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WHAT IS DRAWN AND WRITTEN IS NOT NECESSARILY TRUE. CONTEXTUALISING MELLAART'S FAKES

(*Supplementum Epigraphicum Mediterraneaeum* 45)

Vladimir Stissi

The presentation, in 2017, of a monumental Luwian hieroglyphic inscription, supposedly found in Beyköy in Western Turkey in 1878, caused quite some commotion, not in the least because of the very unclear pedigree of the item. It is known only from a drawing in the estate of James Mellaart (1925-2012), who has long been known as a producer of forged drawings of (non-existing) archaeological finds. While one would have expected a cautious approach and proper research into the pedigree of the inscription before publication, there is little trace of this in the first reports and presentations of it. Even when further exploration of Mellaart's estate revealed much new evidence on his well-known forgeries (and also some lesser known ones), no proper research was done on 'Beyköy 2'.

In this article, the pedigree of the inscription as presented by James Mellaart will be explored – and shown to be spurious. Also, the inscription itself, or rather Mellaart's drawing of it, will be investigated, in combination with the events it appears to describe. Perhaps unsurprisingly, my conclusion is that 'Beyköy 2' is at best highly problematic as an ancient inscription, but perfectly fits various recurring patterns in Mellaart's known forgeries. The 'inscription' is a very good illustration of his way of working and thinking, and seems relevant as evidence for the psychology of a pathological serial forger rather than as an ancient artefact.

Introduction

When first confronted with the news about the hitherto unknown copy of a monumental Late Bronze Age text, supposedly containing a historical narrative which seems incompatible with current archaeological and historical evidence, I could only think of two truisms: *things which look too good to be true, usually are too good to be true* and *extraordinary claims need extraordinary evidence.* The latter seemed particularly apt, as evidence for the text is limited to a 20th century supposed copy of a 19th century drawing of a disappeared original, without a documented provenance. In an era which has brought us several notorious cases centring on unprovenanced documents then revealed to be faked, one would expect scholars to be very cautious when presenting such texts – all the

more so when the sensational new document is found in the estate of a scholar, James Mellaart, who has been known for decades to have produced a series of fraudulent claims, almost all based on drawn supposed copies of originals which are supposedly no longer around.

I was therefore quite surprised, even shocked, when the text, now called 'Beyköy 2' was presented to *Talanta*, with some accompanying material, but without any serious research into provenance and find contexts as presented by Mellaart, in an article which explicitly dismisses his – amply documented – previous frauds, even labelling 'accusations' against him as 'unconvincing'[1]. In my view, such a problematic perspective on a document which was by itself already problematic should not have been published. I still think the case deserved a more balanced and better researched treatment, but after long discussion in the editorial board I accepted the publication as it has appeared online, and promised to present my reservations in an article of my own, as part of a *Talanta* issue mostly dedicated to the new find, and presenting views by various specialists.

To be honest, I soon regretted my commitment. My irritation with the matter grew considerably when Eberhard Zangger, after concluding through further research into the papers in Mellaart's estate that the latter had indeed been producing a series of frauds, presented this as a big surprise and major news[2]. Even though finding and publishing these papers is an important new contribution to the history of archaeology in Turkey, I can only find it utterly shameless that someone who first consciously dismisses already well documented and broadly known and accepted conclusions, then suggests that these very same conclusions are a major new breakthrough coming out through the papers he found.

From there, however, things only got worse. As can be seen in Zangger's contribution elsewhere in this volume, some of the papers in the Mellaart estate clearly indicate that at least part of documentation regarding 'Beyköy 2' and the history of its study are forged. While one would expect it to be a basic academic principle to dismiss archaeological findings only available through recent copies produced by someone who clearly was a serial fraud, particularly when the reality of these findings is not supported by any external evidence and at least part of their pedigree is forged[3], the presentation of 'Beyköy 2' has not been retracted or modified. Indeed, instead of looking further into the documentation provided by Mellaart, or searching for archival or archaeological evidence supporting Mellaart's claims, Zangger has produced a rather 'whitewashed' account (elsewhere in this volume) of Mellaart's fraudulent behaviour which omits important facts and evidence, only to

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[1] Zangger/Woudhuizen 2017, 43.

[2] See mainly: <https://luwianstudies.org/james-mellaart-forged-documents-throughout-life> and <https://www.livescience.com/61989-famed-archaeologist-created-fakes.html?utm_source=notification>.

[3] Ironically, already in the 1950s it was precisely this principle which has stopped publication of the 'Dorak treasure', another likely hoax by Mellaart (Pearson/Connor 1967, 20, 59-60). The principle is also endorsed by Schachermeyr 1959-1960, 232, one of the first reviews of the material presented by Mellaart 1959 (in a non-academic journal).

conclude – without offering any supporting evidence or even new information – that Beyköy 2, and some other problematic items, are still to be taken as serious evidence – simply because there supposedly is nothing proving they are frauds[4]. In my view, the border between non-conformistic and critical research and a pseudo-scientific approach conditioned by a priori beliefs has been crossed here. I have seriously considered resigning from the *Talanta* board and retracting my promise.

Yet, you are reading my article here. In the end, I decided to write a critical review of all the relevant evidence, as I promised, for two reasons. On the one hand, I feel that, in this era of 'fake news' and continuous attacks on academia and scientific research, as seen in the antivax-movement and the prominence of dubious websites and documentaries, it is a task of scholars to expose problematic and pseudo-scientific findings. On the other hand, one could argue that while there may be no proof for the reality of 'Beyköy 2' and its companions, also very little or no proof for their being fakes has been put forward in scholarly publications yet. While it would be easy to sit back and relax confidently, as very few scholars seem to take 'Beyköy 2' seriously, and it generally seems to be included with Mellaart's phantasies, without any supporting evidence and argumentation this actually is not much better than blindly continuing to believe in its reality. Moreover, fakes which are not dismissed conclusively and effectively can have a very disruptive 'afterlife'[5].

Fortunately, my task has been relatively easy, as there is plenty of relevant material which quite consistently points in a single direction. In the following, I will first offer a review of existing evidence and studies on Mellaart as a forger, and then move into the possible historical contexts of 'Beyköy 2' and its companions, starting with the story of their discovery as presented by Mellaart, and continuing with an evaluation of their potential archaeological contexts. Then I will have a look at the 'Beyköy 2' text (and the way it is documented) itself, to consider possible internal evidence of it being a forgery. Finally, after evaluating all the evidence, I will reflect on my conclusions in the light of what we know of Mellaart as a possibly pathological serial forger.

Mellaart as forger

Although there has been ample coverage of Mellaart's fakes since the 1960s, including a book, parts of another book and some very thorough articles on specific cases, and the issue is discussed in most obituaries, sometimes quite prominently, there is no synthetic academic study and almost all of the relevant research is done by journalists and non-academic experts[6]. The overview pro-

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[4] Interestingly, in an interview published on a website directed by him, Zangger appears to be much more open to the possibility 'Beyköy 2' is a fake: <https://luwianstudies.org/interview-eberhard-zangger-spiegel-article-wizen-balloon/>.

[5] See Thompson 2019 for a recent general overview and Mallett 1990; 1992 for a more specific case regarding James Mellaart.

[6] Schachermeyr 1959; Pearson/Connor 1967; Muscarella 1988, 397-398, note 5; 2000, 140-143 ; Mallett 1990; 1992; Balter 2006, 43-54; Mazur 2005a; 2005b; 2005c; Hodder 2015, particularly 414-416.

vided by Zangger in this volume may be regarded as a first attempt to repair this, but although it benefits from the discovery of much archival material, it misses relevant cases and evidence and strongly leans on well-known problematic source material which is largely based on evidence provided by Mellaart himself, mostly in interviews. Moreover, Zangger has a tendency to downplay the more damaging cases and evidence, and to side with Mellaart whenever there is the slightest possibility to do so, also by presenting some rather biased views on clearly damning material. While critical on some points, and offering interesting insights in the psychology of the case, the overall view is far from fair and balanced, and hampers a good evaluation of those cases, like Beyköy, where documentary evidence does not seem to provide sufficient answers – at least at first sight.

Usually, the story of Mellaart's forgeries starts with the so-called Dorak treasure, first published in 1959, but a recent Dutch publication about some events in 1944-1945 may suggest we should perhaps look further back[7]. On the one hand, it is now clear from archival documents that Mellaart indeed worked in the Egyptian department of the Rijksmuseum van Oudheden (the Dutch national archaeological museum) for a while from June 1944, as reported by various biographical texts (including Zangger in this volume), based on stories told by Mellaart himself[8]. On the other hand, several of the details offered by Mellaart seem to romanticize things quite a bit. His 'escape' to Leiden does not seem to have been a more or less spontaneous run to evade being captured by the Germans because of his double (Dutch-English) nationality, as in Mellaart's version. It was apparently not the English nationality, but the German 'Arbeitseinsatz', the obligation for adult Dutch males (except those with an exemption because they were essential locally) to work in Germany which prompted Mellaart to leave the family home. And the move was well planned: a job in the Leiden museum was secured through a recommendation of a friend and colleague of his father, who was a well-to-do art dealer, and a letter by Mellaart himself which mentioned so many archaeologists and museum experts that it apparently completely baffled the Leiden museum director, W.D. van Wijngaarden, who then did his best to create a position for this 18-year old untrained youngster he did not know. The name dropping does remind one of the stories surrounding some of the later forgeries, and one wonders whether young Jimmy Mellaart, perhaps with some help from his father, exaggerated things a bit. There moreover is no mention of an intervention of the Swiss consul, who was the one securing his position in Leiden according to Mellaart's own stories[9]. Further research in the Leiden museum archives may yield interesting insights here.

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[7] Verhart/Pauts 2016.

[8] Pearson/Connor 1967, 29-30; Balter 2006, 12-13; Hodder 2015, 411. Burney 2012a, 11 has a less romantic, more factual summary; 2012b is slightly more dramatic on details. See also Mellaart himself in Mellaart 1992-1993, 81.

[9] Pearson/Connor 1967, 29-30. Balter 2006, 13 and Burney 2012a, 11 and 2012b, iii report Mellaart's father's help in securing his Leiden job.

This also holds for the following period[10]: according to himself and later staff lore, Mellaart used the museum to go underground, not only working there but also living there, to hide from German eyes. This did not last long, however, as Mellaart returned to the family home in the south of the Netherlands the 2nd of September 1944, when the country was in turmoil because of the fast approach of the Allies, and many thought liberation was imminent. While the west remained in German hands till next spring, Mellaart arrived home just in time to be liberated by the Americans ten days later. After the liberation, he returned to Leiden to start studying Egyptology and Classics and agreed on termination of his contract at the museum from October 1st 1944. But then, six months later, he tried in vain to reverse this agreement and be paid till the end of May 1945 at a committee instituted to resolve issues caused by the occupation circumstances. This caused much irritation in the museum, and may have affected Mellaart's career as a student, since museum staff was closely connected to the Egyptology and Classics department. In the end, Mellaart continued his studies, Egyptology and Ancient History, at University College London from the autumn of 1947[11]. In view of later events, a look in the case files of the contract dispute may provide relevant insights in Mellaart's ways of reasoning.

The 'Dorak Treasure'

The next and most controversial event in Mellaart's series of problematic discoveries regards the so-called 'Dorak Treasure'. Although details in Mellaart's accounts vary considerably[12], his stories suggest that a lady of Greek descent he met in a train in the summer of 1958, Anna Papastrati, introduced him to a series of spectacular finds and accompanying documentation, resulting of an illegal excavation near the town of Dorak in the Bursa area, during the troubled period of the Graeco-Turkish war of 1919-1922. Mellaart was then allowed to study and draw the material at Papastrati's home, but could not photograph it. Neither the treasure nor Papastrati were ever seen again, although in October 1958 a letter allegedly written by her, permitting publication of the material, arrived at the British Archaeological Institute in Ankara. Even though Mellaart and others spent much time in preparing a substantial publication, this never appeared, apparently because Seton Lloyd, then director of the British Institute, thought it unwise to publish untraceable material from an illicit excavation[13]. In November 1959, Mellaart did nevertheless publish a sensationalizing and richly illustrated article on the 'Dorak Treasure' in the *Illustrated London News*[14]. This

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[10] The account in this paragraph is based on Verhart/Pauts 2016, 19.

[11] Pearson/Connor 1967, 30; Mellaart 1992-1993, 81; Hodder 2015, 411.

[12] See e.g. Pearson/Connor 1967, 20-21, 34-37, 52, 58-60, 104-106, 145-147, 150-151 (with explicit discussion of differing version on 58-60, 150-151); Muscarella 1988, 398, note 5; Mallett 1990; 1992; Mazur 2005a; 2005b; 2005c; Balter 2006, 45; Hodder 2015, 414-415; Zangger, this volume.

[13] Pearson/Connor 1967, 20, 59-60.

[14] Mellaart 1959.

caused some concern with the Turkish authorities, and a police investigation was started. This led to nothing, even after the Turkish press picked up the story in 1962, leading to new investigations.

A book mostly dedicated to the case, now labelled as 'Dorak Affair'[15], by two journalists of *The Times* and an official investigation by the Ankara institute in 1968[16] did not manage to bring up any substantial new information on what had happened. Both came to the conclusion that an innocent and naïve Mellaart was somehow tricked by a group of smugglers trying to whitewash some of their stock before selling it. The journalists, Patricia Connor and Kenneth Pearson, did nevertheless explicitly suggest a part of the treasure, including the more spectacular items, may consist of fakes[17]. The Ankara institute's report left that matter open: it merely concluded that Mellaart really saw the items he catalogued, leaving out any qualifications of the objects themselves.

Although the case remained much discussed, new evidence surfaced only in 2005, when investigative journalist Suzan Mazur discovered that the letter supposedly written by Anna Papastrati – the only piece of material evidence in the case – was very probably produced by Mellaart and/or his wife, possibly on the Ankara institute's typewriter[18]. Later in the same year, she interviewed David Stronach, Mellaart's main collaborator on the never published Dorak treasure book, then a Berkeley professor. Although part of the wording is somewhat ambiguous, it is quite clear that Stronach considers the Dorak items to be Mellaart's inventions[19]. This has not fully convinced everyone, however, as can be deduced from the ambiguous treatment of the case by Zangger in this volume. Perhaps further study of the publication's typescript, which has resurfaced in the Mellaart estate during Zangger's investigations, could settle the matter.

Meanwhile, there are a few aspects which seem to have been overlooked in existing evaluations of the case. One is the persona of Anna Papastrati. She is presented as member of a Greek family from Izmir. This is quite unlikely. Very few Greeks remained in Izmir after the burning down of the city in 1922 and the subsequent forced population exchange between Greece and Turkey – a recent survey (of unclear reliability, but unlikely to be very far off) mentions 8 families. Even now, and including consular staff, the Greek population of the city is less than 200[20]. While it is perhaps conceivable that Anna Papastrati belonged to one of the very few remaining Greek families in Izmir, it is unthinkable that the local police would not be able to trace or even prove the existence of such a person, also considering the very difficult position of the Greek minority in Turkey at the time (with major pogroms in Istanbul in 1955). Any Greek, and particularly the well-off families, would have been registered as such somewhere, and would have had special police

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[15] Pearson/Connor 1967.
[16] See Daniel 1970, 89-90.
[17] Pearson/Connor 1967, 174.
[18] Mazur 2005a.
[19] Mazur 2005c.
[20] Figures from: <http://omogeneia-turkey.com/en/id/ar/today-GreekMinorityofTR.pdf>.

attention. Furthermore, one would not expect Turkish police in the late 1950s to miss an opportunity to victimize a Greek family.

Things don't stop there. The Dorak treasure is supposed to have been unearthed during the Greek rule of the area. However, Dorak is outside the area of Western Turkey originally occupied by Greece in May 1919. It was conquered only in July 1920, but remained close to the front line till the Greek advance in the summer of 1921, which never produced a stable control of the area till the Turkish reconquest of summer 1922. While this may have been ideal for grave robbing, this was hardly a period and an area offering possibilities for elaborate clandestine excavations – Mellaart indicates the dig was very precisely documented, even supervised by an archaeologist[21]. These two very unlikely situations already arouse suspicion. Moreover, the Dorak area never had a Greek speaking population at the time – even according to the Greek maps as the one produced by Soteriadis in 1918, this was an ethnically Turkish area[22]. Although not completely impossible, it is difficult to imagine Greeks unofficially excavating in an area they did not know well and which was presumably hostile to them[23]. It should also be noted that Smyrna/Izmir was taken by the Turkish armies in a very quick campaign, before they headed further north, which seems to exclude a last minute evacuation of the material from Dorak to Smyrna/Izmir. The least impossible scenario is perhaps that materials from a Greek excavation in 1920-1921 ended up in Turkish hands afterwards and were then handed over to Anna Papastrati or her relatives much later again.

A final aspect to consider is the police investigation into the case. It has repeatedly been stressed that Mellaart was never convicted, but as far as this is relevant at all, it should be noted that apparently the case was closed in the wake of general amnesty, and not because Mellaart was proven innocent[24]. It should also be taken into account that the investigation was not related to forgery, but to illegal excavation and smuggling[25]. In other words, if Mellaart's account of the Dorak finds was truthful, and he did assume he handled illegally excavated items, acquittal would have implied the Turkish police thought the documentation and the objects were faked, rather than Mellaart being completely innocent.

Unfortunately, the only evidence we have for the police investigation are Mellaart's own words and the reports of interviews with various involved officers and informants by Pearson and Connor[26]. These do not offer a very clear account, but two things seem straightforward. First of all, the Turkish police was

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[21] Pearson/Connor 1967, 35; Muscarella 1988, 398, note 5.

[22] See <https://upload.wikimedia.org/wikipedia/commons/d/d6/Hellenism_in_the_Near_East_1918.jpg>.

[23] Apparently, excavation notes were in Greek. It would in any case be even more problematic if finds and notes produced by ethnic Turks of this area would have ended up with a Greek family in faraway Izmir.

[24] Pearson/Connor 1967, 50, 133-134.

[25] Pearson/Connor 1967, 95-96, 129-134, particularly 96.

[26] Pearson/Connor 1967, 95-102, 106, 129-134. Some of this was previously published in newspaper articles. Balter 2006, 43-54 seems strongly based on Pearson/Connor 1967.

not able to find any trace of the 'Dorak Treasure' or Anna Papastrati, and did not even find any evidence either had ever existed. Second, the account by Pearson and Connor strongly suggests that the original investigation after the first publication of the 'treasure' in 1959 was not very thorough and was soon stopped, and a follow up after a sensationalist campaign in the Turkish press in 1962 did not have a high priority either[27]. Otherwise it also seems difficult to explain why the police was so cooperative, showing the (thin) folder of case files, contacting the journalists with potential informers and not stopping them going after an obvious dead end, a supposed visit of Mellaart to Dorak in or around 1956[28] – all hardly the behaviour one expects of the Turkish police if they were still investigating a multi-million dollar smuggling ring which caught much press attention. The amnesty blocking persecution of Mellaart cannot have been the only reason for this relaxed attitude, as this affected only foreigners and would not have extended to local accomplices[29]. While it is perhaps possible the police did not want to find involved locals, the outcry in the press, the general acuteness of the problem of archaeological looting in this period[30], and the fallout of the case among archaeological authorities – who eventually banned Mellaart and found a reason to block further work at Çatalhöyük – do not make this a very convincing explanation. Similarly, from a 2019 perspective, Pearson and Connor's presentation of the Turkish police as lazy, sloppy and slippery seems very much coloured by colonialist and orientalist prejudices. In my view, it is far more likely that the police soon came to the conclusion that their case was a hoax, which they, however, could not publicize as such for political reasons, that is, precisely because smuggling and the British archaeological presence were delicate issues.

In any case, rather than exonerating Mellaart, the stalled and/or failed police investigation suggests that no evidence supporting his story could be found, and there was no indication for looting and smuggling. Not only the police, but also Pearson and Connor were not able to trace Papastrati, although they did a thorough search[31]. Also considering all the above, it seems most likely that the whole Papastrati story is an invention. In view of Mellaart's tendency to link his fantasies to known names and items that ring a bell with part of his audiences (like Beyköy or Perrot), it may not be coincidental that 'Papastratos' was a major tobacco firm and cigarette brand in Greece in the 1950s and 1960s (and later)[32].

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[27] Pearson/Connor 1967, 96, 129-134.

[28] See Pearson/Connor 1967, 124-135, 147-159.

[29] See Pearson/Connor 1967, 50, 133-134, who seem to miss the implications.

[30] As acknowledged by Pearson and Connor 1967, who dedicate a large part of their book to this, and note the prominence of the theme in some press reports on Mellaart (181-182).

[31] Pearson/Connor 1967, 95-102.

[32] Interestingly, according to Pearson/Connor 1967, 102, 130-131, local informants in Izmir remember that a Papastrati family involved in tobacco trade lived in the town before the expulsion of the Greeks in 1922. The Papastratos family is not from Smyrna/Izmir. Did people perhaps remember the brand? As there were more than 100.000 Greek inhabitants till 1922, and many Turkish inhabitants were newcomers, it would seem odd that one family was re-

Even though the background story now seems very problematic at best, all this does not necessarily imply the Dorak treasure itself was a fake. It may not be very likely, but it is theoretically possible that Mellaart's story is a cover up for something even more dodgy, which the police did not manage or did not want to uncover, like a set-up by looters. After all, this was a possibility Mellaart himself stuck to from the late 1960s onwards, albeit in combination with the Papastrati-story[33]. In this case, it remains hard to explain that none of the objects depicted or described by Mellaart have ever surfaced in the art market[34]; neither have any been returned to the Turkish authorities, or have photographs or other reliable documentation ever appeared. Moreover, despite decades of intensive archaeological research, all items and features in the graves which were unique in 1959 remain so, no similar 'royal' graves of the period have been found, and there still is no other evidence for substantial luxury, let alone royal, trade between the Aegean and Egypt during this period. It is not surprising, therefore, that the 'Dorak' objects play no role in the (art) history of the period anymore, and are not mentioned in any recent overviews or handbooks. As often remarked, a final verdict on this possible find may not be possible without the actual objects – assuming these exist. Still, the published drawings should offer some important clues, and deserve further study, particularly taking into consideration David Stronach's claim that the daggers of the treasure were based on tracings of items in Turkish museum by Mellaart himself[35]. The many parallels suggested by him in the text of the *Illustrated London News* publication may also offer some clues[36]. Even as a non-specialist, I can add that the boats incised on one of the most remarkable finds, a silver dagger, look very much like well-known Bronze Age boats illustrated on various objects, like Early Cycladic 'frying pans', Middle and Late Minoan Seals and Middle and Late Helladic pottery[37]. This is not necessarily surprising, but one particularly striking parallel is offered by a (largely reconstructed) Middle Helladic depiction found in Palaia Volos (one of the candidates for ancient Iolkos). It can hardly be coincidental that this item was prominently illustrated in the first 1958 issue of *Archaeology Magazine*, just before the appearance of the 'Dorak Treasure' (Figs. 1-2) and was later mentioned by Mellaart himself as a possible parallel[38]. One can, of course, also wonder what a sword full of ships is doing in a site 30 km from the sea.

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membered so well. Pearson and Connor do not seem to be aware that pre-1922 Smyrna was a predominantly Greek city, and its Greek population was forced to move out (or worse).

[33] Pearson/Connor 1967, 43, 172-175. See Mazur 2005a; 2005b; Burney 2012a, 11; 2012b, iii; Hodder 2015, 415; and Zangger, this volume.

[34] See Mazur 2005b, citing an ancient artefact dealer.

[35] Mazur 2005c.

[36] As already explored by Schachermeyr 1959-1960, 230-232, leading to strong doubts about some of the objects being genuine. See also Muscarella 1988, 397-398, n. 5.

[37] See e.g. overviews in Gray 1974 and Wachsmann 1981, particularly 204-214.

[38] Theocharis 1958, 15, 17-18; Mellaart 1966, 170, Fig. 53, 9; the item is also listed in Gray 1974, G16, cat. no. B22, G43, Abb. 8c, G44, where she actually sees a connection to the Dorak treasure dagger, which she also lists on G29, cat. no AA 5 and G38-40, G39 Abb. 5a-c, G80-81. While Gray notes existing doubts about the Dorak finds and contexts, she cites

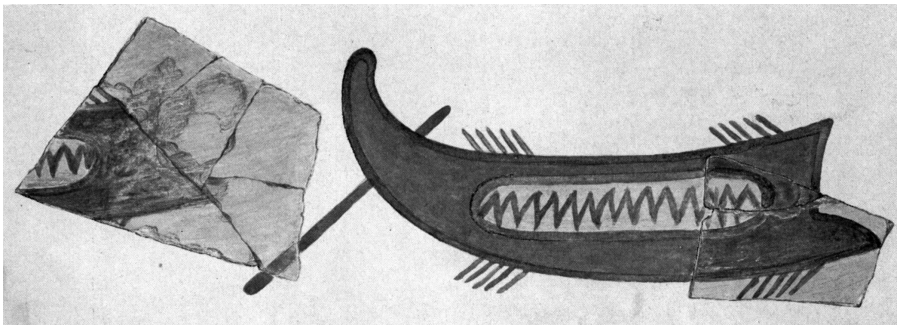

Fig. 1. Drawing by D.R. Theocharis of two sets of Middle Bronze Age sherds with additional reconstruction. The drawing appears to be the direct source of inspiration for at least some of the ships on fig. 2 (Theocharis 1958, 15).

The find location, far from the sea and with no prominent site nearby, is also a problem for the supposed remains of an Egyptian throne, which handily offers a dating for the whole treasure, in the form of a cartouche of a fairly well-known Old Kingdom pharaoh, Sahure. This happens to be the pharaoh at the end of whose reign the first reported expedition to the near-mythical land of Punt took place. More generally, Sahure seems to have been proud of his overseas expeditions bringing in lots of exotic products and metals, mainly from the Levant, as these are elaborately depicted in his pyramid complex. He would therefore seem an obvious candidate for having traded with the Aegean area – if there would have been any evidence for royal Egyptian trade with the area in this period, and/or a depiction in the pyramid reliefs. Quod non. Several inscriptions contain Sahure's cartouche; it is also part of the texts on a royal seal, kept in the Walters Art Gallery in Boston[39]. It would not have been difficult for Mellaart, who had studied Egyptology, to reproduce the cartouche and add some bits of relevant text[40].

All in all, I think it is safe to conclude explicitly what usually seems to have been assumed implicitly: not only the stories surrounding it, but also the 'Dorak treasure' itself are entirely invented by Mellaart. He apparently was willing to spend considerable amounts of work and time on it, also involving others in the work, and to

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similarity with a ship on the Phaistos Disc (her B23) and Early Minoan seals as supporting the dagger to be genuine and does not think the Cycladic frying pans or early Egyptian depictions could have been sources of inspiration of a fake. I can add that the drawing of three ships from the dagger in Mellaart 1966, 170, Fig. 53.1-3 (and reproduced by Gray) shows slight differences with the relevant parts of the complete view in Mellaart 1959, 754, fig. 2. This was already noted by Humphries 1977, 354. Another case of variation between drawings of the same 'Dorak' item is noted in Mallett 1990.

[39] Inv. number 57.1748, see <https://art.thewalters.org/detail/12274/royal-seal-of-king-sahure/>. More relevant objects can be found at <https://www.ucl.ac.uk/museums-static/digitalegypt/chronology/kingsahure.html>.

[40] Schachermeyr 1959-1960, 232 notes Sahure's cartouche text differs from the ones already known, which could also suggest it is a fake.

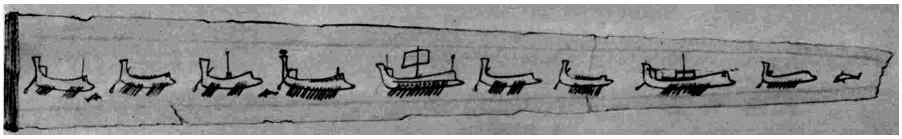

Fig. 2. Drawing of a sword, supposedly made of silver and found in a grave near Dorak in Western Turkey (Mellaart 1959, 754, fig. 2).

risk his credibility. Furthermore, it should be noted that parts of the discovery story, Papastrati's letter and some of the objects, are quite easily recognisable as being problematic. Clearly, Mellaart was not a very good forger, or did not care much about details, perhaps also because he counted on his status protecting him – which it mostly did, at least initially.

Rugs, wall paintings and more

Several of the key elements of the 'Dorak fantasy' returned in the next prominent Mellaart fraud, in the 1980s and early 1990s, when he connected previously unknown wall paintings from Çatalhöyük with much later Kilim rugs. Again, the case centered on drawings made by Mellaart of lost and irretrievable (in this case supposedly decayed) originals, and again Mellaart appears to have spent very much time on a book, also involving others. This time, the book was published, though not in an academic setting[41]. Despite some initial success, mainly in the world of rug collectors, again few people, and hardly any scholars, were impressed by the rather obvious fakes and wild theories derived from it. Just as with Dorak, it also did not help that there are many contradictions between different reports on the same material[42]. Of course, also more generally the limited power of persuasion of Mellaart's fakes is something already notable in the Dorak case.

A new phenomenon was, however, that Mellaart's claims were soon strongly refuted by rug specialists and archaeologists, who brought forward that his theories were incompatible with what was known of textile production technology, but also noted that the new paintings and their supposed find spots did not fit the excavation data of Çatalhöyük as published by Mellaart himself – quite a few of the paintings were even supposed to come from rooms where he had explicitly mentioned a different wall finish[43]. Also new was that Mellaart reacted, partly by introducing new fakes, including drawings of supposed tablets (previously unrecorded, and decayed since) showing the 'impossible' weaving equipment and motifs recurring on the 'wall paintings'[44]. As can be expected, these were not well received either[45]. There were also some other crucial differences between this case and the Dorak

[41] Mellaart *et alii* 1989; the book was preceded by several lectures (the first one in 1983) and articles, including Mellaart 1984b.

[42] See Mallett 1990; 1992; and Zangger, this volume.

[43] Mallett 1990; 1992; Collon 1990; Eiland 1990; Voight 1991; Lamberg-Karlovsky 1992.

[44] Mellaart 1991; see also Mallett 1992; Lamberg-Karlovsky 1992.

[45] See Mallett 1992.

one: there was no complicated, fancy discovery story, the new paintings simply appeared out of Mellaart's personal archive. While this may have been intended to avoid the trouble caused by the Papastrati story, it of course backfired when the discrepancies with the published excavation reports were noted, and Mellaart gave various, partly contradictory explanations for the absence of any photographic documentation[46]. Furthermore, although he presented a few papers on the matter, all in relatively obscure conferences on rugs and similar textiles, and he presented his materials during a lecture at University College London, his academic home, Mellaart does not seem to have tried very hard to impress, let alone convince, the broader archaeological academic world this time[47]. It is not clear whether he had given up beforehand, or after the apparently fierce debate after his 1987 University College London lecture. Perhaps he had hoped to reach academic archaeology by first convincing the rug specialists, and eventually decided to remain silent when already this did not work out.

There is one final detail that needs attention here: among the problematic items Mellaart presents in his self-defence in 1990, he also mentions and illustrates incised pebbles, supposedly found at Beldibi beach, and up to 12.500 years old. According to one, unreferenced, report these items were presented to the Turkish archaeological authorities, who refused to accept them because they were considered fakes[48]. This seems to undermine Zangger's claim, elsewhere in this journal, that 'there is no evidence at all' that 'Mellaart also forged artefacts' (other than documents). In fact, it seems clear that at least some of the incised drawings on stone which Zangger found in the Mellaart estate and describes as 'sketches' are actually these very pebbles. One candidate is even visible in the centre of Zangger's Figure 3, among pieces of slate or similar stone with drawings reminiscent of the fake Çatalhöyük mural paintings – this appears to be the left pebble on the second row of the set of drawings shown by Martha Mallett[49].

A varied collection of inscriptions

Mellaart had not left academia completely aside, though. From the early 1990s onward, he started referring to a number of previously unknown texts which offered much detail on Hittite and Western Anatolian history: a letter from Ashurbanipal to the king of Arda in Lydia, the so-called Beyköy text on a series of bronze tablets, and a monumental inscription on stone in Luwian hieroglyphic

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[46] See Mallett 1992; Zangger, this volume.

[47] As noted by Mallett 1992. Mellaart 1984b; 1990; 1991 are all in conference proceedings or non-academic journals. Interestingly, in Mellaart 1980, a review article on kilims in an academic journal, the only slight hint to a long ancient history mentions an 'ancestry of at least two thousand years, and probably much more'; Mellaart 1984a, a similar article in the same journal, is even more silent on ancient origins; see further Muscarella 2000, 141-143; Mazur 2005a; Hodder 2015, 415-416.

[48] Mazur 2005b; see Mellaart 1990; Mallett 1992; Muscarella 2014, 51.

[49] Mallett 1992, taken from Mellaart 1990, 29.

also from Beyköy[50]. Besides these, his estate contains copies of supposed inscriptions which Mellaart never mentioned in press: two more smaller text fragments said to be from Beyköy, a longer text supposedly from Edremit (on the Aegean coast northeast of Lesbos), one said to be recorded from a rock on a hilltop near Yazılıtaş (not far from Pergamon), and a text supposedly from Dağardı (south of Bursa), which was also given as the findspot for a shorter fragment; a final fragment was supposed to come from Şahankaya (a place I cannot locate)[51]. All these are accompanied by notes on their findspots and in some cases the histories of recording and their disappearance – only the Yazılıtaş inscription is still supposed to be in situ, where it has never been spotted by anyone, however. The elements of the stories surrounding these texts, presented in some detail by Zangger elsewhere in this volume, will by now be familiar: no originals seem to survive[52], and often several, contradictory, versions of their 'biographies' are available, which usually do not look very plausible and cannot be verified. Two new elements are that the discovery stories are placed far back in time (in the mid-late 19th century) and that a series of famous scholars is connected to unpublished studies of the inscriptions, which either have left no trace or have been found in the Mellaart archive – and were clearly produced by Mellaart himself. Needless to say, none of the scholars brought into the story were still alive when Mellaart referred to his new inscriptions, and with one partial exception[53], there is no positive evidence that they even knew about the material. Just as with his previous forgeries, the amount of effort Mellaart has spent in producing both the actual material and the supporting documents and stories is impressive.

Even more strikingly than in previous cases, however, the amount of energy and creativity Mellaart invested did not lead to convincing results. It probably does not come as a surprise to the reader that his references to several of these unpublished inscriptions, and a few conference papers which presented some of the material, were not received very positively by the academic world[54]. Although the phrasing is sometimes (not always) very diplomatic, Mellaart was immediately and very directly accused of inventing the items himself by prominent colleagues – a sharp contrast to the mostly much more subdued criticism, and sometimes even positive reception, of the Dorak treasure, and the almost complete silence among academic archaeologists with regard to the 'additional' Çatalhöyük

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[50] Mellaart 1991-1992; 1992-1993; 1993.

[51] These inscriptions are all discussed in Zangger/Woudhuizen 2017, 46-52; see also Zangger this volume.

[52] As said, the potential exception, the text from Yazılıtaş, has not been traced.

[53] Oliver Gurney gave a lecture on 'Beyköy 2' in 1989, but the text was kept out of the conference publication; see Zangger, this volume.

[54] Mellaart 1991-1992, which led to a fierce discussion (Mellaart 1992-1993; James and Kokkinos 1992-1993; Lewis 1992-1993), after which the journal editors (Gibson 1993, 82) distanced themselves from Mellaart and declared the inscriptions he cites as invalid sources. Likewise, David Lewis (Lewis 1992-1993) concludes an inquiry into Goetze's papers for evidence of his involvement (referred to by Mellaart) with an ominous 'What is written is not necessarily true'.

paintings and related items in the 1980s and 1990s. The fierce attacks possibly explain why Mellaart eventually never published any of the inscriptions, and postponed already (privately) announced publications several times.

The ten Bronze Age texts found in the Mellaart archive clearly form a coherent set, with much overlap and many connections in their content, and shared or connected stories about their discovery and study. They also clearly belong within a single, very problematic, historical framework for Western Anatolia during the last centuries of the Bronze Age, and were used as such by Mellaart. Both as a set and individually, they tick all or almost of all of the boxes of the typical Mellaart-hoax as listed by Zangger elsewhere in this volume – a point I will return to below.

Nevertheless, Zangger and Woudhuizen have now come to the conclusion that some are fakes and others should be considered as real, at least as long as there is no evidence to the contrary. The distinction seems mainly based on the presence or absence within the Mellaart papers of direct evidence for forging, in the form of preparatory documents and draft versions. I find this a strange reasoning, both from a general methodological point of view, and when considering only the specific case. As I have already indicated in my introduction, I think it is a priori unacceptable to consider an item which only exists as a (supposed) copy, and whose existence is not confirmed in any way by external evidence and is even doubtful in view of circumstantial evidence, as a serious historical source. Moreover, it is methodologically unsound (because it is circular) to assume that because an item is supposed to be correct (in terms of making, style and content etc.) we can take it to be real – any successful fake works precisely because it is considered to be what it is not.

In this specific case, we can note that the texts which can be proven to be forgeries (because there is documentary proof) do not stand out in any way from those which are not 'betrayed' by other papers. Meanwhile, there is quite some circumstantial evidence beyond the actual documents: while citing a graphologist and psychoanalyst (as Zangger does, elsewhere in this volume) may not be very relevant academically[55], it is completely clear that Mellaart was a pathological serial forger, and also that those of his texts which have circulated previously were almost universally considered to be fakes by the leading experts in the field. It seems entirely self-evident to me that in such a case, and in absence of any external evidence supporting the documents in question to be genuine, the default assumption should be that the monumental Beyköy inscription and its companions are fake.

Having said that, as stated in the introduction above, the position taken by Woudhuizen en Zangger is also a stimulus to look further into the matter, and to explore the circumstantial and even direct evidence that has a bearing on the case. Indeed, a

[55] Graphology (at least as a method of psychological or psychiatric research) is generally considered to be a pseudo-science, and psycho-analysis is not part of mainstream academic psychiatry anymore in most parts of the world.

closer look at the documents themselves and the stories which Mellaart connected to them offers several additional indications supporting my position. In my view, the evidence clearly shows that the inscriptions which are not directly recognisable as fakes, share more problematical aspects with the certain fakes than Zangger and Woudhuizen suggest, and even offer direct evidence of being forgeries.

Perrot in Turkey

Since Zangger's research has now clearly shown that Mellaart's stories regarding the study of the monumental Beyköy inscription by Prof. Bahadır Alkım are fabricated, and that the supporting documentation was produced by Mellaart himself, I can here focus on the discovery story and original setting of the stones, which do not seem to have been seriously checked yet. Below, I will go into the archaeological context, but before that, there is the historical background: Mellaart claims the inscription was first documented by the eminent French scholar Georges Perrot in 1878, on invitation of the Turkish authorities. While this may seem perfectly reasonable at first sight, there is no supporting documentary evidence for this story, which also describes the retrieval and documentation, during the same trip, of most of the other inscriptions from Western Turkey documented in Mellaart's archives which I have already mentioned above. Leaving aside the rather erratic combination of visited locations, there does not seem to be any evidence Perrot was in Turkey in 1878 and/or knew about any of the inscriptions he is supposed to have documented – none of which are known to have survived, and some of which seem to have disappeared under rather suspicious circumstances, according to Mellaart's stories.

Of course, one could always assume Perrot's trip to Turkey in 1878 has somehow been overlooked, but there is a series of reasons why this seems to be very unlikely. To start with, a series of biographical notes published after Perrot's death in 1914[56] offer very detailed documentation of an impressive amount of travels, including a long trip to Turkey in 1861-1862, but nothing about a 1878 trip. Even if the mission to Beyköy may have been hidden in some way for political reasons, Mellaart states Perrot was already in Turkey when he was involved by the Turkish authorities[57]. There is no reason why the non-official part of his

[56] Reinach 1914; Maspero 1915; de Lasteyrie/Collignon 1916; see also <https://www.aibl.fr/membres/academiciens-depuis-1663/article/perrot-georges?lang=fr> and <https://www.inha.fr/fr/ressources/publications/publications-numeriques/dictionnaire-critique-des-historiens-de-l-art/perrot-gorges.html> which offer additional references to literature and archival material, including unpublished travel reports by Perrot.

[57] Zangger/Woudhuizen 2017, 14. It should be noted that Mellaart consistently mentions the 'Department of Antiquities' as the relevant authority, which did not yet exist as such in this period. Archaeology was handled by the Ministry of Public Instruction and what would become the Topkapi Museum (see e.g., for the general background and specific cases, Uslu 2015, particularly 53-57, 63-87, 99-101, 123-130). More generally, his grasp of institutions in Istanbul during this period and the ways they operated and developed seems limited. This is perhaps not surprising, but Mellaart often seems to have reminded interviewers that his father in law had a significant position in the late Ottoman government circles.

voyage would have been omitted from his biographies, also because the trip as a whole must have led to an absence of several months from Paris – which is very problematic in itself, because 1878 was a very busy year for Perrot, who was teaching at the École Normale Superieure, but must also have spent time on his upcoming professorship at the Sorbonne (starting in early 1879) and his contributions to the first volumes of the monumental *Histoire de l'Art Ancien*, which started appearing in 1882, but took several years of preparation[58].

The window for a trip to Turkey in 1878 must have been very limited in any case, as the Ottoman Empire was at war with Russia till March, with Russian troops almost reaching Istanbul. A final peace settlement was reached only in July. Even in the relatively safe surroundings of Troy, Schliemann could only work in autumn, protected by armed guards[59]. In these circumstances, it is hardly conceivable that a European traveller would have reached Istanbul before August or even September, and then started a major trip into remote inland territories – where winter sometimes starts from the end of October – and with long stretches over minor sea routes. Several articles written and published in Paris in summer 1878 also place Perrot in Paris, or at least nearby, during this period[60].

Apart from the practical (near) impossibility of a 1878 Perrot trip into the Turkish interior, there is another important issue: there is not a single reference to such a journey or any of the resulting finds in Perrot's publications. This is completely out of character, as Perrot always promptly published extensive reports of this travelling, and was very actively discussing all new finds in Turkey, with a special interest in the pre-Classical cultures of Anatolia. Many of the resulting insights were, moreover, subsequently published in the relevant chapters in the *Histoire de l'Art Ancien*. Even if some kind of secrecy was arranged in 1878 – which is unlikely and unparalleled in itself – it is unconceivable that Perrot would have remained silent about one of the most spectacular finds in his lifetime in a field which was one of his main specializations. More specifically, I cannot accept the possibility that Perrot would have left out even a passing or vague reference to the monumental Beyköy inscription or the smaller inscriptions he supposedly documented in the same trip in the 40 page review of all available knowledge about the Hittite language and scripts he published in the *Revue de Deux Mondes* in 1886[61]. The article includes an exhaustive overview of the early scholarship and of known finds, including items (re-)discovered by Perrot himself during his 1861 visit, but does not mention any archaeological

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[58] See Cagnat 1914, 461; Reinach 1914, 123-124; Maspero 1915, 473-475, de Lasteyrie/Collignon 1916, IX, note 1; and the following footnote 60.

[59] Uslu 2015, 75-76. Schliemann started excavating the 30th of September.

[60] Perrot 1878, published in July 1878, refers to several publications dating to May (61, note 9) and June 1878 (62, note 14) which do not seem to have been easily and quickly accessible in Istanbul, let alone inland Turkey. Both the second 1878 and the first 1879 issue of *Revue Archéologique* contain reviews of recent books by Perrot which must have been written in late 1878 or early 1879.

[61] Perrot 1886.

remains in the Beyköy area, even though Hittites are connected to Troy, Phrygia, Lycia and Lydia. Likewise, the relevant chapters in the fifth volume of the *Histoire de l'Art Ancien* (published in 1892) do include a short reference to the fragmentary hieroglyphic inscription found near Beyköy, first published in 1889, but do not mention any of the texts supposedly tracked down and studied by Perrot himself[62]. This is more than 'utterly inexplicable' (as suggested by Zangger elsewhere in this volume) – it simply proves that Perrot was not aware of any 'new' monumental Hittite or related inscription in Southwestern Turkey.

This leaves one final document to discuss: a small note in French accompanying the file in Mellaart's archive on one of the smaller inscriptions supposedly documented by Perrot directly after he went to Beyköy[63]. Zangger and Woudhuizen cite this note, which describes the hieroglyphs as 'en hièroglyphique Egyptiens(?)' [sic] to suggest that Perrot may have had some difficulties in recognizing Hittite/Luwian hieroglyphic. This, however, is very unlikely: in the just cited 1886 article he explicitly notes that [Hittite/Luwian hieroglyphs] 'se distinguent à première vue des hiéroglyphes égyptiens; un œil exercé ne s'y trompera pas'[64]. Of course, 1886 is not 1878, but the 1886 article refers to an 1882 study which already discusses Hittite writing[65], and which in turn refers to signs he already documented and studied during and just after his 1861 trip – during which Perrot spent much time studying Boğazköy, Yazilikaya and other major Hittite sites and monuments. There is no doubt that Perrot was already very much 'un œil exercé' in 1878[66].

The problems with the note go deeper, though. As far as I can see, the four short lines contain seven spelling/grammar errors and two parts which are simply incomprehensible. The errors regard basic incongruencies of gender and singular/plural (like in the quote above), and would be very odd for a highly educated native speaker, like Perrot. They would, however, nicely fit a writer less at home in French with a background in a language where such congruencies are much less relevant – like English or Dutch. Combined with the improbable content just discussed, the obvious conclusion is that the 'French' note was composed by Mellaart and is just one more mystification.

The archaeological context

Even if there is no evidence that Perrot was in Beyköy in 1878, and it seems impossible or at best very unlikely that he recorded a major inscription there, there still is a theoretical possibility that someone else found and/or recorded the

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[62] Ramsay 1889, 181; Perrot/Chipiez 1892, 79; see also further below in this article.

[63] Zangger/Woudhuizen 2017, 46 mention that the inscription is said to have been found in 1854, and suggest the note was written then, but the 1854 date is mentioned in the text and indicates this was written afterwards; in fact their attribution of the note to Perrot (who cannot have been in Turkey at the time, and is not placed there by anyone) seems incompatible with this early dating.

[64] Perrot 1886, 314.

[65] Perrot 1882.

[66] On the Hittite expertise of Perrot from 1861 onwards, see already Menant 1887, 90, 93. It can be added that Perrot also wrote several articles on Egyptian archaeology, and must have had at least basic knowledge of Egyptian hieroglyphs.

monumental remains, and Mellaart just faked the discovery story – a scenario now also left open by Zangger (see elsewhere in this volume). Unfortunately, researching this possibility yields a familiar pattern of dead ends, inconclusive information, but also some very relevant negative evidence.

The problems start right away. Mellaart mentions that 'peasants in the hamlet of Beyköy'[67] found the series of inscribed blocks and reported them. However, Beyköy is a fairly recent settlement which has no significant archaeological remains, and the surrounding fields are equally empty. It seems impossible the inscription is from Beyköy itself or its immediate surroundings. The nearest archaeological sites are at some distance from the village: burial mounds, a small tell and a large amount of rock-cut graves, including some monumental ones[68]. Most of these are not directly on agriculturally used land – shepherds rather than peasants would be the ones being in touch with ancient remains. Many of the sites were, and some still are, full of blocks, including plenty of decorated and inscribed ones. Apparently, interest of local farmers in these remains is limited. It seems therefore very unlikely that they would have recognized blocks with hieroglyphic writing (which was only 'discovered' as such in 1862) as something exceptional, and then, with a disastrous war ongoing, also would have found the means to raise the interest of the distant Istanbul authorities in their finds in a remote province, which at that time was not known for its archaeological remains at all.

Indeed, the first documented archaeological exploration of the area of Beyköy took place only in November 1881, when William Ramsay first visited this part of Turkey[69]. Apparently, he saw some potential: Ramsay returned in August 1884, to document Phrygian graves and other remains he had discovered during his first visit in more detail. During this second visit, local farmers showed Ramsay an odd stone in an artificial hill (probably the nearby tell or perhaps a burial mound, this is not entirely clear). He then dug out the stone, which turned out to be inscribed – in what we now know to be Luwian hieroglyphic. This quite spectacular find was quickly published in Ramsay's report, and has remained well known among specialists[70]. One of the scholars who immediately saw its importance, was Perrot[71] – the two were among those eagerly discussing all new finds from Turkey, and were clearly well aware of each other's work[72].

Ramsay's publications of his work in Phrygia in the 1880s show that he knew the area around Beyköy very well and thoroughly explored it, also by following up what local informants told him. It is unlikely anyone knew the archaeology of the area better than Ramsay at the time. Yet, he never mentions the finding of a monumental Hittite inscription, or even stories hinting in that direction. It seems

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[67] Zangger/Woudhuizen 2017, 14.
[68] Haspels 1971, 288.
[69] Ramsay 1888, 352-353, 363, 372.
[70] Ramsay 1889, 181-182.
[71] Perrot 1892, 79.
[72] See e.g. Ramsay 1888, 353, note 4, 374, 381; 1889, 171-174, 179.

extremely unlikely that such a major find in the area just three years before his visit would have escaped his notice (and/or that of his informers) completely. Unless one wishes to believe in a major cover up, it is moreover inconceivable that neither Ramsay nor (as discussed above) Perrot mentioned such an inscription, or a visit of the area by Perrot, in their intensive discussions of finds in Anatolia, their chronology and their connection to Hittite culture, which was a major focus of interest for both.

Such a cover-up, moreover, would not have been limited to just Ramsay and Perrot. After Ramsay's first visits, other archaeologists have continued to explore the area. They soon noted that the hieroglyphic inscription Ramsay had found and drawn in 1884 could not be found anymore, but did document some of his other finds in more detail. None mentioned a second lost inscription (or an early visit of Perrot to the area). The last of these 'traditional' explorers of the area was Emilie Haspels, assisted by Jaap Hemelrijk, who very meticulously recorded and drew the mainly Phrygian remains in the 1950s. In her monumental publication *The Highlands of Phrygia*, Haspels mentions that the small tell near Beyköy has many Bronze Age artefacts on its surface, but also notes that the inscription found by Ramsay is the only trace of Hittite presence in the area. She suggests the item might not be local – apparently implying (though not stating explicitly) it was a *spolion* brought there during the Phrygian period[73]. It is evident that Haspels, who spent very much time with the local population, but also had good contacts with prominent Turkish archaeologists in Istanbul, was not aware of any major hieroglyphic inscription from Beyköy.

The inscription found by Ramsay finally received a brief but thorough modern publication in 1980[74]. This offers no new information on the findspot and, needless to say, no reference to a second hieroglyphic inscription from Beyköy. In the 1970s and 1980s, the area of Beyköy was thoroughly investigated again, by the Turkish archaeologist Hatice Gonnet-Bağana. She explored the region, excavated large parts of the *necropoleis* and tested the tell and some funerary mounds. Besides regular (preliminary) publication, much of the primary documentation of Gonnet-Bağana's work, about 3000 photographs and drawings, is accessible online[75].

Particularly the visual documentation is not only interesting from a purely archaeological view. 1970s photographs of the village show a very small town, without a large mosque, mostly consisting of houses in unworked stone (including some *spolia*, separately documented by Gonnet-Bağana) directly on bedrock. There does not seem to be any place where large blocks forming a 29 meter monumental inscription can have disappeared in a foundation, as in the story presented by Mellaart. Moreover, even the smallest traceable bit of inscription would have

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[73] Haspels 1971, 288.

[74] Masson 1980.

[75] See Gonnet 1981 and her archive material at <http://digitalcollections.library.ku.edu.tr/cdm/search/searchterm/Beyköy/field/all/mode/all/ conn/and>.

been spotted and documented by Gonnet-Bağana. It is also interesting to note that the surroundings of the village were still full of *spolia* and ancient worked blocks till at least into the 1980s – thus there seem to have been little reason to carry the inscribed blocks from further away to use them in a foundation in the 1870s.
As to the tell, which, being the only site with substantial bronze age remains, seems to be the most likely potential findspot for a major Hittite inscription, the archival documentation brings forward some serious issues. First of all, with a diameter of *ca.* 150 m, the tell appears to be rather small for a 29 meter inscription: not only would this, or a building or structure it would have been part of, hardly fit, but the tell also offers no indication of the presence of monumental Bronze Age architecture, which could be expected as a proper context for such an inscription. One possibility might be that such remains are still very deep down, but that would make one wonder how the inscription (but nothing relatable) made it to the surface, through a clearly Phrygian top layer. As seen, a similar issue may play with Ramsay's inscription, but this is a single, much smaller fragment which has come up accidentally, perhaps because of Phrygian reuse, and not a large and fairly complete set of monumental blocks.
Of course, we could also assume the hieroglyphic inscription is not from the tell but from another nearby site. But which one then? The funerary mounds are again too small and too late, just as the rock cut monuments of the area. It seems, the only feasible possibility would have been the presence of a single, separate structure somewhere in the plains, which must have disappeared without a trace and without being remembered between 1878 and 1881. Considering the amount of visible monumental surface remains in the area, this does not appear to be a likely scenario. Indeed, Hatice Gonnet-Bağana herself has made it very clear that she sees no serious possibility that an inscription as presented on the basis of Mellaart's notes has been found in Beyköy or the surrounding area, and so, according to her, his story (and the inscription) must be a hoax[76].

All this leads to several interconnected conclusions: first of all, it is very unlikely, even inconceivable, that a monumental Hittite inscription was found in Beyköy or its surroundings, and/or ended up there hidden in some foundations. There is no evidence connecting the inscription to the site, there is no site offering a potential, let alone suitable, find spot, and there are no remains in the area which could form a fitting setting for such an extraordinary monument. Mellaart's account of the reuse of the inscribed blocks also seems incompatible with the situation at the village as documented by 20th century reports and photographs.
All this also has implications for a second issue the (lacking) archaeological context brings forward: Not only is there no place for the inscription itself in the Beyköy area, in view of the archaeology of the region it is also impossible to fit

[76] <https://www.haberler.com/prof-dr-bagana-kazi-yapilacaksa-camide-degil-10133242-haberi/> (in Turkish, but even with an automatic translation the frustration, disbelief and anger appears very clearly).

in the historical interpretation as presented by Woudhuizen and Zangger, since there simply is no archaeological evidence for any major or even more than marginally minor role of the site or another place in the area as a Luwian centre in the unstable West-Anatolian margins of the Hittite empire. The tell is far too small for that, and there is nothing else in the surroundings fitting a place of more than very local importance. There moreover is nothing in the wider surroundings suggesting the presence of a significant larger political entity in the area in the 13th-12th centuries BC. Absence of evidence is not necessarily evidence of absence, but some things are extremely unlikely to disappear completely, particularly in an area where preservation of archaeological sites appears to have been fairly good, at least till recently.

Finally, it can be noted that the inscription found and published by Ramsay appears to be a very good candidate for having served as a source of inspiration for a Mellaart hoax. While on the one hand the site would have been vaguely familiar to many as a source of an important early inscription (especially after the 1980 re-publication), on the other hand available publication and documentation about it was limited and not easily accessible till recently, allowing a lot of freedom in inventing a history and context for the supposed find. More generally, the use of a starting point which looks solid and controllable, but starts falling apart even from basic fact checking seems to fit Mellaart's hoaxes very well, starting with the 'Dorak treasure.'

The material itself

A fake discovery story and an impossible find spot still do not entirely exclude a genuine inscription, not even in the collection of serial forger. It is time to look at the item itself. First of all, it may be worthwhile to look at the preserved document[77], supposedly a copy by Mellaart of a 19th century original, before we come to the text. According to Zangger and Woudhuizen, these 'transcripts corresponded clearly to the pre-1900 style transmitted, for instance, in the *Corpus Inscriptionum Hettiticarum* by Leopold Messerschmidt in 1900'[78]. I do not agree. Mellaart's drawings are much more refined and detailed than Messerschmidt's, and more elegant in style. This seems partly related to the limitations in the printing technology Messerschmidt had to work with: the type of engraving used required rather thick lines and avoidance of detail, while Mellaart worked in a tradition based on photographic reproduction techniques, which allowed colour, shading and refined detail. Mellaart's drawings also seem rooted in the elegant Art Deco-based artistic style of the 1940s and 1950s, which is also visible in the drawings of the 'Dorak Treasure' (mostly not by his hand, it seems). All this, however, misses an important point: Messerschmidt's drawing style is not a rel-

[77] The image as reproduced by Zangger/Woudhuizen 2017 (12-13, fig. 1) and in many press releases apparently shows a scan of one of two copies supposedly made by Mellaart. Unfortunately, the pencil sketch which seems to have preceded this well known ink version has not been published, to my knowledge.

[78] Messerschmidt 1900, passim.

evant point of reference, as it is too late. Drawings made by Perrot in 1878 would have looked even simpler and more sketchy, more like Ramsay's reproduction of the Beyköy text in his publication of it[79] – or some of the illustrations in Perrot's *Histoire de l'Art Ancien* or his articles[80].

One could argue that all this is not relevant either, since a copy by Mellaart does not need to have looked exactly like a 19th century original. Yet, Mellaart's drawing is almost photographic in its precision, both in the depiction of the blocks (including damage) and that of the signs, and it seems quite unlikely that such precision could have been based on preliminary drawings or sketches needed to make Ramsay-style illustrations. To complicate matters further, Perrot and others did sometimes offer very precise illustrations of inscriptions, but these were based on photographs – which are explicitly excluded as a source by Mellaart[81]. Although I would not consider this very strong evidence, the style of Mellaart's drawings indicates to me they are not based on 19th century originals.

This slowly leads us to the actual inscription. A tell-tale sign of forged inscriptions is often that breaks and lacunae are aligned in such a way that the remaining text fits nicely, or even that the text seems (partly) written around or in between damaged parts. A notorious recent case is offered by the papyrus snippet showing a bit of 'The Gospel of Jesus' Wife'[82]. Although one may argue that most of the damaged areas seem to follow the vertical columns of signs a bit too well, the monumental Beyköy inscription (as illustrated in Zangger/Woudhuizen 2017, Fig. 1) is not notably clumsy in this respect. Yet, in a few places, like near the right edge of 13 (26), and on 11 (11), 9 (3) and 28, the hieroglyphs seem to be arranged and/or squeezed in in such a way that they fit the damaged areas. On 3 (21) moreover, the left damaged area seems too wide for a single column of signs, but too narrow for two columns; likewise, the damaged area towards the right edge on 25 (18) seems too narrow for a column, but is much wider than the usual margins. 6 (24) is also quite odd regarding columnation, just as the left part of 15 (7), and much of 30. 15 (7) also differs from most other blocks, which have generally quite wide margins on their sides, by the squeezing of the rightmost column, with one sign touching the right edge. The same can be seen in 17 (9). 19 (12) has a sign that crosses the seam between blocks, the only one in the whole inscription. A feature like this could of course be ascribed to ancient clumsiness, but that cannot be said of what can be seen at the damaged areas. Viewed together, the indications are suggestive, but perhaps not conclusive.

The sketch of the inscription supposed to come from Edremit is more problematic, as some of the damaged areas are even drawn in separate columns, and most

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[79] Ramsay 1889, 181-182.

[80] See e.g. Perrot 1882, Pl. XXIV.

[81] See Zangger, this volume. This is actually another problematic aspect in Mellaart's story, since Perrot was a pioneer in the use of photographic documentation. He, more than most contemporaries, can be expected to have taken a camera with him on a mission aiming to record important inscriptions.

[82] See for a discussion on this forgery, e.g., Depuydt 2014; Sabar 2016.

indeed align very well with the lay out of the text[83]. The shorter inscriptions do not offer much in this context, partly also because they show less damage; the Dağardı-inscriptions are much messier than the others in their layout, but this could of course go back to the ancient writers rather than the modern sketch[84]. All in all, again there is no conclusive evidence, although I would say there are enough reasons for worrying.

This slowly brings us to the signs themselves. As I do not have any expertise on Luwian hieroglyphs, I cannot say much here, but I do want to note two issues which made me wonder. Firstly, it seems odd to me that a new inscription, which is the longest Luwian Hieroglyphic text known, and offers a substantial quantitative addition (around 30%, according to Zangger and Woudhuizen)[85] to the existing corpus, offers just four new signs to the around 500 already known. It would be very useful to explore whether this is indeed as odd as it seems. A comparison with the absolute and relative amount of *unica* or signs firstly discovered on some other long inscriptions, or a more general statistical analysis of the frequencies of signs on all inscriptions, could offer very relevant information here, and should be tried as soon as possible.

Pending that, it seems quite remarkable that two of the new signs, representing a loom and a ship, clearly relate to earlier forgeries by Mellaart. The loom played an important role in the discussion around supposed Neolithic rugs, and then appeared among the fake objects Mellaart claimed to have found in Çatalhöyük[86]. Likewise, even though it does not look the same (but rather like ships on Minoan seals)[87], the ship sign immediately reminded me of the silver Dorak sword with a row of ships. Moreover, the ship connects with two other new signs, the silver weight and the gift bearer, which all three relate to trade and international connections, which are central themes in Mellaart's research and also a crucial part of some of his wilder ideas – like those the Dorak treasure was supposed to support. Frankly, it is too good to be true that all previously unknown signs in this inscription align perfectly with Mellaart's research agenda. It is, moreover, worrying that the concepts expressed by these signs in the text were apparently not needed and/or expressed by other signs or different ways of phrasing in previously known Luwian hieroglyphic texts. The 'new' signs appear to indicate that either the content or the language and writing of this text, or both, are exceptional. It is hard to escape the conclusion that this is a reflection of Mellaart's mind, perhaps in combination with his limited grasp of Luwian hieroglyphic. As to the latter: in the translation of Woudhuizen and Zangger, the boat sign is once

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[83] See Zangger/Woudhuizen 2017, 48, Fig. 3.

[84] See Zangger/Woudhuizen 2017, 49, Fig. 5.

[85] Zangger/Woudhuizen 2017, 36-37.

[86] See Mallett 1990.

[87] See Gray 1974, G41, Abb. 6, G43, Abb. 8, G45, Abb. 9; Wachsmann 1981, 204, fig. 19, 208, fig. 24. As there is no concordance between the transcription and the block numbers in the illustration of the inscription, and the transcription has no clear line division, I was only able to trace the boat sign on the image.

(in their § 28) translated as 'ship' and once (in § 25, where two of the signs are combined) as fleet, in both case within quite unclear phrases[88]. It is almost as if the new sign is forced in and does not fit naturally.

The inscription: its content

This brings me to the content of the text. From its appearance, it has been noted by many, already in some of the first reactions in the social media, that a text consisting mainly of long lists of geographical names, partly embedded in formulaic phrasing, is unlike any other Luwian hieroglyphic text and, above all, relatively easy to forge because limited knowledge of the language is needed – even less so because most of the place names have no parallels. The fact that this pattern is repeated in the smaller previously unknown inscriptions connected with the monumental one by Mellaart, makes the situation even more suspicious. Why would a single expedition in the 19th century record eight inscriptions with the same, otherwise unseen type of text, which moreover partly overlap or connect in their content, have very similar dates and regard the same main characters, even though the find spots are very far apart? Reality does not work that way.

Again, as a non-specialist I cannot say much about the content of the inscriptions and the narrative they present. Yet, I cannot refrain from noting a few problematic aspects. First of all, the recurring reference to the location where the inscription itself seems to have belonged (§6, 16, 20 in the division given by Woudhuizen and Zangger)[89] is problematic, since, as I argued above, there is no trace of a relevant archaeological site in the Beyköy area. Particularly the reference to a fortification is impossible in this context, as there are no traces of a Bronze Age fortified site in the area, and precisely these tend to remain recognizable. In any case, as I also already indicated above, it is highly unlikely that 'Beyköy 2' (and 'Beyköy 3' and '4', for that matter) were found and/or originally placed in Beyköy or its immediate surroundings. Considering this situation, we must either conclude that the inscription is a fake (because it cannot refer to a place which does not seem to exist) or that it refers to an original location which is unknown to us. In that case, at least part of the geography as reconstructed by Woudhuizen and Zangger holds no water.

There are several more puzzling geographical features, however. In the order of the text: § 1-10 relate to events and towns in North-western Anatolia, particularly Troy. I do not see why this had to be presented, in a prominent position, in a monumental inscription in Beyköy (or another location in this general area), a generation or more after it happened. Beyköy is a relatively minor place, outside the actual kingdom of the protagonists of the text, almost 500 km from Troy. This simply does not make sense[90].

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[88] Zangger/Woudhuizen 2017, 24-25.

[89] Zangger/Woudhuizen 2017, 22-24 and comment on 30, 32, 34.

[90] The same holds for the mentioning of offerings taking place in Apaisos in the Troad in § 36 and 38. In both lines, however, the toponym is inserted by translators (Zangger/Woudhuizen 2017, 26), as it is not preserved in the original text - § 38 does not even have any meaningful text preserved at all.

§ 24-25 appear to list the towns controlled by the Hittite king and by the king of Mira in Southern Anatolia and the Levant. Neither the division nor the order the towns are listed in makes much sense. The Hittites are connected to Tarsus and Adana in Cilicia and much of the Northern Levant, which is fair enough, but then Mira controls (mentioned in this rather odd order) Perge (in Pamphylia), Philistia (Southern Levant) and Ura and Lamiya which are harbours in Cilicia – not far from Tarsus and Adana[91]. Even leaving aside the discussion whether there really was a Philistia, and if so when, it seems improbable that the Western Anatolian kingdom of Mira included two harbours in Hittite-controlled Cilicia and a bit of the Levant in the middle of the Egyptian sphere of influence, far south of the Hittite-controlled part of what is now Syria and Lebanon. In §28 then, Ashkelon is fortified by Mira's 'princes', under unclear circumstances – the inscription is very incomplete here, and the translation offered by Woudhuizen and Zangger offers a lot of detail and interpretation which I fail to see in the preserved text[92]. Whether one wishes to believe the mainly conjectural story produced by Woudhuizen and Zangger or not, even a minimalist reading of the inscriptions requires belief in a very implausible political entity (a Western Anatolian inland kingdom, expanding to the Aegean, but also controlling loose patches of coastline in Pamphylia, Cilicia and the Southern Levant), and a logistically unfeasible situation (conquering and keeping a Levantine outpost in Egyptian-controlled territory over sea from more than 1000 km away) without any archaeological or textual support – the Southern Levant remained under direct or indirect Egyptian control well into the 12th century and while 'Philistine' culture may have important Aegean components (of hotly debated origin and significance), there is no trace of Luwian or more generally Western Anatolian presence in this very well researched area, which is also covered fairly well by some Egyptian texts.

The historical context implied by the text and constructed more explicitly in the translation, suffers from several more problems and inconsistencies. Firstly, the general timeline is problematic. Zangger and Woudhuizen date the inscription after the downfall of the Hittite empire (which they place *ca.* 1190), 1190-1180[93]. This is based on a rather early dating of the end of the Hittite empire[94], which is more usually placed around 1180 or even a few years later – which would move the inscription to 1180-1170. Such a later dating would also at least allow the appearance of Philistia in the inscription – however problematic in many ways: it seems impossible to assume the existence of this state before Ramesses III's

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[91] Zangger/Woudhuizen 2017, 24, 34-35 suggest that the Hittite situation as described lies in the past, as the king is labelled as a 'hero', but this seems problematic: why would the king of Mira relate his own realm to one which does not exist anymore? Moreover, the patchy presence of Mira along the coastline would not make much sense if the formerly Hittite cities in between were not part of the empire anymore. This issue returns in § 49-50 and will be discussed further below.

[92] Zangger/Woudhuizen 2017, 25, 35.

[93] Zangger/Woudhuizen 2017, 10, 18-19.

[94] This early dating is also promoted by Zangger elsewhere, but has very little support among academic scholars, and is unconvincing in my view as well.

defeat of the Peleset and other Sea Peoples in the early 1170s. A late dating, however, seems to conflict with the initial part of the inscription, which lists events which regular chronology would place around 1235-1230 – the date we know Walmus, who is a protagonist here, ruled Wilusa[95].

Although not entirely impossible, it seems chronologically unlikely that Kupantakurantas, the otherwise unknown king who is supposed to have set up the Beyköy inscription and to be active in the 1170s, could be the son of a king active in the 1230s, Mashuittas. In fact, the latter is only known from a much later document, and probably correctly dated in the late 13th century by Woudhuizen and Zangger[96]. However, by suggesting a survival of Walmus into the late 13th century and placing Troy under control of Mashuittas, Woudhuizen and Zangger silently reshuffle the current historical framework for this period, based on a Hittite letter (*KBo* XVIII.18), which has no place for Walmus and tends to place Troy outside control of the Hittites or their allies (like Mira)[97]. In other words, the story supposedly told at Beyköy either contradicts the only previously known text about the same period and area, or has to be re-dated in a problematic way.

In this context, is also notable that Mashuittas' predecessor as king of Mira, Tarkasnawa/Tarkuwas, is not mentioned in the genealogy at the beginning of the inscription. Placing him on a sideline in the family tree (as an older brother of Mashuittas), as Woudhuizen and Zangger do[98], seems a rather weak solution for a king who must have ruled successfully for several decades. If we assume, however, that the monumental inscription from Beyköy is a forgery, there is a very good explanation for Tarkasnawa's absence in the genealogy: he was only recognized in our source material in 1998, some time after Mellaart produced his drawings[99].

This leaves one more major loose end in the inscription. As noted above, the division in § 24-25 between the Hittite towns on the Cilician and Levantine coasts and those controlled by Mira looks awkward. In § 49-50 some of these towns appear again, in a similarly strange and problematic context[100]. Here, the Hittite king is named, Arnuwandas – this must be Arnuwandas III, who reigned till 1207 or a few years later. Apparently, his death led to the loss of a series of cities, including Ura and Lamiya, which were mentioned in § 25 as controlled by Mira, but also Lawazantiya, which was mentioned in § 24 as under Hittite control. This does not seem to make sense in several ways. Why should developments in 1207 or so be prominently remembered twenty or thirty years later, when, moreover, the Hittite empire had collapsed? How should we explain the inconsistencies (also in the spelling of some place names) between § 24-25 and § 49-50? How does the chronology of

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[95] For a summary overview of relevant events, see e.g. Hawkins 1998, 19, 28; Kelder 2004-2005, 66-67; see also Zangger/Woudhuizen 2017, 29.

[96] Zangger/Woudhuizen 2017, 29.

[97] See e.g. Hawkins 1998, 19-21; Kelder 2004-2005, 66-67, 74-75.

[98] Zangger/Woudhuizen 2017, 29.

[99] See Hawkins 1998.

[100] See Zangger/Woudhuizen 2017, 28, 40.

the two relate, as a Western Anatolian intervention in the Southern Levant is even less likely in the late 13th century than in the early 12th? And how can the loss of the Cilician part of the Hittite empire in 1207 or so be reconciled with a series of Hittite and Ugaritic documents which clearly indicate that Arnuwandas' successor Suppiluliuma (II), reconquered (parts of?) Cyprus and was in control, or tried to be so, of the Anatolian and Syrian coasts till the very end of the empire[101]? If at all realistic in some way, the situation described in § 49-50 could only refer to the period around the accession of Suppiluliuma, who apparently had some internal struggles to overcome, or after the fall of the Hittite empire. The first solution would require a much earlier dating of the inscription, which seems to be excluded by the conquest of Ashkelon described in § 27-28, the second solution seems incompatible with the use of the death of Arnuwandas as the reference moment.

The alternative brought forward by Woudhuizen and Zangger[102], that Kupantakurantas' declaration of loyalty to the Hittite king in § 14-15 is an empty gesture, since he did not recognize Suppiluliuma as Hittite king and conquered the cities lost after Arnuwandas' death, does not address any of the issues just mentioned. It makes things only worse, as § 15 and particulary § 14 do not make much sense after the fall of the Hittite empire. Beside this, they even go as far as concluding 'that Suppiliulumas II had a serious problem along his western and southern borders with a hostile Arzawan great king who was supposed to be his loyal partner'.

This simply disregards solid historical evidence and some of what is stated in § 24-25 – and starting from what? After all, the Beyköy inscription never proclaims that Kupantakurantas now rules the cities lost from the Hittites. On the contrary, its last paragraphs are rather odd because they just describe the fate of the Hittite cities, partly repeating § 24-25, but do not say anything about the kingdom of Mira. Although it seems the end of the inscription is missing, the main character and subject of each paragraph is always mentioned at the beginning, and so must be Arnuwandas here. Inscriptions like this do not tend to focus on puzzling lines about a foreign enemy power of the past, hiding an at best very implicit message promoting the heroic ruler who has produced the text. Again, if we assume the text is a forgery, a convenient interpretation would be that Mellaart at some point chose an alternative draft of § 24-25 to extend his masterpiece a bit. The typescript of his translation (Zangger/Woudhuizen 2017, fig. 2), with a division drawn before § 49, may actually still show a trace of this. If I can speculate a little further: perhaps Mellaart tried to make 'his' inscription longer than the Yalburt text, found in 1970 but published rather later? More generally, by its scale, setup and content, the Yalburt inscription looks like a very suitable source of inspiration for 'Beyköy'[103].

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[101] See e.g. Wachsmann 1981, 187, with references to the relevant ancient sources; and also Hawkins 1998, 21.

[102] Zangger/Woudhuizen 2017, 40.

[103] See for this inscription Poetto 1993.

In addition to the main historical problems and inconsistencies in the inscription, there are also some smaller puzzling details. Thus, an offer of 6.000 rams seems surprisingly, even impossibly massive for a relatively minor kingdom in the Late Bronze Age[104], and would be completely out of scale for Beyköy, where the event is placed by Zangger and Woudhuizen[105]. Similarly, the 8000 troops sent to Hapalla (in § 45) and the garrison of 6000 placed at Mira (in § 48)[106] seem rather too high numbers in this context – considering that apparently the forces of the Sea Peoples who seriously threatened Egypt numbered about 15.000-20.000, the Egyptian army at the famous battle of Kadesh consisted of around 20.000 troops (which was less than the Hittite forces) and while Ugarit seems to have provided 150 ships at one point (*RS* 18.148), one of the Ugarit letters (*RS* 20.238) suggest seven enemy ships caused serious damage and were considered a major threat; another incursion seems to be the work of a fleet of 20 ships (*RS* 20.18)[107]. Perhaps, moreover, we should also consider that, according to the Beyköy inscription, Mira must have had additional substantial forces in Cilicia and the Southern Levant. With such figures, Mira would have been a dominant power in the Eastern Mediterranean.

Finally, several of the lists of places in the monumental Beyköy inscription and the ones supposedly from Edremit, Yazılıtaş and Dağardı contain some quite problematic toponyms or sets of toponyms. Zangger and Woudhuizen connect Kurupiya in § 31 to a very Greek toponym Koruphe[108] – but that is odd since it is placed, also following the logic of the sequence in the inscription, in the area between Ephesos and Smyrna[109], which had no known Mycenaean/Greek presence at the time, and in any case a non-Greek indigenous population. The (Greek) city of Smyrna, also mentioned in the list, seems to have been an Early Iron Age foundation[110]. Similar problems trouble the inscription supposedly from Edremit[111], which is hardly more than a list of place names. As also seen in other cases above, the order of locations is quite messy, as it starts with Antissa, a town on Lesbos, then mentions Lesbos (the whole island), then two towns on Lesbos again (Mytilene and Methymna), then (after a lacuna for which there is no good candidate island) three islands further north (Imbros, Lemnos, Tenedos), and then a long list of towns which (as far as we can locate them) cover the whole

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[104] The linear B tablets from Pylos and Knossos mention around 10.000 and 100.000 sheep respectively (Rougemont 2004, 20); while these are not complete inventories, they do seem to reflect the general range of scale of the flocks managed by the palaces. In any case these are (female) sheep, which would represent the large majority of the animals kept alive. Furthermore, one cannot slaughter large proportions of flocks all at once. 6000 rams would imply six figure flocks, but Beyköy is no Knossos.

[105] Zangger/Woudhuizen 2017, 23, 33.

[106] Zangger/Woudhuizen 2017, 28, 39-40.

[107] For the Ugarit episodes, see Wachsmann 1981, 188, with references to the ancient sources.

[108] Zangger/Woudhuizen 2017, 36.

[109] This is further south than it is generally located.

[110] The few Late Bronze Age finds all seem to come from later layers. See Kelder 2004-2005, 57.

[111] Zangger/Woudhuizen 2017, 46-48, 50.

area on the mainland from Parion at the southwest end of the Sea of Marmara to Adramyttion just northeast of Lesbos, in no logical geographical order. In addition to Lesbos, five of the places (Abydos, Arisbe, Perkote, Pithyeia and Adrasteia) are mentioned in Homer[112], as allies of the Trojans. None of these have any substantial Bronze Age remains, most seem later foundations. The same holds for four other places in the list known from later ancient sources (Chryse, Astyra, Parion and Adramyttion)[113]. Then there is Gargara, which again seems a Homeric reference[114], although the poet just mentions Mount Gargaron where an altar to Zeus was visited by the god himself – not a Bronze Age site, archaeologically. Finally, there is the curious mount Leleges: the Leleges, as an ethnonym in plural, are well known from the ancient sources from Homer onwards[115], but they do not have a clearly defined and fixed origin and a 'Mount Leleges' does not make much linguistic sense either.

The only place in the whole list (probably) known already from Hittite texts, is the island of Lesbos, but oddly some of the most prominent Late Bronze Age sites of the island are missing, while Antissa, a very minor place in the ancient world, is surprisingly prominent[116]. It is probably not accidental that this place is well-known for its 1930s excavation, which did yield some Late Bronze remains – in between much more prominent earlier and later finds. The rest of the known places on the list look more like a selection based on Homer and, sometimes rather obscure, later ancient texts than one that can be related in any way to a Bronze Age situation. Mellaart was trained as a classicist, after all…[117].

The inscription supposed to come from Yazılıtaş[118] repeats many of the names encountered in Edremit, but also adds one Homeric toponym (Mount Ida, in the Troad, near Adramyttion) and one known from later texts (Atarneus, either south of Adramyttion which is also listed, or further south near Pitane)[119]. Likewise,

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[112] Homer, *Iliad* 2.828 (Pithyeia, Adrasteia) and 2.835 (Abydos, Arisbe, Perkote). Zangger and Woudhuizen 2017 seem to have missed that Adrasteia is a known place, near Parion. It is notable that Apaisos, which is in Homer's list as well (*Iliad* 2.828), is mentioned in 'Beyköy 2' but not here, and also that Sestos (*Iliad* 2.835), which is on the European side of the Hellespont, is omitted.

[113] Zangger/Woudhuizen 2017 seem to have missed that Chryse and Astyra are known places, the former is not far from Adramyttion, the latter could also be nearby, but there is also a place of the same name near Abydos. See Mitchell 1998-1999, 143, with further references.

[114] Homer, *Iliad* 8.47-52, 14.292-293, 352-533, XV.151-153. See also Mitchell 1998-1999, 140 with further references.

[115] Homer, *Iliad* 10.429; and later e.g. Herodotos I, 171, Strabo 7.1-2.

[116] For an overview of Bronze Age sites on Lesbos, see Spencer 1995.

[117] It may be also relevant here to consider the many correspondences between the 'non-Greek' toponyms in the Mellaart texts and del Monte/Tischler 1978, which are noted but not generally commented upon by Zangger and Woudhuizen. I suspect these also reveal Mellaart's sourcing rather than an ancient reality. Similarly, the Lycian towns mentioned in the Yalburt inscription, not yet published when Mellaart worked on 'his' inscriptions, are notably absent among the toponyms mentioned, which otherwise cover Lycia quite well.

[118] Zangger/Woudhuizen 2017, 48, 50-51.

[119] See Mitchell 1998-1999, 143, with further references.

'Dağardı 2'[120] repeats several of the same names, including Atarna (sic) again, but also Pergamon, Thyateira (further inland, east of Atarneus) and Pitane (further south on the coast). As before, the geographical order does not make much sense, apart from the concentration of the mainland opposite Lesbos, and none of these places have substantial remains of the Late Bronze Age.

And then, before I conclude this chapter, there is a final bit of evidence which is not directly related to any of the inscriptions in Mellaart's estate, but appears to be very crucial: according to Ramesses III's narrative of the attack on Egypt by the Sea Peoples, as recorded at Medinet Habu in or around 1175[121], not only the Hittites, but also Arzawa (so the kingdom of Mira) was overrun by these invaders. This would seem to leave very little time, or more probably none at all, for the glory days of Kupantakurantas and his princes. When Zangger and Woudhuizen conclude that 'Beyköy 2 was evidently composed after Hittite rule had collapsed', they really present a paradox, because there is no reason to suppose that Arzawa lasted much longer than Hatti – rather the opposite, if the Sea Peoples came from the west, as is often supposed[122].

This confrontation between sources is typical. Briefly summarizing, it seems that the content of the monumental Beyköy inscription and its minor companions leaves only two possibilities for understanding: either we accept them as real, and we will have to re-interpret or dismiss virtually all known relevant ancient texts relating to Arzawa and the end of the Hittite empire, assume our current archaeological knowledge of Western Anatolia is worthless, stretch our existing chronology and accept quite some inconsistencies and oddities in our new inscriptions. Or we take them as forgeries, in which case it appears they reveal Mellaart's sources and ways of working and thinking in many details, and perfectly fit his background and his research agenda. I don't think the choice is very difficult.

Conclusions

All in all, it seems we can safely conclude that the monumental text supposed to come from Beyköy is very unlikely to have been found there or in its surroundings, and was almost certainly not first recorded by Georges Perrot as stated by Mellaart. The stories regarding a set of other inscriptions related to it by Mellaart seem equally spurious. Zangger has already shown that the history of the recent study of these and other texts 'found' by Mellaart are also (almost) entirely fictitious as well. The drawings as preserved in the Mellaart estate, moreover, seems to offer several indications that they are forgeries, not based on ancient originals, and the contents of these inscriptions appear to be problematic in many ways, not it in the least because they are incompatible with our existing corpus of archaeological and inscriptional evidence. Finally, several details in the drawings

[120] Zangger/Woudhuizen 2017, 49, 51.

[121] See e.g. Hawkins 1998, 21; Kelder 2004-2005, 66. Dating these events a decade or so earlier, as some, including Zangger/Woudhuizen, prefer, would not affect my argument here.

[122] See also Kelder 2004-2005, 66.

and the contents of the inscriptions can be connected to probable source materials used by a recent forger. Also considering that almost all scholars who were shown the texts in Mellaart's estate from the 1980s onwards appear to have considered them fakes, I see no reason to believe they are recording genuine ancient documents, and plenty of indications and evidence they are fakes.

Nevertheless, even though they allow some doubt on the surrounding stories, and now admit, albeit not entirely wholeheartedly, that Mellaart has produced forged drawings and documents during his career, Zangger and Woudhuizen continue to argue that 'Beyköy 2' and its seven companion texts go back to ancient remains. One of their main arguments seems to be that there would be no evidence proving them to be forgeries[123]: I think I have addressed that point sufficiently above. This leaves a series of more specific indications they have brought forward in support of their viewpoint, listed as *a.* to *i.*[124], and some related points which are embedded in Woudhuizen and Zangger's rebuttal of some arguments supporting the text(s) being a forgery (listed as *a.* to *h.*)[125]. For the sake of completeness, it seems useful to review these arguments before concluding.

Starting with the rebuttals of indications for forgery, it is notable that most of these rest on circular arguments. Against scholars who have noted that several features in the writing, spelling, grammar and language of the supposed Beyköy inscription are unique and/or do not conform to what is seen in other Luwian hieroglyphic texts, Woudhuizen and Zangger argue that these features are quite regular (*a., b.*), can be explained (*d., e., f., g., h*), are characteristic in other languages or scripts (*b.*), or confirm previous suggestions by Woudhuizen regarding other texts (*c.*). They even go as far as stating that the new inscription proves that some of its supposedly exceptional or problematic features are regular (*c., f., g., h.*) while at the same time suggesting some irregularities indicate that the text must be genuine (*b., e., f.*). However, as long as Beyköy 2 is the only case supporting these points, the reasoning obviously remains circular.

Besides that, a second logic flaw seems to affect most of the cases: the mere fact that a certain feature is a possibility, does not imply the text is genuine. Any forger can invent perfectly legitimate possibilities – indeed that is the essence of forging. Such possibilities can only support the case for, in this case, Beyköy 2 being genuine if on the one hand they cannot reasonably have been guessed or invented starting from existing knowledge, and on the other hand parallels can be found which were not available at the (supposed) time of forging. Zangger and Woudhuizen do not offer any such example. Likewise, they seem to assume that a forger would have avoided new features and mistakes – which may be true but need not result in a flawless text, as intentions do not always match results. Here again, features in the text would only become relevant to the discussion when additional evidence can be found.

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[123] Zangger/Woudhuizen 2017, 45; Zangger this volume (though somewhat less confident).
[124] Zangger/Woudhuizen 2017, 44-45
[125] Zangger/Woudhuizen 2017, 43-44

Similar problems affect the argumentations brought forward to support the text is ancient. First of all, several points have now been invalidated by the new information emerging from the papers found in Mellaart's estate. Thus, it can no longer be maintained that 'Mellaart could not read Luwian hieroglyphs, let alone compose texts with them.' (point *a.*) As 'The scholars who have worked with it' [i.e. Beyköy 2] are now known to be a Mellaart phantom, point *b.* is out as well[126]. Point *c.*, which regards the drawing style of Mellaart's reproduction of the text, has received elaborate treatment above, and can be set aside also. This leaves a series of points which all centre on real or supposed corrections (mainly reshuffling of the blocks and therefore text, *d.* and *i.*) and mistakes (reversed columns and a shift in writing direction, *g.* and *h.*) made by Mellaart during his supposed study of the material.

This brings us to methodological issues as the ones just discussed. I do not see how any of these points offers any information indicating the text is genuine (or forged, for that matter), as there is no reason why mistakes or corrections, either genuine or faked themselves, necessarily imply a specific working process (of ancient writers or modern scholars). Something similar holds for the idea that the inclusion of four new symbols would be incompatible with a forgery – I would rather argue the opposite, particularly taking the actual signs into consideration (see also above).

Furthermore, all this is also valid for the culmination of Zangger's argumentation: 'Above all, one argument kills all accusations of forgery, at least as far as James Mellaart is concerned: Mellaart misinterpreted the contents of Beyköy 2 – and then used this misinterpretation over a period of about twenty years for the production of countless hand-drawn maps and several hundred pages of fantasy stories (the Beyköy Text). If Mellaart had conceived and forged Beyköy 2, he would, of course, have known its actual contents – and would not have wasted the rest of his life exploiting a wrong and distorted translation of this document'[127]. I really cannot follow the reasoning: of course, Mellaart can have produced a text which others then read and interpret differently. It seems entirely obvious that he thought the text expressed what his interpretation of it says, and from that perspective, other interpretations of it are simply irrelevant as proof of forgery. And why would the 'Beyköy Text' be a derivative?

Zangger and Woudhuizen point also out that a fraudulent Mellaart would have put an incredible lot of effort in producing his texts, the annotations going with them, and the many reconstructions and interpretations accompanying the actual documents (point *i.*, also partly *d.,* and the paragraphs after *i.*)[128]. Likewise, the long lists of toponyms, many of which unidentifiable, look quite superfluous (*e.*). I think Mellaart's uncontested fakes already show very clearly that he had

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[126] This also affects *d.*, but not so much that it completely invalidates its content - I will come that below.

[127] Zangger, this volume.

[128] Zangger/Woudhuizen 2017, 45.

no lack of creativity and spent very much time on producing fakes, including elaborate documentation and provenance stories, and often presenting much redundant information. An overkill of unknown toponyms and very thorough preparatory research (and perhaps fake indications of research) leading to very complex documents and surrounding stories perfectly fit the pattern.
This brings me to the last argument brought forward by Woudhuizen and Zangger: they see no reason why Mellaart would have invested so much in producing fakes, and cannot explain the 'how and by whom'[129]. The latter two issues have now of course been solved, as there is no reason to suppose Mellaart could not have repeated the forgeries which are acknowledged by Woudhuizen and Zangger, some of which involve huge projects. Paradoxically, the rest of the answers to this point are offered by Zangger himself. As indicated above, he presents Mellaart as a pathological liar. He also notices that the forgeries tend to appear 'shortly after James Mellaart ha[s] introduced a new theory to archaeology – one that the finds in question would reinforce.' Zangger's conclusion then summarizes things perfectly:

> "It thus appears as if Mellaart's fantasy was fuelled by his and others' recent ideas and that he felt he needed to contribute to the discussion by presenting additional, thus far unknown, evidence that would reinforce his point of view. He had acquired a tremendously broad and deep knowledge and developed a coherent historic panorama. Instead of formulating theories, however, Mellaart then sometimes fabricated drawings of artefacts and translations of alleged documents to reinforce his theories".

'Beyköy 2' and the shorter texts going with it seem to offer a perfect case, as they very nicely support Mellaart's ideas about the prominence of Western Anatolia (in whatever part of the Bronze Age). Moreover, with its companions, it also fits Zangger's more general summary of Mellaart's 'modus operandi':

- "All objects were related to topics that fell under James Mellaart's primary interest;
- All finds supported Mellaart's view;
- All the items are unique and highly relevant – even spectacular;
- All items have disappeared;
- No photos were made or were still available when Mellaart presented the case;
- All scholars involved in the cases had died when the evidence was first introduced;
- All other witnesses that may have existed remained anonymous;
- No supporting evidence could be found elsewhere;
- Mellaart himself never sought supporting evidence;
- Mellaart happened to be the only scholar who knew about the items in question".

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[129] Zangger/Woudhuizen 2017, 45; Zangger this volume.

Perhaps we should add:

- All items are documented only by drawings produced by Mellaart: not only the items but also their original documentation have disappeared;
- All items are connected to familiar existing items ('Beyköy 1'), events (the Greco-Turkish war, known Hittite actions in western Anatolia), persons (Perrot, Sahure), and/or names (Papastratos) which appear to be vaguely relevant, but turn out to be problematic or actually irrelevant when researched;
- All items show rather obvious signs of being fakes – clearly Mellaart was not able to or did not feel the need to produce very good fakes;
- Mellaart usually produces contradictory information about items or the stories of their discovery and study;
- Each item offers far too much: more than enough relevant examples or cases, and a lot of irrelevant detail, some of it very much 'too good to be true'. The discovery stories often have quite fantastic elements as well;
- Aside from the idiosyncratic general historical framework the cases suggest, on a more detailed level they all contain serious internal inconsistencies and incompatibilities with both specific ancient source materials and more general scholarly consensus.

As 'Dorak', the various Çatalhöyük cases and the set of inscriptions relating to Arzawa all seem to share all these features, the obvious conclusion seems to be that they belong in the same category. As long as evidence to the contrary is missing, we have to conclude that all otherwise undocumented material presented by Mellaart is faked, even if there is no supporting paper trail. These controversial items are the products of a scholar who clearly could not stop the urge to produce his own reality, even if there was no academic audience, or even no audience at all (as with the Luwian texts). Products, also, of a scholar who was contained by academic criticism, but never excluded – partly because of his charisma, partly because of his undoubted scholarly qualities and spectacular 'real' projects, but also because 'academia' found (and finds) it difficult to recognize and to handle rogue scholars. Perhaps, we should be more aware of the possibility that even academic icons can get lost in their own fantasies, and of the ways to contain this.

Bibliography

Balter, M. 2005: *The Goddess and the Bull. Çatalhöyük: An Archaeological Journey to the Dawn of Civilization*, New York.

Burney, O. 2012a: In Memoriam James Mellaart (1925-2012), *Paléorient* 38(1-2), 11-12.

Burney, O. 2012b: James Mellaart, *Anatolian Studies* 62, iii-iv.

Cagnat, R. 1914: Georges Perrot, *Journal des Savants* 12, 460-464.

Collon, D. 1990: Subjective Reconstruction? The Çatal Hüyük Wall-Paintings, *Hali. The International Magazine of Antiques, Carpets and Textiles* 53 (October), 119-123.

Daniel, G. 1970: Editorial, *Antiquity* 44, 85-90.

Depuydt, L. 2014: The Alleged *Gospel of Jesus's Wife*: Assessment and Evaluation of Authenticity, *Harvard Theological Review* 107(2), 172-189.
Eiland, M. 1990: Book Review: The Goddess from Anatolia, *Oriental Rug Review* 10(6), 19-26.
Gibson, S. 1992-1993: [Editor's Note], *Bulletin of the Anglo-Israel Society* 12, 82.
Gonnet, H. 1981: Beyköy (Ihsaniye-Afyon) 1979, *Anatolian Studies* 31, 181-183.
Gray, D. 1974: *Seewesen; mit einem Beitrag: Das Schiffsfresko von Akrotiri, Thera von Spyridon Marinatos* (series: Archaeologia homerica, Bd. 1, Kap. G), Göttingen.
Haspels, C.H.E. 1971: *The Highlands of Phrygia. Sites and Monuments*, Princeton.
Hawkins, J. 1998: Tarkanaswa King of Mira 'Tarkondemos', Boğazköy sealings and Karabel, *Anatolian Studies* 48, 1-32.
Hodder, I. 2015: James Mellaart 1925-2012, *Biographical Memoirs of Fellows of the British Academy*, 14, 411-420.
Humphreys, S.C. 1977: review of Gray 1974, *Classical Philology* 72, 347-355.
James, P./N. Kokkinos 1992-1993: [Letter to the Editor], *Bulletin of the Anglo-Israel Society* 12, 80.
Kelder, J.M. 2004-2005: Mycenaeans in Western Anatolia, *Talanta* 36-37, 49-88.
Lamberg-Karlovsky, C.C. 1992, 'Constructing' the Past, *The Review of Archaeology*, 13(1), 37-39.
Lasteyrie, R. de/M. Collignon 1916: Georges Perrot, *Monuments et Mémoires de la Fondation Eugène-Piot* 22, V-XVII.
Lewis, D. M. 1993: [Letter to the Editors], *Bulletin of the Anglo-Israel Society* 12, 80-81.
Mallett, M. 1990: A Weaver's View of the Çatal Hüyük Controversy, *Oriental Rug Review* 10(6), 32-43. Accessed through <http://www.marlamallett.com/ch.htm> (accessed 30/6/2019; without pagination).
Mallett, M. 1992-1993: The Goddess from Anatolia: an Updated View of the Çatal Hüyük Controversy, *Oriental Rug Review* 13.2, 24-31. Accessed through <http://www.marlamallett.com/chupdate.htm> (accessed 30/6/2019; without pagination).
Maspero, G. 1915: Notice sur la vie et les travaux de M. Georges Perrot, *Comptes rendus des séances de l'Académie des inscriptions et belles-lettres*, 453-485.
Masson, E. 1980: Les inscriptions louvites hiéroglyphiques de Köylütolu et Beyköy, *Kadmos* 19, 106-122.
Mazur, S. 2005a: Getting To The Bottom Of The Dorak Affair, *Scoop independent news (scoop.co.nz),* 27/8/2005, <http://www.scoop.co.nz/stories/HL0508/S00224.htm> (accessed 30/6/2019).
Mazur, S. 2005b: Dorak Diggers Weigh In On Anna & Royal Treasure, *Scoop independent news (scoop.co.nz)*, 4/10/2005, <http://www.scoop.co.nz/stories/HL0510/S00059.htm> (accessed 30/6/2019).
Mazur, S. 2005c: The Dorak Affair's Final Chapter, *Scoop independent news (scoop.co.nz)*, 10/10/2005, <http://www.scoop.co.nz/stories/HL0510/S00120.htm> (accessed 30/6/2019).
Mellaart, J. 1959: The Royal Treasure of Dorak. A first and exclusive report of a clandestine excavation which led to the most important discovery since the royal tombs of Ur, *The Illustrated London News,* 754, suppl. I-III.
Mellaart, J. 1966: *The Chalcolithic and Early Bronze Age in the Near East and Anatolia*, Beirut.
Mellaart, J., 1980: Turkish Kilims, *Anatolian Studies* 30, 91-99.
Mellaart, J., 1984a: Anatolian Kilims, New Insights and Problems, *Anatolian Studies* 34, 87-95.
Mellaart, J. 1984b: Some Notes on the Prehistory of Anatolian Kilims, in: Frauenknecht, B. (ed.), *Early Turkish Tapestries*, Nuremberg, 25-41 (also reprinted in *Oriental Rug Review* 4 (1985), 416-421).
Mellaart, J./B. Balpinar, B./U.Hirsch 1989: *The Goddess from Anatolia: Environment, Economy, Cult and Culture*, Milan.
Mellaart, J. 1990: The Earliest Representations of the Goddess of Anatolia and Her Entourage, in: Rageth, J. (ed.), *Anatolische Kelims; Symposium Basel, Die Vorträge*, Basel, 27-46.

Mellaart, J. 1991: James Mellaart Answers His Critics, *Hali. The International Magazine of Antiques, Carpets and Textiles* 55(2), 86-87.
Mellaart, J. 1991-1992: [Book review of James, P. et al. 1991: *Centuries of Darkness*, London], *Bulletin of the Anglo-Israel Society* 11, 35-38.
Mellaart, J. 1992-1993: [Letter to the Editor], *Bulletin of the Anglo-Israel Society* 12, 81-82.
Mellaart, J. 1993: The Present State of "Hittite Geography", in: Mellink, M.J./E. Porada/T. Özgüç (eds.), *Aspects of Art and Iconography: Anatolia and its Neighbors. Studies in Honor of Nimet Özgüç*, Ankara, 415-422.
Menant, J. 1887: Les Hétéens: un nouveau problème de l'histoire d'Orient (review article), *Revue de l'histoire des religions* 15, 88-93.
Messerschmidt, L. 1900: *Corpus inscriptionum Hettiticarum* (series: Mitteilungen der Vorderasiatischen-Aegyptischen Gesellschaft, Vol. 5), Berlin.
Mitchell, S. 1998-1999: Archaeology in Asia Minor 1990-98, *Archaeological Reports* 45, 125-192.
Monte, G.F. del/J. Tischler 1978: *Répertoire Géographique des Textes Cunéiformes* 6 (series: Die Orts- und Gewässernamen der hethitischen Texte), Wiesbaden.
Muscarella, O.W. 1988: *Bronze and Iron: Ancient Near Eastern Artifacts in the Metropolitan Museum of Art*, New York.
Muscarella, O.W. 2000: *The lie became great. The forgery of Ancient Near Eastern Cultures*, Groningen.
Muscarella, O.W. 2014: Forgeries of Ancient Near Eastern Artifacts and Cultures, in: Brown, B.A./M. H. Feldman (eds.), *Critical Approaches to Ancient Near Eastern Art*, 31-54.
Pearson, K./P. Connor 1967: *The Dorak Affair - A true tale of archaeology and intrigue in Turkey*, London.
Perrot, G. 1878: Florence depuis l'annexion au royaume d'Italie, [vol. 2. Juillet 1878:] l'Institut des hautes études, les grands travaux, et la situation financière de Florence, *Revue des Deux Mondes* 48, vol. 28, 294-331.
Perrot, G. 1882: Sceaux Hittites en terre cuite appartenant à M. G. Schlumberger, *Revue Archéologique* 44, 333-341.
Perrot, G. 1886: Une Civilisation retrouvée. — Les Hétéens, leur écriture et leur art, *Revue des Deux Mondes* 76, 303-342.
Perrot, G./C. Chipiez 1892: *Histoire de l'art dans l'antiquité*, Tome 5: *Perse, Phrygie - Lydie et Carie - Lycie*, Paris.
Poetto. M. 1993: *L'Iscrizione luvio-geroglifica di Yalburt. Nuove acquisizioni relative alla geografia dell'Anatolia sud-occidentale* (series: Studia Mediterranea, 8), Pavia.
Ramsay, W.M. 1888: A Study of Phrygian Art, *Journal of Hellenic Studies* 9, 350-382.
Ramsay, W.M. 1889: Syro-Cappadocian Monuments in Asia Minor, *Mitteilungen des kaiserlich Deutschen Archäologischen Instituts, Athenische Abteilung,* 14, 170-191.
Rougemont, F. 2004: The Administration of Mycenaean Sheep Rearing (flocks, shepherds, "collectors"), in: Frizell, B.S. (ed.), *PECUS Man and Animal in Antiquity. Proceedings of the Conference at the Swedish Institute in Rome, September 9-12, 2002* (series: The Swedish Institute in Rome. Projects and Seminars, 1), 20-30 Rome.
Reinach, S. 1914: Georges Perrot, *Revue Archéologique*, 121-127.
Sabar, A. 2016: The unbelievable tale of Jesus's wife, *The Atlantic* (July-August issue) <https://www.theatlantic.com/magazine/archive/2016/07/the-unbelievable-tale-of-jesus-wife/485573/> (accessed 30/6/2019).
Schachermeyr, F. 1959-1960: Die Königsgräber von Dorak, *Archiv für Orientforschung* 19, 229-232.
Spencer, N. 1995: Early Lesbos between East and West: A 'Grey Area' of Aegean Archaeology, *Annual of the British School at Athens* 90, 269-306.
Theocharis, D. 1958: Iolkos: Whence Sailed the Argonauts, *Archaeology* 11.1, 13-18.
Thompson, E.L. 2018-2019: "Official Fakes": the Consequences of Governmental Treatment of Forged Antiquities as Genuine during Seizures, Prosecutions, and Repatriations, *Albany Law Review* 82.2, 407- 447.

Uslu, G. 2015: *Homer, Troy and the Turks. Heritage & Identity in the Late Ottoman Empire 1870-1915*, dissertation University of Amsterdam 13/11/2015.
Verhart, L./H. Pauts 2016: Een onderduiker in het magazijn, *RMO Magazine* 17, vol. 42, 17-20.
Voight, M. 1991: The Goddess from Anatolia: An Archaeological Perspective, *Oriental Rug Review*, 11(2), 32-39.
Wachsmann, S. 1981: The Ships of the Sea Peoples, *The International Journal of Nautical Archaeology* 10, 187-220.
Zangger, E./F. Woudhuizen 2017: Rediscovered Luwian Hieroglyphic Inscriptions from Western Asia Minor, *Talanta*, this volume, 9-56.

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