On the Origins of the Hieroglyphic Luwian Writing System: The Chronological Problem

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ABSTRACT:

This paper critically examines the key problem in the origins of the Hieroglyphic Luwian writing system, namely that of chronology. It argues that the sign CAPERE and the inscriptions of the stag-shaped silver vessel and from Kayalıpınar require dating its emergence to the Old Hittite period at the latest. Nevertheless, the signs INFRA and PES still imply a Hittite contribution, which can only be assumed in a Hittite-Luwian bilingual context, which, in turn should be dated already to the Old Hittite period because of the Luwian grammatical influence documented from Middle Hittite onwards. Finally, the (lack of) evidence from the Old Assyrian Colony period will be discussed and the question why the emergence of the Hieroglyphic Luwian writing system cannot currently be dated to this period.

1. INTRODUCTION

The origins of the Hieroglyphic Luwian writing system[1] are still hotly debated, including the question of when this writing system emerged. The present contribution does not intend to solve this problem but rather to provide a sort of state of the

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1 The term "Anatolian Hieroglyphs," which has become more and more popular in recent years, will be avoided here since it lacks any foundation. As is well known, this writing system was developed for the Luwian language (cf. most recently Rieken 2015, but see already Güterbock 1956: 518, *contra* Marazzi 2018: 46) and there is still no evidence that it would also have been adopted for other languages (transcriptions of non-Luwian names [including divine epithets in Yazılıkaya], of course, do not prove such, *contra* e.g. Morpurgo Davies — Hawkins 1978: 756 [but see Hawkins 1986: 371] and Payne 2008: 120). But even if the Luwian hieroglyphs were adopted for other languages, it is precisely their origin that requires us to call them Luwian: cf., e.g., the Latin script, which is still called "Latin" despite its adoption to write many other languages and not, for instance, "Hungarian script." Note furthermore that the spelling "Luvian" currently used by some American linguists contradicts all known Luwian and English phonological and orthographical facts (cf. already Hawkins 2013a: 25 n. 1).

art based on recent findings and discussions focusing on a specific problem, that of chronology.[2] The starting point of the present analysis is the two most probable hypotheses which have not until now been refuted: a (late) emergence around 1400 B.C. in a Hittite-Luwian bilingual environment, and an (early) emergence in the Old Assyrian Colony period at the latest.[3]

2. CRITICISM OF THE LATE EMERGENCE HYPOTHESIS AND THE PROBLEM OF THE ACROPHONIC SIGNS OF HITTITE ORIGIN

Yakubovich 2008 (cf. also Yakubovich 2010: 285–299) proposed that the Luwian hieroglyphs were developed around 1400 in the Hittite-Luwian bilingual environment of Hattuša, to enable the writing of Anatolian names and titles on durable materials in a local writing system instead of the Akkadian cuneiform out of "nationalistic concerns." This theory has been intensely criticised.[4] Oreshko (2013: 407) argued that it remains unexplained why this writing system was never used for Hittite if it was developed in Hattuša. But we do know that it was developed for Luwian (cf. above), and thus this is not a counter-argument. Furthermore, Oreshko called attention to the problem that there is no need to develop a new writing system if a perfectly functioning one (cuneiform) is already at disposal. He also emphasised that "nationalistic concerns" can hardly apply to Late Bronze Age Anatolia. Waal (2012: 306) added that the cuneiform writing system remained in use despite the appearance of the Luwian

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2   I find it premature to discuss the exact steps and/or creation process of the Hieroglyphic Luwian script until its basic chronology is clarified. Therefore, in the following I refer to the origins of this writing system in the most neutral way as "emergence" without implying any specific details.

3   The alternative hypothesis of Oreshko (2013: 400–409), who sees the starting point in Arzawa, will not be considered since the earliest inscriptions originate in Central Anatolia (see below) and the writing system was developed for Luwian (cf. above), the presence of which in Arzawa is more than dubious (see first of all Yakubovich 2010: 75–160; cf. also Simon 2018a: 381 with refs.; most recently, Melchert 2020: 244 leaves the question open). Besides, the presence of Hieroglyphic Luwian in Arzawa does not antedate the Hittite conquest, so that the most straightforward explanation is that it was introduced by the Hittite conquerors (note that local features can also appear secondarily, *contra* Oreshko 2013: 400–402, cf. also Yakubovich 2013: 117). The same applies to the West Anatolian hypothesis of Hawkins (1986: 374, 2003: 168–169) with the additonal problem that he proposed this region because of the assumed inspirational role of the Aegean scripts (but see now his inclination towards the theory of Yakubovich discussed below, Hawkins 2013b: 5). This assumption is not only based on purely typological commonalities, but also contradicted by the different structure of these writing systems (Neumann 1992: 26–27; Yakubovich 2008: 16–17; Valério 2018: 144 n. 2). For a short overview of the hypotheses, see de Martino 2021: 114–116 and the (superficial) treatment in van den Hout 2020: 120–134.

4   The critique of Hawkins 2013b: 3 that Yakubovich's conclusions on bilingualism are "somewhat extreme" shall be mentioned only for the sake of comprehensiveness, since Hawkins did not adduce any argument.

hieroglyphs. Nevertheless, this argument is not decisive since different media (e.g., inscriptions in stone or on wooden tablets) or audiences (e.g., local administration or local messages) can perfectly well justify the development of a new writing system. Furthermore, self-expression (e.g., in the form of one's own script for a language/ country instead of the writing system of foreigners and enemies) belongs to the set of possible motivations (see also Payne 2008: 121), but Yakubovich's unfortunate wording misled fellow researchers. It is important to note that the entire problem exists only if the adoption of cuneiform preceded the emergence of Hieroglyphic Luwian writing. Accordingly, the exact chronology should now be addressed.

Yakubovich dated the emergence of the Luwian hieroglyphs late because he observed that some of the acrophonic signs could be explained only from Hittite. This in turn requires Hittite-Luwian bilingualism, which, however, cannot be dated earlier than the 14th century. This double claim is of the utmost importance in explaining the origins of the Hieroglyphic Luwian writing system. Still, Waal (2012: 306) dismissed the problem with the single remark that based on the attested personal names, a "mixed language environment" already characterised the Old Assyrian Colony period, which thus could also easily have been the background for the emergence of the Luwian writing system. But this is absolutely not the case. It is well known that the proportion of Luwian personal names and Luwian loanwords in the Old Assyrian texts is small.[5] In other words, the language situation was definitely not conducive to developing a writing system for Luwian (the status of Luwian in the Old Assyrian Colony period will be more closely examined at the end of this paper). Furthermore, a "mixed language environment" is not identical to bilingualism, and the usage of Hittite words to create a writing system for Luwian requires bilingualism *sensu stricto* — one uses only the words of one's mother tongue(s) while creating a writing system for one's mother tongue(s) and not the words of a different language of one's neighbours, as in a "mixed language environment" (cf. also Valério 2018: 156).

Therefore, any judgement of the validity of Yakubovich's proposal should address the following two arguments: first, the existence of acrophonic signs from Hittite; and second, the dating of Hittite-Luwian bilingualism.

Yakubovich identified four groups of Hieroglyphic signs according to their origin: 1) signs of unknown origin, 2) signs of acrophonic origin that can be explained only from Luwian, 3) signs of acrophonic origin that can be explained from both Luwian and Hittite, and 4) signs of acrophonic origin that can be explained only from Hittite. It is the last group that interests us, which consists of the following four signs according to Yakubovich:

5   On the personal names see the optimistic overview of Yakubovich 2010: 208–223 and the critical discussions of Simon 2016, Kloekhorst 2019a: 58–65, and Giusfredi 2020. On the loanwords see Dercksen 2007, Vernet — Vernet 2019 (their Luwian proposals are, however, frequently Hittite, see Simon 2019b), and Simon 2020b; for an additional Luwian loanword see Simon 2015. Hawkins 2013a: 28 claims that "even if only a proportion of the over sixty names proposed are correctly identified as Luwian, it will suffice to show a substantial presence in the environs of Kaneš already at this early date." However, in view of the hundreds of Kanišite Hittite names (Kloekhorst 2019a), it will obviously not suffice.

1) *41 <tà> CAPERE: Hitt. *dā*- 'to take'   Luw. *lā*- 'dto'
2) *56 <ká> INFRA   Hitt. *katta* 'down'   Luw. *zanta* 'dto'
3) *90 <ti> PES   Hitt. *tiye/a*- 'to step'   Luw. *tā*- 'to step, to arrive'
4) *391 <mi> 4   Hitt. *mieu-/miu*- 'four'   Luw. *māwa*- 'dto'

However, Oreshko (2013: 405–406) doubted that these syllabic values can only be explained on the basis of Hittite:

(1) In the case of *41, he pointed out that *lā*- etymologically originates in **da*- and it is possible that there was a Luwian dialect without this sound change, and thus, the syllabic value would reflect this dialectal form. Although the existence of such a Luwian dialect cannot be excluded, no evidence for it currently exists. Moreover, it is incomprehensible why the creator(s) of the script would have used words from dialects other than their own (cf. above). Note that the derivation of the syllabic value is problematic, see below.

(2) In the case of *56, Oreshko claimed that the syllabic value <ká> is restricted to the Empire period, and thus he attributed this value to Hittite scribes. This explanation is, however, a *petitio principii*.

(3) In the case of *90, Oreshko argued that *tā*- is not the etymological cognate of *tiye/a*-, which is not only without relevance but also false. He also emphasised that a derivation from 'to step' is not the only possibility. This is correct, but no alternative was provided. The most important is his third argument that the verb *tā*- is written with the logogramm CRUS, and thus, the comparison with PES fails. However, this does not refute the derivation from *tiye/a*-, and his speculation that the stem *tiya*- is not yet attested or recognised in Luwian cannot be proven, even if it is correct.

(4) In the case of *391, Oreshko rightly remarked that its syllabic value was <ma/i> in the Empire period (cf. also Oreshko 2016: 93–94), which could only be explained from Luwian and not from Hittite. As described by Hawkins 2000: 28 (cf. also Güterbock 1998: 203, who was even quoted by Yakubovich 2008: 25 n. 28 in acknowledging a value <ma$_x$>), there were many cases in the Empire period of <mi> as a phonetic indicator in which the reading <ma> would reflect the underlying word more precisely, and thus, it could even be read as a phonetic complement in some of these cases. Accordingly, this syllabic value was assumed by many scholars; a notable exception was Hawkins himself.[6] This does not prove the syllabic value <ma>, though. However, Oreshko (2013: 394, 2016: 94) quoted an Empire period digraphic seal from the Middle Euphrates region (Gonnet 1991: 200 No. 24) that spelled the name of the owner, H̬imāši-Dagan, as *hi-MI-sa$_5$-tà-ka* and thus proved the syllabic value <ma>. Nevertheless, Weeden (2018a: 339) rejected this interpretation, and since he wanted to save the exclusive reading <mi>, he assumed vowel metathesis, which is of course completely arbitrary and a classic example of *petitio principii*. Moreover, Weeden (cf. also Weeden 2014: 91 n. 35) ignored the other arguments for the value <ma>:

6   E.g., Bossert 1944: 197; Otten 1953: 28 n. 56 (he only entertains the idea); Laroche 1960: 211; Marazzi 1990: 253.

a. Allumari, the Assyrian spelling of a ruler of Malatya, who can only be identi-
fied with PUGNUS-*mili* (Simon 2016).

b. The first sentence of the KARAHÖYÜK inscription in which the traditional
reading *la-mi-ní-ʿ* led to an unknown word (the translation of Hawkins 2000:
289, 291 "at the moment, at the time" based on Hitt. *lammar* 'time' is morpho-
logically not possible). The new reading, however, produced **a-la-ma/i-ní*,
leading to a morphologically regular and semantically fitting word ('name')
(cf. Oreshko 2013: 391 n. 117 with refs.).

c. Finally, the word ("VITIS[")*mi-t*[*u-...*] 'wine' in TELL TAYINAT 2 line 2, Frag.
2b-a, which is written normally as *ma-tu-* (Sasseville 2017), for this reason re-
quires a reading <ma>. Weeden again arbitrarily claimed that this is a dialec-
tal form because he followed the speculations of Hawkins 2000: 373 ("variant
spelling or dialectal form") without any doubt.[7]

To sum up, despite the criticisms of Oreshko, two signs cannot currently be explained
other than as acrophonic signs based on Hittite words: *56 <ká> INFRA from Hitt.
katta 'down' and *90 <ti> PES from Hitt. *tiye/a-* 'to step'. They demonstrate that the
creator(s) of the Luwian script also used the Hittite lexicon as a source. Accordingly,
one still has to assume that the basic tenet of Yakubovich was correct and that the Lu-
wian hieroglyphs emerged in a bilingual environment.

However, recent investigations have uncovered new pieces of evidence that con-
tradict the chronology of Yakubovich: the sign *41 <tà> CAPERE, the graffiti from
Kayalıpınar, and the inscriptions on the stag-shaped silver drinking vessel.

The problem with the sign *41 <tà> CAPERE is that the alleged source of the sign,
Hitt. *dā-* 'to take', has a voiceless initial consonant, but the initial consonant of the
Luwian sign is not voiceless, since this sign and the signs with initial /t/, i.e. <ta> and
<tá>, are in complementary distribution, as demonstrated by Rieken 2008. It is used
in the position of /d/ (but cf. also below). In other words, this sign cannot be derived
from either Hittite or Luwian. More precisely, it cannot be derived from these lan-
guages in a specific period. It could be explained from Luwian if the sign was created
before the Luwian sound law **d-* > *l-* (a similar scenario ["phonetic change or lexical
replacement"] was proposed already by Morpurgo Davies 1987: 211 n. 17). We do not
know when this sound change happened, but it must have happened before the first
attestation of Luwian, i.e. during the Old Assyrian Colony period at the latest.[8] But

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7   It must remain an open question how the value <mi> came about (it must be noted that not
     all double values have been explained). One can speculate that both stems reflect differ-
     ent ablaut grades of the Luwian word for 'four'. Although synchronically Luwian no lon-
     ger shows different stems in this word, the Hittite cognate shows that this must also have
     been the case earlier in Luwian, even if the precise prehistory and reading of this word
     are unclear (for different attempts see Tischler 1990: 178–179, Puhvel 2004: 118, Kloek-
     horst 2008: 572, all with further refs.). If this is the case, it could point to a very early date
     for the formation of the Luwian script, for which see below.

8   There was no middle step **t-*, i.e. this change is independent of the general initial devoic-
     ing, otherwise, no Luwian words with initial /t-/ would have remained.

the sign could also be explained from Hittite, more precisely from Old Hittite, as demonstrated by Rieken (2015: 221). She quoted the investigation of Kloekhorst (2010), according to whom there were two types of initial dentals in Old Hittite (setting aside a problematic case, for which Kloekhorst 2016: 217–218 created a separate category, cf. also Kloekhorst 2019b: 165–167): the first type was spelled consistently with TA (the general reflex of initial dentals) and the second type was spelled consistently with DA (the reflex of initial dentals followed by a laryngeal). The verb *dā-* 'to take' belongs to this latter group (Kloekhorst 2010: 202–207). In other words, *dā-* 'to take' was not yet /tā-/ in Old Hittite, but something like /dā-/ (Kloekhorst assumed a glottalised /t$^?$/, but we obviously do not know its precise phonetics). Alternatively, Yakubovich (2020: 232) claimed that the initial neutralisation spread contemporarily by diffusion and the spelling with DA characterises the last forms affected due to their frequency. Nevertheless, in the case of an ongoing sound change one would expect variable spellings and not strict consistency. However, if Yakubovich's scenario is correct, a derivation from a Hittite word is still possible, just slightly pushed back in time.[9]

However it may be, this sign can be explained from both Luwian and Hittite, but in both cases only from an early period, which cannot be later than the Old Hittite period. In other words, we have the first piece of evidence here that the Luwian writing system emerged in the Old Hittite period at the latest and that the chronology of Yakubovich is therefore problematic.[10]

9 For the sake of completeness, it must be mentioned that Melchert (online) has rejected the analysis of Kloekhorst as "typologically improbable," which is, first, a methodological mistake since typological tendencies are not laws, and thus, they do not present compelling arguments, and second, Melchert did not provide any explanation for the consistent orthographic distinction observed by Kloekhorst.

10 I am pleased to mention that after my talk Alwin Kloekhorst informed me that he has reached similar conclusions in a forthcoming paper (published now as Kloekhorst 2019c). In fact, he even assumed a pre-Old Assyrian Colony period sound change and, accordingly, an early emergence of the Luwian hieroglyphs. The reason for his date is that he excludes the Hittite derivation based on his phonetic assumption (cf. above) and he defined the underlying consonant of <tà> as [ð] because Rieken (2010), modifiying her early views, also attributed the value [d] to <tá>. However, such an early date is possible only if the sign is based on the Luwian word, but a Hittite derivation is equally possible: Kloekhorst assumed the value /t$^?$/ for DA because DA was used not only for [da] but also for [ṭa] in Old Babylonian. While this is correct, Old Babylonian cuneiform was only indirectly transmitted to the Hittites via a still unknown path, and thus, it is problematic to assume the Old Babylonian values automatically. Moreover, we cannot exclude alternative interpretations with sound substitution, i.e., that DA was used only as the phonetically closest equivalent of the Hittite consonant, e.g., [d^h] (Kloekhorst also admitted that there are alternative possibilities and listed [d^ʕ] and t^ʕ]). A value [d^h] would fully conform both to the origin of the sign and to the etymology of the words involved going back to a combination of a voiced dental with a laryngeal. This would make a Hittite derivation absolutely possible. In general, it must be noted that the assumption of /t$^?$/ is only etymologically motivated and rests on the phonological framework of Kloekhorst reducing the Hittite stop inventory to voiceless consonants only, which has no basis (Simon 2020a).

The second piece of evidence is provided by the graffiti from Kayalıpınar from a stratified context on the internal surface of the southern external wall of Gebäude A. They date between the early 17th century and 1400. Since they are not masons' marks but real, carelessly drawn graffiti, they demonstrate the everyday usage of the Luwian hieroglyphs already in the Old Hittite period.[11]

Finally, the stag-shaped silver vessel has two inscriptions, which perhaps should be read as (DEUS$_x$)CERVUS$_x$ and á(DEUS$_x$.FILIA).⸢X⸣, i.e. Kuruntiya and Ala, the names of the gods depicted below the inscriptions (Bolatti Guzzo — Marazzi 2010: 14–21; they read X as *461, which is, however, not possible epigraphically, cf. van den Hout 2018: 120). Although van den Hout (2018) recently claimed to have found a new reading of X, REGIO (i.e., á-X(REGIO) FILIA, the affiliation of the dedicator, who is allegedly mentioned in the other inscription), his reading is neither new nor unproblematic: Woudhuizen (2013) suggested the same reading five years earlier (not mentioned by van den Hout) and already pointed out the problem with this reading: it assumes an irregular REGIO sign, because the sign's triangles are not connected in their bottom part (Woudhuizen 2013: 335). Whatever the case, we are interested in the date of the inscriptions. On this issue, all possible periods have been proposed, from Old Hittite times to the age of Tudhaliya IV. Since I have discussed this question elsewhere in detail (Simon 2019a: 140–142), suffice it to say that none of the previous arguments is compelling and all researchers have neglected the depicted pottery (a so-called *Schnabelkanne* and a pitcher), the forms of which can however be reconciled only with an Old Hittite date.

Thus, there are at least three independent pieces of evidence, the sign <tà>, the graffiti from Kayalıpınar, and the inscriptions of the stag-shaped silver vessel, that demonstrate that the chronology of Yakubovich, i.e. the late emergence of the Luwian hieroglyphs after 1400 B.C., cannot be correct since in the Old Hittite period at least a rudimentary writing system already existed that made use of syllabic signs.[12] Having said that, it would be premature to exclude the hypothesis of Yakubovich and assume an early emergence (e.g., in the Old Assyrian Colony period), because the late dating of Yakubovich's proposal was based on his late dating of Hittite-Luwian bilingualism. So the next question to be analysed is if this late dating of bilingualism is correct.

3. DATING HITTITE-LUWIAN BILINGUALISM

The overwhelming majority of the changes explained by Luwian influence appear for the first time in the Middle Hittite (i.e. in the transitional) period. In one case, the Old Hittite period was also considered, but it cannot be demonstrated unambiguously (on

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11 Their reading is, however, still problematic. The publisher attempted to read them as personal names with title or occupation, which is plausible in itself, but the signs can be identified only partly (Müller-Karpe — Müller-Karpe — Rieken 2017: 73–77).

12 A fourth piece of evidence may be the sign <ma/i>, per above. I deliberately exclude the Old Hittite seals from the discussion for the time being because of their problems in dating and reading; see now Weeden 2018b.

the problem of the substitution of the allative by the dative see Hoffner — Melchert 2008: 76 and now Frantíková 2016). Rieken (2006: 275) noted that the abolishment of the number distinction in the genitive is attested already in the Old Hittite period, and if we are not dealing with a coincidental parallel development, Luwian must have influenced Hittite already in the Old Hittite period. Rieken's cautious formulation was completely justified, since excluding the alternative explanation is not possible: it was not the gen. sg. ending -*aš* that was extended to the genitive plural (as in some dialects of Luwian), but the dat. pl. ending -*aš* (emphasised already by Hoffner — Melchert 2008: 73), so that there would be no reason to assume Luwian influence.[13]

Accordingly, our starting point should be that confirmed changes due to Luwian influence are attested in the Middle Hittite period, and this is exactly the problem. These cases are not of code switching, but changes in Hittite grammar. Such changes require intensive language contact for a longer period, so when the results are attested in the texts, they must already have been preceded by a longer period of bilingualism. In other words, bilingualism must be assumed already for the Old Hittite period. Exactly this circumstance solves the contradiction between the proposal of Yakubovich and the evidence of the texts.

If extensive Hittite-Luwian bilingualism can be dated already to the Old Hittite period, the formation of the Luwian hieroglyphs can be dated to the Old Hittite period at the latest, which fully conforms to the acrophonic signs based on Hittite words, to the sign <tà> reflecting Old Hittite, to the Old Hittite graffiti from Kayalıpınar, and to the Old Hittite inscriptions of the stag-shaped silver vessel. A major Luwian-speaking population in the Old Hittite period has indeed already been proposed by some scholars on independent grounds: On the one hand, Goedegebuure (2008: 173) assumed a great number of Luwian speakers at this time based on alleged structural influences in Hattian (which is, however, false; see below). On the other hand, Yakubovich (2010: 248–260) argued on the grounds of the personal names of this period and the Luwian sections of the Hittite rituals of the state cult that Hittite society was already multilingual at that time, with Hittite- and Luwian-speaking groups.

4. THE PROBLEM OF THE OLD ASSYRIAN COLONY PERIOD

Only one question remains: can the emergence of the Luwian writing system be dated even earlier, to the Old Assyrian Colony period at the latest? This idea is not new, and the reasoning practically since the beginning of research has consisted of the following two arguments, which were most recently evaluated by Waal (2012 with refs.).

First are the so-called *işurtum*-documents of the Old Assyrian texts, i.e. "economic documents prepared by an Anatolian for an Assyrian," that are not cuneiform tablets and thus are identified with the *gulzattar*-wooden tablets. However, Valério (2018: 143 n. 1) has argued that the *işurtum*-documents do not necessarily imply another writing

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13 Similar changes in the pronominal system do not prove the singular origin of -*aš*, *contra* Kloekhorst 2017: 393 with ref. See the discussion of Goedegebuure 2019 that -*aš* is indeed an innovation, *contra* Kloekhorst 2017.

system, since they could have been different from the cuneiform tablets from a functional point of view (e.g., a numerical notation consisting of "marks scratched on wood"). Arguing for the identification, however, is the equivalence of the terms Old Assyrian *iṣurtum eṣērum* = Hittite *gulzattar gulš-* 'to draw, to write with hieroglyphs' (upon wood, stone, metal, but not on clay), if the analysis of Waal (2011) is correct. Although the proposal is very plausible and would demonstrate the existence of the Hieroglyphic Luwian script already in the Old Assyrian Colony period, the obvious problem is that we do not have any *iṣurtum*-document at our disposal, and thus, this theory cannot be proven.

Second is a group of alleged Hieroglyphic Luwian inscriptions from the Old Assyrian Colony period. After earlier discussions, these inscriptions have been re-evaluated in detail by Hawkins (2011), Waal (2012: 298–300; cf. also Waal 2017: 118), and Poetto (2018). Waal cited four examples, which she identified as proprietary inscriptions. Hawkins was more sceptical: he counted only two cases and did not identify them as inscriptions, but as "randomly devised symbols" for names. Poetto was even more sceptical and identified only a single case as an inscription.[14]

Among the four cases of Waal, one is definitely not an inscription, but the usual grip of a vessel (Börker-Klähn 1995: 40–41, thus with question mark in Waal 2012: 299). Even if it were an inscription, it would be nothing else than the sign TONITRUS, the symbol of the Storm-god and thus, it cannot be identified as an inscription (Poetto 2018 did not even include it).

The second case is a vase with three painted figures. Among them, two (a snake and a star) do not correspond to any known Hieroglyphic sign and thus the similarity of the third figure to the sign *215 <ha> is hardly relevant. One could, of course, argue that the writing system had signs in this early period that were later abandoned. This is possible and, in the light of the sign inventory of the Old Hittite seals, even probable, but these figures cannot be obviously identified as a Hieroglyphic Luwian inscription based on unattested Luwian signs (similarly rejected already by Meriggi 1975: 315 with refs., followed by Poetto 2018: 17 with references to the supporters).

The third case is again a vessel with four scratches. Among them, Waal would identify three hieroglyphs, although only a single sign shows any similarity to the attested signs, and this is the sign TONITRUS. It remains, however, doubtful if they are more than scratches, which were also the identification of Waal herself for the second figure from left (rejected also by Poetto 2018: 17, but accepted by Hawkins 2011: 96).

The fourth case is also a vessel, and most scholars see three signs there (Poetto 2018: 19 with refs.). However, only the first one, a donkey head, could be identified. Setting aside the definitely incorrect reading of Archi 2015: 24 (ASINUS-X+*ra/i*), Poetto and Woudhuizen (2011: 84, 464) proposed a more developed reading and reached the same result: ASINUS-*si-li* (even if their own transliterations and thus interpretations are not identical due to their diverging views). Poetto (2018: 20–22 with refs.) argued that the sign *174 can be read not only as <si>, but also as <sa> since it is interchangeable with <sa> and <sà>, and thus read the name as *Tarkasna-salli-*

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14 Only for the sake of completeness shall the rejection of Weeden (2016: 86–87) be mentioned, since he did not offer any arguments for his opinion.

'great (one of the) donkey(s)'. This interchangeability requires further research, but it should be noted that the traditional reading Tarkasnasili, which was rejected by Poetto on the grounds that it is not transparent morphologically, would be a regular *-ili*-adjective from an adjective of appurtenance or toponym *Tarkasnassa-* (type Hattušili). However it may be, the question is if the signs are really there. Here I must indicate my scepticism. The donkey head is there, and the second sign could indeed be <si>. But the third sign is horizontal and not vertical as <li> is usually written,[15] and it is combined with the preceding sign in a ligature (which is why some scholars see only two signs), which is not a regular procedure with these signs. Accordingly, I am not sure if we can really read ASINUS-*si-li* here. Admittedly this case has the biggest chance to be an inscription (also Müller-Karpe — Müller-Karpe — Rieken 2017: 67 n. 3 emphasise that we are dealing with an inscription; similarly, but cautiously Doğan-Alparslan — Alparslan 2017: 53), but it is not fully regular in any case, and one cannot claim the existence of Luwian inscriptions and script based on irregular signs. Thus, I can preliminarily classify it only as an uncertain inscription (similarly rejected also by Payne 2015: 69 n. 114).[16]

All in all, we do not have any confirmed inscription from the Old Assyrian Colony period at the moment. Since there is neither a secure inscription nor an *išurtum*-document at our disposal, the emergence of the Hieroglyphic Luwian script cannot (yet) be dated to the (pre-)Old Assyrian Colony period. If someone maintains that the case of the last vessel is indeed an inscription, then there is evidence for the claim that the script emerged by the Old Assyrian Colony period. However, in this case they would have to explain how and why the creator(s) of the writing system could have used Hittite words. This leads to our last topic, the perspectives of the research.

5. PERSPECTIVES

As presented above, the current evidence implies the emergence of the Hieroglyphic Luwian script in the Old Hittite period at the latest. What are the perspectives?

First, as presented above, the contribution of Hittite to the date of the emergence of the Luwian script is crucial. If the remaining two acrophonic signs (*56 <ká> INFRA from Hitt. *katta* 'down' and *90 <ti> PES from Hitt. *tiye/a-* 'to step') could receive another explanation, one would be completely free to date the emergence independently of Hittite-Luwian bilingualism. Note that the Luwian cognate of Hitt. *tiye/a-* 'to step' (the assumed source of *90 <ti> PES) is not known, so that this sign cannot serve as an unproblematic argument. However, it remains impossible at present to reconcile *56 <ká> INFRA with any known Luwian word.

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15   Massimo Poetto has kindly called my attention to DARENDE line 3, GÜRÜN (lower inscription) line 2, and KÖTÜKALE line 3, where <li> is indeed not vertical. Nevertheless, they are not horizontal either, but somewhere halfway in between.

16   Note also the important *caveat* of Marazzi (2018: 47) that the signs were incised after firing, which in combination with the lack of published details on the findspot allows only a *terminus post quem*.

Second, would unproblematic evidence for the usage of the Hieroglyphic Luwian script already in the Old Assyrian Colony period be found, it would solve the problem. But then we are back to the previous problem: how can the signs from the Hittite words be explained? Or can we date Hittite-Luwian bilingualism back already to the Old Assyrian Colony period? For that it should be demonstrated that Luwians lived in great numbers among Hittites already in the Old Assyrian Colony period. In fact, as mentioned above, Goedegebuure (2008) argued exactly this for Central Anatolia. She claimed that Hattian is a so-called VSO language, which, however, does not correspond perfectly to the type of VSO languages since it has some SOV features, and accordingly a SOV language must have influenced Hattian in a bilingual environment, and this SOV language would be Luwian. This is, however, a methodological mistake. There are no "pure" VSO or SOV languages, and thus, it is a misunderstanding to explain the SOV features of a VSO language exclusively with the influence of an SOV language (setting aside that this SOV language could have been Hittite or Palaic, too, which were excluded by her on insufficient grounds).[17] The remaining evidence for Luwian speakers in this period consists of the small number of personal names and loanwords mentioned above. Accordingly, nothing currently proves the presence of Luwian speakers in huge numbers in Central Anatolia in the Old Assyrian Colony period. Large-scale Hittite-Luwian bilingualism is, accordingly, not very probable. And in this case the acrophonic signs based on Hittite words would remain unexplained.

6. CONCLUSION

The sign CAPERE, the inscriptions of the stag-shaped silver vessel and those from Kayalıpınar require us to date the emergence of the Hieroglyphic Luwian writing system to the Old Hittite period at the latest. The signs INFRA and PES require a Hittite contribution, which can only be assumed in a Hittite-Luwian bilingual context. However, these two circumstances, the Old Hittite period and bilingualism, do not exclude each other, since extensive bilingualism should be dated already to the Old Hittite period at the latest because of the date of grammatical changes beginning in Middle Hittite texts. In the present state of evidence, it must remain open if the Luwian script can be dated even earlier. Confirmed inscriptions are still missing from the Old Assyrian Colony period, and how the Hittite contribution to the sign inventory in the Old Assyrian Colony period (not to mention earlier) could be explained remains unknown. Accordingly, the currently available evidence points to the emergence of the Luwian script in the Old Hittite period.

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17   She excluded Hittite because Hittite does not show any traces of Hattic structural influence, which she expects in a bilingual situation. However, influences are not necessarily
     mutual. She excluded Palaic because it is supposed to be too peripheral, but current research on the toponyms shows that this is not necessarily the case (Simon 2018b: 264).

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