THE PHONETICS AND PHONOLOGY OF HITTITE INTERVOCALIC FORTIS AND LENIS STOPS

Alwin KLOEKHORST

Abstract

In the field of Hittite linguistics there is a longstanding debate on the phonetics of the Hittite fortis and lenis stops in intervocalic position, and on how to phonologically interpret the distinction between these two series. Although it is usually assumed that the two series were distinct in voice, /t/ vs. /d/, respectively, arguments in favor of a length distinction, /t:/ vs. /t/, have been put forward as well (Melchert 1994 and Kloekhorst 2008; 2014; 2016). This article will discuss two recent treatments of this topic: one by Simon (2020), who specifically argues against a length distinction, and adduces new arguments in favor of the traditionally assumed voice distinction; and one by Patri (2009; 2019[1])), who rather posits a distinction /t^h/ vs. /d/. It will be argued that the arguments used by both Simon and Patri are untenable, and that all evidence rather points to a length contrast, /t:/ vs. /t/.

Key words: *Hittite phonology, stop system, cuneiform orthography, historical phonology*

1. Introduction

Since Sturtevant (1932) it is generally acknowledged that Hittite shows in intervocalic position two types of stop series: one, called "fortis", that is spelled with geminates (*VT-TV*) and in principle corresponds to PIE voiceless stops (**t*, etc.); and another, called "lenis", that is spelled with singletons (*V-TV*) and generally corresponds to PIE voiced and voiced aspirated stops (**d* and **d^h*, etc.). There is debate, however, on the synchronic phonetic and phonological interpretation of these two series.

The default assumption, which can be found in many handbooks (e.g. Luraghi 1997: 3-4; Kimball 1999: 54; Watkins 2004: 556; Vanséveren 2006: 39-40; Weiss 2009: 90; van den Hout 2011: 64; Francia & Pisaniello 2019: 19) is that, in line with their Indo-European cognates, the fortis series denote voiceless stops ([t], etc.) and the lenis series voiced stops ([d], etc.), and that the two series thus contrasted in voice. A different view was presented by Melchert (1994: 14-21, 147), however, who argued that the fortis series should rather be interpreted as containing *long* voiceless stops ([t:], etc.). As he explains, the consequence of this interpretation is that the phonological contrast with the lenis series, which in intervocalic position were short voiced stops ([d], etc.), must have been length, which implies that the voice of intervocalic [d], etc. was merely allophonic. The underlying, phonemic system would then have been /t:/ vs. /t/. I myself have in several publications endorsed this view and presented additional arguments in its favor, including evidence that indicates that the length contrast between /t:/ and /t/ can be found in consonant clusters and other positions of the word as well (Kloekhorst 2008: 21-5; 2014: 544-7; 2016: 213-23). The main principle of this model has also been adopted by Yates (2019).[2])

In a recent paper, Simon (2020) offers a new discussion of the phonetics and phonology of the Hittite intervocalic stops,[3]) evaluating all arguments that have been put forward by Melchert and myself. He concludes that they are all invalid and that our postulation of a length contrast thus cannot be substantiated. On the basis of a new type of evidence, namely the rendering of Hittite names and lexemes in the writing systems of other languages, he instead argues that the contrast between intervocalic fortis and lenis stops was rather voice: /t/ vs. /d/. Interestingly, the same type of data was also used by Patri (2009; 2019), who came to a quite different conclusion regarding fortis stops, however, namely that these were in fact voiceless aspirates: /t^h/, etc.

In the following sections I will discuss the arguments and proposals by both Simon and Patri. I will argue that these are based on incorrect premises, and that all evidence indicates that in intervocalic position the contrast between the Hittite fortis and lenis stop series was length.

2. Evaluating Simon 2020

Simon's 2020 article starts with a discussion of seven arguments made by Melchert and myself (two that are specifically geared against the traditionally assumed voice contrast, and five that speak in favor of a length contrast), of which he concludes that they all should be rejected. Then he moves on to present new evidence that would speak in favor of a voice contrast. In the following evaluation of Simon's article, I will maintain this order.

2.1 Simon's discussion of the two arguments against a voice contrast

2.1.1

The first argument against a voice contrast treated by Simon concerns spelling. As was mentioned above, in order to write the difference between the fortis and lenis series, the Hittite scribes used geminate vs. singleton spelling (*Vt-tV* vs. *V-tV*; *Vk-kV* vs. *V-kV*; etc.), but not the voice distinction that is available in the cuneiform script (cf. the voice distinction in sign pairs like PA vs. BA, or TU vs. DU, or KI vs. GI). As I have argued in e.g. Kloekhorst 2014: 544-5; 2016: 214, this situation implies that the phonological distinction

[1]) A review of this book by Alexander Vertegaal has appeared in *BiOr* 78 1/2: 173-7 (Vertegaal 2021).

[2]) Although this new interpretation has not yet found its way into more general handbooks of Hittite, some publications do note the possibility that the traditional interpretation needs to be adapted: cf. Hoffner & Melchert (2008: 35): "For the sake of simplicity we here describe the contrast in stops as one of voicing, but we do not mean thereby to take a definitive stance on this issue"; and Rieken (2011: 39): "Es ist aber nicht klar, ob es sich bei der genannten phonemischen Distinktion tatsächlich auch phonetisch um einen Kontrast zwischen stimmhaft und stimmlos handelt […]. Der Konvention entsprechend ist im Folgenden stets von stimmhaften und stimmlosen Plosiven die Rede".

[3]) The main point of Simon's 2020 article is in fact to refute my 2016 hypothesis that the length contrast that we find in the Hittite stop system was inherited as such from Proto-Indo-European (which I reconstruct as having a stop system of the shape */t:/, */^{h}t/, */t/), and that the voice contrast that we find in the other Indo-European languages (which go back to a system */t/, */^{h}d/, */d/) is the result of a common innovation, which would then constitute an argument in favor of the Indo-Anatolian hypothesis (Kloekhorst 2016). In the present article, I will only discuss the synchronic analysis of the Hittite stop system, however. I plan to discuss the stop systems that can be reconstructed for Proto-Anatolian and Proto-Indo-European on a different occasion (see also n. 51).

between the fortis and lenis series was not voice (/t/ vs. /d/), but instead rather points to a length contrast (/t:/ vs. /t/). Simon admits that this argument has some merit and agrees that if the difference between the two series were voice, "it remains unexplained why the Hittites would have invented a much more complicated system" to note down this difference (2020: 236). Moreover, Simon accepts the studies of myself (Kloekhorst 2010; 2014) and Pascual Coello (2014) that show that "Hittite scribes were aware of the phonetic distinction between the voiced and voiceless series [of cuneiform signs] and even of their original voiced and voiceless value" (2020: 237), and that these values were used as such to indicate a voice contrast in, for instance, the word-initial position (where e.g. the sign KI is used to write initial [ki-] with voiceless [k], whereas the sign GI denotes [gi-], with voiced [g]). This fact makes it even more peculiar that only in intervocalic position the Hittite scribes would not have used these graphic means to mark a voice distinction.

Nevertheless, Simon does not accept the ultimate consequence of this line of thought, namely that the basic contrast between intervocalic fortis and lenis stops cannot therefore have been voice. According to Simon, "the precise origins of the Hittite cuneiform are still unclear […] and thus it is still not possible to exclude that the orthographic practice of single/geminate spelling is inherited, [i.e. that] the Hittites did not invent this system themselves, but rather adopted it" (2020: 237). This statement is gratuitous, however, since it merely replaces the problem. It implies that the Hittite ductus was taken over from another cuneiform tradition that did use the voice distinction that is available in the cuneiform script to indicate the difference between voiceless vs. voiced stops in some environments in the word (e.g. word-initial position), but not in intervocalic position, where instead, for unexplained reasons, geminate vs. singleton spelling was used to mark this distinction. No matter how one assumes that the Hittite scribal tradition first started, it is clear that its ductus ultimately derives from an Old Babylonian tradition that was probably in use somewhere in North Syria. Since in standard Old Babylonian the voice distinction in sign pairs like TA vs. DA and KI vs. GI were used to render a voice difference, whereas geminate vs. single spelling was used to render a length difference,[4]) it remains the simplest assumption that in Hittite, too, geminate vs. single spelling was used to indicate a distinction in length.

2.1.2

The second argument against a voice contrast treated by Simon concerns the form *e-ku-ut-ta* 'he drank'. It is generally assumed that the *u* that can be found in this form is not a real vowel (cf. the alternative spelling *e-uk-ta*), but rather denotes the labial element of a labiovelar stop. Nevertheless, the fact that in *e-ku-ut-ta* this labial element was graphically indicated with the vowel *u* creates a situation in which both the labiovelar and the dental consonant are in graphic intervocalic position, and that therefore their nature can be discerned; the single spelling of the labiovelar stop points to a lenis consonant, whereas the geminate spelling of the dental stop points to a fortis consonant. Within a system in which the contrast between fortis and lenis stops was voice, this form would represent [ʔeg^{w}ta], with a cluster [-g^{w}t-] consisting of a voiced [g^{w}] + voiceless [t]. I have argued on several occasions (Kloekhorst 2008: 23; 2014: 545; 2016: 214-5) that in such a form one would expect voice assimilation of the cluster (which should have yielded either [ʔeg^{w}da], spelled ***e-ku-ta*, or [ʔek^{w}ta], spelled ***e-ek-ku-ut-ta*), and that the absence of such an assimilation rather indicates that the two stops did not differ in voice. This form would thus constitute an argument against the idea that the contrast between fortis and lenis stops was voice. However, if this contrast were length, I argued, the existence of the form *e-ku-ut-ta* would be straightforward: it would represent [ʔek^{w}t:a] with an unproblematic cluster [-k^{w}t:-].

Simon objects to my argument that it is "based on the misunderstanding that voice assimilation is a universal feature, which it is not", and he gives some examples from Georgian where no voice assimilation takes place in clusters of the shape *-bt-* and *-tb-* (2020: 237). I concede that this is a good point: it cannot *a priori* by excluded that Hittite tolerated clusters of voiced and voiceless stops, and that a form [ʔeg^{w}ta], with a cluster [-g^{w}t-], could therefore have existed as such. In that sense, Simon is right that the example *e-ku-ut-ta* does not necessarily exclude a voice contrast for the fortis and lenis stops.

This situation changes, however, if we take Yates' recent discussion of this form into account (Yates 2019). As Yates convincingly shows (2019: 262-71), in all Hittite words containing a stop + stop cluster, the first stop is lenis.[5]) This is best seen in cases in which the first stop is labiovelar: these are always spelled °*V-ku-uT-TV*° (like *e-ku-ut-ta* 'he drank' and *ša-ku-ut-ta-i*° 'thigh'), but we never find the spelling **°*Vk-ku-uT-TV*°. But Yates is able to demonstrate this for other cases as well, like *u̯a-at-ku-* 'to leap' (never spelled ***u̯a-at-tu-uk-*) or *ḫar-ta-a̯k-kV*° 'bear' (never spelled ***ḫar-at-ta-ak-kV*°). To these may be added forms like *e-ep-ta* 'he seized', which is never spelled ***e-ep-pát-ta* or ***e-ep-pa-at-ta*.[6]) Especially examples of the latter type are interesting, since the labial stop of *e-ep-ta* 'he seized' etymologically reflects a PIE voiceless stop: *e-ep-ta* < *$h_1$ép-to*.[7])

According to Yates, the shape of these stop + stop clusters can be explained by assuming that Sturtevant's Law, which he interprets as a sound law according to which PIE voiceless stops were lengthened (PIE **t* > Hitt. [t:]) and PIE voiced (aspirated) stops were devoiced (PIE **d*$^{(h)}$ > Hitt. [t]), was not, as usually thought, fully unconditional, but that its initial

[4]) As is the case in Old Babylonian texts from Alalaḫ VII (Kloekhorst 2010: 231-8; Popova 2016), the ductus of which best resembles the Hittite one. Note that Simon (2020: 237) implies that I have claimed that Alalaḫ was the *direct* source of the Hittite cuneiform script (likewise Popova 2016), but this is not true. I have in my articles always used phrases like "the typical Hittite ductus *best resembles* the ductus as found in Old Babylonian texts from Alalaḫ (Tell Açana), level VII (18-17th century BC)" (Kloekhorst 2010: 231; emphasis added; likewise, Kloekhorst 2013: 125[1]), which is completely in line with the information provided by Rüster & Neu 1989: 15 (cf. also Van den Hout 2012).

[5]) Cf. Kloekhorst 2020: 165 (which went to press before Yates 2019 was available), where I similarly stated that "we may assume that before stops th[e] distinction [between /t:/ and /t/] was neutralized [and] that the phonetic realization of the dental stop in this position was short and voiceless: [t]". When I wrote this, I had not yet realized the consequences of this idea for Hittite phonology as a whole, however, which have been excellently discussed by Yates.

[6]) Whereas forms like *li-in-kat-ta* next to *li-in-ik-ta* for /línkt:a/ 'he swore' and *ša-ak-ka$_4$-aḫ-ḫi* next to *ša-ak-ḫi* for /sák:χ:i/ 'I know' (cf. also Yates 2019: 265) do occur: the absence of the spellings ***e-ep-pát-ta* or ***e-ep-pa-at-ta* thus seems to be significant.

[7]) Cf. the 1sg.pret. form *e-ep-pu-un* 'I seized' < **$h_1$ép-m*, where the fortis character of *-pp-* < PIE **p* is expressed in spelling.

part, i.e. the lengthening of PIE voiceless stops, was blocked in the position before another stop.[8]) This means that in a preform like *$h_1$épto*, containing a cluster of PIE **p* and **t*, the cluster's first member, **p*, was unaffected by Sturtevant's Law, and thus remained a short voiceless stop, whereas its second member, **t*, did undergo lengthening. The result was Hitt. [ʔept:a], spelled *e-ep-ta*, with a cluster [-pt:-].[9])

This latter example is lethal for the view that Hittite lenis stops were phonetically voiced. If we would apply Simon's interpretation of *e-ku-ut-ta* 'he drank' as [ʔeg^{w}ta] (containing a cluster of voiced [g^{w}] + voiceless [t]) to the form *e-ep-ta* 'he seized', we would have to assume that the latter represents [ʔebta], with a cluster consisting of voiced [b] + voiceless [t]. From an etymological point of view, it would be impossible to explain the rise of such a cluster, however: *e-ep-ta* reflects earlier **$h_1$épto*, with two voiceless stops, and there is no reasonable way in which its **p* could have undergone voicing in the prehistory of Hittite. There can thus be no doubt that the *p* in *e-ep-ta* synchronically was a voiceless stop. In view of Yates' demonstration that in stop + stop clusters the first member is always lenis, it is thus inescapable to conclude that *e-ep-ta* contains a *lenis* consonant that is *voiceless*, [p]. By analogy, this should also apply to the lenis labiovelar of *e-ku-ut-ta*, which we therefore must interpret as a *voiceless* [k^{w}].[10]) The voiceless value of these lenis stops is incompatible with the traditional view that the contrast between fortis and lenis stops was voice. It is compatible, however, with the view that this contrast was length: in this way, lenis [p] and [k^{w}] can be distinguished from their fortis counterparts, which were long, [p:] and [k^{w}:], respectively.

2.2 Simon's discussion of arguments in favor of a length contrast

2.2.1

The first argument in favor of length critically discussed by Simon runs as follows. In the prehistory of Hittite a long */ī/ is shortened when occurring in a closed syllable (e.g. 1sg. *kišḫa* 'I become' < **kīšḫa* < **Kéis-h_2e*), but not in an open syllable (e.g. 3sg. *kīša* 'he becomes' < **Kéis-o*). In the word *kitta* 'he lies' < **kītta* < **kéi-to*, shortening of */ī/ is found before fortis *-tt-* < PIE **t*. As I have argued in Kloekhorst 2008: 23; 2014: 418-20, 545-6; 2016: 215, this implies that this stop behaves as a cluster, and thus must have been long: /t:/.

In his discussion of this argument, Simon does not deny that the shortening of pre-Hitt. */ī/ in closed syllables is real, nor that the vowel /i/ in *kitta* is short and should go back to earlier /ī/. However, according to Simon, the short character of the /i/ in *kitta* is not caused by the *-tt-* that follows it. He rather proposes that in this form the short /i/ was taken over from other forms of the paradigm, where it is the result of an earlier long */ī/ that stood in front of a real consonant cluster and therefore was regularly shortened. In order to illustrate this point, Simon gives the following reconstruction of the pre-Hittite stages of the paradigm of *ki-$^{tta(ri)}$* 'to lie', in which stage 2 represents the stage in which earlier */ī/ was shortened when standing before a cluster ('>' indicates a phonological development; '→' indicates an analogical development):

	PIE	stage 1	stage 2	Old Hittite	
sg.	1 **ḱéi-h_2e*	>**kīHa*	>**kīHa*	→ **ki-iḫ-ḫa*	[kiχa]
	2 **ḱéi-th_2o*	>**kītHa*	>****kitHa***	> ******ki-it-ta**	[kita]
	3 **ḱéi-to*	>**kīta*	>**kīta*	→ *ki-it-ta*	[kita]
pl.	1 **ḱéi-wosd^{h}h$_2$o*	>**kīwasta*	>**kīwasta*	→ **ki-wa-aš-ta*	[kiwasta]
	2 **ḱéi-d^{h}h$_2$we*	>**kītuwo*	>**kītuwa*	→ **ki-it-tu-ma*	[kituma]
	3 **ḱéi-nto*	>**kīnta*	>****kinta***	→ (*ki-(ya-)an-da*)	[kiyanda]

Simon states that, within this scenario, "*/i/ already existed in the paradigm of ['to lie'] independently of the [3sg.pres.] *-tta*-ending, and thus one cannot exclude that it simply spread through the entire paradigm by levelling out the alternation **kī-/ki-*" (2020: 239). As a parallel to this development, he cites the verb *kīš-/kiš-* 'to happen, to become', "where the allomorph *kiš-* ousted *kīš-* in New Hittite" (*ibid.*). For this latter statement, he refers to Kloekhorst 2008: 480, but this must be a mistake. I never claimed that the development of, for instance, the OH 3sg.pres. form *ki-i-ša* /kísa/ to NH *ki-ša* /kísa/ is the result of levelling. In fact, I have in Kloekhorst 2014: 471-2 argued that the shortening of the /ī/ of OH *kīša* to the /i/ of NH *kiša* is the result of a regular, phonetic shortening that took place in the Middle Hittite period. The development found in the verb *kīš-$^{a(ri)}$* 'to happen, to become' cannot therefore be used as a parallel to the development that Simon argues to have taken place in *ki-$^{tta(ri)}$* 'to lie'. If anything, it would rather support the view that the short /i/ of 3sg.pres. *kitta* 'he lies' is the result of a phonetic shortening as well.

Another problematic aspect of Simon's scenario is that within the pre-Hittite paradigm of 'to lie' the short */i/ would only have been regular in the 2sg. and 3pl. forms (cf. the forms marked in **bold** in stage 2), whereas all other forms should regularly have had */ī/. The spread of the short */i/ to all forms of the paradigm would thus have been based on these two forms only. According to Simon, this is no problem, however, because "nothing excludes the possibility that analogy starts from the minority of the forms" (2020: 239). Moreover, he refers to the paradigm of *dā-^{i} / d-* 'to take', where the same development would have taken place (*ibid.*). Yet, as we will see below, this is incorrect: the paradigm of *dā-^{i} / d-* does *not* show a similar levelling, and therefore cannot be used as a parallel. Moreover, in the case of *ki-$^{tta(ri)}$* 'to

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[8]) Yates assumes that, when standing before voiceless stops, PIE voiced (aspirated) stops first were devoiced by assimilation, e.g. **$h_1$eg^{wh}-to* > pre-Hitt. *[ʔék^{w}to], after which Sturtevant's Law caused lengthening only of the second member of the cluster, but not of the first member, because this one stood in a position before another stop. The result was thus *[ʔek^{w}t:a], spelled *e-ku-ut-ta* (note that Yates does not assume an initial [ʔ] in Hittite, but this is irrelevant for the present argument).

[9]) Personally, I believe that the length contrast between fortis and lenis stops was present already in Proto-Indo-European, and that the Hittite length contrast was inherited (see also footnote 51). I therefore do not need to assume the existence of Sturtevant's Law as a sound law: to my mind, we would just have to assume that in stop + stop clusters, the first member was neutral to length, and that also when a cluster morphologically consisted of a combination of a fortis (= long) stop + stop, the first stop was phonetically realized as a short one. This difference of opinion with Yates (which I will discuss in more detail elsewhere) has no ramifications for the synchronic interpretation of Hittite phonology, however, for which I completely agree with Yates.

[10]) If we would allow the lenis labiovelar in *e-ku-ut-ta* to be voiced, [g^{w}], we would in fact assume two different types of lenis stops in Hittite, a voiceless and a voiced one, which amounts to inventing a new phoneme.

lie', we have to take into account that synchronically in Hittite only 3sg. and 3pl. forms are attested, but none of the other forms of the paradigm. Given the relative high number of attestations of 3sg. and 3pl. forms in Hittite texts, the absence of 1st and 2nd person forms seems to be systematic. And although these forms may certainly have existed in earlier times (cf. the attestation of 1sg. *siχani* 'I will lie' in Lycian), it is rather unfortunate for Simon that he needs to invoke an analogy that is for a large part based on the 2sg. pres. form **kitta* < **ḱéi-th$_2$o* that itself is unattested in Hittite texts.

All in all, Simon's scenario has little to recommend itself, and I maintain that the presence of a short /i/ in *kitta* 'he lies' is best explained as the result of a shortening of original */ī/ before fortis *-tt-*, which, in turn, implies that this consonant was long: /t:/.

2.2.2

The second argument in favor of a length contrast that is negatively assessed by Simon is based on the fact that any OH long /ā/ is shortened to NH /a/ in closed, non-final syllables.[11]) As argued in Kloekhorst 2016: 215-6, this shortening is also found when /ā/ is followed by an intervocalic fortis stop, for which I gave the examples OH *dātti* > NH *datti* 'you take', OH *dātten* > NH *datten* 'you must take', and OH *šākki* > NH *šakki* 'he knows'. This means that in these words these fortis stops behave as a cluster, and thus must have been long: *-tt-* = /t:/ and *-kk-* = /k:/.

Although Simon acknowledges that the shortening of OH /ā/ to NH /a/ in non-final closed syllables is a regular development, he states that the examples involving forms from the verbs *dā-^{i} / d-* 'to take' and *šākk-^{i}* 'to know' as given above "are not probative, since both are ablauting verbs from Old Hittite onwards, having both /ā/ and /a/ in their paradigms" (2020: 239).

In the case of *šākk-^{i}* 'to know', Simon points out that this verb originally belonged to the class of *ā*/*a*-ablauting verbs (with reference to Kloekhorst 2008: 695), implying that we can assume that in the original 3sg.pres. form *šākki*, which showed the strong stem *šākk-* (< **sókH-*), the weak stem *šakk-* was introduced through levelling. However, as shown in Kloekhorst 2012: 155-6, the weak stem *šakk-* (ultimately from **skH-*) is only rarely attested: it is only found in the OS 2pl.pres. form *šaktēni*, the OH/NS 3pl.pres. form *šakanzi*, and a few MS attestations of the participle *šakkant-*. Already in MH times the verbal paradigm of 'to know' had undergone a levelling by which the weak stem *šakk-* had been fully replaced by the secondary stem *šekk-* (giving rise to newly created forms like 2pl.pres. *šekteni*, 3pl.pres. *šekkanzi*, part. *šekkant-*). Yet, the development of *šākki* to *šakki* does not take place until at the end of the early New Hittite period (Kloekhorst 2014: 269-70, 276), that is, at a time that the original weak stem *šakk-* had already been fully ousted by the secondary stem *šekk-*. It is thus impossible that the NH form *šakki* would be the result of a replacement of the original strong stem *šākk-* by the weak stem *šakk-*. Instead, the development of OH *šākki* to NH *šakki* can only have been caused by a phonetic development, i.e. the shortening of earlier /ā/ to /a/. Since we know that such a shortening regularly takes place in closed syllables, it implies that the fortis stop *-kk-* that follows the vowel /ā/ closed the preceding syllable, which in turn means that it must have been a long stop: [k:].

When it comes to the NH forms *datti* 'you take' and *datten* 'you must take', Simon proposes to explain these forms, too, as the result of a levelling of short /a/ throughout the paradigm of *dā-^{i} / d-* 'to take', which, as he claims, is illustrated by the fact that "New Hittite spellings [of this verb] show a short /a/ also in those cases where Kloekhorst's sound law does not apply" (2020: 240). As examples he cites the following forms (with reference to my own dictionary, Kloekhorst 2008: 803): 3sg.pres.act. *dai*, 3sg.pret.act. *taš*, 1pl.pret.act. *dawēn*, 1sg.imp.act. *talit*, and 3sg.imp.act. *dau*. Upon closer scrutiny, none of these forms is probative, however. The 3sg.pres.act. form "*da-i*", which I indeed cited in Kloekhorst 2008: 803, does not exist: I probably mistook a 2sg.imp. form *da-i* from the paradigm *dai-^{i} / ti-* 'to put' as a 3sg.pres.act. form of 'to take' (cf. Kloekhorst 2014: 392[1521]). Instead, the 3sg.pres.act. form 'he takes' is always spelled *da-a-i*, in Old, Middle and New Hittite texts (dozens of attestations), with plene spelling of *a* that denotes the presence of a long /ā/: its length has thus been retained through time. The 3sg.pres.act. form *ta-aš* is only attested in KBo 18.151, a text that is notorious for its aberrant spellings.[12]) In all other texts, Old, Middle as well as New Hittite ones (dozens of times), we only find the spelling *da-a-aš* (Kloekhorst 2014: 240), again with a plene spelled *a* that marks a long /ā/. This means that in this form, too, the length of the /ā/ was retained through time. The 1pl.pret.act. form is in NS texts indeed attested once as *da-u-en*, with non-plene spelling of its *a* (KUB 26.66 iii 16 (NS)), but we also find four plene spelled attestations, *da-a-u(-e)-en*.[13]) These indicate that the *a* in this form was a long vowel, /ā/, which means that also in this form its length was retained through time. The 1sg. imp.act. form *ta-li-it* (KBo 3.38 rev. 16 (OH/NS)) is indeed attested in this shape, with non-plene spelling of its *a*, once, but since it is only a single form, it does not say too much.[14]) The 3sg.imp.act. form "*da-ú*" that is cited in my dictionary (Kloekhorst 2008: 803) does not exist: it was based on the citation of a form "*da-ú*" by García Trabazo 2002: 514 for KUB 4.1 i 37, where we actually find *da-a-ú*. In fact, this form is in texts of all Hittite periods, including in New Hittite ones, only attested with the spelling *da-a-ú* (dozens of examples, cf. Kloekhorst 2014: 396[1548]). The plene spelling of its *a* again marks the presence of a long /ā/, which had retained its length throughout the Hittite period.

If we now compare the Old Hittite paradigm[15]) of the verb *dā-^{i} / d-* 'to take' to its New Hittite version, we get the following picture:

[11]) Cf. Kloekhorst 2008: 98; 2014: 256-307.

[12]) Cf. e.g. Soysal 2000: 113-4; Kloekhorst 2010: 208[29]; 2014: 240[867]; van den Hout 2012: 166.

[13]) Cf. Kloekhorst 2014: 411[1603]: *da-a-u-e-en* (KUB 13.4 iv 74 (OH/NS)), *da-a-u-en* (KBo 3.45 obv. 5 (fr.) (OH/NS), KUB 8.80, 20 (NS), KUB 13.35 iv 1 (NS)).

[14]) Its spelling with the sign TA is aberrant as well: the verb *dā-^{i} / d-* is virtually always spelled with the sign DA.

[15]) All these forms are attested in OS texts, except 2sg.pret. *dātta* (MS), and 3pl.imp. *dandu* (MS). There can be no doubt, however, that in Old Hittite these forms had these shapes, as well. The 2pl.pret. form *dātten* is assumed on the basis of the OH 2pl.imp. form.

pres. OH	NH	pret. OH	NH	imp. OH	NH
1sg. *dāḫḫe/i*	***daḫḫi***	1sg. *dāḫḫun*	***daḫḫun***	1sg. --	*talit*
2sg. *dātti*	***datti***	2sg. *dātta*	***datta***	2sg. *dā*	*dā*
3sg. *dāi*	*dāi*	3sg. *dāš*	*dāš*	3sg. *dāu*	*dāu*
1pl. *tumeni*	*tumeni*	1pl. *dāu̯en*	*dāu̯en*		
2pl. *datteni*	*datteni*	2pl. [*dātten*]	***datten***	2pl *dātten*	***datten***
3pl. *danzi*	*danzi*	3pl. *dāer*	*dāer*	3pl. *dandu*	*dandu*

The forms in which an OH /ā/ has developed into a NH short /a/ (indicated here in bold)[16]) are all forms in which this vowel stands in a non-final syllable and is followed by a consonant that is spelled geminate. In all other forms that contain an OH long /ā/ this vowel has been retained as such. The rise of /a/ in the NH forms *daḫḫi*, *datti*, *daḫḫun*, *datta* and *datten* cannot therefore have been the result of levelling, since such a levelling should have affected other forms as well. It thus must have been the result of a regular phonetic development, and the only reasonable conditioning of this sound law is that in these forms the /ā/ stood in a closed syllable. This implies, however, that the geminate spelled fortis consonants *-ḫḫ-* and *-tt-* were long consonants: [χ:] and [t:], respectively.

2.2.3

The third argument in favor of a length contrast discussed by Simon concerns the spelling of resonants and of *ḫ* (to which *š* can be added as well, although this consonant is not mentioned by Simon). For these consonants we find in spelling a distinction between geminates and singletons, as well, which, at least in the case of resonants and of *š*,[17]) is generally interpreted as indicating a contrast in length: *Vr-rV* = /r:/ vs. *V-rV* = /r/, etc.; *Vš-šV* = /s:/ vs. *V-šV* = /s/. As I have argued in Kloekhorst 2014: 547; 2016: 216-7, this fact is extra support in favor of interpreting the geminate vs. single spelling in stops as indicating a contrast in length, as well.

According to Simon, however, this argument does not hold, because "the phonemic contrast in resonants is irrelevant for the phonemic contrasts in stops" (2020: 240). As an example, he cites Hungarian, which does make a distinction between single and geminate continuants (e.g. *hal* 'fish' vs. *hall* 'to hear'), whereas in its stop system the basic distinction is voice.[18])

I am afraid Simon has misunderstood my argument. Its point is that the *spelling* of the length contrast in resonants (as well as in the case of *ḫ* and of *š*), i.e. geminate spelling *VC-CV* vs. singleton spelling *V-CV*, is of the exact same structure as the spelling difference between fortis stops (*VT-TV*) and lenis stops (*V-TV*). And since in resonants (and in *ḫ* and in *š*) this spelling difference marks a contrast in length (e.g. /r:/ vs. /r/; /s:/ vs. /s/), the default assumption should be that in the case of stops this spelling strategy likewise marks a contrast in length.

2.2.4

The fourth argument in favor of a length contrast that is critically discussed by Simon concerns the assibilation of PIE **ti̯-* > Hitt. *z-* = [ts-][19]) and of PIE **di̯-* > Hitt. *š-* = [s-]. In Kloekhorst 2016: 219-20, I argued that if the traditional interpretation of Hittite fortis stops as [t], etc. and of lenis stops as [d], etc. is correct, and if the pre-Hittite inputs of these assibilation processes thus were clusters with the phonetic shapes *[tj-] and *[dj-], respectively, we would expect outcome pairs like *[ts-] vs. *[dz-] or *[s-] vs. *[z-], but not the [ts-] vs. [s-] that we actually find. I therefore argued that these outcomes are better explained within the framework that sees the contrast between fortis and lenis stops as length. In this way, we can assume that the inputs of these assibilation processes were pre-Hittite clusters of the shape *[t:j-] and *[tj-], respectively, and that they developed in a symmetrical way, namely *[t:j-] = *[ttj-] > *[tt^{j}-] > Hitt. [ts-] (spelled *z-*) and *[tj-] > *[t^{j}-] > Hitt. [s-] (spelled *š-*), respectively. This would then imply that, in word-initial position, PIE **t-* had yielded a pre-Hittite long *[t:-], which contrasts with PIE **d$^{(h)}$-* that yielded a pre-Hittite short *[t-]. These word-initial values of the outcomes of PIE **t* and **d$^{(h)}$* would then support the interpretation of their intervocalic outcomes in Hittite as /t:/ vs. /t/, i.e. with a contrast in length.

According to Simon, there are two objections to be made against this line of reasoning. First, he states that "the exact phonetic nature of the signs with <z> is undetermined: one cannot exclude that they represented a [z] in specific cases" (2020: 241, with reference to Hoffner & Melchert 2008: 47). I strongly disagree with this point: there simply is no good argument in favor of interpreting *z*-signs as denoting the voiced sibilant [z] anywhere in Hittite.[20]) Moreover, a value [z] for these signs is fully contrary to what is to be expected: in the older stages of Akkadian, including Old Babylonian, the phonemes that in Akkadian linguistics are traditionally noted down as /s/, /z/, and /ṣ/ were in fact dental affricates, [t͡s], [d̪z̪] and [t͡sˀ], respectively (Kogan 2011: 66-7).[21]) This means that in the Old Babylonian ductus the *z*-signs, which are usually used to render the phonemes /z/ and /ṣ/, but in some variants render /s/ as well, only denoted affricates, [d̪z̪], [t͡sˀ] and [t͡s], but not the sibilant [z]. It is therefore fully unexpected that in Hittite *z*-signs would ever be used to render a [z].

Simon's second argument is that, when it comes to the palatalization or assibilation of dental stops, "the direction of phonological changes is neither obligatory nor necessarily parallel. Thus nothing excludes that the reflexes of **di* and **ti* in the same language will be different in terms of voice", for which he cites examples from Italian: *giorno* [d̪ʒorno] 'day' < **di-* vs. *zio* [tsio] 'uncle' < *ti-* (2020: 241). However,

[16]) The one attestation *ta-li-it* has been left out of consideration.

[17]) Simon asserts that in the case of *ḫ*, which he calls "laryngeals" (mixing up etymological origin vs. synchronic value), "the difference between single and geminate spelling […] is not and cannot be the length" (2020: 240, with reference to Simon 2014). However, as extensively argued in Kloekhorst 2018 (where the arguments of Simon 2014 have been discussed), also in the case of *ḫ*, which represents a uvular fricative, the difference between geminate vs. single spelling marks a phonological contrast in length: fortis *VḫḫV* = /χ:/ vs. lenis *VḫV* = /χ/ (albeit that lenis /χ/ was allophonically voiced to [ʁ] in intervocalic position), cf. Kloekhorst 2018: 82.

[18]) Note that Simon remarks that in Hungarian voiceless as well as voiced stops can occur both as singleton and as geminate, which weakens his own point to some degree.

[19]) It is often assumed that Hitt. *z* writes an affricate [t͡s], but see Kloekhorst 2019a for the view that *z* should rather be interpreted as denoting a cluster of [t] + [s]. For the present argumentation, this is irrelevant, however.

[20]) Cf. already Kloekhorst 2008: 26[38]; see also Kloekhorst 2019a: 55-6.

[21]) Whereas the phoneme /š/ was in fact the dental sibilant [s].

the point is that in Hittite the outcomes of *$t\hat{i}$- and *$d\hat{i}$- do *not* differ in voice: both outcomes are voiceless, [ts-] and [s-], respectively. This remains a relevant point: in an article dealing with the typology of stop assibilation, Hall & Hamann (2006) cite dozens of examples of assibilation of dental stops from a wide variety of languages, and in all cases assibilation of voiceless *t yields a voiceless outcome ([s], [ts], [tʃ], etc.), whereas assibilation of voiced *d yields a voiced outcome ([z], [dz], [dʒ], etc.). The fact that PIE *$d\hat{i}$- yields Hittite *š*- (which by all means represented a voiceless consonant [s-]) is therefore relevant and must be accounted for.

When it comes to my assertion that the outcomes of the pair *[tj-] and *[dj-] should have been parallel in structure (either [s-] vs. [z-], or [ts-] vs. [dz-]), I must admit that this was too rash. For instance, in Romanian plural formations, assibilation of *[tj] yields the affricate [ts], whereas *[dj] develops into the sibilant [z] (Hall & Hamann 2006: 1204). Similarly but oppositely, in Greek, original *$t\hat{i}$- assibilates to the sibilant σ- [s-], whereas *$d\hat{i}$- develops into the affricate ζ- [d͡z-]. Howeover, since in the latter language the development of *$t\hat{i}$- to [s-] probably went through the affricate *[t͡s-] (Rix 1992: 92), we may assume a similar development for Romanian: *[dj] should then first have yielded an affricate *[d͡z] which was later deaffricated to [z]. Whatever be the exact paths of development in these languages, the cited examples do show that the fact that PIE *$t\hat{i}$- yielded in Hittite the affricate [ts-][22]) whereas *$d\hat{i}$- yielded the sibilant [s-] cannot in principle be used as an argument against a pre-Hittite phonetic interpretation of these clusters as *[tj-] and *[dj-], respectively.

All in all, Simon is partly right: the fact that PIE *$t\hat{i}$- yields the outcome [ts-] (spelled *z*-) whereas PIE *$d\hat{i}$- yields the sibilant [s-] (spelled *š*-) cannot be used as a direct argument in favor of a pre-Hittite stage with long [t:] vs. short [t]. Yet, the fact that the outcome of PIE *$d\hat{i}$- in Hittite is a *voiceless* sibilant [s-] is remarkable, and would speak in favor of a prestage in which the dental stop was voiceless as well: *[tj-]. However, postulating a value *[t-] as the outcome of PIE *d- would be incompatible with the traditional view that the contrast between (pre-)Hittite fortis and lenis stops was voice. Yet, it would be compatible with the view that this contrast was length: we would then have to assume that at the moment of assibilation the relevant clusters had the shapes *[t:j-] and *[tj-], respectively. The fact that the former of these yielded Hittite [ts-], whereas the latter developed into [s-], would on a structural level be fully compatible with these values as well.

2.2.5

The fifth and last argument in favor of a length contrast discussed by Simon goes back to Melchert (1994: 147), who starts with the observation that PAnat. short accented */á/ is in Hittite lengthened in open syllables, but not in closed ones. Since the Hittite words *ḫuuappa-* 'evil' < PAnat. **Hwápo-* and *ḫatta(ri)* 'he pricks, cuts' < PAnat. **Háto-* (reconstructions according to Melchert *loc. cit.*) contain an *a* that is short (no plene spelling), Melchert states that in both words "the stop [following the *a*] acts to close the syllable, demonstrating that at least by pre-Hittite the intervocalic voiceless stop was geminate".

[22]) Or rather: cluster of [t] + [s], cf. footnote 19.

According to Simon (2020: 241), however, both examples are non-probative. In the case of *ḫuuappa-* 'evil', he assumes that its short /a/ has been taken over from the corresponding *ḫi*-verb *ḫuuapp-^{i}* / *ḫupp-* 'to be hostile, to do evil',[23]) the strong stem forms of which show a short /a/, as well. Simon states that in this paradigm the short /a/ is regular in forms where the ending started in a consonant (shortening in a closed syllable, i.e. before a consonant cluster, for instance in 2sg.pres.act. *ḫuuapti*), and from there already in pre-Hittite times not only spread to all strong stem forms of the paradigm itself, but also to the derived adjective *ḫuuappa-*. Again, Simon is forced to use an innerparadigmatic levelling (this time one that even spreads on into a nominal derivative), whereas we have seen in sections 2.2.1 and 2.2.2 above that there are certainly verbs where such levellings never took place. Melchert's scenario, which assumes shortening of an original long vowel due to the following fortis *-pp-* = /p:/, is much more straightforward.

When it comes to *ḫatta(ri)* 'he pricks, cuts', Simon states that its etymology is unknown, and that "thus this word cannot be used as an argument" (2020: 241). This is too dismissive, however. Within Anatolian, the Hittite verb *ḫatt-$^{a(ri)}$* can be compared to Lyc. *χtta(i)-* 'to do harm', which assures at least a Proto-Anatolian origin of the verbal root. Moreover, in Hittite the verb shows archaic morphological patterns: a medio-passive root-formation with the 3sg. ending *-a(ri)*, *ḫatt-$^{a(ri)}$*, that combines with a *-i̯e/a*-inflected active stem *ḫazzi̯e/a-^{zi}*. There can thus hardly be any doubt that this verb has a long history, and probably is of an Indo-European origin. Moreover, on the basis of our knowledge of the prehistory of Hittite morphology, we can with certainty state that the medio-passive stem *ḫatt-$^{a(ri)}$* belongs to the category that reflects the PIE structure **CéC-o*, which implies that its root can be transposed into PIE phonemes as **$h_2$et-*.[24]) Taken together, we can with certainty assume that its 3sg.midd. form *ḫatta(ri)* goes back to a preform **$h_2$ét-or(-i)* (as implied by Melchert's PAnat. reconstruction **Háto-*). Melchert is thus fully right to state that, if PIE **t* had yielded a Hittite short consonant, the vowel of the stem, through PAnat. */á/, should in Hittite have undergone lengthening.[25]) The absence of lengthening (cf. the consistent non-plene spelling *ḫa-at-ta(-)*, including in Old Hittite originals), thus indicates that this vowel stood in a closed syllable, implying that its *-tt-* closed the syllable, and thus was a long consonant: [t:]. Although Simon thinks that this word cannot be used as an argument because it has no clear etymology, he does add that "one may surmise that a similar scenario [as with *ḫuuapp-^{i}* / *ḫupp-*] applies to *ḫatta-* as well" (2020: 241), i.e. that its short /a/ is the result of a paradigmatic levelling from forms in which the /a/ stood before a consonant cluster. This cannot be the case, however. The only well attested medio-passive forms of *ḫatt-$^{a(ri)}$* are 3sg. *ḫatta*° and 3pl. *ḫattanta*°, and in both cases the root-final consonant *-tt-* is followed by a vowel.[26]) We cannot therefore assume levelling of a shortened

[23]) See Kloekhorst 2008: 369-71 for the fact that this verb originally was *ḫi*-conjugating.

[24]) With the consonants *h_2 and *t that are fully in line with the consonants of Lyc. *χtt-a(i)-*.

[25]) Cf. e.g. dat.-loc.sg. *paddăni* 'basket' < **p(e)th$_2$-én-i* (Kloekhorst 2014: 348-9).

[26]) The only other attested medio-passive form is 1sg.pres. *ḫa-ad-da-aḫ-ḫa-ri* (KUB 17.28 i 6 (MH/NS)), which is clearly based on the secondary stem *ḫadda-* that was formed in analogy with the verb *padda-* 'to dig, to

vowel from forms of the structure *ḫatC*°, since such forms do not seem to have been used at all.

All in all, Simon's attempts to deny the validity of Melchert's examples *ḫuuappa-* and *ḫatta(ri)* are to no avail: Melchert is clearly right in saying that the presence of a short /a/ in these forms is the result of a phonetic shortening, which implies that the consonants following the /a/ closed the preceding syllable, and thus phonetically must have been long, [p:] and [t:], respectively.

2.3 Simon's own arguments in favor of a voice contrast

After having attempted to dismiss all arguments put forward by Melchert and myself against a voice contrast and in favor of a length contrast, Simon presents a "type of evidence, completely neglected by Kloekhorst, that excludes the interpretation of these consonants as voiceless short and long stops", namely the ways in which "Hittite and Luwian words and names as well as borrowings from these languages" are transcribed in the writing systems of other, contemporary languages (2020: 241-2).

2.3.1 Methodological preliminaries

Before he embarks on treating this new evidence, Simon first states that in the works of Melchert's and myself in fact "two competing systems" can be found, namely one that assumes a length contrast of the type "-TT- : -D-" (for which he cites Melchert 1994 and Kloekhorst 2013), and one that assumes a length contrast of the type "-TT- : -T-" (for which he cites Kloekhorst 2016). This statement is based on a misunderstanding, however. All three publications cited by Simon clearly distinguish between the stops' *phonetic* quality and their *phonological* interpretation. In all three works, including Kloekhorst 2016 (see the very explicit remarks in footnote 12 on page 216), it is clearly stated that *phonetically* Hittite intervocalic fortis stops must be interpreted as long and voiceless ([t:], etc.) and intervocalic lenis stops as short and voiced ([d], etc.), but that *phonologically* the two series can be interpreted as showing a contrast in length only, which implies that the voiced character of the intervocalic lenis stops is allophonic. In other words, each of these three publications assumes a phonological length contrast /t:/ vs. /t/ that is based on the (intervocalic) phonetic distinction [t:] vs. [d]. There is thus only a single length contrast theory, and not "two competing" ones.

This is relevant for the following point. According to Simon (2020: 242), there are two ways in which the transcriptions of Hittite lexemes in other languages could prove that the length contrast theory has to be rejected:

(1) "if intervocalic [fortis stops] are not reflected as geminate stops in the languages that can express gemination"
(2) "if intervocalic [lenis stops] are reflected as voiced stops in the languages that can mark voice"

However, this second situation would only affect a length contrast theory that assumes that the phonetic value of intervocalic lenis stops was [t], etc. (contrasting with fortis [t:], etc.), but, as we have seen, none of the publications that speak in favor of a length contrast assumes this. Simon's demonstration that the Hittite intervocalic lenis stops are rendered as voiced stops in other languages (e.g. the name Puduḫepa that is rendered in Ugaritic as *pdġb*, with voiced *d* = [d], *ġ* = [ɣ] or [ʁ] and *b* = [b]) is therefore no argument against the length contrast theory.

So, the evidence adduced by Simon is only relevant when it comes to the way Hittite intervocalic fortis stops are rendered in the writing systems of other languages: if in writing systems that can express gemination these are not reflected as geminate stops, this would speak against assuming a length contrast.

2.3.2 The material treated by Simon

In his treatment of this new type of evidence, Simon notes that not all ancient Near Eastern languages can be used because some of them use writing systems that do not mark the relevant contrasts (Egyptian and Old Assyrian), whereas in the vocabularies of others no relevant lexemes have been found (Armenian and Hebrew). Nevertheless, according to Simon, "Aramaic, Greek, Neo-Assyrian, Neo-Babylonian, Phrygian, Phoenician, Ugaritic and Urartian transcriptions and loanwords do provide relevant data" (2020: 243). After having presented all this data, Simon concludes that Hittite fortis stops "were always and consistently perceived as voiceless consonants and in the languages which could mark geminate pronunciation they were not perceived as geminate consonants. […] Thus it must be concluded that the geminate theory of Melchert and Kloekhorst cannot explain [all the relevant] forms" (2020: 245).

One thing that is remarkable about the material presented by Simon, however, is the fact that the vast majority of the languages he takes into account are only attested in 1st millennium BCE sources (Aramaic, Greek, Neo-Assyrian, Neo-Babylonian, Phrygian, Phoenician, and Urartian), i.e. sometimes several centuries after Hittite ceased to be used as a written (and probably spoken) language. The sources written in these languages are thus not contemporaneous with the attested period of Hittite at all. The reason for Simon to include these languages in his overview is that he does not only treat Hittite lexemes that are transcribed in or taken over by these languages, but also Luwian ones. For instance, Simon cites as an example the Neo-Assyrian spelling *mut(t)alli*/*u*, which renders the name of kings of the Neo-Hittite states Gurgum and Kummuḫ, and which he compares to the CLuw. lexeme *muuattalla*/*ī-* (2020: 244). However, the kings referred to by these Neo-Assyrian attestations ruled in the 9th and 8th century BCE, i.e. more than 300 years after the last Hittite texts were written down. Moreover, these kings probably spoke (Iron Age) Luwian, not Hittite. It is therefore completely unclear to me why the Neo-Assyrian attestations of *mut(t)alli*/*u* would be relevant for determining the phonetics of the Hittite stop system. Apparently, Simon assumes that the (Iron Age) Luwian stop system was identical to the Hittite one, but this can hardly be correct: we know enough of the phonetic interpretation of the Hieroglyphic Luwian script to be certain that its consonant system had different distinctions (e.g. an opposition between stops and fricatives)[27]) than the Hittite one. This means that all 1st millennium material adduced by Simon is irrelevant.

Of all the data discussed by Simon with regard to the value of Hittite fortis stops (2020: 244-5), there remains only

prick' (cf. Kloekhorst 2020: 155[22]). This implies that the *-a-* between *-dd-* and *-ḫḫ-* is linguistically real, which means that also in this form the root final consonant was followed by a vowel.

[27]) See e.g. the discussion in Vertegaal 2019.

one example from a language that is contemporaneous with the attested period of Hittite: Ugaritic *pwt*, also spelled *puwatu* (in the Akkadian of Ugarit) and *puwati* (in syllabic Ugaritic), 'madder', which Simon compares to "Hitt./Luw." *puu̯atti-* 'madder(?)'. According to Simon, this is one of the cases in which a Hittite fortis *-tt-* is rendered in a foreign writing system as a singleton *-t-* (*puwa**t**u* and *puwa**t**i*), which would imply that Hittite fortis *-tt-* was a short voiceless stop [t]. However, there are many problems surrounding this comparison. As CHD (P: 369-70) states, the meaning of Hitt. *puu̯atti-* is not fully clear. It occurs only once (nom.sg. *pu-u̯a-at-ti-iš*) in a lexical list, where it glosses Sum. še-be-da and Akk. *ši-in-du*. This latter form can be read as Akk. *šimtu*, *šindu* "Kennzeichen, Farbe, Marke", but CHD remarks that "[n]owhere else does [this word] translate Sum. še-be-da". Consequently, "[w]ithout a real Hitt. context, and in view of the uncertainty of even the mng. of the Akk. entry, it is risky to assume that either "Akk." *šindu* or "Hitt." *puwattiš* means "mark" or "color"" (P: 369). Moreover, as CHD notes, we do not even know "if *puwattiš* is Hitt. or Luw." (P: 370). In fact, CHD explicitly states that the translation "madder(?)", which goes back to a discussion of these words by Hoffner 1967, is in fact "based on the assumption that Ugaritic *pwt* (a material used in dying and/or tanning) and Arabic *fuwwatu* 'dyers' madder' are related to this word" (P: 370). To all these uncertainties it can be added that, since Ugar. *pwt* has a cognate in Arab. *fuwwatu*, it seems *a priori* more likely that these words have a Semitic origin, which would imply that, if Hitt./Luw. *puu̯atti-* is cognate at all, it may rather be a Semitic loanword into Hittite than the other way around (thus Hoffner 1967: 303). According to Simon, however, this latter idea "is not probable, since the Anatolian word has a plausible etymology", namely one that connects *puu̯atti-* to the Hittite verb *puu̯ae-*^{zi} 'to pound, to grind' (2020: 244[14], with reference to Tischler HEG P: 679 and Puhvel HED P: 148). Yet, this etymology (which in fact was first mentioned as a mere possibility by Hoffer 1967: 303) does not make much sense from a morphological point of view: Hittite does not have a regular nominal suffix *-tti-*. All in all, if Ugaritic *pwt* / *puwatu* / *puwati* and Hitt./Luw. *puu̯atti-* are to be equated at all, it is much more likely that the Hittite/Luwian word was borrowed from Semitic than *vice versa*. This word cannot therefore be used in an argument regarding the phonetics and phonology of Hittite stops.

We can thus conclude that none of the data presented by Simon (2020: 243-5) yields any useful information on the phonetics of Hittite intervocalic fortis stops.

2.3.3 Material not treated by Simon

Does this mean that Simon's approach has no merit at all? This would be too bold a statement. Investigating the way in which Hittite lexemes are written in other languages can be insightful, but one has to choose the right data. In that sense it is quite odd that Simon does not mention Old Babylonian, Middle Babylonian and Middle Assyrian texts as possible sources for relevant data: these three dialects are contemporaneous with Hittite sources (Old Babylonian ca. 20th-16th c. BCE; Middle Babylonian and Middle Assyrian ca. 16th-10th c. BCE), and are written in versions of the cuneiform script that make a distinction between voiceless and voiced as well as long and short stops. They would thus be ideal candidates to assess the way in which Hittite stops are spelled.

And in fact, in these languages we do find interesting cases, especially the words with which they refer to the Hittite kingdom and the Hittites themselves, which all use the stem *ḫatt-* 'Hittite'. In Old/Middle Babylonian and in Middle Assyrian texts, all derivatives of this stem are always spelled *ḫa-at-tV(-)*, with a geminate *-tt-*,[28]) which, according to the spelling conventions of these dialects, represents a long (geminate) voiceless stop [t:]. It therefore stands to reason to assume that also in the Hittite lexemes that contain this stem, like ^{URU}*ḫattuša* 'Ḫattuša', ^{URU}*ḫattušumaš* 'person from Ḫattuša (nom.sg.)' and *ḫattili* 'in Hattic',[29]) the geminate spelled *-tt-* represents a long voiceless [t:]: [χat:usa], [χat:usomas] and [χat:ili].

Although Simon does not specifically say why he does not include Old/Middle Babylonian and Middle Assyrian material in his article, he does mention the stem *ḫatt-* 'Hittite'. He refers to this example in the context of "the methodological problem of the lack of an orthographic distinction between single and geminate voiceless consonants in Hittite cuneiform […]: Data with geminate spelling are not probative *if* we do not know their origin since we cannot exclude that they are originally geminates. These include the toponyms Ḫatti and Ḫattuša" (2020: 243-4; emphasis his). In other words, Simon does take into account the possibility that in some Hittite lexemes geminate spelling of the type *-tt-* may denote long stops, [t:], and he uses the stem *ḫatt-* as a case in point. This implies that he is aware of the fact that in languages other than Hittite (i.e. Old/Middle Babylonian and Middle Assyrian)[30]) the stem *ḫatt-* is consistently spelled with geminate spelling, and that this implies that its dental stop is long: [χat:-]. However, since the stem *ḫatt-* is not of an Indo-European origin (it derives from Hattic), Simon apparently thinks that its phonetic shape has no bearing on the question what the phonetic quality is of stops spelled *-tt-* in lexemes that *are* of an Indo-European origin, for which he assumes that their synchronic value was short, [t] (the outcome of PIE **t*).

I find this reasoning peculiar. It is unclear to me why Simon would accept the presence of a long stop [t:] spelled with geminate *-tt-* in the Hittite forms ^{URU}*ḫa-at-tu-ša-* [χat:usa] 'Ḫattuša', ^{URU}*ḫa-at-tu-šu-ma-aš* [χat:usomas] 'person from Ḫattuša' and *ḫa-at-ti-li* [χat:ili] 'in Hattic', but not in words like *ḫa-at-ta(-ri)* 'he cuts', *da-a-at-ti* 'you take' or *ki-it-ta* 'he lies', where we find geminate spelling as well, but which Simon interprets as containing a short [t]: [χata(ri)], [tāti] and [kita], respectively. Since in all six forms the dental stops are written in the exact same way, it stands to reason to assume that they are phonetically identical as well, i.e. that they all contain a long stop [t:]: [χat:usa], [χat:usomas],

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[28]) E.g. ÉRIN *ḫa-at-ti-i* 'Hittite troops' (VS 22, 85: 11 [OBab., letter]); KUR *ḫa-at-ti* 'the Hittite land' (EA 17: 31, 38 [MBab., letter of Tušratta of Mittanni to the Egyptian pharaoh]; EA 151: 58 [MBab., letter of Abimilku of Tyros to the Egyptian pharaoh]); KUR *ḫa-at-te-e* 'id.' (RIMA 2.0.087.001: 195, 543 [MAss., royal inscription]). Cf. also the attestations gathered in the lemma *ḫattû* 'Hittite' in CAD Ḥ: 151.

[29]) Note that in the Hittite expression KUR ^{URU}*ḪATTI* 'the land of Ḫattuša' the element *ḪATTI* is an akkadogram that represents the Hittite word ^{URU}*ḫattuša-* (Kryszeń 2017). Hittite thus never contained a word '*ḫatti*'.

[30]) But also in Neo-Babylonian and Neo-Assyrian, where the word *ḫatti* (referring to the Neo-Hittite states) is, as far as I am aware, consistently spelled *ḫa-at-tV°* as well as *ḫat-tV°*, with geminate *-tt-*.

[χat:ili], [χat:a(ri)], [t'āt:i][31]) and [kit:a]. Another argument against Simon's interpretation is that he is in fact assuming an extra set of phonemes: next to a series of short voiced stops ([d], spelled *VtV*) and a series of short voiceless stops ([t], spelled *VttV*), he now assumes an additional third series, namely one consisting of long voiceless stops, [t:], which are spelled *VttV*, as well. In this way, Simon is multiplying entities, violating Occam's Razor.

2.4 Conclusions regarding Simon 2020

All in all, we must conclude that Simon (2020) is unable to build a convincing case in favor of the postulation of voice as the phonetic and phonological contrast between Hittite fortis and lenis stops in intervocalic position. His negative assessments of arguments in favor of a length contrast can almost all be refuted, and his treatment of evidence based on the way Hittite lexemes are spelled in the writing systems of other languages is mostly irrelevant. In fact, all data discussed in the sections above rather indicate that the contrast between Hittite intervocalic fortis and lenis stops was length.

3. Patri's treatment of Hittite loanwords in other languages

Simon (2020) is not the first to adduce evidence from the way Hittite lexemes are written in other languages as an argument in the interpretation of the Hittite stop system: this was already done by Patri (2009; 2019: 170-217). Although Patri arrives at a similar conclusion for the Hittite lenis stops as Simon, i.e. that these were phonetically voiced, his conclusions when it comes to the fortis stops is quite different from Simon's ones.

3.1 An evaluation of Patri 2009

In his 2009 paper, Patri discusses Hittite names and loanwords attested in Egyptian, Ugaritic and Old Assyrian. Especially the Old Assyrian material is important for Patri's interpretation of the Hittite fortis stops, and he bases himself primarily on Dercksen's 2007 article that treats all Hittite / Anatolian loanwords that are attested in Old Assyrian texts (e.g. OAss. *kullupinnum* < Hitt. *kullupi-* 'pruning-knife'; OAss. *zuppannum* < Hitt. *zuppa-* 'a metal container'; OAss. *upatinnum* < Hitt. *ubadi-* 'royal land grant'). As Patri duly notes, in Dercksen's overview, "the only example of a voiceless [= fortis] stop (Hitt. *zuppa-* = *zuppannum*) is rendered by geminate spelling" (2009: 103),[32]) which, according to Patri, "evidently suggests an increase in the duration of the voiceless [= fortis] stops compared to the voiced [= lenis] stops" (2009: 105).[33])

Unfortunately, Patri seems to be fully unaware of the fact that the Old Assyrian version of the cuneiform script hardly notes down any contrast in voice and consonantal length.[34]) This also applies to the Hittite loanwords cited by Dercksen. For instance, the OAss. word that is a loan from Hitt. *zuppa-* 'a metal container' is in Old Assyrian always spelled *zu-ba-n°* (Dercksen 2007: 33), with the sign BA whose basic value is *ba*, with voiced *b*, but which in Old Assyrian is also used in the value *pá*, with voiceless *p*. Moreover, since Old Assyrian hardly ever explicitly spells geminates, it is indeed a possibility that this word contained a geminate labial stop, but this cannot be independently proven. In other words, the Old Assyrian spelling *zu-ba-n°* could in principle be read as *zu**b**an°*, *zu**bb**an°*, *zu**p**an°* as well as *zu**pp**an°*. The reason for Dercksen to cite this word as *zuppannum*, with *-pp-* (2007: 33), is only based on the fact that its Hittite base word is in the Hittite ductus spelled *zuppa-*, with geminate *-pp-*. The Old Assyrian spelling itself does not say anything on the exact quality (voice vs. voiceless; single vs. geminate) of the labial stop. In the same way, the word that is cited by Dercksen as "*upatinnum*" 'land grant' (2007: 35) is always spelled *ú-ba-t°*, and can thus in principle be read *u**b**aT°*, *u**bb**aT°*, *u**p**aT°* as well as *u**pp**aT°*. Likewise "*kullupinnum*" (Dercksen 2007: 34), which is spelled *ku-lu-pì-n°*, and thus can in principle be read *kullu**b**i-*, *kullu**bb**i-*, *kullu**p**i-* as well as *kullu**pp**i-* (note that the reading of the geminate *-ll-* is not certain either).[35]) All in all, Patri's 2009 discussion of the phonetics of the Hittite fortis stops on the basis of the Old Assyrian material is based on a misunderstanding of the relevant material.

3.2 An evaluation of Patri 2019

In his 2019 book on Hittite phonology, Patri uses the same type of arguments in his discussion of the phonetics of Hittite fortis stops, i.e. their rendering in other writing systems. This time, however, he does not use the Old Assyrian material,[36]) but refers to "accadien" in general, for which he cites three words (2019: 202):

1. *ḫattū*, *ḫattītu* (adj.) 'Hittite' (attested in Standard Babylonian; CAD Ḫ: 151) < Hitt. *ḫatt-*.
2. *tuppanuru*, *tuppalnuru*, *tuppalanuru* 'an official at the Hittite court' (attested in Ugarit Akkadian; CAD T: 475-6) < Hitt. *tuppa(la)nuri-*.
3. *kappu* 'bowl' (attested in all kinds of Akkadian dialects; CAD K: 188-9) < Hitt. *kappi-*.

It is generally assumed that in the latter case the Hittite word *kappi-* is rather derived from Akk. *kappu-* than the other way around (Puhvel HED K: 63), but the two other words are in fact good examples of Hittite lexemes that are rendered in a foreign writing system. On the basis of these examples, where we find in the Akkadian attestations a geminate

[31]) See Kloekhorst 2010: 205-7 for the postulation of an ejective stop [t'-] in the verbal root *dā-* 'to take'.

[32]) "Le seul exemple de non voisée (hitt. *zuppa-* = *zuppannum*) est […] rendu par une graphie double".

[33]) "… suggère, de toute évidence, un accroissement de la durée des non voisées par rapport aux voisées".

[34]) Kouwenberg 2017: 17-8, 27-9, 89-90; Kloekhorst 2019b: 16-21. See also Simon (2020: 243) for the fact that Old Assyrian in principle does not make any such contrasts.

[35]) The only word of the list cited by Patri (2009: 103) that has any merit is "*padallum*" 'a copper object' < Hitt. *patalli-* 'fetter(?), tether(?)' (thus Dercksen 2007: 37). This word, which is spelled *ba-da-l°* (Prague I 792: 2', 5'), uses the sign DA, which in principle denotes the voiced dental stop [d] (since it contrasts with the sign TA, that in principle denotes voiceless [t], cf. Kloekhorst 2019b: 19). Only in this case it is therefore possible to say anything about the phonetic quality of the intervocalic stop, namely that it was probably voiced (but not whether it was single or geminate).

[36]) Although he does state that "exceptionally, it happens that the Hittite voiceless [= fortis] stop is rendered in Akkadian by a single stop of the voiceless series, like in Hitt. *Ḫattušili* → Cappadocian [= Old Assyrian] *Ḫa-tù-šú-ili*$_5$" (2019: 203). Apparently, Patri was during the writing of his 2019 book still not aware of the spelling conventions of the Old Assyrian version of the cuneiform script.

spelling of the Hittite fortis stops, Patri concludes that "[i]t thus seems that, in the Akkadian perception of Hittite stops, the absence of voice is correlated with an increased duration" (2019: 203).[37]) This conclusion is opposite to that of Simon, who instead claimed that on the basis of the spelling of Hittite lexemes in other languages there was no evidence of a longer duration in Hittite fortis stops (2020: 245).

3.3 Patri's interpretation of duration: aspiration

Although Patri (2019: 203) concludes that Hittite intervocalic fortis stops were perceived by Akkadian scribes as having a longer duration than lenis stops, he does not assume that the fortis stops were phonemically long. According to Patri, the best indication for differentiating the duration of stops is *voice onset time* (VOT), i.e. the time that takes place between the release of the stop and the onset of the vibration of the vocal cords that constitutes the voicing that belongs with the vowel that follows the stop: the longer the VOT, the longer the duration of the stop as a whole. Patri explains that of the different types of voiceless stops that on typological grounds are candidates for being the counterpart to a lenis series that consists of voiced stops,[38]) voiceless aspirates, [t^{h}], etc., have the longest VOT. He therefore states that "[i]f we admit that the [Hittite] voiceless series written '*CC*' [= fortis stops] has a longer duration than that of the voiced series written '*C*' [= lenis stops], the characteristic that justifies this difference is more likely to be aspiration than anything else" (2019: 204).[39])

To this argumentation he adds two other arguments that, to his mind, would support the interpretation of fortis stops as aspirates:

(1) In some Hittite words, we find the presence of a lenis stops (spelled *VCV*) instead of expected fortis stops (spelled *VCCV*) in the vicinity of an [s], e.g. *iš-ta-a-pí* next to *iš-tap-pí* 'it clogs', or *ša-qa-aḫ-ḫi* next to *ša-aq-qa-aḫ-ḫi* 'I know'. According to Patri (2019: 198-201; 206-7), this change of *VCCV* to *VCV* can be interpreted as signaling a deaspiration caused by the [s].
(2) In verbs that show an alternation between a stem ending in *-CC-* and in *-C-*, e.g. *āki* / *akkanzi* 'to die', the lenis stop is found after an accented vowel. According to Patri (2019: 201-2; 206), the lenis stop is in such cases the outcome of a deaspiration of an original fortis stop due to the preceding accented vowel, e.g. *[á.k^{h}i] > *[á.ki], which was reinterpreted as [á.gi], spelled *a-ki*.

On the basis of these considerations, Patri assumes that Hittite fortis stops were voiceless aspirates, [t^{h}], etc., which contrast with the lenis stops, which were voiced, [d], etc.

This interpretation cannot be upheld, however. First, the two phenomena mentioned by Patri as additional proof for an aspirated quality of fortis stops are to be explained differently:

(1) The material adduced by Patri that would show the development of fortis stops into lenis ones in the vicinity of an [s], which he interprets as caused by "deaspiration" (2019: 198-9), actually falls into two groups. The first group consists of cases where the lenis stop is morphologically conditioned. For instance, the presence of a lenis *-p-* in *ištāpi* (OS) is determined by the fact that this verb belongs to the *āki*/*akkanzi*-group, where an alternation between lenis and fortis consonants is also found in verb stems that do not contain an [s].[40]) Moreover, in this case the comparison between OS *ištāpi* and NS *ištappi* rather shows a change from an original lenis stop into a fortis one,[41]) and not the other way around, as Patri has it. Likewise, the lenis dental stop in OH gen.sg. *šeppidaš* 'grain' is clearly the result of the PAnat. lenition rules, cf. Kloekhorst 2014: 562-3. Moreover, the fact that this form has in younger times been replaced by *šeppittaš*, with fortis *-tt-*, would again rather speak in favor of a change of an original lenis stop into a fortis one,[42]) and not the other way around, as Patri has it. The second group of examples consists of occasional simplified spellings. For instance, the form *ša-ka$_4$-aḫ-ḫi* 'I know', with single spelling of the velar stop, does indeed occur twice,[43]) but can hardly be taken seriously when compared to the fifteen attestations in which this form is spelled with a geminate *-kk-* or *-gg-*.[44]) The spelling *ša-ka$_4$-aḫ-ḫi* is therefore better interpreted as a simplified spelling that has no bearing on the phonetic interpretation of this form. In the case of *ḫartakaš* next to *ḫartaggaš* 'bear', it is generally assumed that in this word the velar stop is part of a cluster, /χərtkːa-/, and it is well known that in such clusters fortis consonants are often spelled as singletons.
All in all, none of Patri's alleged examples of "deaspiration" of an original fortis stop in the vicinity of [s] is compelling.
(2) The origin of the alternation between fortis and lenis consonants in the class of *āki*/*akkanzi*-verbs is debated (cf. Melchert 2012; Kloekhorst 2014: 549-53), but it is generally assumed that their alternation has its origin in pre-Hittite, and is not the result of a synchronic phonological development. Moreover, this class also contains verbs that have a stem-final fricative (e.g. *ḫāš-^{i}* / *ḫašš-* 'to give birth', *nāḫ-^{i}* / *naḫḫ-* 'to fear'), where Patri's deaspiration rule would not work: even within Patri's own framework fortis fricatives were not aspirated. Another argument against Patri's deaspiration theory is that we find many forms in Hittite where intervocalic fortis stops are preceded by an accented vowel but where no "deaspiration" takes place. For instance, the 3sg.pres. form *šākki* 'he knows', which in Patri's analysis is [sa̋.k^{h}i] and thus is

[37]) "Il apparaît donc que, dans la perception accadienne des plosives hittites, l'absence de voisement est corrélée à une durée accrue".

[38]) According to Patri, it is a typological given that in two-way stop systems that contain a voiced series /d/, the other stop is either /t/, /t^{h}/, /t^{s}/, /t̪/ or /^{n}t/ (2019: 186).

[39]) "Si l'on admet que la série non voisée écrite '*CC*' a une durée plus longue que celle de la série voisée écrite '*C*', le trait justifiant cette différence est plus vraisemblablement l'aspiration que n'importe quel autre."

[40]) The origins of this alternation between fortis and lenis consonants are debated (cf. e.g. Melchert 2012 and Kloekhorst 2014: 549-53 for a discussion), but surely does not have anything to do with the presence or absence of an [s].

[41]) Which can in fact be explained as the result of levelling of the fortis stop *-pp-* (original in e.g. 3pl.pres. *ištappanzi*) throughout the paradigm.

[42]) See Kloekhorst 2014: 563; 2016: 221-2 for the fact that in the paradigm of *šeppit(t)-* the fortis *-tt-* was at a certain point in time generalized throughout the entire paradigm.

[43]) KUB 40.1 obv. 13 (NH/NS), HFAC 6 iii 6 (LNS).

[44]) Cf. the attestations gathered in CHD Š: 21. Compare also the dozens of other forms of the verb *šākk-* that are spelled with geminate *-kk-* or *-gg-*.

structurally identical to *[á.k^{h}i], the input form of "deaspirated" *aki* [á.gi]. According to the deaspiration theory we would thus expect that [sá́.k^{h}i], through deaspiration, yielded [sá́.ki], which was then reinterpreted as [sá́.gi], spelled **_ša-a-ki_*. Yet, such a form is never attested. Likewise in the case of *ḫatta(ri)* 'he pricks, cuts' which shows a fortis *-tt-* after an accented vowel (as we saw in section 2.2.5). Within Patri's framework, this form, which he would interpret as [xá.t^{h}a(.ri)], should have undergone a deaspiration to [xá.ta(.ri)], which was reinterpreted as [xá.da(.ri)], spelled **_ḫatari_*. Again, such a form is unattested. A third example is *e-ep-pu-un* 'I seized', which, according to Patri's rule, should have undergone deaspiration of [é.p^{h}un] > [é.pun], which would have been reinterpreted as [é.bun], spelled **_e-pu-un_*. In this case, too, such a spelling is never found. The number of counter-examples against Patri's assumption of a "deaspiration" after an accented vowel are easily multiplied, and his theory therefore has little merit.

Another important argument against interpreting the Hittite fortis stops as aspirates is formed by the language universal that is formulated by Hyman (2008: 114, with reference to Hagège 1982: 936) as follows: "if [in a given language] there are aspirated stops, then there is /h/". Since Hittite knows no phoneme /h/,[45]) the postulation of a series of aspirated stops, /t^{h}/, etc., would violate this universal, and thus is unattractive.

We can thus conclude that nothing speaks in favor of Patri's interpretation of the Hittite intervocalic fortis stops as voiceless aspirates, and that there is in fact an important argument that specifically speaks against it. This proposal cannot therefore be maintained.

3.4 Patri's arguments against length

An interesting part of Patri's argumentation regarding Hittite intervocalic fortis stops that does remain valid is the fact that Akkadian scribes wrote them down as geminates (as did Hittite scribes themselves as well), which according to Patri indicates that in intervocalic position these stops had a longer duration than their lenis counterparts. Since nothing speaks specifically in favor of interpreting this longer duration as the result of aspiration and thus of a longer VOT, it seems much more straightforward to interpret the fortis stops' longer duration as the result of a longer closure time. Compare for instance the situation in a language like Kelantan Malay, where intervocalic long /t:/ has a closure duration that is 3.18 times longer than that of intervocalic short /t/.[46]) This would thus speak in favor of assuming that the contrast between the Hittite intervocalic fortis and lenis stops was length, as proposed by Melchert and myself.

According to Patri, however, our assumption of a length contrast is untenable, because, cross-linguistically, "[n]o language distinguishes two series of stops on the basis of length" (2019: 186[65], with reference to UPSID and PHOIBLE).[47]) This statement is incorrect, however. For instance, UPSID mentions the Waray language (spoken in Australia; UPSID nr. 8348), for which it lists the stops [p, t, c, k] next to [p:, t:, c:, k:]. This language has thus a two-way contrast in its stop system that consists solely of length. Another well-known example is Swiss German, which knows only two series of stops, which are distinct in length, as well: cf. Ehrenhofer e.a. (2017: 209), who cite for this language the plosive phonemes [p, t, k] as well as [p:, t:, k:]. Compare also Old Tamil, for which in intervocalic position a distinction was made between voiceless geminates ([p:], [t:], etc.) and voiced singletons ([b], [d], etc.) (Kuiper 1958: 209), and which thus shows a system that would be comparable to the Hittite system as argued for by Melchert and me.[48])

When it comes to the argument that fortis stops close the preceding syllable (see sections 2.2.1, 2.2.2 and 2.2.5, above), Patri (2019: 189[69]) acknowledges that plene spelling occurs less often before fortis stops than before lenis stops, but does not view this as an indication that fortis stops shortened preceding vowels, because:

(1) a closed syllable does not necessarily cause shortening of its nucleus;
(2) Hittite does show long vowels in closed syllables;
(3) a shortening of vowels can, *a priori*, take place in many contexts; and
(4) it is in general improbable to assume that one series would be distinct from another by the fact that it closes the syllable.[49])

All these points are a *non sequitur*, however:

(1) Although closed syllables may indeed not *necessarily* undergo shortening of their vocalic nucleus, it is in general a very common phenomenon. There is therefore nothing wrong with assuming such a development for Hittite.
(2) In Old Hittite we indeed still find long vowels in closed syllables, but these are all regularly shortened in later times (see Kloekhorst 2008: 98; 2014: 256-307 for the fact that the shortening of original long /ā/ in closed syllables is a development that first starts in the Old Hittite period).
(3) The shortening of vowels can cross-linguistically indeed be caused by several factors, but in Hittite, such a shortening is well attested before clusters. It thus makes sense to investigate the possibility that the shortening of vowels before fortis stops are caused by the same mechanism.

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[45]) The Hittite phonemes spelled *-ḫḫ-* and *-ḫ-* are nowadays commonly regarded as uvular fricatives, cf. Kümmel 2007: 331; Simon 2014; Weiss 2016; Kloekhorst 2018 (note that Patri 2019: 221-9 interprets them as velar fricatives, /x/ vs. /ɣ/).

[46]) According to Hamzah e.a. (2016: 147), the mean closure duration of intervocalic long /t:/ in Kelantan Malay is 197 ms, whereas in the case of short /t/ it is 62 ms. Note that intervocalic short voiced /d/ has in Kelantan Malay a mean closure duration of 49 ms, which means that the closure duration of long voiceless /t:/ is 4.02 times longer than that of short voiced /d/.

[47]) "Aucune langue ne distingue deux séries de plosives par la durée".

[48]) Note however, that I have argued that Hittite knows a series of ejective stops as well (Kloekhorst 2010: 202-7; 2013: 127-31; 2020), which, as I will argue elsewhere, probably knew a distinction between long and short variants as well (Kloekhorst fthc.). I therefore assume that the Hittite stop system in fact contained four types of stops: /t/, /t:/ and /t'/, /t':/.

[49]) "Mais un tel jugement néglige que […] le caractère fermé d'un syllabe ne cause pas nécessairement l'abrègement de son noyau, qu'il existe, en hittite, des témoignages de voyelles allongées sous accent en syllabe fermée, qu'un abrègement vocalique (ou: non allongement) peut *a priori* se produire dans bien des contextes (en autres, devant plosive dévoisée), et qu'il est, de façon générale, improbable de postuler qu'une série entière de plosives se distinguerait de l'autre série par le fait qu'elles ferment les syllabes."

(4) The shortening of vowels before long consonants is well attested in other languages,[50]) so it certainly is not "improbable" to assume that this happened in Hittite as well.

3.5 Conclusions regarding Patri 2009 and 2019

Though most of Patri's discussion of the phonetics and phonology of the Hittite intervocalic stops cannot be maintained, it does contain one important contribution, viz. the recognition that Akkadian scribes wrote the Hittite fortis stops as geminates (just as Hittite scribes did), and that this indicates that these stops were longer than the lenis ones. This fact cannot be interpreted as an indication that Hittite fortis stops were aspirated, as Patri has it, but, in spite of Patri's claims of the opposite, fits perfectly within the length contrast theory.

4. Overall conclusions

The interpretation of the phonetic value of the Hittite intervocalic fortis and lenis stop series as long voiceless ([t:], etc.) and short voiced ([d], etc.), respectively, and of their phonological contrast as one in length (/t:/ vs. /t/), as had been advocated by Melchert (1994: 14-21, 147) and myself (Kloekhorst 2008: 21-5; 2014: 544-7; 2016: 213-23), remains unsurpassed. All Simon's (2020) and Patri's (2009; 2019) objections against the arguments supporting this length contrast theory have turned out to be based on either the usage of incorrect data (§ 2.2.2, § 3.4), the misunderstanding of the original argument (§ 2.2.3), not having taken into account the newest insights (§ 2.1.2), or the postulation of scenarios that are clearly less suitable for explaining the relevant data than the scenarios proposed within the length contrast theory (§ 2.1.2, § 2.2.1, § 2.2.4, § 2.2.5, § 3.4). Moreover, Simon's and Patri's treatments of evidence based on the rendering of Hittite lexemes in the writing systems of other languages have turned out to be either irrelevant (in the case of Simon 2020), faulty (in the case of Patri 2009) or leading to interpretations that are otherwise untenable (in the case of the aspiration theory of Patri 2019).

The conclusion that Hittite really showed a length contrast between its fortis and lenis stops in intervocalic position is not only an important piece of information for our interpretation of the synchronic phonological system of Hittite, but also needs to be taken into account in our interpretation of the phonology of the entire Anatolian language family, as well as into our views on the relationship between Anatolian and the other Indo-European branches.[51])

[50]) Cf. Kubozono (2017: 2, with references), who states that "many languages such as Bengali, Berber, Hindi, and Italian shorten pregeminate vowels just as they shorten vowels in closed syllables as against open ones".

[51]) In Kloekhorst 2016, I argued that also Proto-Anatolian must have had a length distinction in its stop system. This idea has been rejected by Yates (2019), who rather assumes that the Hittite length contrast was a specifically Hittite innovation. Yet, in view of new insights into the phonology of the Luwian languages, which, according to Vertegaal (2019; 2020: 127-58), also knew a length distinction in their stop systems, it seems indeed more likely that the length contrast was a Proto-Anatolian phenomenon: I plan to expand on this topic elsewhere. Likewise in Kloekhorst 2016, I argued that even Proto-Indo-European knew a length contrast in its stop system (*[t:], *[ʔt], *[t]), and that the voice contrast known from the other Indo-European languages (traditionally noted down as *t, *d, *d^h, but phonetically probably *[t], *[ʔd], *[d]) was a later innovation. This idea has been rejected by Kümmel 2019, who claims that linguistic parallels rather indicate that the Anatolian length contrast should have derived from an earlier voice contrast, instead of the other way around. I plan to assess this point on another occasion as well.

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Leiden University
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