come from *$Hou(H)d$- with *o*-grade and this is not an ablaut variant that can be reconciled with *ud-. It is therefore problematic to posit *$udd^h h_1 r̥$ as the predecessor for all the IE reflexes. As it is clear from strong phonological and semantic similarities that the Latin word should be connected to those from the other previously mentioned IE languages, this reconstruction must be rejected.

10. Gr. δῆρις 'battle' and Skt. *dāri* 'to split'

As already stated by Beekes (2010: 326), Gr. δῆρις 'battle' and Skt. *dāri* 'to split' formally look like they are related. He, hesitantly, reconstructs the underlying PIE root as *der- 'to flay', connecting it to δέρω 'to skin, flay', but this cannot directly account for the long vowels in both languages. Garnier (2014: 9) suggests to derive the Greek and Sanskrit forms from an original reduplicated formation of the shape *de-h_1r-i- < *de-$dr(H)$-i-, comparable to what we found for Skt. *dāśvāṃs-* 'devout, pious'.

To my mind, this is a plausible solution. This reconstruction enables us to explain both Gr. ῆ and Skt. *ā* without having to assume an unfounded lengthening of the vowel in PIE. Immediately reconstructing a laryngeal without a prestage of *d would serve this specific purpose equally well, but it would create the problem of having to explain *deh_1ri- by connecting it to a verb with the same root and meaning, if we want to avoid having to accept it as an isolated formation. A verb with the same root and meaning already exists if we reconstruct the reduplicated formation *$dedr(H)i$- derived from something like *$derH$-, and the parallel from Skt. *dāśvāṃs-* strengthens this assumption. This example can therefore be added to the list of most certain examples of the Kortlandt effect. The phonetic conditions suggested by this etymology, also keeping possible influence of preceding consonants in mind, are the following:

[15] Weiss (2009: 75f.).

Pre-PIE *d > PIE *h_1 / *(d)..._r

11. OHG *gizāmi* and Goth. *gatemiba* 'appropriate'
At the end of the discussion of Gr. δῆρις and Skt. *dāri*, when giving an overview of the stages these forms went through, Garnier introduces a different root, *$demh_2$- 'to tame', and reconstructs the same stages for this verb as those discussed for *$derH$- in the previous section. Derived from this verb we namely find OHG *gizāmi* and Goth. *gatemiba*, adjectives meaning 'appropriate', both with a long vowel that we would not expect from *$demh_2$-. In the same way as with *$dé$-$dr(H)$-i-, he therefore assumes there must have been a reduplicated *de-$dm(h_2)$-i, resulting in *$déh_1mi$- due to the Kortlandt effect and finally leaving a lengthened *$dēmi$-.
This is all theoretically possible, but he does not make very explicit why we would need to reconstruct this reduplicated form and how it would work for the different daughter languages. While the long vowel in these Germanic words indeed needs to be explained somehow, it does not correspond to a long vowel in Greek or Sanskrit, like with the previous case. Garnier himself gives Gr. δεδμημένος 'tamed' as a reflex of this original participle formation *$dedm(h_2)$-, clearly without the long vowel that we would expected in the first syllable, had the Kortlandt effect indeed occurred. One could argue that Greek simply generalised the form with *d while Germanic perhaps favoured the *h_1 variant, in a similar way that Ved. *ā* arose next to Lat. *ad* and Goth. *at*, but we have seen from δῆρις that Greek did exactly the opposite. It might be thinkable to accept that Greek in some places could have generalised the other reflex, that of *h_1, but as the structure and formation of *de-$dm(h_2)$-i- and *$dé$-$dr(H)$-i- are identical, it seems unlikely that they would have yielded different results in the same language, if still in PIE the Kortlandt effect had affected them. It then follows that *$dedm(h_2)i$- did not in fact undergo this change and was not the basis for the long vowel in Germanic. The fact that this formation is so similar to the previous one in which the Kortlandt effect did occur makes it a very interesting case for our knowledge of the phonetic conditions. The only difference that *$dedm(h_2)i$- shows, is that the second *d, which, based on the evidence from *$dédr(H)i$-, was the candidate for the Kortlandt effect, is followed by *m here instead of by *r. Therefore, because this example explicitly does not exhibit the sound change at hand, we can use it to add the following to the conditioning:

Pre-PIE *d > PIE *d / *_m

12. Greek τρητός 'pierced'
Garnier (2014: 2) saw a strong similarity between the Vedic root *$tr̥d$- 'to pierce' < PIE *$tr̥d$- and Greek τρητός 'pierced' < PIE *$tr̥h_1$-to-, which led him to posit this as another instance of alternation caused by the Kortlandt effect. The adjective *$tr̥h_1$-to- then originally goes back *$tr̥d$-to- with *d, but due to the following *t, this was dissimilated to *h_1. Semantically, it is clearly very attractive to connect the roots *$terd$- 'to pierce' and *$terh_1$- 'to pierce' and it would almost be problematic to not be able to connect roots of such similar shape with the exact same meaning. I therefore believe this can certainly be seen as an alternation caused by the Kortlandt effect. A problem seems to occur when looking at Ved. *$tr̥ṇṇa$- 'pierced', which reflects *$tr̥d$-na- < *$tr̥d$-to-, instead of the expected shifted variant *$tr̥h_1$-to-, like in Greek. However, this can be explained, as Garnier states, by accepting that analogy occurred in several parts of the paradigm, so that we find reflexes of *d throughout the Vedic paradigm. The opposite is true for Greek. We find no forms with *d and various instances, such as τρητός 'pierced', τετρημένος 'pierced' and τρήσω 'will pierce', where η points to a vowel lengthening due to *h_1. This combined with the fact that the η

shows up before a wide range of consonants strongly suggests that *h_1 was introduced analogically in some of the cases. Inconveniently, that also means it is hard to determine which forms acquired *h_1 by sound change and which by analogy, so we cannot establish a phonetic conditioning here. I do, however,
Garnier (2014: 14) extends the discussion of this root by setting up a relative chronology where the Kortlandt effect postdates a metathesis rule *$CRH\text{-}i/u\text{-}to$- > *$CRi/uH\text{-}to$-, which, if correct, could be very useful as an indication of where to place the Kortlandt effect chronologically between other sound changes. Garnier sets up this chronology in order to explain Gr. τρύω 'to wear down, exhaust' from *$t̥rd$-. This is mainly to account for the υ in the Greek root, but requires a rather speculative, as Garnier admits, adaptation to the metathesis rule in order to work. Since this adaptation is really only proposed in the article to match this specific word and had not been suggested elsewhere by independent evidence, it does seem a bit ad hoc. On top of that, the sequence with υ has already been explained by Beekes (2010: 1514) as taken over from the passive perfect τέτρυμαι. It might still be useful to further investigate Garnier's interpretation of the metathesis rule, but as it does not immediately provide any strong new evidence for the Kortlandt effect, I will refrain from doing that here.

13. PIE *$tm̥d$- ~ *$tm̥h_1$- 'to slice, cut'
These roots are reflected respectively in the Greek verbs τένδω 'to gnaw, cut' and τέμνω 'to cut, split'. Beekes (2010: 1466) mentions that these two roots have been connected with the *d > *h_1 change, but he himself seems to question whether the two roots are to be unified. He reconstructs *$tend$- and *$temh_1$-, stating that the *m would otherwise be unexplained in view of τένδω. To my mind, however, it is well possible that the original *m simply assimilated to the following *d and therefore became *n. Garnier (2014: 10) very briefly mentions that he posits more or less the same scenario for PIE *$tm̥d$- and *$tm̥h_1$- as for *$t̥rd$- and *$t̥rh_1$-. PIE *$temd$- would indeed be the basis for Gr. τένδω, *$tm̥h_1$-*(to-)* specifically that of the adjectival form τμητός. If *h_1 indeed only turned up before this *-to- suffix, we would have a nice distribution, but as we have seen, this is not the case: τέμνω < *$temh_1$- exists as a separate verb with all kinds of different suffixes and endings and therefore no coherent phonetic environment. However, connecting these two roots, thus accepting the occurrence Kortlandt effect here, is in my opinion still the most attractive scenario. The fact that *$temd$- and *$temh_1$- both show up in Greek as separate verbs with a similar meaning can only lead us to believe that an original complementary distribution must have existed between them at a PIE stage, but this is now completely obscured by paradigmatic levelling.

14. PIE *$(s)pn̥d$- ~ *$(s)pn̥h_1$- 'to stretch, pull'
Another addition of the same type by Garnier (2014: 14), PIE *$(s)pn̥d$- ~ *$(s)pn̥h_1$- 'stretch, pull', runs into the same problems as the previous two etymologies. We find reflexes of the form with *d in e.g. Lat. *pendo* 'to hang' and Pol. piędź 'span' and with *h_1 in Gr. πένομαι 'to toil' and Eng. *spin*. I accept that the connection of these two roots with the *d ~ *h_1 alternation is likely, but we cannot reconstruct the conditions for the occurence of *h_1 for the same reasons as with the other words of this type.

15. Ved. *vena* 'to spy on'
Next to the widespread root *$u̯eid$- 'to see', we find Ved. *véna*- 'to spy on' with a similar meaning, but a disputed etymology. Gotō (1987: 298) proposed to connect it to a different Indo-Iranian root *$u̯aiH$- meaning 'to follow', but Garnier (2014: 11) rejects his explanation due to several difficult required semantic developments that make it less plausible. Instead, he suggests we also trace

véna- back to a form of **u̯eid̯-*, namely **u̯oid̯-no-* 'vision, observation'. This cannot directly account for the Vedic form, so he derives **u̯oih₁-no-* from it by means of the Kortlandt effect. Subsequently, he assumes, the laryngeal was dropped due to the Saussure effect, which means that in this case the sequence **-oRHC-* caused the laryngeal to disappear.
Evaluating this possibility, there seems to be no strict counterargument to a connection between **u̯eid̯-* and *véna-*. Semantically, 'to see' and 'to spy on' are very easily reconcilable and cause no problems. Phonetically, there are precedents to the sounds changes we have to accept in order to get from **u̯oidno-* to **u̯oino-*, so this might very well be accurate and this possibility should be taken seriously. However, purely in terms of demonstrating the Kortlandt effect, *véna-* is not the most reliable candidate. There is no part of *véna-* itself that unambiguously suggests an original **h₁*, let alone a **d*, and the assumption is purely based on semantic similarities. This is not a reason to reject the etymology, but it is too indemonstrable on the phonological side to serve as individual evidence for the Kortlandt effect, so it will be not be taken into account for establishing the conditions.

16. PIE **med- ~ *meh₁-* 'to measure'
In Indo-European, we find two very similar roots for 'measure': **med-* and **meh₁-*. The former is reflected in e.g. Lat. *medeor* 'to measure' and Dut. *meten* 'to measure', the latter in e.g. Gr. μῆτις 'skill, plan' and Lat. *mētior* 'to measure'. Garnier (2014: 11) proposes to connect these two roots and assumes **meh₁-* was derived from **med-* with the Kortlandt effect. He does this to be able to account for several forms with long vowels that are related to roots with a short vowel elsewhere. These are Gr. μήδομαι 'to consider' and the athematic imperfect μῆδτο, as opposed to the related Gr. μέδομαι 'to sleep, meditate'; and the isolated Vedic middle formation *ámāsi* 'I have measured'. These forms with η/*ā* have in common that they mostly go back to a construction where the original **h₁* is followed by a consonant, e.g. *mētior* < **meh₁-ti-*, *ámāsi* < **h₁e-meh₁-s-h₂*. A seeming counterexample is μέτρον 'measure', which must clearly be from the same root but has neither the δ we would expect from **med-* nor the η we would get from **meh₁-*. Beekes (2010) simply derives μέτρον from zero grade **mh₁-* to get ε. Garnier (2014: 14) explains it by assuming that the Hackstein effect occurred after the Kortlandt effect, stating that **CVh₁-CC* > **CV-CC*. In this case, therefore, **med-tro-* developed into **meh₁-tro-* due to the Kortlandt effect, which in turn became simply **metro-* due to the **-tr-* sequence giving rise to the Hackstein effect. The former explanation seems a bit more straightforward, but either way, μέτρον does not pose a problem for the assumption of the Kortlandt effect.
The connection between **med-* and **meh₁-* to me seems like a very plausible solution to the previously mentioned problems, on top of already being an attractive scenario due to the semantic and phonological likeness of the two roots. It is likely that an original complementary distribution existed between **med-* and **meh₁-* dependent on certain adjacent sounds. However, we find a lot of different formations in different languages with different environments and both variants are so widespread that they exist as separate roots, so the original distribution is no longer traceable.

17. Gr. φιτρός 'trunk, block'
Gr. φιτρός 'trunk, block' is in part a similar case as μέτρον. It is regarded as a derivation from the root **bʰiH-* 'to hew, cut' with the instrument suffix **-tro-*.[16] Garnier (2014: 14) therefore uses the Hackstein effect to explain the lack of length on ι that would otherwise be expected with a

[16] E.g. Chantraine (1968: 1163). Beekes (2010: 1574) hints at a Pre-Greek origin, but does not give any reasons supporting that idea nor any arguments against derivation from **bʰiH-*.

following laryngeal. This all seems fine to me, but in reconstructing the prestages, he says we must "sans doute" accept the following development: *b^hid-tro- > *b^hi-h_1-tro > *b^hitro-. The last two stages are clear and the first would theoretically be possible within the relative chronology between the Kortlandt effect and the Hackstein effect, but, as far as I can tell, there is no actual need to reconstruct a preform with *d. As we already have a perfectly fine connection to a verb with a laryngeal and Garnier himself does not explain why we would have to reconstruct a stage with *b^hid-, there is no reason to assume that this etymology has anything to do with the Kortlandt effect.

18. *h_2ed- ~ *h_2eh_1- 'to dry up'
Another connection that Garnier (2014: 12, 14) proposes to make is that between *h_2ed- and *h_2eh_1- 'to dry up'. The former is reflected in Hitt. *ḫāt-* and Gr. ἄζομαι 'id.' and Lat. *ador* 'coarse grain', the latter in e.g. Lat. *āra* 'altar', Hitt. *ḫāšša* 'earth', Pal. *ḫā-* 'to make warm', Ved. *āsa-* 'ash' and possibly Hitt. *ḫāttar* 'cereal'. The distinction between the two PIE roots arose by adding suffixes that caused the *d to become *h_1. More specifically, Lat. *āra* 'altar', Hitt. *ḫāšša* 'earth', Hitt. *ḫāttar* 'cereal' and Ved. *āsa-* go back to PIE *h_2eh_1-s-h_2- < *h_2ed-s-h_2- and Hitt. *ḫāttar* goes back to PIE *h_2eh_1-t_r^o < *h_2ed-t_r^o. As Pal. *ḫā-* is a complete root, the original environment is no longer visible. The forms with a reflex of PIE *d go back to forms followed by a vowel or, in the case of Gr. ἄζομαι, a *-*i̯e/o-* suffix (Kloekhorst 2007: 372). On top of this, Garnier proposes to derive Gr. ἦμαρ and Arm. *awr* 'day' from *h_2eh_1-m_r^o, going back to the same *h_2ed- root. This would have been derived from a meaning of 'heat' to 'heat of the day'. Garnier also adds Gr. ὀστέον 'bone' to the descendants. The connection between *h_2ed- and *h_2eh_1- seems quite possible to me. Reflexes of both roots show semantics involving dryness and phonology points to a complementary distribution between the two variants, so reconstructing one original root becomes plausible. Some element of doubt might arise due to the fact that most forms reflecting *h_2eh_1- show a following *-*s*-, which puts forward the question as to whether the root was not simply *h_2eh_1s-. Kloekhorst (2007: 372), for example, is reluctant to analyse this form as *h_2eh_1-*s*-, because the evidence for a root *h_2eh_1- without *-*s*- is scarce. In view of the reflexes of only *h_2eh_1*(-)s*-, it would indeed be more attractive to connect *s directly to the root, but with the assumption that it derives from *h_2ed- due to some complementary distribution, it is actually very logical that we would find *h_2eh_1- followed only by specific consonants. Additionally, the example of Hitt. *ḫāttar* suggests that *h_2eh_1- did in fact also occur before other sounds than *s. All in all, I do think an alternation between *h_2ed- and *h_2eh_1- might have existed. Only a few of the alleged descendants might not actually be correct. The connection to Gr. ἦμαρ and Arm. *awr* 'day' does not seem entirely evident to me. If the semantics had been strikingly similar, this derivation might have been possible, but they are not necessarily. Garnier proposes to derive the 'day' meaning from a starting point of 'heat', for which he gives a parallel in Germanic. While this clearly shows that such a development is possible, the original semantics of *h_2ed- have to do with dryness rather than heat necessarily. Some of its descendants might have something to do with heat, but because some of the others really do not, a meaning 'to dry up' is more plausible, which is not very easily connectable to 'day'. Also the addition of Gr. ὀστέον 'bone' seems a bit weak on the semantic side. On top of that, the nature of initial laryngeal is not entirely certain; Beekes (2010: 1119), for instance, reconstructs *h_3esth_1-. What the other forms tell us is the following:

Pre-PIE *d > PIE *h_1 / *_*s*, *t*

19. PIE *ǵʰed- ~ PIE *ǵʰeh₁- 'to gape'

Garnier (2014: 13) sets up a connection between the PIE roots *ǵʰed- and *ǵʰeh₁-, which he both translates with 'to be gaping'. The former (with different ablaut grades) would be the ancestor of e.g. Eng. *gate*, Gr. χόδος 'anus' and Ved. *had-* 'to defecate', the latter that of Gr. χώρα 'space, location' and χήρα 'widow'. Without immediately ruling out the possibility of a connection between *ǵʰed- and PIE *ǵʰeh₁-, it must be said that the descendants and meaning he posits for these roots are not undisputedly true. Eng. *gate* goes back to a PIE root *ǵʰod- (with e-grade variant *ǵʰed-) 'to find (a way)' and is not related to Gr. χόδος 'anus' and Ved. *had-* 'to defecate' (Kroonen 2013: 170). Kroonen argues that while the meaning 'anus' is found for descendants of this root in several Germanic languages, it is secondary. The Greek and Vedic forms go back to a different PIE root *ǵʰed- meaning 'to shit'. Gr. χήρα 'widow' has also already been traced back to a semantically different root: Beekes (2010: 1631) reconstructs *ǵʰeh₁-ro with the verb root meaning 'to leave', making a widow a 'person left behind'. The origin of χώρα 'space, location' is disputed. Beekes (2010: 1655) suggests a relation with χήρα, so that χώρα would go back to the same root with o-grade. He admits that this is not entirely certain and, in my opinion, it is not the strongest connection from a semantic point of view. Deriving it as a Kortlandt effect variant from *ǵʰed-, as Garnier does, is not more likely in terms of semantics and is in any case too uncertain to use as evidence for the change. In conclusion, the only thing that might cause us to consider a *d/*h_1 alternation here is the fact that the roots *ǵʰed- and *ǵʰeh₁- both existed, but semantically there is no strong reason to connect them.

20. Lat. *sponte* 'will, volition'

De Vaan (2008: 583) mentions a reconstruction of Lat. *spons*, *-ntis* 'will, volition' with *h_1, namely *(s)penh₁- 'to spin', but rejects it due to lack of semantic relation and of further evidence besides a similarity in form. Garnier (2014: 3) later on again argues for the opposite and believes that a fossilised ablative *sponte* is to be traced back to *(s)ponh₁-t- 'to pull'. In his view, the root *(s)penh₁- itself is a Kortlandt effect variant of *(s)pend- 'to pull', whence Lat. *pondus* 'weight'. The Kortlandt effect in this case would have occurred due to the following *t.

I agree with De Vaan that tracing Lat *spōns* back to *(s)penh₁- purely based on shape and with rather diverging semantics is not very attractive. Looking at Garnier's suggestion then, I think we run into the same problem, so accepting this etymology it is not necessarily preferable. In my opinion, *spōns* and the rest of its paradigm should indeed be derived from a form *(s)ponh₁-, which could still reflect an outcome of the Kortlandt effect, but rather because it is a variant of the root *spond- 'to libate', whence also Lat. *spondeō* 'to pledge, promise'[17]. The semantics of pledging and one's will seem quite reconcilable. What is left to account for is the fact that we find no trace of the laryngeal in Lat. *spōns*. This can be explained by Garnier's theory that the Saussure effect occurred after the Kortlandt effect, so that the laryngeal disappeared in the sequence *-*onh₁C*-. Lat. *spōns* must go back to a preform *sponts in which the *t regularly dropped and caused compensatory lengthening on the *o. All forms in this paradigm therefore go back to the stem *(s)ponh₁-t- whereas this *t was not present in the predecessor of e.g. *spondeō*. The data therefore suggest the following development:

Pre-PIE *d > PIE *h_1 / *_t

[17] For a more elaborate discussion of *spondeo*, see De Vaan (2008: 582).

21. Gr. κῡ́ριος ‘powerful, authoritative’

Van Beek (2016) suggests an etymological connection between Gr. κῦδος ‘glory’ and κῡ́ριος ‘powerful, authoritative’. Traditionally, κῦδος had been derived from PIE **keud-s* ‘miraculous power’ (Beekes 2010: 796) and κύριος from **keuh₁-* ‘to swell’ (Beekes 2010: 806), but Van Beek proposes to view κῡ́ριος as the original adjective to κῦδος. It would then go back to **kud-ro-*, which yielded **kuh₁-ro-*. Van Beek describes the conditions of **d* > **h₁* as “at least word-internally before **r* if **d* was part of the syllable coda”.

In my opinion, Van Beek makes a good case for connecting the functions of κῦδος and κῡ́ριος, especially considering his point that κῦδος used to denote ‘power, authority’, making their semantics practically identical and more easily reconcilable than with **keuh₁-* ‘to swell’. The phonetic side is plausible as well, because there is a clearly identifiable distribution between the **d* and **h₁* reflexes and it fits well with what we have seen so far of the Kortlandt effect. This therefore seems like a reliable example, showing the following conditions:

PIE **d* > PIE **h₁* / **_r*

22. PIE **h₁ed-* ‘to eat’ ~ **h₁oh₁s* ‘mouth’

The PIE word for ‘mouth’, reflected in e.g. Hitt. *aiš*, Skt. *ā́s-* and Lat. *ōs*, is often reconstructed as **HeH-es-*.[18] Several scholars have reconstructed **h₃eh₁-s* (e.g. Rieken 1999: 185, De Vaan 2008: 436) and a form **h₃oh₁-s* has also been proposed (Wodtko, Irslinger & Schneider 2008: 387). However, as Kloekhorst (2007: 201) had already pointed out earlier on, this cannot be correct, because the word initial **h₃* would have yielded *ḫ-* in Hittite. Reconstructing **h₂* is out of the question for the same reason and because of the vocalism in e.g. Latin, so our only option is to reconstruct initial **h₁-*. Kloekhorst then reconstructs **h₁eh₃-s*. Ligorio (2019b: 1ff.) regards this as an option, but points out that reconstructing **h₁oh₁-s* is equally possible and would yield the same results. He suggests that accepting a PIE form **h₁oh₁-s* might allow us to connect the word for ‘mouth’ to **h₁ed-* ‘to eat’, by assuming an *o*-grade form **h₁od-s-* in which the Kortlandt effect occurred before the **s* suffix, thus yielding **h₁oh₁-s*. Additionally, he proposes to derive Gr. ἦτορ ‘heart’ through **h₁oh₁-tr-ø* from the **h₁ed-* root as well, with a semantic development from ‘to eat’ to ‘stomach’ to ‘heart’.

The connection between ‘mouth’ and ‘to eat’ seems quite attractive to me. Reconstructing **h₁oh₁-s* enables us to account for descendants in the IE daughter languages and it gives us a clear distribution for the **d* > **h₁* development, making the existence of reflexes from both **h₁ed-* and **h₁eh₁-* possible. The connection with ‘heart’ seems a bit of a stretch. Although it is theoretically possible to posit a development ‘to eat’ > ‘stomach’ > ‘heart’, there is no indication that the ‘heart’ word originally had something to do with eating, so this would merely be based on the hypothetical phonological reconcilability of **h₁ed-* and ἦτορ. On top of that, cognates of ἦτορ such as OHG *ādara* ‘vein’ and OIr. *inathar* ‘intestines’ are even harder to derive from ‘to eat’ and rather point to something like PIE **h₁eh₁t-r* ‘intestines’ (Beekes 2010: 527). On the basis of **h₁oh₁-s* ‘mouth’ we can posit the following scenario:

Pre-PIE **d* > PIE **h₁* / **_s*

[18] See e.g. Kloekhorst (2007: 201).

23. PIE *meld- 'to soften' ~ *melh₁- 'to mill, grind'

We find numerous IE words from the same root meaning 'to crush, grind', such as Lat. *molō* 'to grind', Goth. *malan* 'id.', Hitt. *mall(a)-* 'to mill, grind'. Scholars agree that the PIE root must be *melH-, but the nature of the laryngeal is heavily disputed. This disagreement is caused by CLuw. *malḫu-* 'to break' and Myc. *me-re-u-ro* 'to grind': the former requires *melh₂-[19] due to the retained *ḫ*, the latter implies *melh₁-[20] due to the second *e*. Ligorio (2019b: 7ff.) proposes to stop explaining either of these two as secondary and reconstructs two separate roots *melh₁- 'to mill, grind' and *melh₂- 'to break, crush', splitting up the IE reflexes between these roots. The root *melh₁-, he then proposes, can be viewed as a Kortlandt effect variant of *meld- 'to soften', found in e.g. Eng. *melt* and Gr. μέλδομαι 'to melt'.

I agree that a connection between *meld- 'to soften' and *melh₁- 'to mill, grind' looks quite plausible, both from a phonological and semantic point of view. We would have to assume that *melh₁- originally arose under specific circumstances, but became more widespread due to analogy of related forms and finally developed into a complete, separate root without the original conditioning. This scenario is possible. However, this means either accepting the *melh₁- reconstruction for all of the IE forms or the split that Ligorio suggests. The latter obviously solves the issues with CLuw. *malḫu-* and Myc. *me-re-u-ro*, but in my opinion, despite Ligorio's clear explanation of how the division would work, it does seem a little bit ad hoc. We have a rather extensive list of IE forms with approximately the same meaning and shape that can all go back to either *melh₁- or *melh₂-. This means that with such a split we would be creating a complete, additional PIE root on the basis of only one form that does not give the expected outcome (respectively either *me-re-u-ro* or *malḫu-*), whereas all the other related forms easily fit under one root. This is not usually a very favourable thing to do and I think we should try to avoid it here too. We are thus back to the old question of which of the two forms is secondary or might need revision. If we can reconstruct *melh₁- for all of these forms, I think we should accept Ligorio's suggestion of *meld- > *melh₁-. Because there is still some room for doubt, however, it is safest to exclude it from our further analysis of the Kortlandt effect for now.

24. PIE *sed- ~ *h₁eh₁s- 'to sit'

In the IE languages, we seem to find two different words for 'to sit'. Based on e.g. Eng. *sit*, Lat. *sedeō* and Gr. ἕζομαι we reconstruct a PIE root *sed-, but based on Hitt. *eša* and Skt. *ās* we also reconstruct PIE *h₁eh₁s-. Ligorio (2019b: 11ff.) suggests to derive *h₁eh₁s- from *sed-, by means of, among other steps, the Kortlandt effect. He envisions this as follows: from *sed- a reduplicated present *se-sd- was formed. When this was followed by endings starting with a dental, *s*-insertion occurred, yielding *se-sdˢ-, which was then dissimilated to *se-dˢ-. This created an environment for the Kortlandt effect, so we find *se-h₁s-. Finally, this form was rereduplicated to *h₁e-h₁s-.

While Ligorio gives parallels for several of these steps, I think not all of his assumptions can be justified. First of all, if 'to sit' had been a reduplicated present *se-sd- in PIE, we would expect to find this reflected in other IE languages as well. Instead, most of the evidence (Eng. *sit*, Lat. *sedeō*, Gr. ἕζομαι) points to a simple *sed- in the present. We do find evidence of a reduplicated present of the shape *si-sd- (Lat. *sīdō*, Gr. ἵζω, Skt. *sídati*), but reduplicated *se-sd- could at most have been a reduplicated aorist (Beekes 2010: 376). Most problematic is the last step of Ligorio's theory, where *se-h₁s- is rereduplicated to *h₁e-h₁s-. At the point where *se-h₁s- existed, speakers would not have been aware of all the stages it went through before and therefore would not analyse *se-

[19] This variant is reconstructed by e.g. Kloekhorst (2007: 633) and De Vaan (2008: 387).

[20] As reconstructed by e.g. Beekes (2010: 897) and Kroonen (2013: 351).

as a separate reduplication element and *h_1s- as the root; synchronically, it would simply have been *seh_1s. Thus, if this form were to be reduplicated again, it would have been as something like *$se\text{-}seh_1s$, which defeats the purpose of trying to explain *h_1eh_1s- through this way. To my mind, it is therefore better to keep regarding *sed- and *h_1eh_1s- as separate roots that partly ended up in different IE branches, in which case their relevance for the Kortlandt effect disappears.

25. Instrumental *-h_1

Kortlandt (2010: 41) proposes to connect the more common PIE instrumental ending *-h_1 to the *-t we find in Anatolian (Hitt. -t). He suggests to reconstruct an early PIE *-t that somewhere after the split of Anatolian turned into *-d and finally ended up as *-h_1 with the Kortlandt effect. As most elaborately explained by Beekes (2011: 187), the Hittite ending could theoretically be traced back to each of the PIE dental stops, but since we assume that the ablative -z, which can only go back to *-ti, originally consists of the instrumental ending plus a locative marker *-i, the instrumental must have been *-t. The original environment in which the instrumental ending changed into *-h_1 was, according to Kortlandt, after the full grade *-en- suffix of the n-stems.

This reconstruction is attractive in the sense that it explains the absence of ins. *-h_1 in the Anatolian languages and seems to coincide nicely with a known sound change. However, there are a few issues with this theory. The assumption that Anatolian still had a *t that only in post-Anatolian PIE turned into *d and became a candidate for the Kortlandt effect collides with the fact that we also find reflexes of the Kortlandt effect in Anatolian. We have seen CLuw. $u̯a\text{-}a\text{-}ar$ 'water' as Kortlandt effect variant of PIE *$u̯edr$- 'id.'; Hitt. $ḫāšša$ 'earth' and $ḫāttar$ 'cereal' from PIE *h_2ed- 'to dry up'; and Hitt. $aiš$ 'mouth' from PIE *h_1ed- 'to eat'. The change *d > *h_1 therefore needs to be posited for a stage of pre-PIE that still included Anatolian, making this suggested development of the instrumental problematic. Alternatively, it could be suggested to simply trace the Anatolian instrumental directly back to PIE *d and explain the ablative differently, but the connection between the instrumental and ablative in Hittite is supported by the fact that we find Hittite pronouns in -t with ablative meaning, e.g. $kēt$ 'on this side',[21] and the fact that in other IE languages, *-$(e)t$ is found as the ablative ending. Additionally, the conditioning environment that Kortlandt proposes, namely after full grade *-en- in n-stems, is rather limited in occurrence. Although n-stems are frequent, the majority of nouns belonging to this type are hysterodynamic[22] and would not have had full grade in the suffix in the instrumental, so that group is excluded from the change. According to Kortlandt's theory, we are then only left with the proterodynamic n-stems at the origins of the *-h_1 instrumental. Considering that the *d reflex would have been preserved in all the other stems, so the vast majority of the cases, makes it questionable why *h_1 would have become the standard instrumental ending. One could propose to extend the occurrence of *-d > *-h_1 to after other suffixes as well, but this would all be purely speculative as we have no direct evidence for such a distribution *-d and *-h_1 after different stems. All in all, the connection between the two instrumental endings unfortunately seems too problematic to establish.

[21] Beekes (2011: 187).
[22] Beekes (2011: 193).

3.2 Conditioning

After identifying the most reliable instances of the Kortlandt effect, we are left with the following individual conditions:

- Pre-PIE *d > PIE *h_1 / *_$\acute{k}$ (Skt. *dāśvāṃs-*)
- Pre-PIE *d > PIE *h_1 / *d..._$(\acute{k})$ (Skt. *dāśvāṃs-*, Gr. δήκατο)
- Pre-PIE *d > PIE *h_1 / *_$\underset{\frown}{u}$...d (Gr. εἴκοσι)
- Pre-PIE *d > PIE *h_1 / *_$\acute{k}$(...t) (Gr. εἴκοσι)
- Pre-PIE *d > PIE *h_1 / *_$\acute{k}$...t (Gr. ἑκατόν and decades τριάκοντα - ἐνενήκοντα)
- Pre-PIE *d > PIE *h_1 / *_$\underset{\frown}{u}$...d^h (Skt. *ávidhat*, Eng. *widow*)
- Pre-PIE *d > PIE *h_1 / *_$(\underset{\frown}{u})$...t (PSl. **vъtorъjь*)
- Pre-PIE *d > PIE *h_1 / *_r (Ved. *vār*, Gr. κύριος)
- Pre-PIE *d > PIE *h_1 / *(d)..._r (Gr. δῆρις, Skt. *dāri*)
- Pre-PIE *d > PIE *h_1 / *_s, t (PIE *h_2eh_1-)
- Pre-PIE *d > PIE *h_1 / *_t (Lat. *sponte*)
- Pre-PIE *d > PIE *h_1 / *_s (PIE *h_1oh_1s)

And several environments in which *d remains *d:

- Pre-PIE *d > PIE *d / *_...d (Skt. *dāśvāṃs-*)
- Pre-PIE *d > PIE *d / *_$\underset{\frown}{u}$...h_1 (PIE *$d\underset{\frown}{u}oh_1$)
- Pre-PIE *d > PIE *d / *_$\underset{\frown}{u}$...$\underset{\frown}{i}$ (PIE *$d\underset{\frown}{u}e\underset{\frown}{i}$-)
- Pre-PIE *d > PIE *d / *_m (Gr. δεδμημένος)
- Pre-PIE *d > PIE *d / *_V (Lat. *ador*)
- Pre-PIE *d > PIE *d / *_...$\acute{k}$ (PIE *$de\acute{k}$-)

The one thing that all cases of *d > *h_1 have in common is that *d was followed directly by a consonant in PIE, so this seems a certain part of the conditioning, as already suggested before. This is supported by the fact that we find cases where *d is retained when a consonant that would otherwise provoke the Kortlandt effect is separated from it by a vowel, such as in and PIE *$ded\acute{k}\underset{\frown}{u}os$- (> Skt. *dāśvāṃs-*) and *$h_2edōs$ (> Lat. *ador*). Some cases (*meh_1-, *h_2eh_1-, etc.) seem to suggest the change also occurs simply in root final position, but this contradicts the fact that forms like *h_3ed- 'to smell, stink', *ped- 'to walk, step' and *$(s)teud$- 'to push' exist without a *h_1 variant. We can therefore set up as preliminary condition:

Pre-PIE *d > PIE *h_1 / *_C

Moving on, we find that this rule alone cannot account for every form, as the change does not occur in e.g. PIE *$d\underset{\frown}{u}e\underset{\frown}{i}$- and Gr. δεδμημένος, so not every consonant seems to trigger it. If we now look at phonetic correspondences between these consonants, it is striking that many are dental or coronal sounds, supporting the previously formulated idea[23] that it occurred as a type of dental dissimilation. The previous assumption that a consonant must directly follow *d receives additional support from the fact that, for example, the first *d in *dāśvāṃs-* (< *$ded\acute{k}\underset{\frown}{u}os$-) and δεδμημένος are not dissimilated while clearly in the proximity of a following dental. The different outcomes before *$\underset{\frown}{u}$ can be neatly divided in a group with *t, *d and *d^h following in the next

[23] See e.g. Lubotsky (1994).

syllable, yielding *h_1, and a group without a dental following, retaining *d. This does suggest that the consonant directly following *d does not already have to be dental itself, as long as there is one closely following. These data seem to indicate a specific connection with *$u̯$, but most of these instances derive from the same prefix *$du̯i$-, so they should not be accumulated. On top of that, the same occurs before *$ḱ...s$ in *$dedḱu̯os$- and *$ḱ...t$ in e.g. *$penk^wedḱomt$. To support this further, it would be nice to find more examples of the structure *$dC...C_{[+dental]}$ > *$h_1C...C_{[+dental]}$, but it must be noted that such a structure is in a basic root impossible with most consonants. In addition, two consonants that the root constraints do allow to stand in this position, *l and *r, are in themselves already enough to provoke the Kortlandt effect, so a dental later on in the word is not relevant anymore. Thus, a large part of the data can be accounted for by the following rule:

Pre-PIE *d > *h_1 when directly followed by a consonant and in close proximity to a following dental, where the following consonant and dental can be the same element. If the dental is not the consonant that follows directly, it must be at latest at the start of the next syllable.

What remains to be accounted for is Greek εἴκοσι '20'. As explained more elaborately in **3.1 Etymologies**, the development from *$du̯idḱm̥ti$ to εἴκοσι appears to involve two separate instances of *d > *h_1. The initial *d first changes into *h_1 due to the following *$u̯...d$. Because the second *d was the condition for this change, it can itself only have changed at a later stage. It could therefore be argued that this was not by the same dental dissimilation, because it would have occurred at the same time, but by *$ḱ$, as had already been proposed. This, however, leaves us with on the one side a clearly conditioned change due to (semi) adjacent dentals and on the other the exact same change but with an oddly specific, less phonetically sensible condition. An alternative solution is to explain the second *d > *h_1 change in *$du̯idḱm̥ti$ also as a dental dissimilation, but a later shift. We could make a division between dissimilation before *d and before other dentals and posit the former at an earlier stage. This could simply be explained by the idea that more similar sounds are more prone to dissimilation, but it is also not unlikely supported by the root constraint against two mediae. The dissimilation of two *d's was therefore more urgent and only later did it spread to before other dentals. Although *$du̯idḱm̥ti$ happens to be the only instance found so far where both conditions are present and that actually indicates a specific chronology, it is only logical to reconstruct the same fate for the other decades and '100', as the final element is the same word every time. We have to place most of our other examples in the later shift as well. Such a chronology begs the questions whether all of the branches were still together at this time or whether some had perhaps only undergone the first shift. This is immediately answered by the fact that we have CLuw. *u̯a-a-ar* 'water', Hitt. *ḫāšša* 'earth' and Hitt. *aiš* 'mouth', which lack a dental stop due to *d > *h_1, all in environments with a dental other than *d, so both changes must have been before the split of Anatolian.

4 Phonetics

4.1 Typology of debuccalisation

The phonetics of *h_1 and *d at the pre-PIE stage of the Kortlandt effect will be discussed more elaborately in the next two paragraphs. With the knowledge that *d is in any case a dental stop and *h_1 is some kind of glottal sound, the change *d > *h_1 can already be characterised as a type of debuccalisation. We must then be dealing with one of the following four debuccalisation scenarios: voiceless stop > glottal stop, voiceless stop > glottal fricative, voiced stop > glottal stop,

voiced stop > glottal fricative. In this paragraph, I will look at typological parallels for these developments.

Voiceless stop > glottal stop

Quite a common type of debuccalisation is that of voiceless stops to a glottal stop (Fallon 2002: 124). The phonetic logic behind this, as Fallon explains, can be understood as follows: the original sequence of a stop plus whichever sound comes after is simplified by replacing this first stop with a glottal stop. That way, namely, the oral articulators are freed up and can already work on preparing for the next sound while the stop element is retained through the glottis.

Also outside of Indo-European, there are plenty of parallels for this type of debuccalisation, sometimes with only one particular stop, sometimes with a complete series. For systems in which only one sound changes to *ʔ*, we find numerous examples where this happens to *k*. This change even occurs unconditioned in Samoan and Hawaiian, where *ʔ* is the regular outcome of earlier **k* (Crowley 1992: 86), showing that even without external influences, this is a phonologically plausible development. It has been observed that it is overall most common for velars and uvulars to debuccalise into glottal stops (Kümmel 2007: 107), although other cases are known as well. An example of a whole stop series shifting is Middle Chinese. Here, the voiceless stops *p*, *t*, *k* are reduced to *ʔ* when in syllable final position (Chen 1973: 44).

Although the previous examples have shown it is not a prerequisite, Fallon (2002: 130) has observed that debuccalisation to glottal stop is easier when a stop already carries some sort of glottalisation inherently. This way, the stop does not have to become glottal, but merely loses its buccal feature so that only its glottalic part is retained. Because of this, it is not surprising that we find frequent debuccalisation of ejectives. For example in Yucatec Maya, where the series of ejectives becomes *ʔ* when followed by a homorganic stop, with a special position of *k'*, which debuccalises before any consonant (Lombardi 1990: 383). Other instances include Ethiopian Semitic (McCarthy 1988: 88) and several Caucasian languages (Catford 1992: 196).

A modern Indo-European language known for replacing voiceless stops with a glottal stop in some dialects and contexts is English. Here, the development might be aided by the fact that in many varieties of English, voiceless stops in themselves are already pre-glottalised.[24] As Fallon (2002: 124f.) summarises, there are several types of debuccalisation found in English. In some dialects, there is a word final development *p*, *t*, *k* > *ʔ*, as found in e.g. *map*, *mat* and *mack*, all [mæʔ]. In American English, specifically *t* is commonly debuccalised to a glottal stop when followed by a syllabic nasal, such as in *kitten* [kɪʔn̩], and in some variants also before a syllabic lateral, as found in *bottle* [bɑʔl̩]. A notable feature of this development with *t* is that it only seems to occur with following coronal sounds; it does not occur in e.g. *bottom*. This suggests a similar type of dissimilation as described for **d* > **h*$_1$. All in all, a development voiceless stop > glottal stop is clearly a very plausible scenario.

Voiceless stop > glottal fricative

Although more common with fricatives, we also find instances of voiceless stops to *h*, so becoming both glottal and fricative. Yucatec Maya, as described by Lombardi, is an interesting case. Not one but two series of stops undergo debuccalisation as a regular process. Next to the debuccalisation of ejectives, the series of plain voiceless stops becomes *h* in word final position when the next word starts with a homorganic stop, in order to avoid geminates. Another instance of the development to *h* is Armenian, where PIE **p* becomes *h* in anlaut (Beekes 2011: 121). Kümmel

[24] For the origins of which, see Kortlandt (1997).

(2007) is not convinced that such a change would necessarily have happened directly from voiceless (or voiced) stop to *h* and suggests that it might have gone through an intermediate stage of fricativisation. A more common basis for debuccalisation to *h*, though still less common than fricatives, are aspirated voiceless stops, because it is it is easier for a voiceless stop with an already present aspiration element to become fricative *h* than for a plain voiceless stop. This is found for instance in Klamath, where it occurs after plain sonorants (Blevins 1993: 268ff.).

Voiced stop > glottal stop

There does not seem to be much evidence for the debuccalisation of voiced stops to glottalic consonants. Kümmel (2007: 102) explains that when voiced stops lose their buccal part, they are usually vocalised or lost altogether. Phonetically, it makes sense that voiced stops are less prone to this change than voiceless ones, because they are more steps away from a glottal stop. There are some instances known of voiced implosive stops debuccalising to a glottal stop. We have already seen that in Arbore, ejectives can be debuccalised, but this is possible for implosives as well (Harris 1990). However, this development is merely optional and only one of the two implosives involved is actually voiced; the other implosive and all of the ejectives are voiceless and could easily have contributed to the voiced implosive being debuccalised as well. Interestingly, in the other languages where implosives can be debuccalised, Dime and Ik, they are also accompanied by an ejective series that undergoes the same development (Fleming 1990). I have not found evidence for a language with a single implosive series undergoing this type of debuccalisation. It is therefore questionable whether such a process is likely to occur independently and we must conclude that this is not the most attractive scenario.

Voiced stop > glottal fricative

Even more so than with the voiceless stops, voiced stops are not likely to develop into glottal fricatives. We do, for instance, find the regular Indo-Iranian development in which PIE *$\acute{g}^h$ yields *h* (e.g. Beekes 2011: 123). However, this only happens with an aspirated stop, where the step to *h* is not as big, so this does not mean that such a development would easily arise from a plain voiced stop as well. Combined with the previously discussed tendency of voiced stops to vocalise or disappear when debuccalised, this suggests that a change voiced stop > glottal fricative in general is not very common.

The main conclusion we can draw from this is that typology strongly suggests the PIE development *d > *h_1 started out with a voiceless stop rather than the voiced *d as traditionally reconstructed, as there hardly seems to be evidence for the latter debuccalising to a glottal consonant. There might also be a slight advantage for stops that already contain some type of glottalic feature, such as ejectives and implosives, but as we find several parallels with plain voiceless stops as well, this is not an absolute rule. Additionally, we find that the outcome of a debuccalised voiceless stop is a glottal stop in the vast majority of cases, whereas *h* is usually the result of a fricative. Purely on typological grounds, it would therefore be preferable to reconstruct *h_1 as a glottal stop and *d as a type of voiceless stop. As a final remark, the fact that debuccalisation to glottal stop happens most often with velars and uvulars, might give us some understanding of why the Kortlandt effect only seems to occur in proximity of other dentals; an additional motivation for dissimilation might have been needed. On top of that, as we have seen, American English and Yucatec Maya provide parallels for debuccalisation specifically when close to homorganic stops.

4.2 *h_1

In the discussion of the phonetics of *h_1, the realisations [h] and [ʔ] seem to be the main contestants. What is usually agreed on is that *h_1 was a consonant that in a consonantal environment yielded a vocalic reflex and that, because of its lack of colouring feature on adjacent *e*, it must have been something glottal, so a rather neutral position without any buccal elements (e.g. Rasmussen 1983: 67ff., Beekes 2011: 147).

Rasmussen (1983: 75ff.) argues for fricative [h] as the original consonantal realisation. Based on his reconstruction of *h_2 and *h_3 as [x] and [ɣʷ] respectively,[25] he assumes *h_1 must also have been a fricative. As an additional reason he adduces the fact that PIE had aspirated consonants and therefore a consonant /h/ must already have been present. Beekes (1989: 147) reconstructs [ʔ]. The argument that a lack of colouring on adjacent **e* points to a glottal consonant could also hold for [h], but he indicates several ways in which [h] would not fit the IE data. As he explains, in word initial position before a consonant, one would not expect [h] to yield a vowel, which we do find in Greek ε-. In word initial position before e.g. **r*, one would expect [h] to yield aspiration in Greek, but we do not find this. Additionally, within the Glottalic Theory, laryngeals sometimes seem to have the same effect as the pre-glottalised stops, such as in Winter's Law (Winter 1978), because at a certain point they had all merged into a glottal stop. Beekes remarks that such a merger suggests that at least one of them likely must have been a glottal stop to begin with. As more direct evidence, Kloekhorst (2004) argues for a glottal stop in Hieroglyphic Luwian as the direct reflex of PIE *h_1. After showing that the sign *á* could be used to write [ʔ] or [ʔa], he argues that it diachronically corresponds to forms with *h_1, such as *á-ma'/i-* < **h₁me/o-* 'my', suggesting that *h_1 was a glottal stop and was simply preserved as such in Hieroglyphic Luwian. In his 2006 article, he argues that *h_1 was also retained as a glottal stop in Hittite in certain positions. Although this has met with criticism, most arguments rejecting these ideas have been addressed by Simon (2013), who shows that Kloekhorst's ideas can largely be upheld.

While [h] and [ʔ] are both plausible options to reflect the voiceless glottalic nature of *h_1 that is suggested by IE descendants, I think the problems with [h] that Beekes describes are to be taken seriously and some of the arguments in favour of [h] cannot hold. Rasmussen's fricative interpretation of *h_1 is, as said before, mainly based on his assumption that *h_2 and *h_3 were fricatives. His only argument in favour of this assumption that seems tenable to me is the fact that we find a fricative *ḫ* reflex of *h_2 in some places in Anatolian. However, by this reasoning it seems only logical to follow the Anatolian evidence in identifying *h_1 as well, which would point us to a glottal stop rather than a fricative. His other argument, stating that it is logical to have a consonant /h/ present because PIE contained aspirated stops, can only be maintained if one accepts the traditional reconstruction of the PIE stop system. In the light of the glottalic system, without an obvious aspirated series and with glottalised consonants, it would be much more attractive to reconstruct /ʔ/ as a separate consonant. As a final supporting argument it must be added that as shown in the previous section, it is typologically far more common for stops to debuccalise to a

[25] Rasmussen's (1983: 71ff.) interpretation of *h_2 is based on Anatolian evidence where the reflex of the laryngeal is represented by /h/, in Lycian even denoted by Greek χ, and on the idea that Hitt. *ḫaršar-*, *ḫaršn-* 'head' is derived from **ḱérh₂-s-r*, **ḱrh₂-s-n-ós* by means of a assimilation **ḱ...h₂* > **h₂...h₂*, so that *h_2 must have shared features with *$ḱ$. This assimilation rule has, however, been rejected by Kloekhorst (2007: 367). Rasmussen's view on *h_3 also being a velar fricative stems from the fact that PIE **g*ʷ*ih₃-u̯ó-s* 'alive' became PGm. **kwikwaz*, which he explains by *h_3 assimilating to the previous *g^w before *$u̯$. However, this development has been explained more regularly by the Germanic Cowgill's Law, stating that *h_2/*h_3 (> pre-Gm. **g*) > PGm. **k* before *$u̯$ (see e.g. Cowgill 1965: 143 and Ringe 2006: 68).

glottal stop than to a fricative. Combining all these considerations, I believe it is most credible to assume PIE *h_1 was a glottal stop.

4.3 PIE stop system

Let us now take a look at how the Kortlandt effect would fit into different interpretations of the PIE stop system, with regard to typological tendencies of debuccalisation and the phonetics of *h_1. As the aim of this research is not primarily to argue for one of the reconstructions of the whole stop system, I will not provide a complete discussion of the arguments on those, but merely investigate whether the change *d > *h_1 independently favours any of the variants.

4.3.1 Traditional

Within the traditional system, with a plain voiceless, plain voiced and voiced aspirated series, the plain voiced *d is the one affected by the shift. If we take the example of *$dḱm̥tom$ '100', we would have to assume the following steps in order to end up with the glottal stop *h_1: in *$dḱm̥tom$ a dissimilation of *d was triggered by the following *t. This *d therefore, logically, lost its dental feature. To retain the stop element in this place, it is understandable that it shifted to another stop that did not require the buccal elements needed for a dental, so a glottal place of articulation is explainable. Since in this interpretation *d in itself did not already contain some sort of glottalic element, it does require a complete shift in the place of articulation, but as we have seen in the section on typology, this is not unparallelled. However, the *d also needs to become voiceless in order to end up with a glottal stop and while it could be argued that in *$dḱm̥tom$ this could happen due to the following voiceless *$ḱ$, it would set up quite a complicated scenario with *d having to dissimilate (to *t) and assimilate (to *k) at the same time. Moreover, this would be unable to account for other instances such as *$du̯id^h$- in 'widow', where the *d is clearly in a very voiced environment. We could disregard this as a problem and assume that glottal stop is simply the most probable outcome of the debuccalisation of any stop, with it being what Lass (1976) calls the "reduction stop", like the schwa of consonants. However, we would then like this assumption to be supported by typological data on debuccalisation of stops and as we have seen, typology instead contradicts this idea. Glottal stop is a very common outcome of debuccalised stops, but only of voiceless ones; plain voiced stops rather tend to become vocalised or are dropped, as we have seen explained by Kümmel (2007: 102). Explaining Kortlandt effect with *d as reconstructed in the traditional PIE stop system is therefore problematic.

4.3.2 Glottalic Theory

4.3.2.1 Implosive stops

As described most extensively by Kümmel (2012), one way to interpret the Glottalic Theory is by reconstructing a system with a voiceless, voiced implosive and plain voiced (later to develop into aspirate) series. The implosive series here is the equivalent of the traditional plain voiced stops, so for the Kortlandt effect this means a development *$ɗ$ > *h_1. As a general rule, implosives are usually voiced (Greenberg 1970) and are pronounced by lowering the glottis during the formation of the stop. Due to their voiced nature, the glottis is not completely closed, so they are normally not accompanied by a full glottal stop. If we take the example of *$dḱm̥tom$ again, the development would be as follows: *t triggered the dissimilation of *$ɗ$. Consequently, *$ɗ$ is debuccalised to *ʔ*, possibly aided by the already present glottalic feature. However, the scenario is again one with a voiced stop and as we have seen, these are not likely to debuccalise to *ʔ*. Contrary to with plain voiced stops, we do find some evidence of the debuccalisation of voiced implosives to glottal stop,

as previously stated, but this is not much and some of the evidence is not very strong. In the few languages where voiced implosives could be debuccalised to glottal stops, such as Arbore and Dime, there was always a series of voiceless ejectives undergoing the same development. Since we have seen that this change is much more likely with voiceless stops, we might have to assume the ejectives actually led the change and the implosives were merely dragged along the same path. If so, it is not certain that implosives would undergo this type of shift in a stop system without the "leading" voiceless stops debuccalising. As there are a few parallels for the debuccalisation of voiced implosives to glottal stop, we must conclude that this variant of the glottalic stop system might be able to account for *d > *h_1, albeit not very convincingly.

4.3.2.2 Ejective stops

The view that the PIE stop system originally contained voiceless ejective stops instead of the traditional plain voiced series, as proposed independently by Hopper (1973) and Gamkrelidze & Ivanov (1973), would provide a less complicated scenario. In explaining *t' > *h_1, under the same conditions, we do not have to account for a change in voicing and the shift to glottal stop is less radical, because a glottalic element of *t' was already present. Accordingly, we have seen that it is typologically common for ejectives to undergo debuccalisation to glottal stop. For this purpose, a stop system with an ejective series is therefore very suitable.

4.3.2.3 Pre-glottalised stops

Kortlandt (2003: 259) proposed a PIE stop system without voice or aspiration distinction, but rather with a fortis-lenis distinction in an all voiceless system, consisting of plain fortis stops, pre-glottalised lenis stops and plain lenis stops. In this interpretation, we would have a change *$'t$ > *h_1 to explain. For this specific sound change, it amounts to a very similar situation as discussed for the ejective stops. Once again, we assume that as a result of dissimilation, a glottalised voiceless stop, *$'t$, is debuccalised and ends up as *ʔ*. The fact that in this case the glottalisation precedes the dental does not make a difference for the shift. As we have seen, typology strongly favours voiceless stops in debuccalisation to a glottal stop, so any element of glottalisation inherent to that stop would be a contributing factor. Like in the previous case, we can determine that this system is perfectly compatible with the sound change.

5 Conclusion

After this study, we can first of all conclude that the Kortlandt effect was indeed a widespread pre-Proto-Indo-European sound law. It has been shown to have affected different branches and a wide range of words. With a more complete overview of the instances known thus far, we have been able to connect the phonetic environments and describe the conditioning as "PIE *d > *h_1 when directly followed by a consonant and in close proximity to a following dental, where the following consonant and dental can be the same element. If the dental is not the consonant that follows directly, it must be at latest at the start of the next syllable.". Under influence of Gr. εἴκοσι '20' we seem forced to assume two layers of the change, the first only in dissimilation to *d. A discussion of typology has shown that this type of change, usually referred to as debuccalisation, is a common and therefore also in this case plausible scenario, but voiceless stops and occasionally voiced implosives are the only ones debuccalising to glottal stops. After having determined that PIE *h_1 must phonetically have been a glottal stop, it therefore became clear that the traditional view of the PIE stop system could not account for this change. The Kortlandt effect is best explained

through the Glottalic Theory, of which the interpretation with either a voiceless ejective or a voiceless pre-glottalised stop is most suitable. For a very broad estimation of the dating, we can say that both layers of the change must clearly have arisen before the colouring or loss of laryngeals, before the loss of the glottalic element of the PIE pre-glottalised series, and its effect on e.g. Hitt. *ḫāšša* 'earth' *aiš* 'mouth' suggests that it must have occurred before the split of Anatolian. Garnier (2014) additionally suggested a relative chronology between the Kortlandt effect and a few other specific sound laws, positing a laryngeal-semivowel metathesis rule before the Kortlandt effect, and the Saussure effect and the Hackstein effect after it. The metathesis is, to my opinion, still too speculative in its relation to the Kortlandt effect to securely set up a chronology, but for the other two we can accept this order.

A few suggestions for future research might be made. It would be useful to find more instances like εἴκοσι, where the consecutive change of two **d*'s suggests a relative chronology between two different layers, to strengthen this assumption. As a related question, it should be examined more systematically what approximate date we can assign to the Kortlandt effect by establishing a relative chronology with other sound laws. Finally, Ligorio's (2019a) suggestion that pre-PIE also showed a change **g* > **h₁* deserves serious attention. If this indeed turns out to be an existing sound law, it would be interesting to see whether the conditions are in any way similar to those for **d* > **h₁*.

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