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, /tː/ vs. /dː/, 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)), who rather posits a distinction /tʰ/ 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, /tː/ vs. /dː/.

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 *dʰ, 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 ([tː], 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. /d/. 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 /tː/ and /dː/ 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).

In a recent paper, Simon (2020) offers a new discussion of the phonetics and phonology of the Hittite intervocalic stops, 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: /tʰ/, 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; V-kV vs. V-ktV; 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...
between the fortis and lenis series was not voice (/t/ vs. /d/), but instead rather points to a length contrast (/t/ vs. /tː/). Simon admits that this argument has some merit and agrees that if the difference between the two series was 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 Coelho (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 dactus 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 dactus 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,5 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 [ʔegʷta], with a cluster [-gʷt-] consisting of a voiced [gʷ] + 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 [ʔegʷda], spelled **e-ku-ta, or [ʔekʷta], 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.

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.6 This is best seen in cases in which the first stop is labiovelar: these are always spelled **V-ku-ut-TYº (like e-ku-ut-ta ‘he drank’ and ša-ku-ut-ta-iº ‘thigh’), but we never find the spelling **V³ku-ut-TYº. But Yates is able to demonstrate this for other cases as well, like ya-at-ku ‘to leap’ (never spelled **ta-at-ta-uk-) or har-la-at-kVº ‘bear’ (never spelled **har-at-ta-ak-kVº). To these may be added forms like e-ep-ta ‘he seized’, which is never spelled **e-ep-p³-ta or **e-ep-pa-ta-¹. 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 < *hwp³t-.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 voiceless (aspirated) stops were devvoiced (PIE *d³ > Hitt. [t]), was not, as usually thought, fully unconditional, but that its initial

---

5) As is the case in Old Babylonian texts from Alalah VII (Kloekhorst 2010: 231-8; Popova 2016), the dactus of which best resembles the Hittite one. Note that Simon (2020: 237) implies that I have claimed that Alalah 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 dactus best resembles the dactus as found in Old Babylonian texts from Alalah (Tell Āṣarana), level VII (18-17th century BC)” (Kloekhorst 2010: 231; emphasis added; likewise, Kloekhorst 2013: 125), which is completely in line with the information provided by Rüster & Neu 1989: 15 (cf. also Van den Hout 2012).

6) Cf. the 1sg.pret. form e-ep-p³-ku-un ‘I seized’ < *h³wp³-n, where the fortis character of *pp- < PIE *p is expressed in spelling.

7) Cf. Kloekhorst 2020: 165 (which went to press before Yates 2019 was available), where I similarly stated that ‘we may assume that before stops [t] distinction [between /t/ and /d/] 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.
part, i.e. the lengthening of PIE voiceless stops, was blocked in the position before another stop.\(^5\) This means that in a preform like *h₁epta, 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. [ʔepta], spelled e-ep-ta, with a cluster [-pt-].\(^6\)

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 [ʔegwta] (containing a cluster of voiced [g] + voiceless [t]) to the form e-ep-ta ‘he seized’, we would have to assume that the latter represents [ʔepta], 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 *h₁epta, 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 [kʷ].\(^7\) 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 [kʷ] can be distinguished from their fortis counterparts, which were long, [pː] and [k wː], in this way, lenis [p] and [k w] can be distinguished from their fortis counterparts, which were long, [pː] and [k wː].

### 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 *f/i/ is shortened when occurring in a closed syllable (e.g. 1sg. kīša ‘I become’ < *kīša < *Kēis-h-e), but not in an open syllable (e.g. 3sg. kīša ‘he becomes’ < *Kēis-ō). In the word kīta ‘he lies’ < *kīti < *kēi-to, shortening of *f/i/ is found before fortis -it- < 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: /tː/.

In his discussion of this argument, Simon does not deny that the shortening of pre-Hitt. *f/i/ in closed syllables is real, nor that the vowel /i/ in kīta is short and should go back to earlier f/i. However, according to Simon, the short character of the /f/i in kīta 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 *f/i/ 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 ki₂-proto (to lie), in which stage 2 represents the stage in which earlier *f/i/ was shortened when standing before a cluster (’→’ indicates a phonological development; ‘→’ indicates an analogical development):

<table>
<thead>
<tr>
<th>PIE</th>
<th>stage 1</th>
<th>stage 2</th>
<th>Old Hittite</th>
</tr>
</thead>
<tbody>
<tr>
<td>sg. 1 *kēi-hē</td>
<td>&gt;*kīHa</td>
<td>&gt;*kīHa</td>
<td>*kī-ih-ta</td>
</tr>
<tr>
<td>2 *kēi-thō</td>
<td>&gt;*kīHa</td>
<td>&gt;*kīHa</td>
<td>*kī-it-ta</td>
</tr>
<tr>
<td>3 *kēi-to</td>
<td>&gt;*kīta</td>
<td>&gt;*kīta</td>
<td>*kī-it-ta</td>
</tr>
<tr>
<td>pl. 1 *kēi-wood</td>
<td>&gt;*kīwa</td>
<td>&gt;*kīwa</td>
<td>*kī-va-š-ta</td>
</tr>
<tr>
<td>2 *kēi-dhwe</td>
<td>&gt;*kītuwo</td>
<td>&gt;*kītuwo</td>
<td>*kī-it-um-ša</td>
</tr>
<tr>
<td>3 *kēi-nto</td>
<td>&gt;*kīnta</td>
<td>&gt;*kīnta</td>
<td>*kī-(ya-jan-da)</td>
</tr>
</tbody>
</table>

Simon states that, within this scenario, ‘*f/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 *ki/-ki-’ (2020: 239). As a parallel to this development, he cites the verb kīš-kiš- ‘to happen, to become’, ‘where the allomorph kīš-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 Hittite development of, for instance, the OH 3sg.pres. form kī-i-śa /k̞is/ to NH ki-ša is the result of levelling. In fact, I have in Kloekhorst 2014: 471-2 argued that the shortening of the f/i of OH kīša to the f/i of NH kīša 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 ki₂-proto (to lie). If anything, it would rather support the view that the short f/i of 3sg.pres. kīta ‘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 *f/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 *f/i/. The spread of the short *f/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 dā₂/ /d- ‘to take’, where the same development would have taken place (ibid.). Yet, as we will see below, this is incorrect: the paradigm of dā₂/ /d- does not show a similar levelling, and therefore cannot be used as a parallel. Moreover, in the case of ki₂-proto ‘to

---

\(^5\) Yates assumes that, when standing before voiceless stops, PIE voiced (aspirated) stops first were devoiced by assimilation, e.g. *h₁ept-o > pre-Hitt. *[ʔek-ta], 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-qa], spelled e-ku-ut-ta (note that Yates does not assume an initial [ʔ] in Hittite, but this is irrelevant for the present argument).

\(^6\) 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 synchronous interpretation of Hittite phonology, however, for which I completely agree with Yates.

\(^7\) If we would allow the lenis labiovelar in e-ku-ut-ta to be voiced, [gʷ*], 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.

---

Content extracted from the image appears to be a discussion on the phonetics and phonology of Hittite intervocalic fortis and lenis stops, focusing on the distinction between fortis and lenis stops and how they are interpreted in the Hittite language. The text analyzes the length contrast between fortis and lenis stops and discusses Simon’s scenario, comparing it with the development of other Hittite verbs. The text also examines the possibility of a phonetic shortening affecting the short /i/ of 3sg.pres. kīta ‘he lies’.
lie', we have to take into account that synchronically in Hit-
tite 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 ear-
er times (cf. the attestation of 1sg. sigani ‘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 < *kēi-th₂o 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 /ā/ in kita ‘he lies’
is best explained as the result of a shortening of original *
/ā/ before fortis -t-, 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 syl-
lables.1 As argued in Kloekhorst 2016: 215-6, this shorten-
ing is also found when /ā/ is followed by an intervocalic
fortis stop, for which I gave the examples OH dātti > NH
datti ‘you take’, and OH dāttten > NH datten ‘you must take’, and
OH sākki > NH šākki ‘he knows’. This means that in these
words these fortis stops behave as a cluster, and must thus
have been long: -t/ = ft/ and -kk = fk/.

Although Simon acknowledges that the shortening of OH
/ā/ to NH /a/ in non-final closed syllables is a regular develop-
ment, he states that the examples involving forms from the
verbs dā-i / d- ‘to take’ and sākk- ‘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’

In the case of sākk- ‘to know’, Simon points out that this
verb originally belonged to the class of dā-a-ablauting verbs
(with reference to Kloekhorst 2008: 695), implying that we
can assume that in the original 3sg.pres. form sākki, which
showed the strong stem sākk- (< *sōkH-), the weak stem
sākk- was introduced through levelling. However, as shown
in Kloekhorst 2012: 155-6, the weak stem sākk- (ultimately
from *sēkH-) is only rarely attested: it is only found in the OS
2pl.pres. form sakteñi, the OH/NS 3pl.pres. form šakanzi,
and a few MS attestations of the participle šakkannt-. Already
in MH times the verbal paradigm of ‘to know’ had under-
gone a levelling by which the weak stem sākk- had been fully
replaced by the secondary stem šekk- (giving rise to newly
created forms like 2pl.pres. šekkeni, 3pl.pres. šekkannt-, part.
šekkant-). Yet, the development of sākk- 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 origi-
nal strong stem šakk- by the weak stem šakk-. Instead, the
development of OH šakk- to NH šakki can only have been
casted by a phonetic development, i.e. the shortening of ear-
lier /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 sātt ‘you take’ and satt-
ten ‘you must take’, Simon proposes to explain these forms,
too, as the result of a levelling of short /ā/ throughout the
paradigm of dā- / d- ‘to take’, which, as he claims, is illus-
trated 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,
prect. davēn, 1sg.imp.act. talit, and 3sg.imp.act. dau. Upon
closer scrutiny, none of these forms is probative, how-
ever. 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- / ti- ‘to put’
as a 3sg.pres.act. form of ‘to take’ (cf. Kloekhorst 2014:
192(152)). 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-āš is only attested in KBo
18.151, a text that is notorious for its aberrant spellings.13) In
all other texts, Old, Middle as well as New Hittite ones (doz-
ens of times), we only find the spelling da-a-āš (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 throughout time. The 1pl.pres.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)-(e).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 throughout time. The 1sg.
imp.act. form ta-ā-ī (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-ū’ that is cited in my dictionary
(Kloekhorst 2008: 803) does not exist: it was based on the
citation of a form “da-ū” by Garcia Trabazo 2002: 514 for
KUB 4.1 i 37, where we actually find da-ū-ā. In fact, this
form is in texts of all Hittite periods, including in New Hittite
ones, only attested with the spelling da-a-ū (dozens of exam-
plars, cf. Kloekhorst 2014: 396(548)). 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 paradigm15) of the verb
dā- / d- ‘to take’ to its New Hittite version, we get the fol-
lowing picture:

12) Cf. e.g. Soysal 2000: 113-4; Kloekhorst 2010: 208(10); 2014: 240(867);
vand den Hout 2012: 166.
NS)), da-a-ū-e (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-ā-i d- is
virtually always spelled with the sign DA.
15) All these forms are attested in OS texts, except 2sg.pres. dāta (MS),
and 3pl.imp. dāndu (MS). There can be no doubt, however, that in Old
Hittite these forms had these shapes, as well. The 2pl.pres. form dāten
is assumed on the basis of the OH 2pl.imp. form.
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 has been retained as such. The rise of /ā/ 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 -hḫ- and -tt- were long consonants: [χ] and [tʰ], respectively.\(^{17}\)

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 ʃ 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 ʃ,\(^{15}\) is generally interpreted as indicating a contrast in length: \(V_r\cdot rV = [rʃ]\) vs. \(V_s\cdot sV = [sʃ]\), etc.; \(V_s\cdot sV = [sʃ]\) vs. \(V_s\cdot sV = [sʃ]\). 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 ʃ), i.e. geminate spelling \(VC\cdot CV\) vs. singleton spelling \(VC\cdot CV\), is of the exact same structure as the spelling difference between fortis stops (\(VT\cdot TV\)) and lenis stops (\(V\cdot TV\)). And since in resonants (and in ʰ and in ʃ) this spelling difference marks a contrast in length (e.g. \(fr\) vs. \(f\); \(ʃʃ\) vs. \(ʃ\)), 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 assimilation of PIE *\(dʰi\) > Hitt. \(z\cdot z\) = [ts]-\(^{19}\) and of PIE *\(d\) > Hitt. \(s\cdot s\). 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 assimilation processes thus were clusters with the phonetic shapes *\(t\cdot t\) and *\(d\cdot d\), 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 assimilation processes were pre-Hittite clusters of the shape *\(t\cdot t\) and *\(t\cdot t\), respectively, and that they developed in a symmetrical way, namely *\([ts]\) = *\([tt]\) > *\([t\cdot t]\) > Hitt. \([ts]\) (spelled \(z\cdot z\) and *\([tt]\) > *\([t\cdot t]\) > Hitt. \([s]\) (spelled \(s\))), respectively. This would then imply that, in word-initial position, PIE *\(dʰ\) had yielded a pre-Hittite long *\([t\cdot t]\), which contrasts with PIE *\(d\), that yielded a pre-Hittite short *\([t]\). These word-initial values of the outcomes of PIE *\(dʰ\) and *\(d\) would then support the interpretation of their intervocalic outcomes in Hittite as \([t\cdot t]\) vs. \([d\cdot d]\), 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 [s] were in fact dental affricates, [θ̥], [dz], and [θ̥], 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 [s], but in some variants render [s] as well, only denoted affricates, [dz], [θ̥] and [θ̥], 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 assimilation of dental stops, “the direction of phonological changes is neither obligatory nor necessarily parallel. Thus nothing excludes that the reflexes of *\(dʰ\) and *\(d\) in the same language will be different in terms of voice”, for which he cites examples from Italian: giorno [dʒorno] ‘day’ < *\(dʰi\)- vs. zio [tsio] ‘uncle’ < ti- (2020: 241). However, it is often assumed that Hitt. z writes an affricate [θ̥], 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.

\(^{15}\) The one attestation ta-ši- is been left out of consideration.

\(^{16}\) 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 \(Vb\cdot V = [g]\) vs. lenis \(V\cdot V = [g]\) (albeit that lenis /g/ was allophonically voiced to [ŋ] in intervocalic position), cf. Kloekhorst 2018: 82.

\(^{17}\) 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.

\(^{18}\) It is often assumed that Hitt. z writes an affricate [θ̥], 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\(^{36}\); see also Kloekhorst 2019a: 55-6.

\(^{21}\) Whereas the phoneme /θ̥/ was in fact the dental sibilant [θ].
the point is that in Hittite the outcomes of *ṭj-i- and *ṭd-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 assimilation, Hall & Hamann (2006) cite dozens of examples of assimilation of dental stops from a wide variety of languages, and in all cases assimilation of voiceless *ṭ yields a voiceless outcome ([s], [ts], [tʃ], etc.), whereas assimilation of voiced *ṭ yields a voiced outcome ([z], [dz], [dʒ], etc.). The fact that PIE *ṭd-i- yields Hittite ֶ (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 *ṭ-[i]- and *ṭd-[i]- 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, assimilation of *ṭ[i]- yields the affricate [ts], whereas *ṭd[i]- develops into the sibilant [s] (Hall & Hamann 2006: 1204). Similarly but oppositely, in Greek, original *ṭd[i]- assimilates to the sibilant σ [s]-, whereas *ṭ-i- develops into the affricate Ҫ [ts]-. However, since in the latter language the development of *ṭi- to [s]- probably went through the affricate *ṭs-[i]- (Rix 1992: 92), we may assume a similar development for Romanian: *ṭ[d-i]- should then first have yielded an affricate *ṭ[dz]- 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 *ṭd-i- yielded in Hittite the affricate [ts]-22 whereas *ṭd-[i]- yielded the sibilant [s]- cannot be used as an argument against a pre-Hittite phonetic interpretation of these clusters as *ṭ[i]- and *ṭd-[i]-, respectively.

All in all, Simon is partly right: the fact that PIE *ṭ-i-[yields the outcome [ts]-] (spelled Ҫ) whereas PIE *ṭd-i-[yields the sibilant [s]-] (spelled σ) cannot be used as a direct argument in favor of a pre-Hittite stage with long [t] vs. short [ṭ]. Yet, the fact that the outcome of PIE *ṭd-i- in Hittite is a voiceless sibilant [s]- is remarkable, and would speak in favor of a prestige in which the dental stop was voiceless as well: *ṭ-[i]. However, postulating a value *ṭ-[t]- as the outcome of PIE *ṭ-d- should be compatible 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 assimilation the relevant clusters had the shapes *ṭ[t]- and *ṭ-[ṭ], 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 *ṭʌ is in Hittite lengthened in open syllables, but not in closed ones. Since the Hittite words ḫuappapa- `evil' < PAnat. *Ḥeappo- and ḫattar(i) `he pricks, cuts'. Simon states (2020: 241) that its short /a/ has been taken over from the corresponding bi-verb ḫuappap- / ḫupp- `to be hostile, to do evil' 23) the strong stems 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. ḫuappap-), 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 ḫuappapa-. 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- = /pl/, is much more straightforward.

When it comes to ḫattar(i) `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 ḫattar,ā(i) can be compared to Lyc. ḥutta(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), ḫattar-ā(i), that combines with a -iē/i-affixed active stem hazzī/ia-ā-. 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 ḫattar-ā(i) belongs to the category that reflects the PIE structure *CēC-o, which implies that its root can be transposed into PIE phonemes as *hëyet-.24) Taken together, we can with certainty assume that its 3sg.midd. form ḫattar(i) goes back to a preform *hēr-ēr-ū(i) (as implied by Melchert’s PAnat. reconstruction *Hāto-). Melchert is thus fully right to state that, if PIE *ṭr had yielded a Hittite short consonant, the vowel of the stem, through PAnat. *ṭʌ, should in Hittite have undergone lengthening.25) The absence of lengthening (cf. the consistent non-plene spelling ha-at-ā(-), including in Old Hittite originals), thus indicates that this vowel stood in a closed syllable, implying that PIE *ṭr was closed the syllable, and thus was a long consonant: *ṭt-. 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 ḫuappap- / 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 ḫattar-ā(i) are 3sg. ḫatta2 and 3pl. ḫattanta2, and in both cases the root-final consonant -ṭr- is followed by a vowel.26) We cannot therefore assume levelling of a shortened

---

22) Or rather: cluster of [t] + [s], cf. footnote 19.
23) See Kloekhorst 2008: 369-71 for the fact that this verb originally was bi-conjugating.
24) With the consonants *h₂ and *ṛ that are fully in line with the consonants of Lyc. ḥutta(i)-.
25) Cf. e.g. dat.-loc.sg. paddānī `basket' < *p(e)ṭh₂-rēn-em-i (Kloekhorst 2014: 348-9).
26) The only other attested medio-passive form is 1sg.pres. ha-ad-da-ab-ha-ri- (KUB 17.28 6 (M/HNS), 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 hatC°, 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 huayappa- and hatta(ri) are to no avail: Melchert is clearly right in saying that the presence of a short /ə/ in these forms is the result of a phonetic shortening, which implies that the consonants following the /ə/ closed the preceding syllable, and thus phonetically must have been long, [pː] and [tː], 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 Kloeckhorst, 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 Kloeckhorst 2013), and one that assumes a length contrast of the type “-TT- : -T-” (for which he cites Kloeckhorst 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 Kloeckhorst 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 ([tː], 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 [tː] 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 [tː], 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 pdgreSQL, with voiced d = [d], g = [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 Kloeckhorst 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 Kummuh, and which he compares to the CLuw. lexeme muqattalalli- (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 has different distinctions (e.g. an opposition between stops and fricatives) 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

---

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. puwätti′ ′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- (puwatu and puwati), 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. puwätti is not fully clear. It occurs only once (nom.sg. pu-ya-at-ti-江山 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, šinđu ′Kenneichen, 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 mg. of the Akk. entry, it is risky to assume that either ′Akk. šinđu or ′Hitt. puwätti′ means ′mark′ or ′color′′′ (P: 369). Moreover, as CHD notes, we do not even know ′if puwätti 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 fuwatu ′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. fuwatu, it seems a priori more likely that these words have a Semitic origin, which would imply that, if Hitt./Luw. puwätti- 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 puwatti-to the Hittite verb puwae-i ′to pound, to grind′ (2020: 244), 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. puwätti- 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 hatt- ′Hittite′. In Old/Middle Babylonian and in Middle Assyrian texts, all derivatives of this stem are always spelled ha-at-tu-I(-), 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 Ḫattuṣa ′Ḫattuša′, Ḫattuṣumāš ′person from Ḫattuša (nom.sg.)′ and Ḫattili ′in Hattic′,29) the geminate spelled -tt- represents a long voiceless stop: [ʔatuṣa], [ʔatustomas] and [ʔatiili].

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 hatt- ′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 Ḫattuṣa′ (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 hatt- 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) the stem hatt- is consistently spelled with geminate spelling, and that this implies that its dental stop is long: [ʔat-]. However, since the stem hatt- 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 ha-at-tu-ša-[ʔatustasa] ′Ḫattuša′, URU ha-at-tu-ša-ma-ša-[ʔatustomas] ′person from Ḫattuša′ and ha-at-ti-li [ʔatiili] ′in Hattic′, but in words like ha-at-ta-[ɾi] ′he cuts′, da-a-at-ti-ı′ you take′ or ki-li ′the lies′, where we find geminate spelling as well, but which Simon interprets as containing a short [t]: [ʔa-ti], [t] 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]: [ʔatustasa], [ʔatustomas], [ʔatiili]...
For instance, the OAss. word that is a loan from Hitt. *zuppa* ‘a metal container’ is in Old Assyrian always spelled *zu-ba-nū* (Dercksen 2007: 33), with the stem BA whose basic value is *bu*, with voiced *b*, but which in Old Assyrian is also used in the value *pā*, 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-nū* could in principle be read as *zubānū*, *zubabū* or *zupanū* as well as *zuppanū*. The reason for Dercksen to cite this word as *zuppanum*, with -pp- (2007: 33), is only based on the fact that its Hittite base word is in the Hittite dactylus 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 ḫḫ-tā-tāp-, and can thus in principle be read *ubatāp*-, *ubbatāp*-, *upatāp* as well as *uppatāp*-. Likewise “*kullipinnum*” (Dercksen 2007: 34), which is spelled *ku-lu-pi-nū*, and thus can in principle be read *kulabī-, kulbabī-, kulupī- as well as *kulippī- (note that the spelling of the geminate -ll- is not certain either).33) 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.

3.3 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. *hattā, battīru* (adj.) ‘Hittite’ (attested in Standard Babylonian; CAD H: 151) < Hitt. *battā-

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

33) The only word of the list cited by Patri (2009: 103) that has any merit is *tuppalnuru* ‘a copper object’ < Hitt. *palali- ‘tether(?)’, 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 is it 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).

34) 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. *Hattālūlī* → Cappadocian [≠ Old Assyrian] *Hu-ia-sīlli*,” (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.

32) “Le seul exemple de non voisée (hitt. *zuppānū* ‘pruning-knife’; < Hitt. *zuppa*- ‘royal land grant’). As Patri duly notes, with Dercksen’s overview, “the only example of a voiceless stop [≠ fortis] is rendered in Hittite 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).32)

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

30) “Le seul exemple de non voisée (hitt. *zuppānū* = *zuppanum*) est [...] rendu (une) espèce de ‘témoin’.

31) “... suggère, de toute évidence, un accroissement de la durée des non voisées par rapport aux voisées”.

32) See also Simon (2020: 243) for the fact that Old Assyrian in principle does not make any such contrasts.
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 voiceless stops,38 voiceless aspirates, [tʰ], 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-t-a-a-pí next to iš-tap-ši ‘it clogs’, or sa-qa-ah-hí next to sa-ag-qa-ah-hí ‘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. akki / 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. *[a.šk[i]*] > *[a.ški]*, which was reinterpreted as *[a.gš]*, spelled a-kši.

On the basis of these considerations, Patri assumes that Hittite fortis stops were voiceless aspirates, [tʰ], 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:

---

37) “Il apparaît donc que, dans la perception académienne 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 [b], [n], [-devellopt], [̥] or [ɾ] (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.”

(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 ʾštāpī (OS) is determined by the fact that this verb belongs to the ʾāki/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 ēštāpī and NS šētāppī 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. šēppidasā ‘grain’ is clearly the result of the PAnat. lenition rules, cf. Kloeckhorst 2014: 562-3. Moreover, the fact that this form has in younger times been replaced by šēppiṭās, with fortis -tr-, 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-ah-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-ah-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 /g/ /ka/ and it is well known that in such clusters fortis consonants are often spelled as singeltons. 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 ʾāki/akkanzi-verbs is debated (cf. Melchert 2012; Kloeckhorst 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- / ḫašš- ‘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 šākkī ‘he knows’, which in Patri’s analysis is [sā.kk[i]] and thus is

---

40) The origins of this alternation between fortis and lenis consonants are debated (cf. e.g. Melchert 2012 and Kloeckhorst 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. ʾištappanzi) throughout the paradigm.

42) See Kloeckhorst 2014: 563; 2016: 221-2 for the fact that in the paradigm of ʾšēppiṭi- the fortis -tr- 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 *[ âmːi]*, the input form of “deaspirated” *aki* (ági). According to the deaspiration theory we would thus expect that *[ âmːi]*, through deaspira-
tion, yielded *[āːki]*, which was then reinterpreted as *[āːgi]*, spelled **̓a-a-ki**. Yet, such a form is never attested. Likewise in the case of *hatta* (ri) ‘he pricks, cuts’ which shows a form -it- after an accented vowel (as we saw in section 2.2.5). Within Patri’s framework, this form, which he would interpret as *[āːtːa* (ri)], should have undergone a deaspiration to *[āːta* (ri)], which was reinterpreted as *[āːda* (ri)], spelled **̓a-ha* (ri). Again, such a form is unattested. A third example is *e-e-pu-un* ‘I seized’, which, according to Patri’s rule, should have undergone deaspiration of [é*pu*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 Hit-
tite fortis stops as aspirates is formed by the language uni-
versal that is formulated by Hyman (2008: 114, with refer-
cence to Hagegè 1982: 936) as follows: “if [in a given
language] there are aspirated stops, then there is /h/”. Since
Hittite knows no phoneme /h/, the postulation of a series
of aspirated stops, /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 Hit-
tite intervocalic fortis stops that does remain valid is the fact that
Akadian and Hurrian texts (as geminates were known to
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 /tː/ has a closure duration that is 3.18
times longer than that of intervocalic short /t/. This would
thus speak in favor of assuming that the contrast between the
Hittite intervocalic fortis and lenis stops was length, as pro-
posed by Melchert and myself.

According to Patri, however, our assumption of a length
costant is untenable, because, cross-linguistically, “[n]o
language distinguishes two series of stops on the basis of
length” (2019: 186⁴⁴⁵, with reference to UPSID and
PHOIBLE)⁴⁴⁶. 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 distinc-
tion was made between voiceless geminates ([pp], [tt], 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.⁴⁴⁷ 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: 189⁴⁴⁸) 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 pre-
ceeding 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.⁴⁴⁹

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 gen-
   eral 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 syl-
   lables 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 short-
   ening 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.

⁴⁴⁵ “Aucune langue ne distingue deux séries de plosives par la durée”.
⁴⁴⁶ Note however, that I have argued that Hittite knows a series of ejec-
tive 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 15-6.). I therefore assume that the Hittite
stop system in fact contained four types of stops: /h/, /t/ and /t'/, /k'/.
⁴⁴⁷ “Mais un tel jugement néglige que […] le caractère fermé d’un syl-
lable ne cause pas nécessairement l’abrègement de son noyau, qu’il existe,
en hittite, des témoignages de voyelles allongées ([pp], [tt], etc.)
et voix singly etons ([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.”
(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 the fortis contrast as one in length ([t] vs. [l]), 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], *[l], *[d], *[b], and that the voice contrast known from the other Indo-European languages (traditionally noted down as *h₂, *d, *p, but phonetically probably *[t], *[d], *[b]) 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.


Melchert, H.C., 2012, Hittite hi-verbs of the type -aC1i, -aC1C1anzi, Indogermanische Forschungen 117, 173-185.


Leiden University
May 2021