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Assyria and the West

A Fresh Look at the Unshakeable Pillars
of Late Bronze and Iron Age Chronology
in the Eastern Mediterranean World

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Dating Hartapu: The Troubled Relationship between Archaeology and Texts

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This paper aims to analyse and contextualise a recently discovered inscription from Türkmen-Karahöyük in southern Anatolia. The inscription is in Luwian hieroglyphic and refers to a certain 'Great King' Hartapu and his military exploits against the Mushka (a people often identified as the historical Phrygians). It has provisionally been dated to the 8th century BC. This date, however, presents us with a number of problems, most notably in terms of historical context. For if this Hartapu was indeed a powerful ruler in the 8th century BC, why do we not hear about him in contemporary Assyrian texts (which do mention other kings in the area)? And how could this be squared with the fact that the Phrygian kingdom flourished precisely at a time when Hartapu claims to have defeated various kings and supposedly ruled over a significant swathe of land? These and other problems warrant a re-examination of the evidence, especially now that the 8th century BC date has already been adopted as a reference point for the interpretation of archaeological data from Türkmen-Karahöyük. This paper highlights several methodological pitfalls and discusses the possibilities and limitations of interpretations of the currently available historical, archaeological, linguistic and epigraphic evidence.

Introduction

The centuries between the collapse of the Hittite empire, conventionally date to around 1200 BC, and the rise of the Phrygian kingdom in the historical 8th century BC, remain an ill-understood period in Anatolian history. Consequently, the importance of the stele that is discussed in this paper is immediately clear. Found near the newly discovered archaeological site of Türkmen-Karahöyük, the stele bears an inscription (TÜRKMEN-KARAHÖYÜK 1) that identified it as the work of a 'Great King' Hartapu. Since a king with this very same name was already known from other stelae in the region and was associated with the mighty kingdom of Tarhuntassa, the seat of a viceroy during the Hittite Empire period, it has been proposed that Türkmen-Karahöyük was in fact the site of this long-lost kingdom. Moreover, apart from naming Hartapu, the stele also referred to a military victory over the Mushka, a people commonly identified with the Phrygians from later, classical sources. The stele therefore is of tremendous historical importance, but since its discovery in 2019, the exact date and historical context of the stele have been questioned.

The *editio princeps* of the stele by Goedegebuure *et al.* (2020) suggests an eighth-century BC date, and this date has now been adopted by archaeologists working at Türkmen-Karahöyük. The step is a logical one, but not without risks: for if the eighth-century BC date turns out to be incorrect, all subsequent interpretations



Figure 1. Map showing the location of Türkmen-Karahöyük and other key sites mentioned (U. Zerbst).

and hypotheses that build on it must be rejected, too. That this is not a hypothetical danger may be illustrated by the archaeological report on the site by Osborne *et al.* (2020), published in the same issue of *Anatolian Studies* as Goedegebuure's study.

The Contribution of Survey Work

Although the site remains unexcavated, some general trends in the development of the site can be surmised based on survey results. Osborne *et al.* indicate that the



Figure 2. The recently discovered inscription of Hartapu (TÜRKMEN-KARAHÖYÜK 1; photo by Fadime Arslan and James Osborne, from Goedegebuure et al. 2020, 30).

site reached its largest extent (of some 125 to 150ha) in the Hittite Empire period. They also suggest that, following the collapse of the Hittite kingdom, Türkmen-Karahöyük retained much of its habitation during the conventional 12th through the historical 7th centuries BC. If this were true, it would have been one of the largest cities in the world at that time. The pottery distribution at the site is presented as evidence for this scenario. Mapped during the 2019 survey, the material is not always easily identifiable. Indeed, Osborne *et al.* (2020, 13) note that:

The difference between the Early (ca. 1200–900 BCE) and Middle Iron Age (ca. 900–600 BCE) pottery in this region is almost impossible to discern. Pieces that can only belong to the Late Iron Age (ca. 600–300 BCE) are more readily identifiable, and, when collection units that produced ceramics restricted to the Late Iron Age are mapped, it is apparent that settlement at Türkmen-Karahöyük retracted to roughly 50 ha. *It follows, therefore, that the Late Iron Age witnessed the start of Türkmen-Karahöyük's gradual decline [emphasis added] and that its maximum extent lasted from the Late Bronze Age until the Middle Iron Age.*

This conclusion does not hold. As the authors already indicated, the archaeological evidence – i.e., the distribution of Late Iron Age pottery – indicates that the settlement had *already* shrunk to its much-diminished size at the inception of the Late Iron Age. Unless one wants to argue for a single, sudden depopulation right at the end of the Middle Iron Age, the only plausible conclusion is that settlement at the site had already

decreased in the previous period: that is, the Middle Iron Age and possibly even the – in light of the almost indistinguishable pottery – Early Iron Age. This, of course, is difficult to reconcile with the notion of a Great King Hartapu, who was able to defeat the mighty Phrygians. A critical review of the arguments for an eighth-century date of the stele (which, in the absence of a good archaeological context, must be established based on palaeographic and linguistic assessments) is therefore imperative. This is something that will have to be done by specialists, and indeed, some Hittitologists, such as Oreshko (2020) and Adiego (2021), have already expressed doubts regarding the dating and the reading/understanding of parts of the text. As an archaeologist, I am not qualified to judge all of the (linguistic and epigraphic) assessments presented in those studies, though some observations – such as the point made by Adiego (2021, 15–16) that some of the signs that were presented as ‘late’ in fact seem to have decidedly archaic features – seem to ring true (but see Goedegebuure forthcoming, for a rebuttal of some of these criticisms).

The KIZILDAĞ 1 Conundrum

Where I can judge the evidence, however, the arguments for an eighth-century date seem remarkably thin, and often cascade into a string of assumptions. One example is the inscription's comparison with the KIZILDAĞ 1 inscription of king Hartapu which flanks the only known representation of a king of this name (see Figure 3). This inscription has already been known for several years, and both Hawkins (2000, 429) and Oreshko (2017,

48) argued that it belonged to the conventional 12th century BC. However, it is increasingly thought that the accompanying relief of a seated king was probably contemporaneous with the inscription. Yet the style of the relief seems like Assyrian examples from the 8th century (Goedegebuure *et al.* 2020, 41; Hawkins and Weeden 2021, 391), and thus it has been suggested that the KIZILDAĞ 1 inscription must date to that era as well. If the newly discovered stele from Türkmen-Karahöyük dates to that same period (the 8th century BC), such a suggestion would seem to make sense. Yet there are various methodological problems with these arguments, not least because the association between the inscription and the relief is far from certain. Hawkins (2000, 434), for instance, was quite explicit: ‘it may be necessary to assign the inscriptions and the figure to two widely differing dates’, while Hawkins and Weeden (2021, 393) have taken recourse to the idea that there were two kings called Hartapu (I and II), while acknowledging the problem this raises: ‘Why should an 8th century Hartapu echo the inscription of a 12th century namesake (possibly his actual ancestor)? Could the memory in this area have extended back more than 400 years?’

In addition, given the dearth of Assyrian reliefs prior to the 9th century, we should not be too confident in assigning *post quem* dates to given iconographical features: we simply know too little of the development of sculptural traditions even in Assyria itself, let alone in remote places such as southern Anatolia, prior to Ashurnasirpal II. Certain features may have developed well before they are archaeologically attested. On top of all this, the figure on the throne, whilst undeniably ‘Assyrian-inspired’, displays a number of peculiarities (such as the upward-curved shoes) that seem more Anatolian, whereas other, ‘Assyrian’ features (such as the hairstyle and tripartite beard, as well as the position of the fingers holding a cup) are, according to d’Alfonso and Pedrinazzi (2021, 146), ‘peculiar to the representation of Ashurnasirpal II and Shalmaneser III’; both these Kings ruled in the 9th (not the 8th) century BC. Dating the KIZILDAĞ 1 inscription based on the accompanying relief is thus fraught with numerous problems and uncertainties.

In addition to these iconographical considerations, there are historical problems with assigning the new inscription at Türkmen-Karahöyük to the 8th century BC. For example, Hartapu’s designation as a ‘Great King’ – a highly coveted title that had previously been used by the Hittite emperors – suggests that he ruled over an extensive swathe of land. Yet this seems difficult to reconcile with our understanding of the political geography of 8th century BC Anatolia (which was dominated by the kingdom of Phrygia [which reached the zenith of its power in the second half of the 8th century], with Assyria exercising some sort of control over various minor kingdoms in southern Anatolia):

put simply, there is no space on the map for another eighth-century BC Great King.

This problem might be solved by assuming that the title of Great King was, by this time, merely an obsolete heirloom with no connection to any real power or territorial rule. But this again boils down to special pleading, for we have no evidence for a similar flippant use of royal titulature elsewhere at that time – or any time before or after the 8th century BC.^[1] More importantly, while suggesting that Hartapu was, despite his claims to Great Kingship, merely a petty chief, this contradicts his claims that he conquered the land of the Mushka. One might argue that this victory did not really amount to much more than a successful raid (as per Goedegebuure *et al.* 2020, 41), but this – though not impossible – is again special pleading and only permissible if there are overriding arguments to do so. Even if one were to argue that the ‘conquest’ merely was a successful raid into Phrygian territory, such an endeavour still seems completely at odds with the capacities of a petty king. The Phrygians reached the pinnacle of their power precisely under Mita/Midas, in the late 8th century BC, and would have surely retaliated. If Hartapu were indeed capable of inflicting a defeat on Mita, moreover, one would – as Goedegebuure *et al.* concede – expect references to him in contemporary Assyrian sources; *quod non*.

Suggesting that this raid may have taken place just before Mita’s reign, in the earlier 8th century BC, and before Tiglath-pileser III invaded Tabal (Goedegebuure *et al.* 2020, 41; see now also Massa and Osborne 2022, 98), is hardly more plausible. Whilst there are no Assyrian sources on the region from the early 8th century BC, the early eighth-century BC inscription of Yariri from Carchemish does refer to the region and mentions only the *Musha*, *Mushka* and *Sura*. And although the identification of these three terms is not wholly secure, the general consensus seems to be that these are terms for the Lydians (Greek *Mysioi*), the Phrygians (Greek *Moschoi*, Assyrian *Muški*)^[2] and possibly Urartu

[1] In fact, the only other king in the wider area who is known to have borne the title ‘Great King’, Wasusarma (ruled c. 740–730 BC), seems to have done so with good reason: in the so-called Topada inscription, he claims control over several vassal kings (though he was, at same time, also listed as a tributary to the Assyrian king Tiglath-pileser III; Bryce [2010, 143–144] suggests that his eventual removal from the throne may have been the result of his flaunting of imperial titles).

[2] Though there may have originally been a distinction between the Mushka and Phrygians, the two ethnonyms appear to have become virtually indistinguishable by the reign of Mita at the latest. Before that time, though, it is likely that the Phrygians and Mushka represented different peoples; the latter are most likely situated in the region east and southeast of Tabal (Wittke 2004; Kopanias 2015, 216). I owe this observation and the references, to Peter James. In a recently published paper, James (2021–2022) argues for a ninth-century BC date for Hartapu.

(Hawkins and Weeden 2016, 14; Rollinger 2006; though Simon [2012, 172] argues convincingly against this identification) or Tyre; apparently, and probably tellingly, omitting the supposedly mighty kingdom of Hartapu. The easiest explanation for such an omission is, of course, that there was, at that time, no such powerful state in southern Anatolia.^[3] This is corroborated by an earlier reference from the time of Shalmaneser III (837 BC) to no less than 24 (clearly petty) kings of Tabal (Bryce 2010, 141).

We are thus faced with a conundrum. If one argues that Hartapu ruled in the late 8th century BC, one must accept that a king who clearly ranked amongst the most powerful rulers of his time – powerful enough to invade the realm of king Mita – was not mentioned in the otherwise well-informed contemporary Assyrian annals. If one on the other hand argues for an early eighth-century date of Hartapu, then one must assume that, despite his grandiose title, Hartapu was merely one of the myriads of unnamed kings ruling a minor kingdom in southern Anatolia. Yet at the same time he was apparently able to raid the territory of a next-door emergent superpower with impunity. The latter scenario seems just as unlikely as the first, if only because Phrygia was, already by the late 9th century BC, a powerful kingdom and almost certainly under the control of a single ruler (Muscarella 1995, 96; see Kealhofer *et al.* 2019, 511 for the most recent ¹⁴C dates).

The Wider Historical Picture

There are other problems with placing Hartapu in the 8th century BC. The realisation that Phrygia must have already been a more or less united kingdom with a single ruler, forces one to disassociate the 13 vanquished kings mentioned in lines 3 and 4 from Hartapu's victory against the Mushka. Yet there is nothing in the inscription that suggests that Hartapu's victory and resettlement of these 13 kings was not an integral part of his campaign against the Mushka. Quite to the contrary in fact, for the text indicates that these kings came forth *while* Hartapu was campaigning in the land of Mushka (as Goedegebuure *et al.* themselves note, the literal translation is 'the enemy came down into the land' – there is no need to translate this as 'the enemy descended upon (his) territory' [emphasis

added]). Indeed, disassociating Hartapu's conquest of the country of the Muška (line 1), with his defeat of 13 otherwise unidentified kings (line 3) in battle (line 2) who were subsequently settled in ten strong fortresses (line 4), raises more questions than answers. How could an otherwise unattested, and thus presumably minor king, be able to raid with impunity an emergent superpower, and then defeat a coalition of 13 Tabalian states? Instead, when arguing from the inscription itself, it seems most logical to consider the 13 kings as rulers of the Muška, who were defeated in Hartapu's campaign against this land, and subsequently settled as vassals 'under ten strong-walled fortresses'. Whether these kings were being held in or controlled from these fortresses or given the command (as vassals) over these fortresses, is open to interpretation, but as far as the inscription itself is concerned, the most logical reading indicates that Hartapu was not only able to defeat the Muška, but also that they, at that time at least, were not ruled by a single king but by no less than 13 leaders.^[4]

This, of course, does not at all fit the eighth-century context proposed in Goedegebuure *et al.*, and instead rather suggests a much earlier period; a period of tribal leaders and petty kings, who were on the move or, as the inscription has it, were 'coming down into the land'. The most plausible historical setting for such a situation, as far as I can see, is the conventional 12th century BC; i.e. the period immediately following the collapse of the Hittite empire and, not wholly coincidentally, also the period to which the Hartapu from the KIZILDAG 1 inscription and various other inscriptions (Meriggi 1964, 52; Hawkins 2000, 434) were originally thought to belong. If the TÜRKMEN-KARAHÖYÜK 1 inscription does indeed belong to the 12th century BC, it would confirm suspicions of the survival of a Hittite rump-state at Tarhuntassa into the Iron Age. I note in this respect that Hawkins (2000, 429) already suggested that the 'visiting Great King' Ir-Teshup referred to in the stele from Karahöyük (Elbistan) was a king of Tarhuntassa, rather than a king of Carchemish: we may now be able to gain a better understanding of that king's royal line. A 12th century date would also shed new and important light on the development of Luwian

[3] Although it cannot wholly be excluded, the identification with Tabal is extremely unlikely. In his inscription, Yariri boasts of the various languages and scripts he has mastered. Seeing that the script at Carchemish was Luwian hieroglyphic and thus not distinct from the script in use at Tabal, there would be little reason to boast of mastering this language. Younger Jr. (2014, 181) suggests that Sura may refer to (the alphabetic script used at) Tyre. Assyria has also been proposed, though one would then have to explain why that country is listed twice in the KARKAMIS A15b §19 text.

[4] Peker (2020) suggests that the fortresses belonged to the respective kings and were sacked during Hartapu's campaign, though I find such a reading less convincing (if only because we would then have to account for 13 kings ruling 10 fortresses). The impression is rather of (mobile) war bands with their respective leaders, who were subsequently settled in fortified (settled – and thus more easily controllable) places. Being trained as a classical archaeologist, I cannot help but note that the 'mighty fortresses' in the inscription may be compared to later Homeric tradition (*Homeric Hymn to Aphrodite* V.111–112), which describes Phrygia as a land 'rich in fortresses', ruled by one Otreus (cf. *Homer Iliad* 3.186), where Mygdon is mentioned as the co-ruler with Otreus over the 'multitudes' of Phrygian warriors on campaign near the River Sangarius in central Anatolia.



Figure 3. The KIZILDAĞ 1 relief with the only known representation of Hartapu (from Rossner, E. P., 1988. *Die hethitischen Felsreliefs in der Türkei. Nördlingen*).

Hieroglyphic writing traditions. Rather than unique examples of a local tradition ‘that adhered more than anywhere else to the traditions of the empire period’, as Goedegebuure *et al.* suggested, the inscription’s idiosyncrasies could now easily be explained in the context of a transitional phase.

Concluding Remarks

By way of conclusion, I should like to make three points. First, that the archaeological evidence suggests that Türkmen-Karahöyük’s heydays were the Late Bronze and Early Iron Age (and not, as we argued above, the Middle Iron Age). Textual evidence, moreover, seems to support this notion. The numerous petty states that are mentioned in both Assyrian and local (Luwian hieroglyphic) texts, moreover, indicate that whatever overarching power that may have existed throughout

the Early Iron Age had collapsed by the 8th century BC. In the absence of any unequivocal evidence to the contrary, I would therefore be inclined to date the newly found inscription from Türkmen-Karahöyük to the Early Iron Age – most probably in the conventional 12th century BC. Such a date requires no special pleading and fits the evidence that is currently at our disposal. There are, of course, other solutions to explain the inscription’s idiosyncrasies. One way to explain some of these might be to assume that the inscription stems from the period shortly after the fall of the Hittite Empire, but was reworked at some later date. This is not impossible, for there are clear differences in the execution of line 1 and the rest of the text. Yet it would raise numerous questions, most notably why a given king would want to alter an earlier text and when (the latter becomes especially important when we realise that the site was in decline during the Middle Iron Age). Another solution might be to accept that something is terribly wrong with our (conventional) understanding of Late Bronze and Early Iron Age chronology, and that the cultures and reigns that are now associated with 12th century BC belong, instead, to the 10th or 9th century BC. The effects of such a major adjustment would be immense, as the ripple effect of such a modification would force us to reconsider the chronologies of essentially all other parts of the ancient world (including, most worryingly, the sequence of kings of Egypt and their absolute regnal dates, which is generally – though not universally – considered to be relatively secure).

Second, and on a more methodological note, I should like to stress that special pleading only ought to be permissible if it facilitates our understanding of how various *undisputed facts* may relate to each other – and not as a way to weave various *assumptions* into a single narrative. Perhaps this view is a reflection of my own ignorance (as an Aegean prehistorian dabbling in the field of Luwian inscriptions) but the use of terms such as ‘expertly archaizing’ to explain the use of signs that were hitherto only found in secure Bronze Age contexts in apparent Iron Age contexts (Hawkins and Weeden 2021, 391) raise more than a few red flags – especially given our less than satisfactory understanding of the chronological sequence at the site (Carchemish) where these signs were inscribed. Similarly, whilst one cannot *a priori* exclude the possibility that line 1 of the inscription from Türkmen-Karahöyük is a pastiche of an earlier text (KIZILDAĞ 4) – the carving of this line does indeed appear markedly different from the rest of the text – it seems inherently implausible that the content of this finely carved first line has no real relation to the rest of the text (*pace* Hawkins and Weeden 2021, 392–393). We must assume that the text was meant to be read by someone, and simply using a random line mentioning an earlier, illustrious, namesake before continuing, *in medias res*, with an entirely different,

contemporary story hardly makes sense (as indeed, Hawkins and Weeden seem to admit when they note that this was ‘made for whatever reason’). Rather than resorting to a historically inexplicable, and (to my mind at least) elaborate and unprovable scenario, it would seem prudent to review the entire paradigm that has spawned the various conflicting dates. When our current idea of the development of the Luwian Hieroglyphic script forces us to assume the existence of otherwise unattested local, archaizing (as per Goedegebuure *et al.*) or even ‘expertly archaizing’ (*supra*) signs, and now even forces us to account for, essentially, two entirely different dates (a pastiche and later addenda) within a single text, one cannot help but wonder whether our entire understanding of the development of Luwian Hieroglyphic writing might not be deeply flawed.

Third, and perhaps most importantly, I hope to have highlighted not only how important it is to take into account evidence from neighbouring fields, but also how wary one must be when taking given ‘facts’ from a field that is not one’s own (in this case: the supposed eighth-century date for the inscription) as a point of departure for interpreting new evidence (the archaeological data). This is an important point, for ‘facts’, once they have entered academic debate (and prestigious publications), are only rarely revisited, yet they often serve as lynchpin for entire archaeological paradigms. The discoveries made by Osborne, Massa and Bachhuber offer us a rare chance to revisit our current paradigms and significantly add to our understanding of southern Anatolia’s ‘Centuries of Darkness’. Let us proceed with caution and not jump to pre-existing conclusions.

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