ARGUMENTS FOR THE AUTHENTICITY OF THE LUWIAN HIEROGLYPHIC TEXTS FROM THE MELLAART FILES

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Given that James Mellaart forged numerous documents which he claimed were of Late Bronze Age origin, the authenticity of the Luwian hieroglyphic texts retrieved from his estate inevitably has to be re-examined. However, the inspection of Mellaart's study (see Zangger's contribution "Mellaart's Fantasies", this volume, pp. xxx) yielded no proof that Mellaart had fabricated these Luwian hieroglyphic texts, the most prominent of which was Beyköy 2. The complexity of the grammar, the many idiosyncrasies and the severity of Mellaart's misunderstandings of the text virtually rule out the possibility that he himself fabricated this document. It never changed in appearance while it was in Mellaart's possession. He did not alter even a single letter of the attempted translation that he attributed to Alkım. On the other hand, four different stages of arrangements were needed before the order of the blocks was correct. If the document was falsified, there are a number of idiosyncrasies suggesting that the forger must have been a fool; at the same time there are intricacies that would have required the forger to have been a highly-skilled linguist. What is more, the text contains expressions which were not known in 1989, when Beyköy 2 was first publicly presented. For instance, the title of "great prince" was discovered only much later in the rock reliefs at Latmos. Among the most important indications that Beyköy 2 was not a fake created by Mellaart are his utter misinterpretations of the contents. Mellaart lacked the skills to interpret the texts, translated a number of phrases wrongly, and missed even the gist of the document. As a consequence, there is enough reason to assume that the Luwian hieroglyphic inscriptions from the Mellaart files, and particularly the Beyköy 2 text, are not falsifications but genuine discoveries.

Introduction
During the last week of February 2018, one of the present authors (EZ) received permission to examine the English prehistorian James Mellaart's (1925-2012) study in North London. Zangger was looking for additional evidence to

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reinforce the credibility of what Mellaart had described as an English translation of cuneiform tablets, said to have been found at the village of Beyköy in western Anatolia during the 19th century – the so-called Beyköy Text (BT). The findings in London, however, proved that Mellaart had fabricated these texts in their entirety (see Zangger this volume, pp. xx).

The revelation that a scholar who ranked amongst the world's most renowned archaeologists had indeed fabricated documents on such a massive scale, claiming they were over three thousand years old, will inevitably draw attention to other finds presented by this individual. In particular, one wonders whether the Luwian hieroglyphic texts that were given to Zangger by Mellaart's son Alan eight months earlier, also derived from James Mellaart's estate, may have been forged as well. Several experts in Luwian hieroglyphic had already questioned the authenticity of these inscriptions in personal communications, even before news about the latest revelations of Mellaart's forgeries broke. One therefore wonders whether these texts may not also have been the product of Mellaart's imagination. During his inspection of the study, Zangger did indeed find notebooks from the late 1940s and early 1950s proving that Mellaart had closely studied the signary of the Luwian hieroglyphic script. This nullifies Mellaart's earlier statements that he was an ignoramus in this field.

The publication of the largest Luwian hieroglyphic text, Beyköy 2, we received in June 2017, and published in this volume, already contains a substantial discussion of arguments for and against forgery with respect to this particular document. However, the new disclosures about Mellaart's fantasies justify additional scrutiny of the potential authenticity of this text, as well as of the other smaller hieroglyphic documents retrieved from the same estate. Additional scrutiny is all the more justified given that the newly found material contains a scholarly treatment of Beyköy 2 that James Mellaart evidently wrote (since the carbon copies were still attached to the original typewritten pages) but claimed was composed by the Turkish archaeologist Uluğ Bahadır Alkım, who had died in 1981. In other words, Mellaart was clearly untruthful about this matter as well. A reaction to the reception of Beyköy 2 by scholars in the field (this volume) would also seem in place.

Material in Mellaart's estate
The scrutiny of Mellaart's estate led to the discovery of what Zangger considers to be a "kit" that the prehistorian used to put together the massive amount of text, comprising about five hundred sheets of paper in total, that he attributed to the BT. This kit consisted of numerous pieces of cardboard on which Mellaart had written notes about individual episodes of Hittite history that allowed him to move things around to establish a sensible sequence. Mellaart's next step was to write by hand a first draft of alleged history in prose. This was then embellished in later versions

until the final draft in typewriting was produced (see Zangger this volume, pp. xx). These are indeed precisely the steps one would assume necessary to forge such a massive document.

Zangger's scrutiny of Mellaart's study, however, yielded no earlier drafts proving that Mellaart also fabricated the Luwian hieroglyphic texts. The kind of material found, in particular the chronological sequence of how the document was processed, is indeed the complete opposite of that of the BT. Mellaart's possession of the text begins with a pencil drawing on copy paper evidently drawn by himself and most likely produced as a tracing (of a drawing?) on a light table (Fig. 1). The next step in production is a copy in which Mellaart traced his own pencil drawing, now in ink and on vellum (Fig. 2). The illustrations of Luwian hieroglyphic texts provided by Zangger & Woudhuizen (this volume, pp. xx) consist of these ink drawings by Mellaart. In short, the material handed down by Mellaart would suggest that he had come across the complete Luwian hieroglyphic document at the outset, perhaps in the form of photographs, or more likely drawings, and that he had an opportunity to copy these documents by manually tracing them.

Of course, there is always the possibility that a kit to compose hieroglyphic texts might have existed elsewhere. However, time constraints meant that the February 2018 investigation could not extend to a thorough screening of apparently less significant material stored in two collapsed garages. What is more, Mellaart had a large study in his parent-in-law's summerhouse on the Bosporus, which burnt down in 1976. In theory, he could have produced the hieroglyphic texts in Istanbul and then taken tracings of the finished product with him to London.

Mellaart's scholarly texts discussing the contents of Beyköy 2 yield additional insights (see Appendices 1 and 2). The prehistorian frankly admits that he was not a linguist, and that his knowledge of the Luwian language was limited – a claim clearly illustrated by the interpretation of the text he included, which is quite successful as far as the identification of geographic names is concerned but fails to grasp the meaning of the phrases between the enumerations of place and country names. In this manuscript, Mellaart's *alter ego* Alkım declares that the geography of western Anatolia, as recorded in the hieroglyphic texts, confirms the reconstructions worked out by no lesser individual than Mellaart himself! This shows how eager Mellaart was to show himself to be right. However, the geographic information yielded by the Luwian hieroglyphic documents does NOT verify Mellaart's reconstructions (Fig. 3). Evidently, he lacked the skills to interpret the texts correctly and thus did not apply the information given in the Luwian hieroglyphic texts to the many maps he kept drawing. The alleged cuneiform BT, on the other hand, was purposely invented by Mellaart to prove that his geographical reconstruction of western Anatolia was correct.

Fig. 1 Initial pencil drawing of Beyköy 2 by James Mellaart.

Fig. 2. Tracing of initial pencil drawing by James Mellaart, now using felt tip
pen on vellum.

Fig. 3 Political geography of Anatolia at the end of the thirteenth century BC.
Revised version of Fig. 6 on p. 54, this *Talanta*.

The research history

In his handwritten notes, Mellaart informs us about how Beyköy 2 was discovered and handled initially:

"Around the middle of the eighteen seventies [*ca*. 1875] news reached the Department of Antiquities [in] Istanbul that peasants search?ing for building materials in the mount of the village of Beyköy (Afyon Karahisar) had revealed a large number of blocks with an inscription similar to the Hama stones, removed by the Governor of Syria and sent to the newly founded Imperial Museum at Istanbul in 1872. (Soon after a similar find was reported from the town of Edremit, – where the local authorities had taken the blocks and kept them with other remains in the Belediye Garden).
George[s] Perrot, a French art historian, then in Turkey on a visit, was engaged to make facsimile copies and attempt to photograph the inscription – and was sent off with a suitable escort to Afyon. He found the local people troublesome but managed to record the Beyköy Inscriptions to his satisfaction, as well as the Edremit one. Orders were sent to have the blocks taken into custody for transport to the Imperial Museum in Istanbul.
Impatient with the delay, the Director of Antiquities set off for Afyon Karahisar – and found upon arrival at Beyköy that the inscribed stones had been built into the foundation of a new mosque. Furious he had the village searched and recovered a number (xxx?) of large metal tablets covered with cuneiform writing and had them confiscated".

One of the waste papers Mellaart used is a letter dated 1983, thus providing a *terminus post quem* for the production of these notes, as the paper may already have been old when used.

The story about Beyköy 2 having been drawn by Georges Perrot turns out to be problematic. Perrot did indeed visit and work at Beyköy. But in his book with Charles Chipiez of 1892 entitled *History of Art in Phrygia, Lydia, Caria, and Lycia,* he only refers on page 79 to the fragmentarily preserved inscription Beyköy 1 (now lost) as referred to by William Ramsay in 1884 and published by him in 1889, without mention of the much more important Beyköy 2. If Perrot really had drawn this, why did he not mention it in this particular context, and why did he not publish it, here or elsewhere? We owe this observation to Jorrit Kelder and Alwin Kloekhorst, in e-mails of October 17, 2017.

There is an alternative explanation of how the documents had turned up, one that Mellaart also mentions in his notes: Alkım, or someone else, may have tapped an unknown source. Mellaart's wife Arlette published an elaborate article with memories of the social life at her parents' summerhouse, the Savfet Pasha Yalı in Kanlıca (Mellaart 2002). Accordingly, this was a vibrant place frequented by illustrious guests including famous spies such as Kim Philby (1912–1988) and influential collectors such as the German banker Hans Sylvius von Aulock (1906–1980). Mellaart's notes imply that quite frequently, distinguished guests dropped by with artefacts that deserved scrutiny. In this way, he may have come across drawings or photographs of hieroglyphic texts that were perhaps acquired outside the law. Considering that the Dorak Affair had already damaged his reputation, he may have aimed to "whitewash" the catch with an alleged forthcoming publication by a Turkish scholar in English and Turkish. In his handwritten notes, Mellaart tells a story similar to this scenario:

> "Bahadir Bey tapped a[n] unknown source of information, not accessible to (most) western scholars of Hittitology and in recognition of their contribution, wanted this to be an all Turkish publication. As LBA geography is one of my 'specialities', and as I had married a T[urkish] girl and been invited to lecture at Istanbul University .... I was granted the honour of being a contributor .... (added: a magnificent gesture to? me? 1970!) I was shown the actual material, or photographs/copies thereof and was supplied with the copies, and Bahadir's translations for the purpose of writing my – mostly geographical part of the publication. That was all I needed –".

> "Inscriptions collected by Bahadir Bey from printed papers, etc. of several high Ottoman dignitaries ... c. 1854-1876 where datable – kept by their descendants".

These handwritten notes were retrieved from a dossier written on waste paper, dated 9 April 1987, 17 January 1989, and 23 January 1989. So Mellaart claims

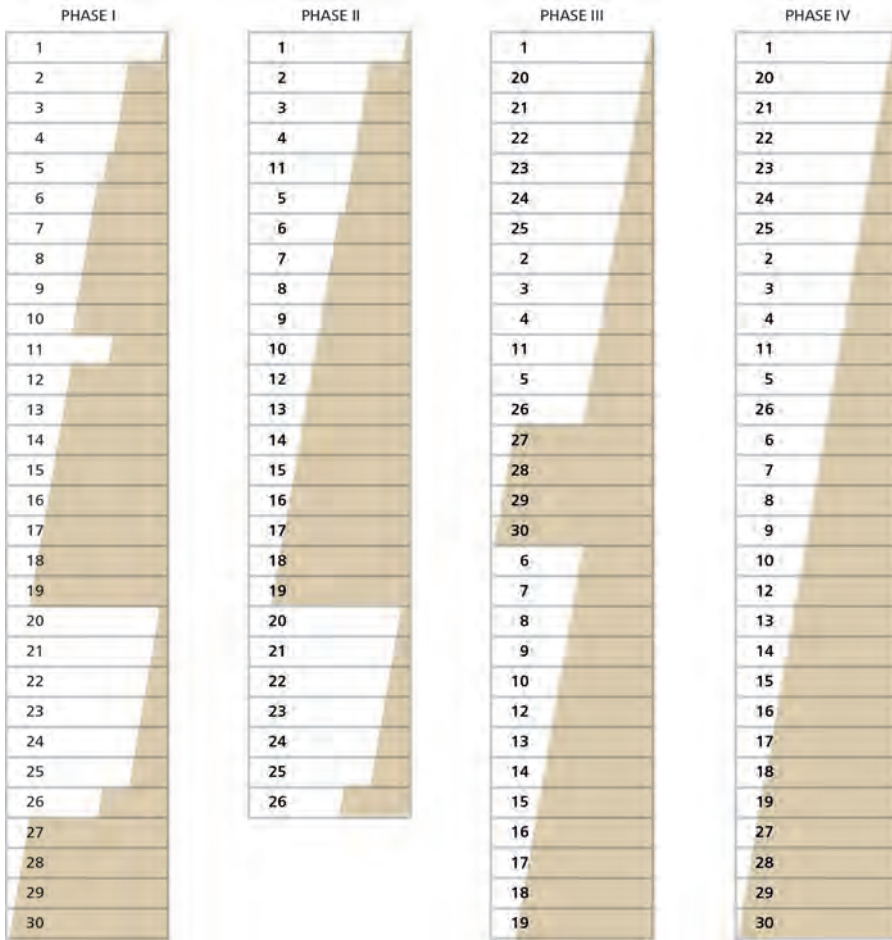| PHASE I	PHASE II	PHASE III	PHASE IV
1	1	1	1
2	2	20	20
3	3	21	21
4	4	22	22
5	11	23	23
6	5	24	24
7	6	25	25
8	7	2	2
9	8	3	3
10	9	4	4
11	10	11	11
12	12	5	5
13	13	26	26
14	14	27	6
15	15	28	7
16	16	29	8
17	17	30	9
18	18	6	10
19	19	7	12
20	20	8	13
21	21	9	14
22	22	10	15
23	23	12	16
24	24	13	17
25	25	14	18
26	26	15	19
27		16	27
28		17	28
29		18	29
30		19	30

Fig. 4 Order of the blocks in the different phases of Mellaart's attempts at reconstruction.

that he had received the copies of the Luwian hieroglyphic inscriptions from Alkım, who in turn had assembled them (according to the typewritten information) from a working group of scholars and laymen who were active in the 1960s and 1970s and specified as descendants of dignitaries of the Ottoman Empire. The typewritten document also claims that a bad photograph of some blocks of Beyköy 2 and drawings by a certain Subhi Sami of the Yazılıtaş and Edremit inscription were included in a copy of Perrot and Chipier's book *Histoire de l'art dans l'Antiquité* of 1884 as it was presented to Safvet Paşa (see Appendix 2). That Yazılıtaş and Edremit were indeed drawn by the otherwise unknown Subhi Sami is evident from the handwritten captions to the drawings of these texts:

"Longeur près de six metres, hauteur 30-35 cms. Inscriptions rupestre en hièroglyphique Egyptiens(?) sculptée en haut relief près du sommet de Mandira Dagh ("Yazılıtaş" sa Ayarmend) visitée en 1854 et copiée par Subhi Sami + variante chose".

"Edremit Belediye bahçevi 1871. SS [= Subhi Sami]".

The poor French has been considered an indication that the drawings were forged, since Perrot's French would, of course, have been flawless. However, if the captions were initially written by the illustrator Subhi Sami, these mistakes could be excused. All this information on the background of the inscriptions may, of course, be a product of Mellaart's imagination, similar to his writing in the name of Alkım. We simply do not know.

The sequence of the blocks
Falsifying a text of the length of Beyköy 2 is a complex matter: one has to lay out the individual signs in segments and distribute them over the individual blocks. The series of signs needs to be arranged in an order to form meaningful sentences – all in all comprising 50 phrases that, of course, include 50 verbs with the right conjugations. The falsifier therefore needs a very clear insight into the content of the text he wants to reveal, with all grammatical details in place. This already complicated undertaking was exacerbated by mashing up the correct order of the blocks. Mellaart's documents record four different stages of arrangements, with the first one represented by his initial pencil and ink drawings. Only the last sequence turned out to be correct.

In the course of time, Mellaart (or his informants) discovered that the order of the blocks needed to be rearranged to enhance understanding of the text (Fig. 4). First, block 11 made more sense when following block 4 (phase 2). Then it turned out that the missing genealogy of the principal of the text, Kupantakuruntas, could be found in block 20, prompting the insertion of the entire sequence of blocks 20-25 between block 1 and block 2 (phase 3). Finally, it was realised that of the sequence of blocks 26-30, inserted between block 5 and block 6 in phase 3, only block 26 belonged there, whereas blocks 27-30 should be reassigned to the end of the text (phase 4). Mellaart accredits the final sequence in a handwritten note to Alkım and dated it to 1980.

Considering the epigraphic and linguistic arguments involved in each step, the rearrangement of the order of the blocks would have complicated the business of falsifying Beyköy 2 – if it is a falsification − in a way which in our opinion is beyond the capabilities of even the best specialists in Luwian hieroglyphic, let alone a non-linguist like Mellaart. We think it is more sensible to assume that Mellaart (or his informant), in the course of studying the text, really did discover that the order of the blocks they were provided with required rearrangement.

One point should be emphasised: while attempts were made to improve the understanding of the text by rearranging the order of the blocks, Mellaart never changed a single letter of the translation attempt that he attributed to Alkım. He only cut and pasted the existing text a few times over, and eventually retyped it in phase 3 order, while sticking to the original word order down to the last comma. In our opinion, this speaks volumes about the limits of Mellaart's linguistic capabilities.

Another indication that Mellaart was not fully aware of what he was dealing with is the indication of the total length of Beyköy 2 provided in his pencil and later ink drawings. Initially, it indicates a total length of 17.55 metres; later Mellaart replaced this value with 25.5 metres. However, applying the scale provided by Mellaart himself, one inevitably ends up with a total length of between 29 and 30 metres.

The date of the drawing
When discussing the potential authenticity of the Luwian hieroglyphic texts, it would naturally be helpful to know when the drawings were produced, since this would define the knowledge available at the time in this swiftly evolving field. Mellaart claims that Alkım's invitation to contribute to the publication of the set of texts with a bearing on geographical aspects goes back to 1970. Beyköy 2 has much in common with Yalburt, since both inscriptions appear on long limestone friezes. Yalburt was discovered in the soil during backhoe operations in 1970. A full set of photographs was published only in 1988, but these were still inadequate to recover the contents of the text. This only became possible with the publication by Massimo Poetto in 1993. Mellaart may, however, have had ways of getting hold of the supporting material on Yalburt before publication.

Mellaart ascribed the ultimate rearrangement of the blocks to Alkım in 1980. The other handwritten notes discussed above are associated with letters dated between 1983 and 1989. In conclusion, it is not possible to define the date when the work on the text was conducted – apparently at some point between 1970 and 1989. Beyköy 2 was indeed presented by the British hittitologist Oliver R. Gurney at the *Rencontre Assyriologique Internationale* in Ghent in July 1989 – and thus became public knowledge amongst experts. The document never changed in appearance before or after this date.

The following analysis of linguistic details of Beyköy 2 is, of course, the work of Fred Woudhuizen (who sometimes comments on it in the first person). It is arranged into four different categories. Some idiosyncrasies in the text would suggest that, if the document was forged, the forger must have been a fool (I). Other peculiarities, however, require the forger to have been a highly-skilled linguist (II). The text contains expressions which were not known in 1989, meaning that the forger would have had to have been clairvoyant (III). There are also similar indications that the other Luwian hieroglyphic texts from Mellaart's estate are

indeed genuine (IV). Finally, Woudhuizen discusses areas where his own inter-pretation of Beyköy 2 now differs from his initial version.

Arguments for why the forger must have been a fool
1. The forger does not work along the lines of the new reading – as introduced in 1973. This applies to the reading of sign *376. In Beyköy 2 it is rendered both as *i* (= old reading) and *zi* (= new reading), in conformity with the adjusted old reading as proposed by me (Woudhuizen 2011, 89-98). However, the new read-ing by Hawkins, Morpurgo-Davies and Neumann (1973) requires that the sign *376 be read in all instances as *zi*, or, in the Late Bronze Age texts, also as *za*.

(1) *ma-sa-hù+*376-ti* (Bk-2, §§ 1, 5) = Hit. *Mashuittas*
   376-ku-wa-na (Bk-2, § 50) = Hit. *Ikkuwaniya* (mod. Konya)
(2) *mi-*376+r(i)* (Bk-2, § 28) = Hit. *Mizri* (mod. Egypt)
   *ka-*376* (Bk-2, § 28) = *Kaza* (mod. Gaza)

Signs *377 with two strokes at the lower side for vowel lengthening was until recently (see sub 12 below) only known from the Early Iron Age. It would there-fore have been foolish for a forger (in the 1980s) to use this sign in the Beyköy 2 text dating to the Late Bronze Age.

The forger may have been inspired by Bossert's (1960, 441) reading of *mi-*377+r(i/a)* as *Mi-za+ra* "Egypt". Bossert argued that *376 and *377 are in certain cases to be read as *zi* and *za*, but in others the old reading (as *i* and *ī*) still applies.

2. The text contains some occurrences where *n* is written before *t* – something not known in Luwian hieroglyphic (thus far). It would therefore have been fool-ish for a forger to introduce this spelling. The writing of *n* before dental *t* is used inconsistently in Beyköy 2. It occurs, for instance, in *la-la-na-ta* (Bk-2, § 45), but not in *la-la-ta* (Bk-2, § 19), the name of the Hittite place *Lalanda*. In cuneiform Luwian, however, the *n* is indeed written before the *t*. Principles of one script could perhaps have occurred in the other, since scribes were versatile in both scripts. The arguments listed below (sub 6) will show more such interfer-ences between the two script traditions. Indeed, it seems relevant to note in this context that a Middle Bronze Age Luwian hieroglyphic seal bears the testimony of the writing of *n* before dental *t:* the legend *ARMA-na-ti-à* "Armantis" occurs on the stamp seal in form of a foot from Nevşehir (Erdem 1969; Woudhuizen 2018, 38, sub 6).

3. In Beyköy 2 the men's names are mostly not written with the ending of the nominative singular *-sa*, as is expected from an Indo-European point of view. However, in two instances the expected ending of the N(m/f) sg.in *-sa* does oc-cur. In the Late Bronze Age writing tradition this spelling is suppressed, whereas

in the Early Iron Age tradition it occurs. A forger would therefore have been foolish to deviate from the standard by using the endings in the men's names inconsistently. Remarkably, it turns out that the men's names where the *-sa* ending occurs are of non-Luwian type: Muksus or Muksas and Walmus. If the text is authentic, maybe this inconsistency resulted from the scribe's unfamiliarity with non-Luwian names, since he could not distinguish the ending from the root of the name.

4. Beyköy 1, found and published by Ramsay (1884; 1889), contains the verb *tá-ta+r-ha-* for "to continue to be victorious". This verb is characterised by reduplication for emphasis. Beyköy 2 includes a variant of the same verb in the form of *sa-ta+r-ha-* (Bk-2, § 46). Again, for a forger it would have been very foolish to use a spelling which deviates from a text that is already well known and indeed was discovered at the very same place.

Arguments for why the forger must have been an expert linguist
5. Some terms in Beyköy 2 include the laryngeal *h: ha+r-wa-na*, *HARWAN* "road" (Bk-2, §§ 12, 13). In other instances, reflexes of the same root are written without the initial *h*: *á+r-wa-na-* "to send", *á+r-wa-na-* "mission" (Bk-2, §§ 46, 48). The incidental loss of laryngeal *h* is attested for the Arzawan language as exemplified in onomastics by the royal name *Uhhazitis* < *huhha-* "grandfather" (Yakubovich 2010, 91) and can further be found in the later Luwian dialects Lydian and Lycian (Woudhuizen 2011, 413). To incorporate these variations, the forger would need to have been an experienced linguist.

6. Beyköy 2 contains a number of elements which are known from cuneiform Luwian (CL) but not thus far acknowledged for Luwian hieroglyphic.

(1) preposition *sa* "of" (Bk-2, § 48) = CL *SA* "of" (*KUB* 35.54 Vs. ii 40)
(2) preposition *ā-na*, *ā-nà* "in" (Bk-2, § 26; Ed, § 1) = CL *A-NA* "in"
(3) preposition *ī-na* "in" (Bk-2, § 33) = CL *I-NA* "in"

In addition to the examples from Beyköy 2, a case of interference with cuneiform Luwian can also be found in Köylütolu § 2. Here, the preposition *i-na* is used for the expression "on behalf of" (*i-na la+PÁRNA* for "on behalf of the great king") as was first realised by Woudhuizen (1994-95, 164-165).
In order to recognise these elements (before they were published elsewhere) and to include them in Beyköy 2, the forger would have had to be a well-trained linguist with experience in cuneiform Luwian.

7. A unique occurrence in Beyköy 2 is the use of the demonstrative pronoun *i-* ("this") written with the sign *ya* (Bk-2, § 29). This may become understandable when compared with the cuneiform Luwian *i-ya-a* ("these") as attested for a song of Istanuwa *(KUB 35.39 Rs. i 27, see Woudhuizen 2016-17, 348-349).

Note that the songs of Istanuwa, a cult place in Hapalla (= Sangarios basin), bear testimony of the Arzawan language. Thus, it so happens to be the case that, alongside *za-*, *iya-* is used as a demonstrative in the Arzawan language. So, if *ya* occurs in Beyköy 2, it is either an indication of a genuine Arzawan feature – or of the unbelievable craftsmanship of the forger.

8. The expression ***infans**^mnana-* is usually used as a kinship term ("son"). In Beyköy 2, the expression is used (§ 20), but only makes sense here as a reference to a junior official:

> (1) ***infans**^mna-na*^{UMINA} "town (of) a junior official"

The same use can indeed be found in Early Iron Age texts. Note especially the text from Cekke, in which one can find an enumeration (§§ 17-24) of places (in Loc. sg.) with their respective mayors (called "fathers") and deputies (called "sons") (Woudhuizen 2011, 200-201). Hence, in a political context, ***infans**^mna-na-* was also used as an honorific title for a junior official. – The forger would need to have been a highly-skilled linguist to make use of this expression in this particular context.

9. In Beyköy 2, in a few instances, enclitic forms of the pronoun of the 3rd person are attached to a verb later on in the phrase, even though in Luwian hieroglyphic in general they regularly occur as attached to the first element of a phrase.

(1)   *ā-la-na-ti -li* "he will covet for himself" (Bk-2, §10)
(2)   *lu-lu-ti -li* "he will placate for himself" (Bk-2, § 39)

Only after Beyköy 2 was retrieved from James Mellaart's study was this phenomenon indeed recognised in a Lydian text, where in Lyd. no. 22 as many as six instances can be observed (Woudhuizen *forthcoming.*) – the first two already being acknowledged in earlier literature (see Gusmani 1964, 89; 131):

(1-2) *mλimnś iit -λ* "the Mermnad (brotherhood) will make for Her" (Lyd. no. 22, §§ 5, 11)
(3)   *t₁ẽnt -λ* "one will take for Her" (Lyd. no. 22, § 8)
(4)   *mλimnś kaττir -ś* "the Mermnad (brotherhood) will have sacrificed for themselves" (Lyd. no. 22, § 3)
(5-6) *sivraλmis kaττir -ś* "the congregation will have sacrificed for themselves" (Lyd. no. 22, §§ 10, 12)

In the case of a falsification, this would indicate that the forger was a well-trained linguist with detailed knowledge of the Lydian language.
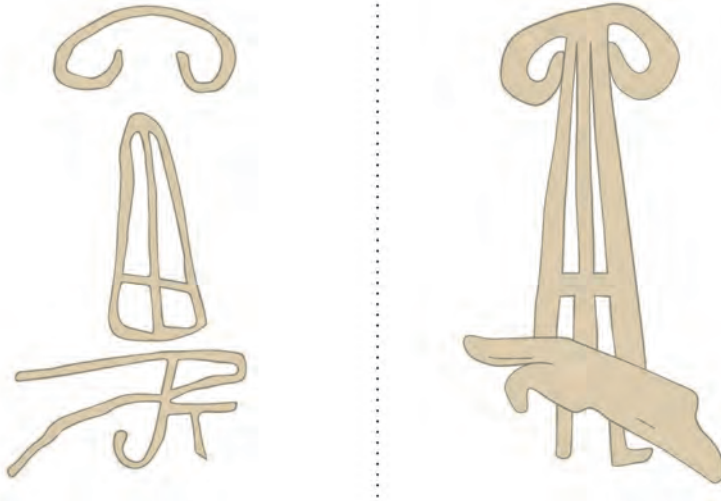
Fig. 5. Rock inscription for the term "great prince" Kupaā at Latmos (Peschlow-Bindokat/Herbordt 2001, 373, Abb. 7a) and the same expression as used in Beyköy 2.

Arguments indicating that the forger included things that were not yet known
10. Beyköy 2 contains the expression for the title of "great prince" in two distinct writing variants. Indeed, the late Annelies Kammenhuber, in her letter of 14 September 1989 to James Mellaart, cites the fact that the title "great prince" occurs in the document as an argument that it must be a falsification.

(1)   *URA+HANTAWAT+**infans**^m* "great prince" (Bk-2, § 26; Ed, § 1)
(2)   ***infans**^m ... URA+HANTAWAT* "great prince" (Bk-2, §§ 27, 39)

However, this honorific title has meanwhile been discovered in the rock reliefs at Latmos in the hinterland of Miletos (Peschlow-Bindokat/Herbordt 2001; see Fig. 5), thus providing a strong argument in favour of the authenticity of Beyköy 2.
Mellaart was unfamiliar with this title. In the translations that accompanied the drawings of Beyköy 2, the expression is wrongly translated as "X, great king, son (of) Y". This mistranslation seriously distorts Mellaart's understanding of the relationship between great king Kupantakuruntas and his henchmen or chief vassals, the great princes Muksus, Tuwatas, Piyamakuruntas, and Kulanamuwas. The latter's name is indeed even rendered as Kuwatnamuwas, a form that was outdated already in the 1980s, thereby indicating that whoever produced the translation was not up to date and not very skilled in Luwian hieroglyphic. However, this should not distract us from the main point: an allegedly forged text that was publicly presented in 1989 cannot possibly include information that did not come to light until 2001!

11. The text contains a few abbreviations – which are always hard to recognise in Luwian hieroglyphic (or ancient texts more in general). As a consequence, most of them were not identified by Mellaart or the translators of the text from whom he drew:

(1)   *ARA*, *ARA-wa* (Bk-2, §§ 23, 31, 40) = Arzawa
(2)   *AS[UW]A* (Bk-2, § 21) = Assuwa
(3-4) *MI WI* (Bk-2, § 26; Ed, § 1) = Mira and Wilusa
(5)   *MA* (Bk-2, § 39) = Masa

Mellaart translates Bk-2, § 26 wrongly by assuming that Muksus was king of a non-existent country Amiwina, whereas in reality he is presented as "great prince in (*ā-nà*) the land(s) and town(s of) Mira (and) Wilusa".

12. This argument goes back to the use of *376 – in its variants with two oblique strokes at its lower side, *377 *ī*, *zā* (both values being valid in the adjusted old reading as per Woudhuizen 2011, 89-98). Thus far this had been unattested for LBA texts. However, Willemijn Waal (2017) recently discovered *377 on a Hittite clay tablet, thereby proving that it was used earlier than previously thought. This is once again information that a possible falsifier could not have possessed.

Arguments for the authenticity of the other Luwian hieroglyphic texts from Mellaart's estate
13. The verb *warpa-* occurs in the inscription from Edremit for the expression of the meaning "to represent the crown":

(1)   *WARPA-ta$_6$* "he represented the crown" (Ed. § 2)

This term, as a verb, is recorded for the first time in the inscription from Afyon, published by Seracettin Şahin and Recai Tekoğlu in 2003. There, in § 1, it is stated that the dedicator, Tarpamaliawatas, *WARPA* "represented the crown". The expression is used in an undeclined form for the 3rd person singular of the past tense. Moreover, it is further specified that Tarpamaliawatas did so in the land of Kuwaliya. Since the stele was found in Afyon, we now know that Kuwaliya is in this region (Woudhuizen 2013, 9-12). A possible falsifier did not have this information and therefore it is unlikely that he would have used the verb *warpa-* in this sense.

14. The Edremit text also contains a form of *harnas-* "fortress" with the ending of the neuter singular *-sa,* where it is followed by an enumeration of 43 towns. This nominal inflection is obviously used here for the expression of the plural, even though the plural of "fortress" should be written with the ending *-a* in Late Bronze Luwian (e.g. *HARNAS-a*).

(1)   *HARNAS-sa* "fortresses" (Ed. § 1)

This feature is paralleled in other texts in LBA scribal tradition:

(1)   *satusaà* "belongings" (Ed, §§ 18, 22)
(2)   *TALMI²sà UMINA* "all towns" (S, § 17)
(3)   *²⁶³tánasa₅* "(type of building)s" (Ke, § 17)

One has to be a well-trained linguist to realise that the ending of the neuter singular can be used for the plural.

Major misinterpretations by Mellaart

Among the most important indications that Beyköy 2 was not a fake created by Mellaart are his utter misinterpretations of the contents. Frankly put, the Luwian hieroglyphic text tells one consistent story, but Mellaart's interpretation fails to recognise it and thus tells quite another. As Mellaart, or his literary *alter ego* Alkım, admits in the unpublished treatment discussing Beyköy 2, "linguistic detail is beyond my competence" (see Appendix 1, page 3). No matter how many lies Mellaart has produced, here he speaks the truth.

The misinterpretation of the titular expression *URA+HANTAWAT+**infans**ᵐ* "great prince" (also written as ***infans**ᵐ ... URA+HANTAWAT*) suffices to distort the whole contents of the account. Since he is unfamiliar with this title, Mellaart does not see that it stands for the ruler of a vassal petty kingdom. He thinks it represents a "great king" – and therefore ends up with a handful of great kings who were ruling in Arzawa and Mira at the same time. Mellaart assumes that the great king of Arzawa, Kulanamuwas, was succeeded by his son Muksus or Muksas, who also became great king of Arzawa (Bk-2, first part of block 17 [= our §§ 39-40]). In reality, however, there is only one great king, Kupantakuruntas, who was assisted in his undertakings by four great princes: Muksus/Muksas, Kulanamuwas, Tuwatas and Piyamakuruntas (Bk-2, §§ 26-27). Of these great princes, Muksus/Muksas is specified as the vassalruler of Wilusa, seated at Apassawa or Apaisos, and Kulanamuwas as the vassalruler of Masa (Bk-2, §§ 38-39); the realm of the other two great princes goes unspecified.

Mellaart's assumption that Kupantakuruntas is great king of Mira and Kulanamuwas likewise of Arzawa is based on a wrong interpretation of Bk-2, §§ 21-23. There it is stated that altogether six kings are headed by the great king of Mira: the vassalkings of Wilusa (in variant form *wa-lu-sa*, wrongly read by Mellaart as *lu-ka* "Lukka"), Hapalla, Urawalwas' land (wrongly read by Mellaart as Walwara) or Seha, and Assuwa, in my opinion wrongly read by Mellaart as Asaha or Seha.

§ 23   à-wa i *URA-HANTAWAT ARA-wa*   "(and) this (one [= great king of Mira] is also) the great king (of) Arzawa."

So Kupantakuruntas rules as great king both over Mira and Arzawa − two variant indications of the same country, the latter of which for historical reasons is the most prestigious. He is the only great king mentioned in the document; all others are great princes, thus rulers of vassal kingdoms. Mellaart, however, assumes a succession of the throne of Arzawa from Kulanamuwas to Muksus/Muksas on the basis of a wrong interpretation of Bk-2, §§ 29-31 and §§ 39-40. Specifically, § 29 is rendered by Mellaart as "The great king of Arzawa [now presumed to be written *19-*376-*439] sat for 40 years on his throne", whereas in reality it reads:

§ 29   à-wa *[ ] URA+HANTAWAT á-i-wa*   "[   ] I, great king, will sacrifice
        *UTNA ASATAR ya 40 *? (= weight)   (from that) land(for) a throne these
                                            40 (metal units)".

In this phrase, "(that) land" refers to the region of Ashkelon and Gaza in the Levant as mentioned in the preceding § 28.

Furthermore, block 17 [= our §§ 39-41] is interpreted by Mellaart as "And Kuwadnamuwa, my brother, the Great king became God Kuwatnamuwa and Muksa xxx ancestral throne ascended as the Great King of [the] Land Arzawa. And there brought peace/gifts/tribute the kings of the lands of Tarhuntassa, Gasga, Masa, …..". − In reality it reads:

§ 39   à-wa *KULANA-MUWA **infans**^{m}*   "Kulanamuwas, great prince (of)
        MA URA+HANTAWAT   Ma(sa), will placate for himself
        ^{MASANA}*KULANA[ ] mu-ka-sa<+r>* the god of the army (by)
        lu-lu-ti -li   invocation."

§ 40   *ASATAR [ASA]-wa ARA^{UTNA}*   "I, great king [Kupantakuruntas],
        URA+HANTAWAT   will [remain seated] (on) the throne
                                              (of) Arzawa."

§ 41   à-wa ^{MASANA}*TARHUNT-ti-sa-sa^{UMINA}* "I regularly campaigned (in) the
        UTNA AMU TIWA-TIWA   divine land (of) Tarhuntassa, I
        ka-sa-ka^{UMINA} UTNA AMU   regularly campaigned (in) the land
        TIWA-TIWA ma-sa^{UMINA} UTNA   (of) Kaska, (and so also) in the

Accordingly, Kulanamuwas did not die at all, neither did Muksus/Muksas succeed him on the throne of Arzawa. As great prince of *MA* (an abbreviation of Masa), however, Kulanamuwas *lu-lu-ti -li* "will placate for himself" ^{MASANA}*KULANA* "the god of the army" *mu-ka-sa<+r>* "(by) invocation" (cf. *mu-ki-SARA-* in Karahöyük-Elbistan § 6). Note further that in § 40 the "I" is clearly Kupantakuruntas, the dedicator of the text. The text next continues in § 41 with one of the latter's deeds featuring twice the subject *AMU* "I" and the endingless verb

TIWA-TIWA "to regularly campaign" (in the reduplicated form of *tiwa-* "to go" as attested for in Yalburt § 6), so that it is Kupantakuruntas who marched against Tarhuntassa (written with determinative *225 *UMINA*, wrongly taken by Mellaart for its look-alike *17 *HANTAWAT*), Kaska, and Masa.

In addition to the severe distortions caused by Mellaart's misinterpretation of the term "great prince", he wrongly translates a number of other phrases and thereby misses even the gist of the document.

  - In the first place he takes ha_6+*r-wa-na* "road" in Bk-2, § 12 for a geographic name, *Harziuna*, and therefore he entirely misses the point that this and the following phrase deal with the building of roads.

  - Next, he assumes that "the Great king of Mira[, I] trampled all the lands of Hattusa and I burned the land of Hatti" after which "thousands of cattle" are brought back "to Mira" as booty. In reality, the verb in § 14 reads $TIWA_2$-$TIWA_2$-*wa*, the 1st person sg. of the present/future in -*wa* of $TIWA_2$-$TIWA_2$- "to continue to provide" (as in Yalburt § 39), and the verb in § 15 reads *sá-sá-ha*, the 1st person sg. of the past tense in -*ha* of *sá-sá-* "to continuously render support" (as in Karkamis A15b, § 15 in the variant writing *sa-sa-*). Instead of war with Hatti, what really is stated is that Kupantakuruntas respects the Hittite sanctuaries in his land and up to the moment of writing had supported Hatti. The 6,000 rams mentioned in § 16 are therefore not booty, but no doubt meant to facilitate offerings in the Hittite cult places located in his realm.

  - Furthermore, Mellaart is at a total loss in connection with § 9 (he "interprets" it as follows: "And .. to the? land Mira he took (?) ...."), which quite straightforwardly reads:

§ 9   à-wa i-á ta-sa -ha mi+*r(a)*-*à*^{UTNA} *tà-ha*   "I placed these stelai also (for/in) Mira."

The following § 10 also happens to be beyond Mellaart's capabilities of interpretation. He assumes it to feature a writing variant of the MN Alantallis − in my opinion wrongly so, as we are more likely confronted here with the verb *ālana-* "to covet" (cf. Karatepe § 65: *álánaī-* "to be covetous") in 3rd pers. sg. of the present/future in -*ti*, which has the reflexive -*li* "for himself" attached to it in like manner as *luluti -li* in § 39. The second part of the phrase is ruled by the verbal form *TUZI-mi*, an endless 2nd pers. sg. of the imperative: "may you guard" (cf. Köylütolu § 6 *TUZI-mi-tu* "may they guard"). In sum, then, we arrive at the following translation:

§ 10   *à-wa ā-la-na-ti -li*   "The prince (or) representative (of)
        infans^m+*HANTAWAT URA+PARNA*   the palace (who) will covet (it) for
        infans^m *TUZI-mi wi-lu-sa^{UTNA}*   himself: may you guard Wilusa
        á URA+HANTAWAT mi[+r(a)]-à$^{[UTNA]}$ (as) the great king (of) Mira (did)!"

Finally, it deserves attention that Mellaart – and most likely even the illustrator who produced the original drawing – did not distinguish between two signs of

similar appearance. Mellaart sees the determinative for mountain, *209 *WANTI* (so certainly in §§ 24 and 31 in connection with the mountain names *à-ma-na* "Amanos" and *ti$_4$-ma-la* "Tmolos") where in a number of instances I indeed identified the negative *332 *na$_4$* for "not" without its usual legs (when not in association with a mountain name, as in §§ 8 and 33). The problem of deciding between these two options is particularly acute in Bk-2, § 50, where the sign is followed by *hu-wa-ta$_6$-nú-wa-ta$_6$* which might correspond to Mount *Huwatnuwanda* of the Lower Land. For contextual reasons, however, I am rather inclined to read *na$_4$ hu-wa-ta$_6$ nú-wa-ta$_6$* as "he (= Hittite great king Arnuwandas III of § 49) did not run into (and) renew (his hold on)", otherwise a verbal form would be missing.

Whatever the merits of this latter suggestion, what primarily concerns us here is that it is impossible that a falsifier with limited linguistic capabilities randomly wrote a series of hieroglyphs which on closer study turn out to be grammatically correct phrases.

Enhancements based on Mellaart's additional notes
While Mellaart misinterprets the phrases of Beyköy 2, the identification of geographic names he used (or made) has its merits. This became even clearer in the additional notes found during the examination of Mellaart's estate in February 2018. In this context, first of all mention should be made of his further identifications among the toponyms from the land of Arzawa as enumerated in § 31, namely: *Lapati* "Lebedos", *ā-ku-ma-na* "Kume", ^{WANTI}*ti$_4$-ma-la* "mount Tmolos", and perhaps *hu-wa-la-?-?* "Koloe". Most stunning is his identification of the islands, indicated by a determinative in form of an oval (*543 *insula*, a sign that much puzzled me; cf. Egyptian hieroglyphic N 18 [numbering according to Gardiner 1994]), in § 42. Accordingly, we can now read *ā-la-sá$^{insula-UTNA}$* "Alasiya", *a$_5$-lu-sa-$^{insula-UTNA}$* "Ialysos", *ka-pi-ta+r(a)$^{insula-UTNA}$* "Crete", *TALMI insula* "all islands".

Similarly, in §§ 25 and 28 on the naval expedition to the Levant, in the first mentioned phrase, the reading *sà-ka+r(a)-sa^{UTNA}* "the land of the Shekelesh" can be discerned, referring to a group of Sea Peoples that originated in the west, probably Sicily. This occurs alongside *pu-la-sà-ti^{UTNA}* "Philistia"; whereas in the second phrase alongside *ā-sa-ka-lú-na^{UMINA}* "Ashkelon" mention is made of *ka-za* "Gaza" (without determinative of town). The action in the latter case is also supported by *292 *HARSANTAN* "chariot(s)", presumably preceded by an odd variant of the number *399 "100".

The *pièce de résistance* is the identification of *r(a)-ā-ma-sa-sa* in § 43 as the name of Egyptian pharaoh Ramesses III (1184-1153 BC), with the somewhat awkward writing of initial *r(a)* by placing the usually enclitic *383, 2 +r(a/i)* in front of *209, 1 ā* – an ad hoc solution for rendering a sound otherwise absent in Luwian hieroglyphic. The translation of the phrase is hampered by the problematic reading of what appears to be *mi$^?$-ā-na* "my [A(m/f) sg.]" (with *210 for *ā*),

but given the context it may perhaps be suggested that Ramesses, acknowledged as the great king of the land in which Kupantakuruntas was militarily active, i.e. the southern Levant, brought gifts to Kupantakuruntas's citadel there in a similar manner as the kings enumerated in the preceding § 42.

Also, with respect of § 20 on the province of Atapali or Atipali, which Mellaart identifies as Tabal (see Appendix I, page 5), one has to admit that this identification coincides with the mention of the place-names *pàr-su-ha-na-ti* "Parsuhanda", *na-hi-ta* "Nahita (= mod. Niğde)", and *hu-pi-sa-na* "Hupisna (= Class. Kybistra)", all located in the Lower Land and the bordering region of Tyana (see Fig. 3). In this connection it is important to realise that the text dates from the 1170s BC, some 15 to 20 years after the fall of the Hittite Empire around 1190 BC, and that apparently Arzawa in the meantime had taken possession of the Lower Land and the region of Tyana as it had in the period of Tarhundaradus (*ca.* 1370-1350 BC), when it had established its frontier at *Tuwanuwa* (Class. Tyana) and *Uda* (Class. Hyde) (Bryce 2010, 146-147).

Note in this connection that in the following § 24 Hatti is referred to as a rump of the former Empire, consisting of the former provinces of Kizzuwatna and North Syria, and that its king is specified as being a hero (*á-ya*), i.e. having died.

Comparing Beyköy 2 with the Sea Peoples inscriptions in Medinet Habu
Beyköy 2 provides an account of the events in the eastern Mediterranean at the end of the Bronze Age around 1180 BC as seen from the perspective of a powerful western Anatolian ruler. Obviously, this account must be compared with the only other description we thus far possess of these events, the Sea Peoples inscriptions on the mortuary temple of Ramesses III in Medinet Habu. The gist of the two documents is the same: a coalition of naval forces from a number of petty kingdoms raided the shores of Syria and Palestine. However, discrepancies occur in some details.

In the Medinet Habu inscriptions it is said that Arzawa succumbed to the attacks by the Sea Peoples much as the Hittite Empire did – whereas according to Beyköy 2, it would have been the primary attacker (as Michael Bányai, this volume, points out):

> "As for the foreign countries, they made a conspiracy in their isles. Removed and scattered in the fray were the lands at one time. No land could stand before their arms, from Ḫatti, Kodi, Karkemis, Yereth [= Arzawa], and Yeres [= Alasiya] on, (but they were) cut off at (one time)".
> Medinet Habu, Inscription of year 8 (= 1176 BC); Edgerton/Wilson 1936, 53.

Not just Arzawa, but also Karkamis, is mentioned as a victim of the Sea Peoples' raids in the Medinet Habu inscriptions. Experts, however, tend to agree that this

kingdom's royal line continued across the transition from the Bronze to the Iron Age. After all, a continuous line of kings with Hurritic names can be reconstructed for the period of the latest phase of the Bronze Age to the earliest one of the Iron Age. Talmitesup I (*ca*. 1235-1210 BC) is regularly succeeded by his son, Kuzitesup, who reigned in the period of the upheavals of the Sea Peoples, say 1210-1175 BC. The latter was apparently succeeded by the Aritesup addressed as great king in the Luwian hieroglyphic inscription from Karahöyük-Elbistan, *ca*. 1150 BC (Woudhuizen 2003). Aritesup in turn is likely to have been directly succeeded by Initesup II, the contemporary of the Assyrian great king Tiglathpileser I (1114-1076 BC). Evidence for discontinuity in this period is only forthcoming for the coastal region of the former Karkamisian realm, which was settled by a branch of the Philistines after the smokescreen was lifted at the end of the 11th century BC thanks to the Luwian hieroglyphic text Aleppo 6 (Hawkins 2011; Woudhuizen 2015).

If there is continuity in Karkamis in the period of the Sea Peoples, so might there be in Arzawa too. All this implies that the contents of the Sea Peoples' inscriptions from Medinet Habu should be taken with a grain of salt (Zangger 1994, 28-40).
In Medinet Habu, Alasiya (Cyprus) is also listed as a victim of the Sea Peoples' raids, even though the island was particularly flourishing at the time – another discrepancy between documentary evidence and archaeological record that is completely independent of Beyköy 2. Cyprus, however, is likely to have experienced a change in regime and to have developed from a vassal of the Hittite king into a stronghold dominated by the incoming Sea Peoples, in line with Ramesses III's observation.

With respect to Arzawa, the situation may have been more complicated if we take a closer look at what we now know about its rulership. According to Beyköy 2, Arzawa is governed by great king Kupantakuruntas III and at the height of its military power during the period of the upheavals of the Sea Peoples. Nevertheless, the naval expedition to the Levant is led by the great prince Muksus/Muksas whose seat is Apassawa (Bk-2, §§ 26, 36, 38) in Wilusa, thus in the Troad. Confronted with the attack by Muksus/Muksas on his Levantine province, Ramesses III may well have ascribed this assault to one of Sea Peoples' groups, namely the Tjeker or Teukroi, who originated from the Troad (Zangger 1994, 81; Zangger 1995, 27; Woudhuizen 2006, 107-110).

Added to this is the fact that Muksus/Muksas is definitely not a Luwian but a Phrygian name, and Phrygians from the European continent are commonly assumed to have invaded Anatolia via the Troad in the period of the upheavals of the Sea Peoples. In my opinion, this is a simplification of reality, since the movements of Thracian and Phrygian population groups from the European continent to Anatolia began as early as the 16th century BC. What primarily concerns us here is the fact that the Medinet Habu monument features a captured chief of the

Sea Peoples who is specified by the ethnic *m-*ᵃ*- š3-3-k-n* (KRI V 24, line 14; cf. Redford 2007, 300), corresponding to Luwian hieroglyphic *ma-sà-ka-na* "Phrygian" (Kızıldağ 4, § 2). Accordingly, this particular chief may have been of the same ethnic affiliation as Muksus/Muksas. This may have reinforced Ramesses III's belief that the kingdom of Arzawa, which Muksus/Muksas represents, had been taken over by the Sea Peoples.

According to Mellaart, the various groups of the Sea Peoples all originated from western Anatolia, but this is most likely also an oversimplification. The Tjeker, the Lukka, the Teresh and possibly part of the Peleset indeed came from western Anatolia and the adjacent Aegean islands, but the Ekwesh and Denyen most likely originated from the Greek mainland. Part of the Peleset came from Crete, whereas the Sherden, Shekelesh and Weshesh might be traced back to Sardinia, Sicily and mainland Italy. The validity of the latter part of this statement is underlined by the fact that with respect to the handmade Barbarian Ware in the wider Aegean, Sardinian, Sicilian and mainland Italian variants can now be distinguished (Jung 2017).

Conclusions
In conclusion, Beyköy 2 provides a link between the script, material and physical appearance of the Yalburt inscription in central Asia Minor and the contents of the Medinet Habu inscriptions in Upper Egypt. Yalburt also represents a long narrow band of text (around a water basin) with its blocks reassembled in the wrong order. The grammar is similar, since both are long Bronze Age Luwian hieroglyphic texts dating to the end of the Bronze Age. Yalburt was found in 1970, but good photographs were not available until 1993, long after Beyköy 2 was publicly shown.

The complexity of the grammar, the many idiosyncrasies and, above all, the severity of Mellaart's misunderstandings of the text make it virtually impossible that he faked this document. As an archaeologist, he would not have been capable of falsifying a text of this length. Annelies Kammenhuber and Rudolf Werner thought that a student of Bossert (such as Franz Steinherr) might have produced it, but even for an expert in linguistics it would be too complicated to fake the grammar of a Bronze Age text, because so little material was (and still is) available. There are far more Iron Age documents, in which the verbs always have endings. Bronze Age Luwian hieroglyphic verbs, on the other hand, sometimes have endings, and sometimes not; Beyköy 2 strictly adheres to this principle. Above all, the text contains the title of "great prince", a term that as of 1989 was not known to have existed in Luwian hieroglyphic writing. Indeed, there is no evidence of forgery as far as the Luwian hieroglyphic texts from Mellaart's estate are concerned. The sequence of work on the documents began with a pencil drawing (most likely traced from another drawing) – just as one would expect if the text were authentic.

Although the evidence of Beyköy 2 seems at first sight to collide with that of Medinet Habu, on closer inspection the matter appears to be more complex, and a synthesis lies at hand.

All in all, we think there is enough reason to assume that the Luwian hiero-glyphic inscriptions from the Mellaart files, and particularly the Beyköy 2 text, are not falsifications but genuine discoveries. The question that remains is: how did Mellaart acquire them? And even more importantly: where are the original drawings that Mellaart saw and was able to trace? Perhaps this will always re-main a mystery.

Appendix 1

The typewritten manuscript of a scholarly treatment of Beyköy 2 below was found in James Mellaart's estate. Although certainly composed by Mellaart himself, it is unclear whom he had in mind as the author. He starts out by describing his background (non-linguist, study of Egyptology) and then ends up talking about his work in the third person as if the manuscript should be attributed to B. Alkım.

[p. 1]
The Inscription

The nineteen blocks of light grey limestone and of various length, put by side as in L. Messerschmidt's copy, amount to c. 17.55 m. of inscription and are even then incomplete. There was a completely destroyed 20th block, and there could have been more. The beginning of the text is preserved, the end is not. The inscribed field is some 0.3 m. in height and the inscription reads from left to right, like the newly discovered Yalburt (Ilgın) inscription of Tudhaliyas IV, still unpublished. Having been buried, its preservation was, barring three badly damaged blocks, ex-cellent, to which the nature of the material, semi-crystalline limestone, undoubted-ly contributed. Most of the signs are pristinely clear, but there are of course broken and worn parts, as in virtually every inscription of the Hittite Empire period, to which it must be assigned on palaeographical and historical grounds (see below). One might well wonder what caused such a unique (?) enormous text to be carved to decorate the right side of an entrance gate − to a city, a palace, or a temple? − in Western, non-Hittite Anatolia? The text, if our interpretation is correct, provides the answer: the overthrow of Hatti, i.e. the Hittite hegemony in Anatolia. This, if anything, is a victory monument, a return to independence from Hittite vassalage, as I shall tend to show.
At this point I must caution the audience that unlike my eminent colleagues, like Bossert, Meriggi, Laroche[,] I am an archaeologist, not a Hittitologist let alone an expert on Hittite Empire inscriptions. Yet, having studied Egyptology, and thus being aware of the mode of New Kingdom inscriptions, I perceive a similarity in the Beyköy (and Yalburt) inscriptions to Egyptian historiography. This consists of

the epigraphic formula of name of king, with titles and genealogy[,] followed by "what I did"; ["]the lands and cities I took, the booty I brought back[,] the cities I built (to accommodate them), the respect I paid to my Gods, and the bliss they, in recompense, created for my people and my descendants".

This particular Beyköy text seems to me very much conceived in the same style: with a minimum of grammatical expressions, where self evident ideograms would amply convey the meaning, but with a love for detail of "how many kings, lands, places, temples, gods", etc.; intended to impress the readers. With grammar reduced to a minimum, even moderately literate readers would be able to guess the message of the royal inscription. "I, PN[,] four temples built" is a sufficient signal to the reader who knows the temples, but not the name of their builder, and over the ages, such minimal information has not been questioned. Except of course by later historians. Ramses III's account of his war against the "Sea Peoples" can only be described as "Laconic", i.e. reduced to an absolute minimum of historical information. Yet on such evidence history is built.

[p. 2 is missing]

[p. 3]
The text
Without going into linguistic detail which is admittedly beyond my competence, the following interpretation of the text is offered hereby.
[summing up of blocks with interpretation of the text in phase 2 order, with blocks 20-26 on a separate unnumbered page and with omission of blocks 27-30]

[p.4-6]
Commentary
Compared to the detailed building activities of no less than fifty towns (nine in Mira, the rest in Arzawa)[,] the historical passages occupy much less space.
Kupanta-Kurunta, Great king of Mira-Kuwaliya[,] was active in Pitasa, Harziuna, Salapa, Marasa and the Marassantiya[']s riverland. He 'trampled and burnt the Land of Hatti entirely and carried off thousands and then built nine cities in the Land of Mira. Blocks 1-4, followed by block 11.
There follows a war in which six kings combined: the kings of Lukka, Hapalla, Walwara, the Seha Riverland and two great kings, of Mira and Arzawa conduct a campaign by land and sea along the Mediterranean coast which finds the Hittite king in Ura and can be followed to Byblos (Blocks 6-8[)], probably led by the king of Mira (end of block 8).
At this point the second participant comes in with the forces of Pulusati, Lasatina, Ura, Luka, Sakarasa and Lamiya, presumably led by Muksus, king of the land Amiwina, and some of Kuwatna-ziti/muwa, great king, son of Asuwati, Great king, son of Piyama-Kurunta, Great king of (almost certainly) Arzawa (Block 9)[.] After this interruption, the narrative continues with the statement

that the frontier of Egypt was reached, and Askalon and Gaza (Block 10)[.]

The rest of the inscription reveals only domestic and dynastic matters[,] the death of Kuwatna-muwa after a reign of 40 years, the accession of his son, Muksus as great king, perhaps by his uncle, Kupanta-Kurunta[,] and the tribute, gifts and greetings received on this occasion from other kings, providing us with a telling picture of the political geography of the time (Blocks 17-18).

Only one Great king is mentioned, and by name, Riamasasa (Ramses III[)], among the foreigners. All the others are merely king; whether they come from islands like Alasia (Cyprus), Iyalusa (Rhodes), Kapitara (Crete), are northern neighbours of the Arzawans, like Masa and Kaska[,] or eastern ones like Tarhuntassa, Atapaliya (in which we see the Assyrian name Tabal(u)[)], Kizzuwadna and [the] Neo-Hittite kingdom of Karkamisa (Carchemish). One expected name is missing in this list, Lukka, at the end of block 17. It may well have been there originally.

From this list the new politically situation in Anatolia is painfully evident. The two Arzawan rulers, of Arzawa and Mira[,] do not even mention their confreres in the context of a coronation − the list seems to include only foreign, non-Arzawan states of greater or lesser importance.

This calm and dignified narrative of a war that spelled the end of the Hittite Empire, seen for the first time from the other side, is to me strangely moving in its lack of exultation of victory, its absence of hatred or g??ating and the main purpose of the inscription; building for men and deities, and implied in it, for prosperity and peace. Up to now we only know the Arzawans as inveterate enemies of the Hittites, and only in the writings of the British scholar James Mellaart, can I find a sympathetic attitude towards them. The historian's task is to weigh the evidence and strike a balance; most Hittite scholars have failed to do so, and write Hittite history with an unseemly bias.

This is not right and the whole so-called Sea-People episode can now be seen to have been defective in interpretation; the Beyköy inscription is less dramatic and a near contemporary account of what happened in those fateful years in the eleven eighties B.C.

Two further items demand our attention: the historical role of the three persons mentioned in the text and the impact of the text on the study of Hittite geography. The role of Kupanta-Kurunta, brother of Kuwatna-muwa, king of Arzawa and thus uncle of Muksus[,] suggests that the Arzawan king was old or infirm at the time of the war. The young prince is however given a fair share in it after the combined armies had reached Amurru and Byblos, he may well have led the raid into Palestine up to the frontiers of Egypt, which can be dated to the eight[h] regnal year of Ramses III, c. 1175 on Kitchen's chronology.

The apparent solicitude for the young prince certainly suggests that the heir to Arzawa was being tutored by his uncle — this was an age of many old rulers and therefore political solicitude for the careers of young princes, in an otherwise Dark age. Muksus' (Muksas, Moxos, Mopsos) fame as a founder of cities "after the Trojan war" was remembered in Maeonia, Pamphylia and Cilicia and Soli-

nus records that he defeated the king of Egypt at Askelon and Pelusium at the mouth of the easternmost branch of the Nile. In the famous bilingual inscription from Karatepe, Azatiwata records his or his overlord − Awarikus/king of Adana's descent from the house of Muksas! (8th century B.C.). Our text suggests that Kizzuwadna regained its freedom with the destruction of the Hittite Empire. The Beyköy inscription gives Muksus's genealogy, but one is struck by the lack of Kupanta-Kurunta's line, which seems out of character in a royal inscription of this magnificence. I would like to suggest that there is a block missing, which might have contained this information, between blocks 1 and 2.

In this preliminary examination of the Beyköy inscription it would be premature to enter into the problems of Hittite, or better Anatolian[,] Late Bronze Age geography. My views on the subject are in basic agreement with Mellaart's reconstruction of Anatolian topography and together we have prepared the map (Fig. 3) that accompanies this article. Such a reconstruction is only possible by working from the known to the unknown, i.e. from the Hittite capital onwards. The alternative view which uses Miletus, equated with Millawanda, as a starting point and is based almost entirely on classical toponomy leads to unsatisfactory overcrowding in southwest Anatolia, which I believe to be incorrect. It is, however, possible to recognise classical equivalents for thirty of the forty names of cities in the land of Arzawa, suggesting perhaps a stronger degree of continuity from the Late Bronze Age into classical times than demonstrable elsewhere (Fig. 4 *list*). This welcome acquisition of two score names of towns or cities greatly strengthens Professors Garstang and Gurney's location of Arzawa (minor) in the area occupied by the Iron age kingdom of Lydia. The Beyköy inscription, the first Arzawan document, alas, throws no light on the controversial issue of the location of Ah[h]iyawa, Milawanda, Atriya, Iyalanda, Attarimma etc. where Hittite kings so long strove to impose their hegemony. It tends to vindicate James MacQueen and Mellaart's view that these regions are to be located north of Arzawa, south of the Sea of Marmara.

Appendix 2

The following typewritten document, also from Mellaart's estate, goes into details about the background of the rediscovered Luwian hieroglyphic texts. It is possible that Mellaart himself had discarded page 2 of the above manuscript (Appendix 1) in order to produce a second, more elaborate draft of this section – the text provided below – which then ended up covering one and a half pages.

During the nineteen sixties and seventies, a group of professors of archaeology in the University of Istanbul organised a very selective *Beyler toplantisi* who regularly met in each other's houses to discuss matters of archaeology, and ways to extend their knowledge from previously untapped evidence buried in Ottoman archives. The circle was widened to include people with archaeological interest among their forebears and who had access to the private correspondence of those dignitaries

of the Ottoman Empire, who either through service abroad or by their initiatives at home had founded Istanbul University (1868), the Ottoman Imperial Museum (1874), Galata Saray (1868), a *lise* where children of dignitaries were educated in French, and finally the Department of Antiquities, with its laws for regulating foreign excavations, aimed at preserving antiquities.

Those efforts bore fruit and Prof. Bahadır Alkım was able to find recordings of a number of previously unknown Hieroglyphic-Hittite inscriptions in northwest Anatolia, preserved in family papers, of at least six pashas.

1. Çapankale, now called Şahankaya, between Akhisar and Gördes. 1854 or thereabouts[.] Rock inscription in relief.
2. Dağardı, Eğrigözdağı. 1854 or thereabouts. idem[.]
3. Yazılıtaş, near summit of Mandira Dağı. 1854[.] idem[.]
4. Edremit Belediye bahcesı, from Mandra tepesi. 5 blocks. 1871[.]
5. Beyköy (Ihsanye), Afyon. over 30 blocks, from the hüyük. 1878. Another, 1884, W. Ramsay *Ath.Mitt.* 1889, 181 ff.
6. Demircikale, Beyköy[.] one block[.] probably 1878[.]
7. Sarıoğlan, Beyköy[.] one block. probably 1878.
8. Levke (Osmanneli)[.] one block[.] ?mention only 1870[.]
9. Çardak, Yenişehir[.] three blocks. ?mention only 1870[.]
10. Gediz mosque[.] one block. 1854 or thereabouts. 1869 recopied.
and
11. One well preserved cuneiform tablet from the hüyük of Beyköy. 1878.

As far as can be seen, Ottoman determination to safeguard and record monuments extends from *ca*. 1854 (the first year of the Crimean war) to 1878, a period of some thirty years, during which enlightened and pro-French and British ministers of state (Subhi Paşa who saved the Hama inscriptions, but failed to secure the Aleppo ones in 1874; Safvet Paşa, the friend of Sir Henry Layard, the British Ambassador and excavator of Nineve, Necib, Arif and Kadri pashas) tried to install a respect for antiquities.

Recording inscriptions was one thing, securing them for transport to the Imperial Ottoman museum in Istanbul another. It needed cooperation from the local authorities and this was rarely forthcoming. As vali (governor) of Syria, Subhi pasha had been able to [secure] the Hama inscriptions with the help of the military, hence his fury when the governor of Afyon was unable to secure the thirty blocks or so of the Beyköy inscription, which the locals[,] it is said, used in the foundation of a new mosque. The Edremit blocks likewise were despatched by boat, which is said to have sunk in or near the Dardanelles. The vali of Bursa reported that the Cardak and Levke blocks were stolen en route. The rock inscriptions are probably still there and the Beyköy tablet was saved. The Beyköy block copied by W. Ramsay is said to have disappeared and the block from Gediz, repeatedly destroyed by earthquakes, has not been retraced.

We are, of course, here dealing with a period before photography reached Turkey, outside the capital. G. Perrot is said to have taken the first photographs of

the ruins of Boğazköy in 1874 and in a copy of his and Chipiez's book, *Histoire de l'art dans l'Antiquité*, presented to Savfet Paşa he added a (bad) photograph of some of the Beyköy blocks. In the same volume were paste in drawings of the Yazılıtaş and Edremit inscriptions, copied by Subhi Sami.

When Professor B. Alkım came to England in 1976 he was prepared to give a few lectures on these new discoveries and as I had been honoured to deal with geographical aspects of these texts, I had received copies of his translations beforehand. To my knowledge he may have lectured outside London on this subject, but not in London, or at least not at the Institute of Archaeology. Bahadır's Bey's deteriorating health and his premature death in May 1981 left me with his translations of his material, but without the autographies and the philological commentary on the texts, on which he had been working.

Handan Hanim's request, shortly before her death in 1985, to ensure that Bahadır's work in this field should not be forgotten causes a dilemma, I have his translations and conclusions, but not the transcripts of the texts and the necessary textual commentary. Handan Alkım's note shows that as the Beyköy tablet text was privately distributed, my comments will be confined to historical and geographical matters, as originally agreed.

Additional note

In his contribution on the topic (this volume), Vladimir Stissi duly notes that the place-names in the inscriptions from Edremit and Yazılıtaş are all from sites without (substantial) Late Bronze Age archaeological remains. It deserves our attention in this connection that the same verdict also applies to the Lycian place-names mentioned in the Yalburt text. Instead of drawing the conclusion that these places did not exist in the Bronze Age, their mention in the Yalburt text proves the opposite and it should rather be inferred that archaeologists have been looking for their Bronze Age remains in the wrong place. By the way, nobody ventilated similar objections in connection with Linear B forms like *ra-mi-ni-ja/jo*, *i-mi-ri-jo*, *ra-pa-sa-ko*, and *pe-ri-te-u* related to the toponyms Lemnos, Imbros, Lampsakos, and Perinthos (Woudhuizen 2009: 185-192 [with refs.]).

We acknowledge Mellaart's fascination for kilim motives and weaving, which in principle may be at the background of the newly attested sign in the form of a loom. It should be noted, however, that a similar loom sign is attested in the Luwianizing Cretan hieroglyphic for early seals featuring also ships, ingots, and pots (= crucibles) which are indicative of the fact that the bulk of Minoan maritime trade in this early period consisted of metals and finished cloth, see Ulanowska 2017 and cf. Woudhuizen 2016, 121-125.

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