How old was the Ankara Silver Bowl when its inscriptions were added?

S.P.B. Durnford

c/o British Institute at Ankara

Abstract

The artefact known as the Ankara Silver Bowl bears two short Hieroglyphic Luwian inscriptions, each in a different 'handwriting'. They tell us about the origin of the bowl in the year that Tudhaliya *labarna* conquered *Tara/i-wa/i-zi/a*. Unparalleled phrasing and tantalising historical allusions make dating and interpretation problematic. The conquest mentioned is widely held to be that of Taruisa in the Troad by the 14th-century bce Hittite king Tudhaliya I/II, but epigraphy points to a Karkamiš origin for the inscriptions and probably to a post-Empire date. Treating the text as contemporary with the conquest requires either that the bowl be classed as an exceptional Empire document or that a later Tudhaliya is intended. This paper offers a new approach. It accepts a late date, offers an amended translation and proposes that the narrative be viewed as literature alluding to the past and not as contemporary chronicle. The bowl's possible status as a relic prompts questions about the transmission of history, motives for alluding to the past and the words chosen for the purpose. An interpretation of sign *273 is ventured within a speculative scenario that encompasses the bowl's various oddities.

Özet

Ankara Gümüş Kasesi olarak bilinen eser, herbiri farklı 'elyazısıyla' yazılmış iki kısa Luvice resim yazısı taşır. Bu yazıtlar, Tudhaliya *labarna*'nın *Tara/i-wa/i-zi/a*'yı fethettiği yılda kasenin kökeni hakkında bilgi verir. Benzeri olmayan anlatım tarzı ve yanıltıcı tarihi göndermeler, tarihlendirmeyi ve yorumlamayı sorunlu hale getirmektedir. M.Ö. 14. yy.'da Hitit kralı I. veya II. Tudhaliya'nın Troad bölgesindeki Taruisa'yı ele geçirmesinden genişçe bahseder fakat epigrafi, yazıtlar için Karkamış kökenine ve muhtemelen İmparatorluk sonrası bir tarihe işaret eder. Metnin bu fetihle aynı döneme ait olduğunu düşünmek için, ya kasenin sıradışı bir İmparatorluk belgesi olması ya da daha sonraki bir Tudhaliya'dan bahsedilmiş olması gerekmektedir. Bu makale yeni bir yaklaşım ortaya koymaktadır. Geç bir tarihi kabul etmekte, düzeltilmiş bir çeviri sunmakta ve hikayenin o döneme ait tarihsel bir kayıt olarak değil de, geçmişten bahseden edebi bir eser olarak algılanması gerektiğini öne sürmektedir. Eski eser olarak kasenin olası durumu, tarihin aktarılması konusunda bazı sorular akla getirmektedir, geçmişten bahseden motifler ve kelimeler bu amaç için seçilmiştir. Kasedeki çeşitli çelişkileri de içeren şüpheli bir senaryo dahilinde, *273 işaretinin yorumlanması riskli bir girişimdir.

The Ankara Silver Bowl resides in the Ankara Museum of Anatolian Civilizations, but lacks a provenance, so that its origin and subsequent history have to be deduced from the object itself. Incised on the bowl are two Hieroglyphic Luwian (HL) texts, one of which, if interpreted a certain way, claims a provenance from the 14th century bce. The texts, however, do not look as old as that.

It is conventional to assign each HL document a unique designation in upper case recording its primary geographical association. The bowl had not received such a designation by November 2009, when the present paper was completed. After discussion with Professor J.D. Hawkins the bowl is informally designated ANKARA 2 in this paper, to distinguish it from the ANKARA stone fragment (Hawkins 2000: 559).

Figure 1 shows the bowl. It has the shape of an oblate hemisphere, 20.2cm in diameter and 7.3cm deep (Mora 2007: 517). Round the brim is a lip which suggests that ANKARA 2 might once have had a lid. The remainder of the bowl is smooth and unornamented, apart from two sinistroverse HL inscriptions on opposite sides of the bowl, of unequal length and running horizontally below the lip.
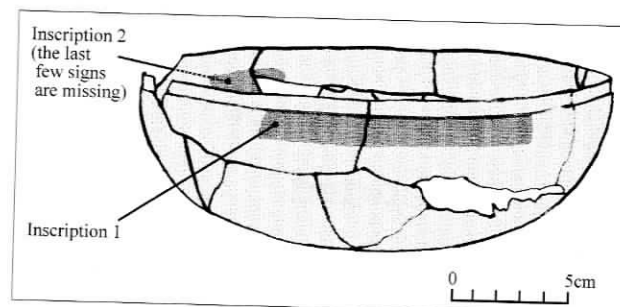
Fig. 1. ANKARA 2, showing the fragmentation and missing pieces. The two hatched areas show the positions of the inscriptions, which run from right to left on the outside surface

ANKARA 2 is of beaten silver and was evidently found in a fragmentary state. It has been reassembled, though when and by whom is unknown. Jansoone 2007 shows another view of ANKARA 2. Though largely complete, the bowl lacks some fragments, leaving four jagged gaps. The longer text, Inscription 1, is complete, apart from one sign which lies under the solder holding the fragments together, but whose identity is relatively certain from the context. One of the missing fragments has carried away the latter part of Inscription 2, the shorter text.

ANKARA 2's fragmented state

The bowl's state of fragmentation is consistent with the effects of stress corrosion cracking (SCC), a form of spontaneous decay to which objects made of unannealed ductile metal sometimes fall prey. SCC is a scourge of ageing silver objects and is associated with, for example, the presence of embedded zinc atoms. Appropriate analysis of ANKARA 2 may be able to reveal not only how it has fared metallurgically, but also, possibly, the region where the silver came from.

Without the artefact having to suffer any physical trauma to trigger the process, SCC causes cracks to develop and then to propagate in straightish, forking lines. This accounts for an impression that much of ANKARA 2 was deliberately cut up. There is no predicting whether or when SCC may begin, nor how long it will continue actively wreaking its destructive effect.

Does the presence of SCC offer any scope for historical deduction? One has to wonder when smiths first realised the need to anneal silver before cold working, when or if annealing became the normal practice and how many other silver objects from Bronze and Iron Age Anatolia show the effects of SCC. Was ANKARA 2 created, though not necessarily inscribed, relatively early, having been left unannealed because of general ignorance, or was it created and inscribed in the Iron Age and left unannealed through lack of personal ability or knowledge?

The lack of distortion from impact or pressure shows that fragmentation must have occurred while the bowl was safe in some sort of hollow space for a very long time. This suggests that ANKARA 2's original find-spot could have been a tomb, and it seems improbable that the bowl would have been used as grave goods if already showing signs of SCC damage.

The fact that some fragments are missing suggests that the tomb may have collapsed or that the find-spot was a midden (see below), and that the finder of the bowl did not recover more of it nor realise at the time that there were more fragments to look for. Be that as it may, the recovered fragments would have looked like a handful of valuable scrap silver, yet someone had the means and the motivation to reassemble the bowl. Subsequently, rather than being kept as a private treasure, the bowl found its way to a museum in Turkey and not to an institution somewhere else.

Although silver is generally very corrosion resistant, an alternative hypothesis to SCC may be offered, which stems from the metal's susceptibility to ammonia and its derivative compounds and cyanide salts. Thus the unannealed bowl could have cracked up relatively rapidly if it had lain in a midden among urine residues or perhaps even been used as an (albeit rather small) up-market chamber pot before disposal. Of course, SCC may also have been active as well.

Soldering silver can be done in more than one way, and the joints may alter with the passage of time. For instance, if the solder contains lead, the atoms of the lead interdiffuse with the atoms of silver, causing both to change progressively. Would scientific examination of the soldered joints used tell us anything about how, when, where or by whom the repair was made?

Did the bowl have a lid?

If ANKARA 2 was made with a matching lid, as implied by the lip, there is nothing to show which material was used for it. If also of silver, the lid would presumably have been made from the same metal as the surviving bowl and in the same workshop. The variability of SCC's onslaught is such that one cannot tell whether the lid too would have suffered or might have remained in one piece instead.

The lack of any lid fragments suggests that, if a lid ever existed, it may have either remained undiscovered or been entirely removed, presumably unfragmented. Removal could have happened at any time between manufacture and the donation of the reassembled bowl to the Ankara Museum. It is even conceivable that the putative lid may be sitting in another collection somewhere.

It is also conceivable that the hypothetical lid could have been ornamented and even had inscriptions of its own. This possibility prompts one to wonder whether the

two texts were added to the bowl before the loss of the lid or afterwards and whether the extant texts may have related to the lid in some way. However, the texts as we have them give no such hint.

The HL signary includes a single logogram for a bowl, which is lidless. The sign appears once in each of ANKARA 2's inscriptions. This cannot be taken to indicate that the texts were incised after both the lid and knowledge of it had already been lost, because no sign depicting a lid-covered bowl has yet been identified, such as might have been used if the inscriptions predate the loss of a lid. The possibility of a lid thus remains open, but investigation of ANKARA 2 and its texts need not, as things now stand, pay any further attention to that question.

Scholastic treatments of ANKARA 2

ANKARA 2's existence was apparently first made known to scholars via a brief mention by Laroche with the comment 'provenant de Kargamis(?)' (Laroche 1960). Full details were first published in Hawkins 1997, where a possible conflict between epigraphy and subject matter in dating the texts was first identified and explored. At that time Hawkins 2000 was in an advanced state of preparation and the bowl is briefly mentioned there in a notice of recent discoveries (Hawkins 2000: 19).

The longer inscription is reproduced untranslated in Hawkins 2003 (144–46) with a summary of its dating implications. Hawkins 1997 was updated and republished as Hawkins 2005, which is the primary reference for this paper and sometimes quoted without attribution. The longer text, with translation, is reproduced from Hawkins 1997 in Bryce 2006 (109). The transliteration and translation of both inscriptions from Hawkins 2005 are reproduced in Mora 2007 (517–19) with a summary and critique of Hawkins's commentary. Mora adds several significant observations, to which the present paper makes appropriate reference.

Professor Hawkins wishes to point out (personal communication) that adverse circumstances affected the publication of Hawkins 2005 and resulted in the omission of the tracings of the HL inscriptions themselves. Also, a reference to Güterbock in note 34 wrongly appeared in note 33 instead. Figure 2 in the present paper reproduces both inscriptions after an original drawing kindly loaned by Professor Hawkins.

An apparent conflict emerges when the texts are examined.

[Inscription 1] dates the dedication of the object to the time of a Tudhaliya *labarna* and is written largely in syllabic Luwian. For this reason alone it would naturally be attributed to the time of the Hittite
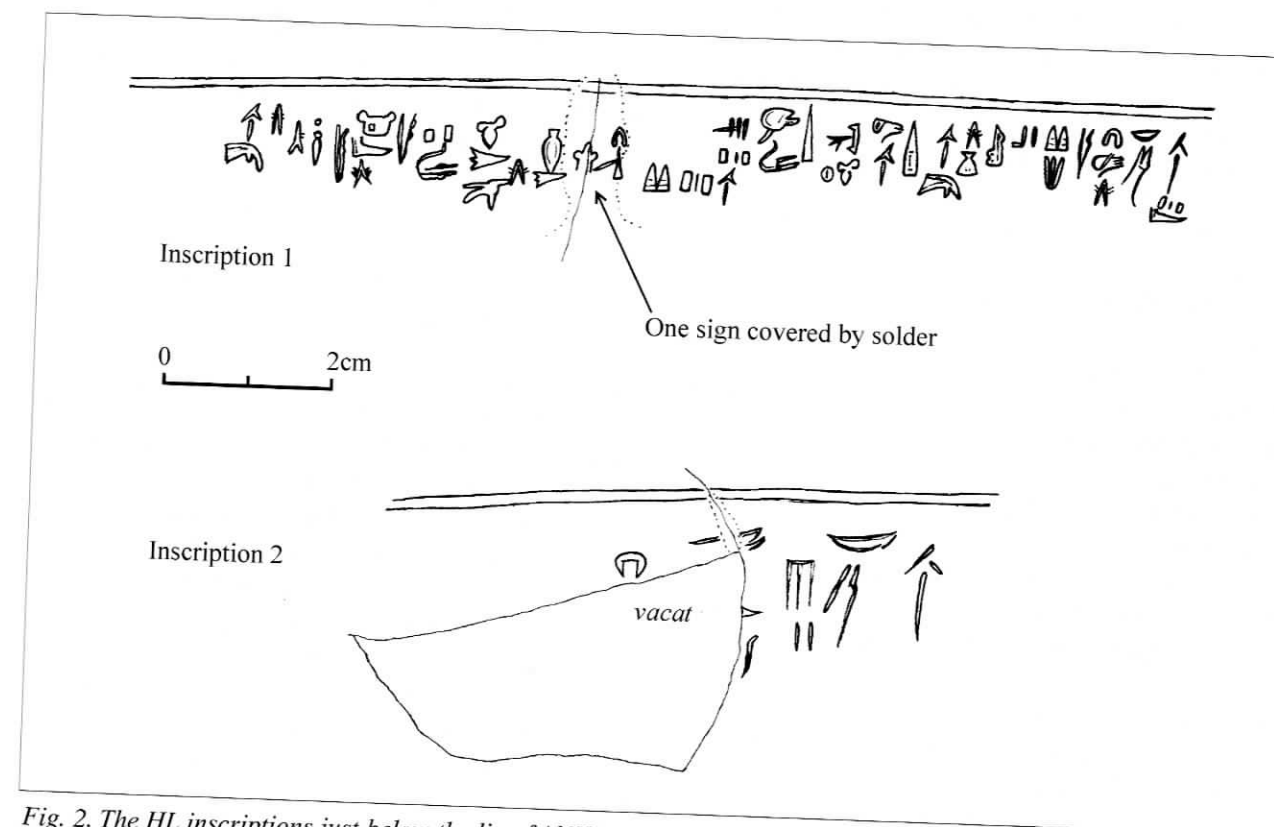

Fig. 2. The HL inscriptions just below the lip of ANKARA 2

emperor Tudhaliya IV, yet strong arguments can be advanced for thinking that this should rather be the time of Tudhaliya I/II, six generations earlier. That such an inscription might be written in Hieroglyphic at this date (ca. 1400 BCE) would have a revolutionary implication for our view of the origin of the script, but it does not seem possible to exclude it (Hawkins 2003: 146).

This paper seeks to offer an alternative approach to the conundrum that would avoid such a radical solution.

ANKARA 2's metallurgy, design and intended uses are matters for the relevant specialists. Despite the question in the title, it is not the aim of this paper to establish the absolute age of the bowl, but to show that its inscriptions are very much more likely to belong to the Late corpus (ca. 1100–700 bce) than to be of Empire date. That events apparently mentioned in one of the texts might go back to actual Empire history is then treated as a matter of literature and not of contemporary record. While seeking to demonstrate the probability of a relatively late date for the texts, this paper poses a number of incidental questions, whose resolution must await future evidence and research.

Text transliteration conventions

The HL script appears to possess more syllabic signs than needed for the spoken language itself. Consequently some spoken syllables are denoted by more than one sign. Such apparent homophones are distinguished in transliteration by a sequence of diacritics. For instance, the three signs transliterated respectively as *ta*, *tá* and *tà* are all provisionally taken to be alternative representations of a single phonetic value.

When more than three homophones have been identified, the sequence is continued by means of subscript numerals, such as in *sa₅*. The choice of diacritic or numeral assigned to each homophonous sign reflects, if anything, little more than the sequence in which scholars agreed the values of signs. A similar but independent convention is used in transliterating cuneiform too. Thus, for example, HL and cuneiform each have a sign transliterated *má*, but the use of the acute accent in both instances carries no implication of any phonetic similarity or other special relationship between the two scripts.

Logographic signs in HL are transliterated using upper-case. Anatolian names, such as *HATTI*, are italicised; while Latin is used for vocabulary words. In a minority of instances a single semantic value appears to be represented by more than one sign, and subscript numerals are used to distinguish them. Thus 'man' may be represented by either VIR or VIR₂ without any obvious difference in significance.

A different use of italicised upper-case is made in transliterating cuneiform, to show that an Akkadian term has been written in place of a native Hittite or Luwian word. These Akkadograms often contain elements of Sumerian origin, which are transliterated unitalicised. The native equivalent of each Akkadogram is presumed to be what was actually spoken when the text was read out loud.

There is a curious HL writing habit, a form of aphaeresis dubbed 'initial-*a*-final', whereby a word starting with the sign for *a* is written with this sign at the end of the word instead. Some texts exhibit this; some do not. A comprehensive analysis of the phenomenon is found in Hawkins 2003 (159–61). In words where the true sequence is to be reconstructed, the displaced first syllable is marked *a. The surviving part of Inscription 2 offers no opportunity for 'initial-*a*-final', while three instances of it occur in Inscription 1.

Hawkins 1997 appeared before 'initial-*a*-final' was recognised and transliterates the apparently functionless word-final *a* with an unattached acute accent. Hawkins 2005, however, transliterates with *a and moves the syllable to the front of its word. More recent practice is to retain the sequence as written, leaving the reader to interpret instances of word-final *a. In transliteration of continuous text this paper therefore retains word-final *a. However, in subsequent reference to individual words the *a* is shown in the presumed initial position and with the asterisk omitted.

The asterisk that characterises an 'initial-*a*-final' vowel must be distinguished from the asterisk marking signs whose values have not yet been identified, namely *273 and *414 in ANKARA 2. These signs are identified instead by their catalogue numbers, as allocated in Laroche 1960. An interpretation of *273 is ventured later.

ANKARA 2's inscriptions

Table 1 gives the transliterated and translated inscriptions. There are differences from Hawkins 2005 in that syllabically written names have been retained in their transliterated form where it is not clear what the normalised transcription should be, and a new translation is ventured for the larger text. The notes in table 2 focus on points covered later in this paper, and the reader is referred to Hawkins 2005 for the original word-by-word commentary and to Mora 2007 for additional discussion.

Inscription 1, the longer text, contains two sentences, consisting of one clause and two clauses, respectively. The text mentions three people and two places and refers to what purports to be an historical event. However, the passage is fraught with peculiarity, because the three names *A-sa-ma-i(a)*, *Ma-zi/a-Karhuha* and *Tara/i-wa/i-zi/a* are one-offs in HL, as are the two turns of phrase REGIO.*HATTI* VIR₂ and MONS.[*tu*] *LABARNA+la*.

Transliteration and translation

Inscription 1 (three clauses)

§1   *zi/a-wa/i-ti* CAELUM-*pi*   *sa-ma-i(a)-*a* REGIO.*HATTI* VIR₂
This bowl for himself   *A-sa-ma-i(a)*, man of Hatti/Hattusa,

     *273 *i(a)-sa₅-zi/a-tá*   REX *ma-zi/a-kar-hu-ha* REX PRAE-*na*
     forged?   during the reign of? king *Ma-zi/a-Karhuha*.

§2   *tara/i-wa/i-zi/a-wa/i* (REGIO)   REL+*ra/i*
The land of *Tara/i-wa/i-zi/a*   when

     MONS.[*tu*] *LABARNA+la*   *hu-la-i(a)-tá*
     Tudhaliya *labarna*   smote,

§3   *wa/i-na-*a*   *pa-ti-i(a)-*a* ANNUS-*i(a)*   *i(a)-zi/a-tà*
it   in that year   he made.

Inscription 2 (presumed to consist of one clause only)

§1   *zi/a* CAELUM-*pi*   SCRIBA 2 *pi?-t[i?]-x[...]*   *414   [...
This bowl   the second(-rank) scribe *Pi?-t[i?]-[...]*, the? *414,   [... -ed].

Table 1. The HL inscriptions – transliterated and translated

Inscription 2, of which the latter part is lost, is much shorter than Inscription 1. Although no verb survives, the text is evidently short and appears to consist of a single clause naming the scribe who, we are presumably being invited to believe, added at least one of the texts to the bowl.

Approaches to the challenge of the texts

Whichever language one is investigating, comparing a new text with the rest of the corpus is, generally speaking, a two-way activity. On one hand a pre-existing corpus acts as a guide to deciphering and dating. On the other hand the corpus expands to include the new text, and our understanding, we would hope, increases as well. New insights and hypotheses may then lead in turn to a reassessment of the pre-existing texts. The process is ideally one of perpetual review, re-energised by each discovery.

In recovering the history and languages of the ancient world it is customary to treat each new document at face value, until and unless some other approach is shown to be relevant. Every so often during this process the weight of new evidence goes beyond merely increasing our understanding and forces a shake-up of the consensus. The challenge to scholarship has been to reconcile ANKARA 2's epigraphy with Inscription 1's narrative by accounting for the apparent two-century gap between them. Opinions published hitherto differ as to which Tudhaliya is the most likely, and all have treated the texts as contemporary with whatever event Inscription 1 is referring to.

Some recent literature restates the challenge without attempting to resolve it (Hawkins 2003: 145, 167; Bryce 2005: 125–26, 360; 2006: 109). This neutral response appears to exhibit a tacit leaning towards Tudhaliya I/II. It has also been mooted that resolution may not be achieved without a significant reworking of our understanding of HL epigraphy (Hawkins 2005: 200).

In contrast to the neutral response is one which proposes a later Tudhaliya as very much more likely. Yakubovich, as part of a larger study with a different purpose, presents a vigorous case for Tudhaliya IV, suggesting that the latter's Seha River campaign could have been the occasion for the conquest, 'if Tarwiza is indeed to be identified with Tarwisa of the cuneiform sources, and by extension with the classical Troy' (Yakubovich 2008: 359–62).

Textual notes

(most are discussed more fully elsewhere in this paper)

Inscription 1

(a) *zi/a-wa/i-ti*: The *-ti* appears to be the reflexive pronoun, the indirect object of (d).

(b) *A-sa-ma-i(a)*: The bowl's provider seems to bear a Hurrian name.

(c) REGIO.*HATTI* VIR₂: Hawkins 2005 has 'Hattusa', but Hawkins 1997 has 'Hatti'. Both seem equally possible in this phrase, which is unparalleled in HL.

(d) *273 i(a)-sa₅-zi/a-tá*: This otherwise unknown verb is echoed by (h) in some way. It has *zi/a* ... CAELUM-*pi*, 'this bowl', as its direct object and the *-ti* from (a) as its indirect object. Hawkins 2005 suspects a verb of dedicating, as found in other bowl texts, but the resemblances to *i-(ia-)sa-*, 'buy', and YALBURT's obscure *i(a)-sa-zi/a-* or *i(a)-zi/a-sa-* (opinions differ over the reading) add to the mystery. Elsewhere *273 determines 'conquer', 'smite' and 'skill', bringing to mind the craftsman's hammering of the bowl into shape, so 'forge?' is the meaning ventured here. If *i-(ia-)sa-* is relevant, then combining purchasing with manufacture could mean that *A-sa-ma-i(a)* commissioned the bowl, rather than making it himself. The possibility is explored below that *273 may represent a silversmith's anvil, known as a 'stake'.

(e) REX *ma-zi/a-kar-hu-ha* REX PRAE-*na*: This unknown king's name seems to contain that of Karhuha, a god found only in Karkamiš. The *kar* sign used to write both *Karhuha* and *Karkamiš* (HL *kar-ka-mi-sà-*, etc) is also found only in Karkamiš. Based on comparable Hittite phrasing it is proposed that the meaning 'during the reign of' for PRAE-*na* is not only syntactically acceptable, but also makes appropriate sense. For emphasis the phrase has been placed after the verb and sits alongside the apparent date formula that follows it.

(f) MONS.[*tu*]: The solder-covered sign is almost certainly *tu*. Preceded by MONS, it provides the distinctive writing of the royal name Tudhaliya. The allusion to his conquest of *Tara/i-wa/i-zi/a* has the look of a date formula, more specific than that in (e) and taken with which it appears to provide a synchronism.

(g) *LABARNA+la*: The coupling with MONS.[*tu*] is another construction without parallel. Hawkins 2005 (197, 200) summarises the other usages of *labarna*.

(h) *i(a)-zi/a-tà*, 'He made': This verb in some way echoes that in (d) and could be referring to the physical creation of the bowl. Alternatively, a more general meaning such as 'did' may be more appropriate, as in BABYLON 2, if (d) should later prove to have a meaning unlike that proposed in this paper.

Inscription 2

(i) SCRIBA 2: Four scribal ranks are known, and all but this instance are on seals.

(j) *Pi?-t[i?]-[...]*: The scribe may bear a Hurrian name, to be read *Benti(p)-*.

(k) *414: If not of phonetic value here, *414 may denote something else of the scribe's role or title.

Table 2. The HL inscriptions – textual notes

The observation that ANKARA 2 is probably a late document is offered by Mora too, who also explores some implications of a Karkamiš origin, including whether *Tara/i-wa/i-zi/a*, whatever the name underlying this spelling may be, could be a place in or close to north Syria (Mora 2007: 519). Mora furthermore asks whether the Tudhaliya mentioned could be either Tudhaliya IV or, indeed, another king of this name and as late as the 11th century bce. Starke maintains that the Troad is too far away from Karkamiš. He analyses *Tara/i-wa/i-zi/a* as a name quite different from Tarwiza or the like and does not exclude even a 15th-century bce date for ANKARA 2 (Starke 1997).

An alternative approach to the challenge

As already mentioned, all of the scholars just cited take ANKARA 2's inscriptions to be contemporary with the event to which Inscription 1 refers, and only Mora ventures beyond the Empire period into the Late corpus. This paper aims to outline the case for treating ANKARA 2's texts as not of necessity contemporary with that event, a proposed approach which has two main consequences. Firstly, epigraphy becomes the dominant means of dating. Secondly, interpretation of Inscription 1's content moves from the domain of historical chronicle into that of literature.

Both of ANKARA 2's inscriptions refer to the bowl itself, but the surviving parts are otherwise distinct from one another in subject matter. They also differ in the way they are written, and there seems to be some value in comparing them, not only with the rest of the HL corpus, but also with each other. Even though no firm conclusions can be drawn from the observations below, there is sufficient cause, in my opinion, to raise doubt about the role of ANKARA 2 as a strict historical record.

This paper seeks to identify indications that ANKARA 2 may be harking back to an age of remembered glory in order to present itself as a prestige heirloom. It is rare for a year to go by nowadays without the discovery of another HL inscription, and so the corpus is constantly expanding, and our understanding along with it. In the hope that future finds will resolve the conundrum of ANKARA 2, this paper poses many questions that cannot yet be answered.

ANKARA 2's texts compared with each other

Let us for now set aside all questions of grammar, vocabulary and what the narrative appears to be telling us and let us look only at how the texts are written. There are then two distinct aspects to consider, namely, how a sign is shaped and how it is deployed, i.e., the handwriting and the spelling.

As can be seen in figures 2 and 3, Inscription 2 'is executed in somewhat larger, more coarsely incised signs' than Inscription 1. This alone gives cause to wonder why the look of the two texts should differ at all. Did they have separate authors? Were they incised on two different occasions? Which was incised first? What additional evidence can be brought to bear?

The three signs *zi/a*, CAELUM and *pi* are found at the start of both inscriptions and thus provide a small sample for comparing the two writing styles. Over and above the differences in size and degree of coarseness, some small differences are also visible in the shapes of the three shared signs, as described next.

The sign *zi/a* occurs a further four times in Inscription 1, and these later instances have the central vertical stroke straight and are effectively identical to one another. The initial *zi/a* is slightly taller than the other four, and the lower part of its central stroke shows a slight curve to the left. In contrast, the *zi/a* at the start of Inscription 2, the only instance in that text, has its central stroke curving slightly to the right.

The logogram CAELUM depicts a bowl at its simplest. It consists of a horizontal line with ends joined by an arc hanging beneath it. Inscription 1's CAELUM has a straight horizontal, and the arc is drawn with a single and evenly curving stroke. The horizontal in Inscription 2's CAELUM bulges down a little in the

centre, and the arc has a break in it, showing that it took two strokes. The impression given is that the logogram in Inscription 2 was incised by a less fluent hand than that which wrote Inscription 1's.

Inscription 1 contains just the one instance of *pi*. Inscription 2 contains one definite instance and may contain a second, but it is too damaged to be certain. Of the three signs shared by the inscriptions the writing of *pi* shows the greatest difference between the texts. It is made with five strokes in Inscription 1, but the complete instance in Inscription 2 with just three, reproducing the essential outline only.

Turning next from the epigraphy to the wording, there is very little to go on. As examined above, both inscriptions begin with the phrase *zi/a* .. CAELUM-*pi*, but there is a difference in that Inscription 1's first word is longer by two signs, *wa/i* and *ti*. The *ti* goes with the verb later in the clause and is discussed further below.

The *wa/i* represents the quotative particle *-wa*, which is enclitically attached to the first stressed element in a clause. It can be seen occupying the corresponding position in both of the other clauses in Inscription 1 as well. This particle is used the same way very frequently in other HL texts too. Being stylistically optional, it is generally left untranslated, as with Inscription 1. (In a minority of texts *-wa* introduces a true quotation and is then rendered by means of inverted commas.) Inscription 2 appears to have contained a statement of the same narrative kind as found in Inscription 1, but it lacks Inscription 1's use of *-wa*. Is this stylistic difference significant?

Our first assumption is that the two inscriptions would have been incised in succession on a single occasion. What, then, of the differences in size and coarseness of the characters, in the shapes of the three shared signs and in the use of *-wa*? Did one person incise both inscriptions, but separated by such an interval that his writing habits had changed meanwhile? ANKARA 2


Fig. 3. Signs occurring in both texts (shown to scale and read from right to left)

Inscription 1	𝔐	⌣	⌐	₀₁₀	↑	First word of the text
					4x ↑	Later in the inscription
Inscription 2	𝔐	⌣			↑	First word of the text
	⟍		⊢			Later in the inscription (uncertain)
	pi	CAELUM	*ti*	*wa/i*	*zi/a*	Transliterated

is a small object, easily handled, and could have existed for a considerable time before being first inscribed. It is also easy to suppose that each text could have been added by a different person and perhaps on different occasions, but, if so, which was added first?

If the author of Inscription 1 was not also the author of Inscription 2, did one of them choose to remain unnamed or is there another name in the lacuna in Inscription 2? Was the author of Inscription 2, who defines himself as a second-rank scribe, less deft of hand than the author of Inscription 1, who was then perhaps a top-rank scribe and so fit to incise the main narrative on such a prestige object as a silver bowl? Alternatively, was Inscription 2 added by the scribe named in it, seeking to imply a claim to authorship of Inscription 1 as well? Furthermore, if the texts were not both made by the same hand and on a single occasion, what certainty can there be that ANKARA 2 was not already old when first inscribed?

It is not suggested that ANKARA 2's inscriptions date very far apart, and certainly not from different eras. Whatever the age of the bowl when first inscribed, the evidence favours both texts being relatively late and 'from the same stable', so to speak.

ANKARA 2's epigraphy and the rest of the corpus

Whereas one might expect, in the general case, a sign's simpler and more cursive form to appear later than its more complex ancestor, the actual record shows, roughly speaking, that both kinds of variant are found alongside one another in HL documents of all kinds and perhaps from all eras. The Late corpus exhibits this mixture clearly, containing as it does stone monuments showing some cursive forms as well as generally cursive documents, such as the Kululu lead strips, in which occasional monumental sign-forms appear.

The same phenomenon may be at work in the preceding Empire inscriptions, though there it is difficult to judge since we have so few incised inscriptions to represent a lost handwriting (Hawkins 2003: 162).

Both of ANKARA 2's texts show HL signs in cursive form, consistent with the process of incision, and feature several instances of the two signs which are transliterated respectively as *zi/a* and *i(a)*. The vowel in these syllables is ambiguous and is to be understood by the reader as either *a* or *i*, according to the context. One of the differences between the Empire syllabary and that of the Late corpus is that the latter had developed variants of these signs which mark the *a* form of each explicitly. From the two older signs four are thus created, transliterated as *za*, *zi*, *ia* and *i*.

There is a transitional group of inscriptions that postdates the fall of Hattusa, but does not exhibit all the features characteristic of the Late corpus (Hawkins 2003: 146). These texts retain the undifferentiated *zi/a* and *i(a)*. Let us suppose that the names on ANKARA 2 had been unremarkable, then its texts, rare instances of early syllabic writing and cursive script though they would be, might have been dated before the differentiated *za*, *zi*, *ia* and *i* were established in the Late corpus. This in theory would allow the texts to be dated any time from almost the start of the HL era until several generations after the fall of Hattusa. Likewise, the phenomenon of 'initial-*a*-final', described above, appears in Empire texts and 'continues in texts of the early first millennium' (Melchert 2003: 210). Even when no longer in regular use, these writing habits, being so visible on monuments, would serve as obvious indicators of claimed antiquity.

The earliest HL texts are logographic. Phonetic complements and grammatical elements came to be added as time went by, such that the latest texts are predominantly syllabic. We might suppose that vernacular writing habits were always more advanced than formal and monumental styles, and, as mentioned above, individual cursive signs are indeed found intermingled with more elaborate forms from the start, implying the existence of less durable documents with greater narrative content, which would have made wider use of syllabic signs.

Narrative texts, functional documents on wood and correspondence of the kind conducted centuries later on strips of lead are thus believed to have existed from early times, but have failed to survive. The need for clarity in that sort of text would surely have stimulated innovation in the phonetic and grammatical uses of signs, but circumstances have denied us the Empire corpus where this might be observed.

Given no more than a cursive sign-form on a seal or a stone monument we cannot tell the extent to which a fully cursive text of the same date would make use of phonetic syllabograms. To see the silver bowl as an isolated example of a lost body of Empire handwriting is to require vernacular habits attested only centuries later to have coexisted with a monumental tradition that must then have seemed inordinately conservative in comparison. That is not of itself impossible, but unlikely, because this scenario would further require those vernacular habits, by nature likely to be relatively volatile, to then have remained substantially unaltered during the subsequent centuries before the Late corpus came into being.

The progressive appearance of phonetic signs in monumental texts is a very crude indication of relative age, and it also varies with location. Of the Late corpus,

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'it is noteworthy that full phonetic writings, without but also with the logogram, are particularly characteristic of the latest group of Tabal inscriptions' (Hawkins 2000: 5). A notable aspect of ANKARA 2's texts is that the grammar is spelled out, except for the case endings of the main nouns, which are simply missing. The details follow.

The verbs are fully inflected, but their subjects lack a nominative ending and their object nouns an accusative ending. This is unexceptional for a fully logographic text and hence could stand as a mark of old-fashioned writing, but it contrasts with the thoroughness with which syllabograms are otherwise used to spell out so much of Inscription 1. It is hard to resist the suspicion that the text's composer wished to safeguard the clarity and completeness of his message while feeling able to sacrifice those parts of it that its readers would easily supply for themselves.

CAELUM-*pi*, 'bowl', is accusative but endingless, whereas *a-wa/i-na*, referring back to the bowl, shows the accusative common gender ending -*(a)n*, written with dead vowel as -*na*. Alternatively, and less plausibly, if CAELUM-*pi* is actually neuter and thus with no ending, then the scribe has forgotten its gender when writing *a-wa/i-na*. The gender of CAELUM-*pi* has not yet been established, and, whichever it turns out to be, the construction CAELUM-*pi* .. *a-wa/i-na* will remain anomalous. If it contains no unintended error, then we must see CAELUM-*pi* as common gender, with its case ending, like that of other nouns, deliberately omitted.

The dative-locative is seen in *a-pa-ti-i(a)* ANNUS-*i(a)*, but is absent from *Ma-zi/a-Karhuha*. It is possible that a genitival ending for the latter might be intended instead, according to the intended meaning of the subsequent PRAE-*na*, usually 'before; in the presence of; prior to'. The key point is that *Ma-zi/a-Karhuha*, like the nominative and accusative nouns, has no case ending at all. The overt dative-locative ending to ANNUS-*i(a)* at first sight seems to gainsay the hypothesis that case endings are all deliberately omitted, but the word forms part of the adverbial phrase *a-pa-ti-i(a)* ANNUS-*i(a)*, 'in that year', i.e., *abadi usi* in normalised transcription. This phrase correlates with *kwari*, 'when', and ANNUS-*i(a)* is thus not on a par with those load-bearing nouns that show bare stems, but is instead treated as part of the syntactical skeleton in which the load-bearing nouns and verbs are embedded.

Such omissions of case endings may appear in any phase of written HL, but, like using 'initial-*a*- final' and undifferentiated *zi/a* and *i(a)*, they provide highly visible signals that a text could be of Empire vintage or was intended to appear so. The narrative on ANKARA 2 would, however, be hard to convey without the full

phonetic writing, and this, on present evidence, is more indicative of a later date instead. In order to give the reader the impression of antiquity the composer of Inscription 1 seems to have larded it with just about as many perceived archaisms as could be fitted in without obscuring the thrust of the message.

The pull towards post-Empire Karkamiš

None of the evidence is conclusive, but all the epigraphic indicators point to post-Empire Karkamiš for dating ANKARA 2's texts. It is the apparently historical allusions that swing the focus back into the centuries of Empire, and the clarity of these allusions is examined later.

One indication that ANKARA 2's texts come from a Karkamiš orbit is the stylistic resemblance of some signs to some on inscribed stone fragments from Karkamiš. This recently recognised similarity is particularly noticeable in the ram's head sign *ma* (Hawkins, personal communication). Figure 4 shows ANKARA 2's two instances of *ma* beside those in KARKAMIŠ A19, fragment n1 (Hawkins 2000: 201–06, pl. 81) and KARKAMIŠ A28, fragment h (Hawkins 2000: 215–17, pl. 85). Characteristic of Karkamiš are the longer, thicker, upward curling horn and the presence of the mouth. These are clearly depicted on ANKARA 2, despite the differences in detail between the two instances.
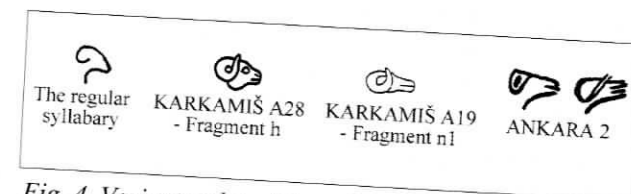
Fig. 4. Variant styles of the syllabogram ma

A stronger indication comes from *Ma-zi/a-Karhuha*, whose 'name is striking, apparently incorporating the name of the Karkamiš god *Karhuha*, written as regularly with the *kar* sign, a usage attested only in Karkamiš inscriptions' (Hawkins 2005: 196). It was familiarity with this connection that presumably led Laroche to describe the bowl as 'provenant de Kargamis(?)' (Laroche 1960).

Karhuha must surely have been worshipped in Karkamiš for a long time before being incorporated into the royal name *Ma-zi/a-Karhuha*. The logographic [D]KAL, a god of Karkamiš in the cuneiform *Annals of Suppiluliuma I*, had long been presumed (Hawkins 2005: 196) to stand for *Karhuha* or its alternative *Karhuhi*. This was confirmed by Singer, who rejoined a broken tablet of Suppiluliuma II to reveal [D]K]*ar-ḫu-u-ḫi-iš* among the chief gods of Karkamiš (Singer 2001: 638–39).

Of the HL corpus as known prior to ANKARA 2 it has been observed that 'curiously, the latest Karkamiš inscriptions show a marked archaizing style, in which the earliest largely logographic practice is consciously affected' (Hawkins 2000: 5). There are late Karkamiš instances of full syllabic writing with only the case ending omitted, just as exhibited by ANKARA 2 and strongly reminiscent of syllabically written names in the opening clause of cuneiform Hittite royal proclamatory texts. This type of archaising in HL could perhaps be designated pseudo-logographic. It is as though a truly logographic rendering might fail to convey the full grammatical sophistication of the message, or perhaps the HL readership of that late era were felt to lack the will or ability to appreciate the finer points of antique writing habits.

All in all there is a considerable case for supposing ANKARA 2's inscriptions to be of a late date, from the Karkamiš area and of archaising intent.

The verb -ti *273 i(a)-sa₅-zi/a-

This obscure verb holds the key to the origin and purpose of the bowl. *A-sa-ma-i(a)* did something to the bowl 'for himself'. Given the HL verb *i-(ia-)sa-*, 'buy', *A-sa-ma-i(a)* could have simply bought himself the bowl, but the determinant *273 seems to require a more vigorous action. The argument is advanced below that *273 *i(a)-sa₅-zi/a-* refers to the physical creation of the bowl, hence 'forge', or, possibly, 'commission the forging of'. 'Purchase' is not ruled out, but seems too placid to be preceded by *273.

Some instances of the verbs *muwa-*, 'conquer', and *tupi-*, 'smite; incise text onto stone', occur with *273 as determinant. This sign is also found determining the noun *warpi-*, 'skill'. Combining these apparently disparate meanings in relation to the beaten silver of ANKARA 2 leads to 'manufacture a metal object by hammering'. This could refer to the original forging of the bowl or to its completion by engraving the text or, by extension, to commissioning it from the relevant professionals. Although we cannot yet tell which of these actions is meant by ANKARA 2's verb *273 *i(a)-sa₅-zi/a-*, the context does seem to confine the possible range of meanings within close limits. The verb should, in my view, be either one of fabrication or one of commissioning.

The pictogram *273 could possibly be interpreted as an anvil, perhaps the small kind known as a 'stake', used by a metalworker in gold, silver or copper for beating a bowl into shape. Figure 5 shows *273, as drawn in ANKARA 2 and TELL AHMAR 6 §33, set beside a modern silversmith's stake. The upper portions of all three share a remarkably similar profile, while the differing shapes of their lower parts makes a good match for the wide variety of shapes with which individual stakes are made.

Incidentally, the metaphor of the forge would also suit KARKAMIŠ A24a2+3, §7, which Hawkins, italicising the unknown words, translates as 'he *smote* Assyria with the *firebrand*'. The verb there is *273 *hu-ta-i-li-* and the metaphorical instrument used is (FLAMMAE?) *ma-ru-*

The smoothly curving head of a small anvil, known as a 'stake', has concave and convex surfaces, on which objects such as a silver bowl are beaten into shape.

0 ———————— 5cm

This modern stake has a plain shaft, which is clamped at the angle shown.

Compare the modern stake with two versions of *273 seen below at the same angle.

*273 from TELL AHMAR 6   *273 from ANKARA 2

These two instances of *273 both show a similar head to the modern one, but all three clearly differ in their lower parts. The variations in shape would then reflect differences in purpose. Modern stakes and anvils are also made with a wide variety of projecting cones, horns, beaks, etc. Its flat base shows *273 to have been a stake or larger anvil that was free-standing with a heavy base and used upright, as indeed are many kinds of stake in use today.

*Fig. 5. Does the undeciphered HL sign *273 represent the metalworker's anvil?*

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sà-na- (Hawkins 2000: 135). If *273 does indeed depict an anvil, then it offers a conqueror an alternative to a hammer when boasting of smiting his enemy.

Turning briefly from the logographic value of *273 to its phonetic value, the final word of the TELL AHMAR 6 inscription is *wa/i-ra/i-*273-na*, recognised as the infinitive of an unknown verb (Hawkins 2006: 16). The Luwian infinitive ends in *-una*, which would thus identify the vowel component of *273 as *u*.

A-sa-ma-i(a), the reported provider of ANKARA 2, might therefore be the silversmith or the purchaser of the latter's professional services. The wording of Inscription 1 suggests that the bowl had no further recipient and stayed in the ownership of its originator. If both texts were indeed added much later than the conquest of *Tara/i-wa/i-zi/a*, a very long time later in my opinion, then they would have been added by the proud inheritors of the bowl. Perhaps what was incised was little more than a family legend about the bowl's prestigious origin, and family legends can be notoriously tendentious and inaccurate.

The usage of PRAE-na

On three other HL bowl texts, KASTAMONU (Hawkins 1993), BABYLON 2 (Hawkins 2000: 394–96, 397) and BABYLON 3 (Hawkins 2000: 396–97), the beneficiary is a god. On KASTAMONU and BABYLON 3 the bowl has been 'placed' or 'set up' (*tuwa-*, as for a stele) to or before the god by its donor. On BABYLON 3 the recipient's name, in the dative case, is followed by PRAE-*na*, presumably reinforcing the simultaneous acts of placing the bowl physically and dedicating it, and the PRAE-*na* phrase is followed by the verb, which, as normal, comes at the end of the clause.

Instead of PRAE-*na* some HL inscriptions show *pa+ra/i-na*, the syllabically written alternative. The Cuneiform Luwian (CL) counterpart is *pár-ra-(a-)an* (Melchert 1993: 166). The Luwian form underlying the HL and the CL is reconstructed as *parran*, the direct equivalent to Hittite *pēran*, allowing for the regular sound changes that distinguish Luwian from Hittite.

An HL clause is usually closed by its verb, but the third singular preterite verb *273 *i(a)-sa₅-zi/a-tá* does not mark the end of its clause. Its direct object is CAELUM-*pi*, and its indirect object is the enclitic dative reflexive pronoun *-ti*. This leaves REX *ma-zi/a-Kar-hu-ha* REX PRAE-*na* as an independent adverbial phrase, which is here placed after the verb for emphasis (Melchert 2003: 582). An appropriate idiomatic meaning for PRAE-*na* is suggested by Hittite phraseology in a comparable context, as follows.

Both HL and CL attest *parran* as postposition and preverb in Luwian. Hittite, with its larger corpus and greater range of genres, shows *pēran* in various contexts

as postposition, adverb and preverb. The basic meaning is 'in front of', and usage ranges from the physical to the abstract in much the same way as in English.

In Hittite texts *pēran* is sometimes spelled out in full and sometimes concealed behind the Akkadogram *PĀNI* or *ANA PA-NI*. Akkadian has prepositions where Hittite uses postpositions, and the Akkadian word-order when a whole Akkadographic phrase appears in the Hittite text. This mixed way of writing is conveniently illustrated by *istanani peran PĀNI* DINGIR-*LIM*, 'before the altar in front of the god' (*KBo* 4,9 i 14–15).

The Chicago Hittite Dictionary (*CHD* P: 291–311) catalogues the various meanings of *pēran*, one of which is 'during the reign of', for example the Akkadographic *ANA PA-NI ABI=YA*, 'during the reign of my father' (*CHD* P: 304–05), which would presumably have been read as Hittite *atti-mi pēran*. The known range of meanings of Luwian *parran* is rather narrower than that of Hittite *pēran*, reflecting the much smaller corpus of the Luwian languages. No Luwian construction for 'during the reign of' has been identified.

It is proposed that translating REX *ma-zi/a-Kar-hu-ha* REX PRAE-*na* as 'during the reign of king *Ma-zi/a-Karhuha*' is not only plausible as a parallel to Hittite usage, but also makes good syntactical sense of Inscription 1. It then remains to make deeper sense of the narrative in Inscription 1.

Who was Ma-zi/a-Karhuha?

The phrase REX *ma-zi/a-Kar-hu-ha* REX PRAE-*na* is translated above as 'during the reign of king *Ma-zi/a-Karhuha*'. The only time available for this unknown monarch to have reigned in or near Karkamiš is before the start of Hittite domination there in the middle of the 14th century bce under Suppiluliuma I (Hawkins 2005: 200). After that the rulers of Karkamiš are known, firstly as viceroys of Hatti, later as autonomous kings.

Opinions have differed over whether there were two kings of Hatti named Tudhaliya before Suppiluliuma I, or three, but the earliest of these came to the throne no more than 50 years before Suppiluliuma did. That half century is the window in which to place *Ma-zi/a-Karhuha* as the king of an as yet unconquered land. Synchrony with Tudhaliya's Assuwan campaign then looks entirely possible.

To continue the list of kings named Tudhaliya: between Suppiluliuma I and the fall of Hattusa came Tudhaliya IV, but Karkamiš had no kings during that time, only viceroys of Hatti. Local kings arose after Hattusa fell, and their names and successions are becoming clearer, but a *Ma-zi/a-Karhuha* is not attested. A post-Empire king Tudhaliya of Karkamiš appears in three texts, KARKAMIŠ A16c (Hawkins 2000: 82),
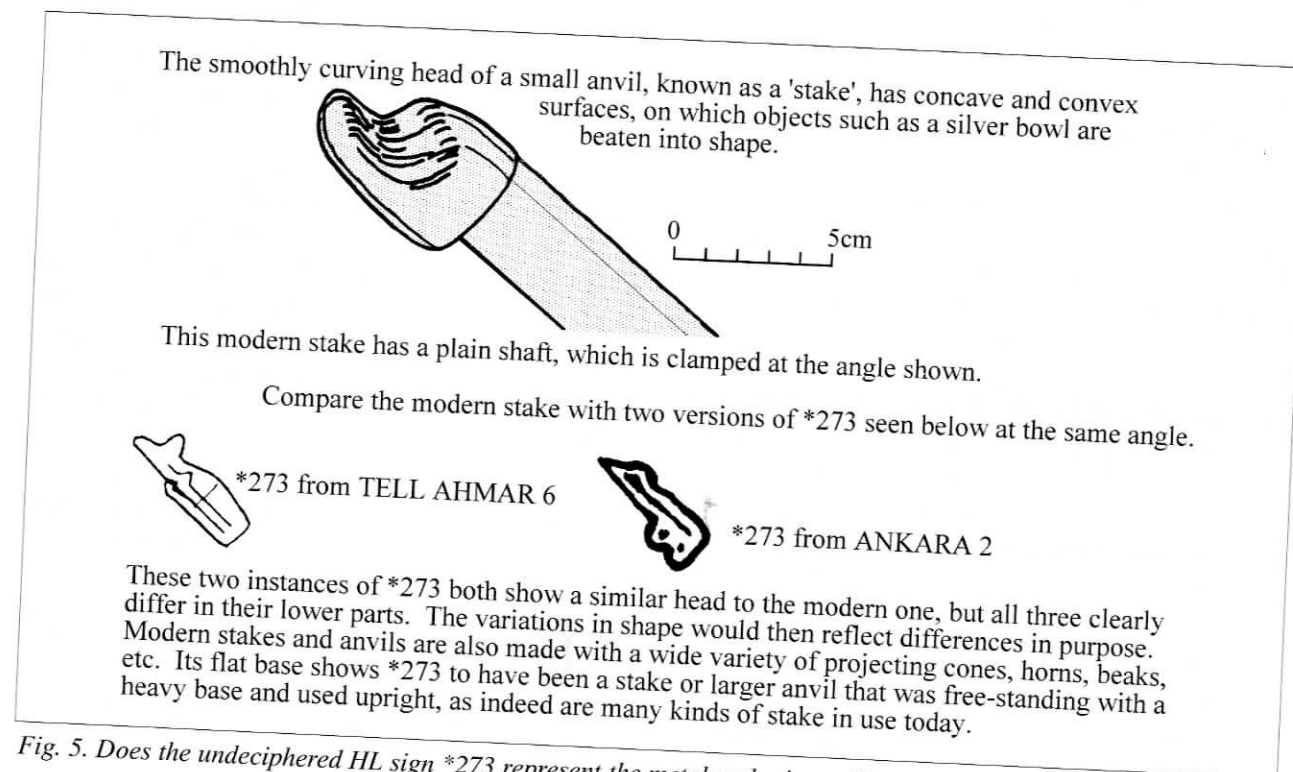
KELEKLİ (Hawkins 2000: 92–93) and KARKAMIŠ fragments a/b (Hawkins 2000: 590–91), and it is not clear whether there was more than one king of that name. Nevertheless, a king *Ma-zi/a-Karhuha* is difficult to fit in alongside any post-Empire Tudhaliya.

Linking Taruisa to Wilusa

Inscription 1 claims that ANKARA 2 is contemporary with the smiting of the land of *Tara/i-wa/i-zi/a* by *Tudhaliya labarna*. Neither of these names is an exact match for any known place or king, but both of them do closely resemble the names of the place and king featuring in one event known to current scholarship, namely, the successful Assuwan campaign conducted by one of the several kings named Tudhaliya. It is now believed that the conqueror was the first king of that name in the early 14th century bce, but debate about how many different kings bore the name has led to his being commonly designated Tudhaliya I/II.

The location and extent of Assuwa is not quite clear. It was probably the northernmost area within the Arzawa lands of western Anatolia and contained a number of small kingdoms acting in confederacy against Tudhaliya. Last in a list of 22 places conquered by him come Wilusiya and Taruisa. Under the alternative name 'Wilusa' the first of this pair of places features again in the 13th-century bce treaty with its ruler Alaksandu. Taruisa, in contrast, is not mentioned again anywhere, unless it be as *Tara/i-wa/i-zi/a* on ANKARA 2.

European culture is steeped in the story of the Trojan War. It is hardly surprising that scholars have been almost irresistibly excited by the mention in Hittite records of Alaksandu, the conjunction of Wilus(iy)a with Taruisa, the generally accepted location of these places in the Troad and their similarity to the Greek names Alexandros, Wilios/n and Troia, respectively. This paper too is concerned with Wilusa and Taruisa, but only from an Anatolian perspective. Investigation is centred on Taruisa, but, given the absence of this name from all sources apart from the list of Assuwan toponyms, we cannot do other than trace the history of Wilusa and hope that Taruisa, if not exactly the same place as Wilusa, at least shared its fate.

Was there a Hittite conquest of Wilusa?

The treaty between Muwattalli II and Alaksandu of Wilusa begins with a historical summary, whose opening clauses are damaged and problematic in places. One interpretation runs:

Formerly, when my forefather Labarna had conquered all the lands of Arzawa and the land of Wilusa, thereafter the land of Arzawa began war, and the land of Wilusa defected from Hatti, but, because the matter is

long past, I do not know from which king. [And] when the land of Wilusa [defected] from Hatti, its people were indeed at peace with the kings of Hatti from afar, [and] they regularly sent [them messengers]. But when Tudhaliya came [...] against Arzawa, he did not enter [the land of] Wilusa. It was] at peace [with him] and regularly sent [him messengers. ...] and Tudhaliya [...] forefathers in the land [of ...] (*CTH* 76, B i 2–14; after Beckman 1999: 87).

The momentum of context appears to justify the restorations shown, but opinions differ over the verb rendered above as 'to defect from', a translation which seems incompatible with unbroken peaceful dealings between Wilusa and Hatti. Starke, writing in German, offers instead 'abfallen von' (Starke 1997: 473, n. 79), an interpretation supported by Singer (personal communication) 'to be lost to' (personal communication). The Starke-Singer interpretation not only makes more plausible sense, but it also alters the implications of the whole passage.

Further episodes of Arzawan hostility matched by Wilusan peacefulness are mentioned before the tablet breaks off. The excerpt quoted above conveys the sense that Wilusa defected several times before the time of Muwattalli II, but nevertheless maintained diplomatic relations with Hatti indirectly, either during every single such episode or, duplicitously, before each episode and right up to the moment of defection. Yet the surviving treaty text not only contains no hint of condemnation of repeated Wilusan treachery, but also explicitly states that Wilusa was left alone during each campaign against Arzawa.

In contrast, the Starke-Singer view means that the Arzawa which revolted on each occasion excluded Wilusa and that Muwattalli is recalling Wilusa's unflagging loyalty throughout those episodes, despite not being able to identify the Hittite king at the time of the first episode. Wilusa remained at peace with Hatti throughout subsequent Hittite campaigns against Arzawa, and direct contact was interrupted while the Arzawan territory inbetween was hostile, requiring indirect dealings to be maintained 'from afar' instead. From the Manapa-Tarhunda letter we know that the part of Arzawa separating Hatti from Wilusa was the Seha River Land.

The Starke-Singer interpretation implies that the strength and independent nature of Wilusa was such that it never itself fell victim to Arzawan aggression, nor had it ever been a vassal of Hatti. Its location beyond the fringe of Hittite direct control and its apparent ability to sustain itself for several centuries implies a status for Wilusa as a strong and desirable ally or, indeed, as a coveted prize. A direct reference to international compe-

tition over Wilusa is found in the Tawagalawa letter (*CTH* 181), sent by a Hittite king to the king of Ahhiyawa: 'Now, as we have reached agreement on Wilusa, over which we went to war, ...'. No reference is made to a conquest of Wilusa itself.

To Muwattalli's eye Wilusa was evidently within Arzawa geographically, but was not one of the rebellious Arzawa lands. The annals of Tudhaliya I/II describe four campaigns against the Arzawa lands, including the one against the Assuwan confederacy from which the roll-call of the defeated ends with Wilusiya and Taruisa. Muwattalli II, however, writing about a century later, refers to only one campaign by Tudhaliya against Arzawa, a campaign throughout which, moreover, Wilusa remained either an ally of Hatti or at least on terms of friendly neutrality. If these two conflicting accounts are reporting the same Arzawa campaign, then the amnesia apparently displayed by Muwattalli II seems, at first sight, either a deliberate diplomatic tactic towards Alaksandu or the result of gaps in the state archive.

However, a further interpretation is possible, one that requires neither Tudhaliya nor Muwattalli to have been economical with the truth and one also that does not require Muwattalli to have been too grossly ignorant of his own country's history. This interpretation would maintain that Tudhaliya's capture of the Assuwan states was not entirely by force of arms and that Wilusa was won over by diplomacy instead, though his terse summary of the campaign did not stretch to that detail. One may then further speculate that Wilusa had been playing a minimal role in the Assuwan confederacy as a strategy for self-preservation, which would be entirely consistent with its behaviour on later occasions when neighbouring parts of Arzawa took up arms against Hatti.

How reliable are historical allusions?

Of the Alaksandu treaty's preamble a leading authority writes that,

the passage is of dubious historical value. To judge from sections 1–4 of the Telepinu proclamation, Labarna I's military exploits, impressive though they were, did not extend into western Anatolia, certainly not on the scale indicated by Muwattalli (Bryce 2003: 48).

Telepinu reigned some two and a half centuries before Muwattalli II and perhaps only about a century and a half after Labarna and thus much closer to the events. I suggest that a somewhat kindlier view of Muwattalli's confessed ignorance is to hand.

The same scholar remarks elsewhere that,

there must have been major disruptions to the capital's archives on several occasions during its history – when Hattusa was sacked in the early fourteenth century, in the course of the capital's relocation to Tarhuntassa in the late fourteenth or early thirteenth century, in the course of conflicts in Hattusa between the opposing forces of Hattusili and Urhi-Teshub, and perhaps most of all during the major redevelopment of Hattusa in its final years (Bryce 2002: 65).

Hattusa itself had been captured and burnt down not much more than a half century before the time of Muwattalli II, so it is hardly surprising that the record was somewhat patchy and evidently not complete enough for Muwattalli to recover the earlier history in full detail. It is laudable that he felt able to confess his uncertainty. Perhaps he supposed that Wilusa's own records might be more complete and able to gainsay anything incorrectly asserted. In attempting to fill the gaps, Muwattalli may have had to rely upon oral tradition and, presumably, also upon the monuments set up by previous kings. That in turn suggests the possibility that history recovered that way was more akin to saga or legend and was also acknowledged in royal circles to be so.

Let us suppose, for the sake of argument, that Hattusa's royal archives stayed completely intact from the earliest times right to the final collapse of the Hittite state, and that local copies then survived till much later in Karkamiš, or indeed anywhere else where continuity was maintained after the fall of Hattusa. How likely is it that the populace at large would have had access to the records? It would seem very unlikely. Would the records be available to someone of relatively elevated status who was making ANKARA 2, or having it made, at some time or other during those centuries? It still seems unlikely. How accurately would a tradition of oral record among the people maintain several centuries of close harmony on every nuance and historical point with the parallel but unpublished official record? That also seems most unlikely.

The royal archives, however, did not stay intact. They suffered periodic disruption, and so vernacular versions of Hittite history, even if somehow 'refreshed' from the official version, would have diverged progressively from it. In contrast, it would seem that 'refreshment' may have flowed in the opposite direction, if interrupted royal annals did indeed need to be made good from less formal sources.

Muwattalli's archivists and advisers could not establish which of his royal forebears had been the first to suffer loss of direct contact with Wilusa, while his

mention of the prior conquest of Arzawa and Wilusa by his forefather Labarna may have been a retrospective assumption and not a matter of accessible record. We cannot tell what sources Muwattalli had concerning Labarna, but, as mentioned above, they appear to conflict with the Telepinu proclamation, which may thereby have been among the documents unavailable to Muwattalli. A comparable case may be made for Muwattalli's having suffered a lack of detailed information about Tudhaliya I/II's Assuwan campaign as well.

If Muwattalli II truly had little more than a half century of fully reliable historical record, it seems that Labarna I's reputation could have outgrown historical fact during the two centuries between Telepinu and Muwattalli. The concommittant impression is that this reputation had been transmitted orally from one royal generation to the next and that Muwattalli had nothing better as a source. If Labarna's conquests did indeed not extend as far as Wilusa, then it would not have come under Hittite control in his time, and defection from Hatti was, thus, not a possibility. It is of course conceivable that Labarna might have made contact with Wilusa without military conflict and established the long-term alliance being celebrated by Muwattalli, but it may be that alliance with Wilusa was in fact no older than the Assuwan campaign, which itself was recalled only vaguely. Despite gaps in the record, then, Muwattalli is correctly celebrating the complete absence of Wilusan hostility.

Whereas recent political events and the last few generations of one's family may be relatively accurately recalled and retold, informal narratives about the more distant past tend to coalesce around a few legendary figures and prestige events. Outside the orbit of the palace, the stories told round a campfire or over an aristocratic dinner may have had more in common with *Beowulf* or the *Iliad* than with bureaucratic annals. It is suggested above that this oral factor may have been active in the framing of the initial clauses of the Alaksandu treaty's historical introduction. The *Catalogue des textes hittites* lists the narratives set in the time of Hattusili I in the 17th century bce under the significant heading 'Legendary accounts of the Hurrian Wars' (*CTH* 16), an explicit acknowledgement of this interplay between literature and history.

What became of Taruisa?

Taruisa was last mentioned many paragraphs above, and investigation has focused on Wilusa. The justification for this is the linking of the two places in Tudhaliya's summary of his Assuwan campaign, coupled with the close bonding in Homeric literature between Troy and Ilium. The hope is that the fate of Wilusa can be presumed to be that of Taruisa too.

If the Alaksandu treaty's reference is indeed to the same Tudhaliya as the one who conquered the Assuwa confederacy, and if, of the campaigns against Arzawa that he may have made, it is the Assuwan campaign that Muwattalli is citing, and if the missing place-name in the treaty preamble quoted above is correctly restored as 'Wilusa', and if Taruisa and Wilusa were indeed as closely bonded as Troy and Ilium, then Taruisa would presumably have suffered the same defeat, or enjoyed the same lack of assault, as Wilusa had. The Alaksandu treaty's version of events, in which no hostility occurred, seems very unlikely to make a conquest of Wilusa, and hence, possibly, of Taruisa too, remembered as Tudhaliya's greatest victory.

The conquest of Assuwa has a memorial in the form of the bronze sword, of probable Aegean origin, found in Hattusa and inscribed in cuneiform without the dedicator's name but dated stylistically to Tudhaliya I/II. It is held to be part of the spoils from that campaign, dedicated by Tudhaliya to the Storm God. Can we take this find as an indication that the Assuwan campaign was something exceptional and did indeed leave a lasting mark in the popular mind? If so, could an evolving oral narrative come to focus on just one of the places in Assuwa, Taruisa, and lose interest in Assuwa as a whole and in all its other 21 members? This is not impossible, though why it should have happened is a matter of conjecture, and a certain passage of time would need to have elapsed while history was mutating into legend.

One conjecture is that, if ANKARA 2 is indeed referring to Taruisa, could the survival of this name but of no other be related to its being the final entry in Tudhaliya's list of Assuwan rebels? According to this conjecture *Tara/i-wa/i-zi/a* would be the orally boiled down residue of a shorthand phrase such as 'the list of places ending with Taruisa', leading to Taruisa thereby standing for the whole Assuwan confederacy, whether or not force had been used against Wilus(iy)a and, by presumed extension, against Taruisa too.

History or legend?

A comparable but simpler question arises concerning Arthurian tradition in Europe: whether the *Camalot*, *Camelot*, etc, of 12th-century ce French romance continues the name, though probably not the location, of *Camulodunum*, the capital of Roman Britain from some seven centuries earlier (Morris 1977: 138). Lack of evidence about the intervening years has allowed scholars to maintain an inconclusive debate for and against. The *Tara/i-wa/i-zi/a* = Taruisa question is more complex in that the readings of the toponyms themselves are part of the debate too.

Taruisa is a non-Hittite name, written in cuneiform as *Ta-ru(-ú)-i-ša*, while *Tara/i-wa/i-zi/a* is the HL rendering of a toponym of unknown location and linguistic affiliation, which offers several alternative transcription possibilities. The spelling rules of HL would allow not only *Tarwiza*, but also, for instance, *Triwanzi*. No candidate toponym other than Taruisa is known that might match any of the possible combinations of HL sign values. This is very much a join-the-dots puzzle where many of the dots are of uncertain position or have been erased altogether.

In treatments of the question one sometimes sees *Ta-ru(-ú)-i-ša* and *Tara/i-wa/i-zi/a* rendered as *Tarwisa* and *Tarwiza* respectively, as though presupposing their equivalence. Whether or not this turns out to be a valid assumption, it is the only line of enquiry explored in this paper. If both names are non-Hittite, some form of Luwian is generally presumed for each, with a derivation possibly from *taru-*, 'wood'.

The sibilant series are among the more problematic elements of many deciphered ancient scripts, and even languages with living descendents are not immune. For instance, the pronunciation of *z* and *ss/tt* in the Greek dialects of pre-Classical times is debated to this day. Luwian speech is believed to have been in use for a long time and over a large area, and our knowledge of its dialectology is sparse. With such a background of uncertainty it seems hardly productive to attempt to reconcile cuneiform *š* with HL *z* in a single toponym of undocumented linguistic provenance, especially as a considerable distance in space and time may separate the two writings.

Returning to the conjectured displacement of Assuwa by Taruisa in ANKARA 2's date formula, we may speculate that, if indeed the equation *Tara/i-wa/i-zi/a* = *Ta-ru-(ú-)i-ša* = *Troia* holds good, then Bronze Age Troy may have enjoyed the same reputation east of the Aegean as in Mycenaean Greece to the west of it. Troy controlled both the southern trade route between Anatolia and Thrace and the sea route between the Mediterranean and the Black Sea. The wealth and power of the place would have made it famous throughout the region as a tough nut to crack and a rich prize for a conqueror.

Some three centuries before Troy fell to the Mycenaeans Tudhaliya's supposed capture of it, even if possibly bloodless, or perhaps even because no battle was needed, may have grown in popularity as a saga among the Hittites and eclipsed all else that was achieved in his Assuwan campaign by conventional force of arms. The story would then have been cherished by the heirs of Hatti in their post-Empire Karkamiš home.

What does a bowl have to say?

The native Celts of northern Britain in the second century ce seem to have used their skills in enamelling to create

a souvenir industry for those of their Roman overlords who were stationed on the frontier along Hadrian's Wall, or perhaps only visited it. 'Soldiers could buy *paterae* decorated in colourful enamel that depicted aspects of the Wall. Some, like the Rudge Cup and a *patera* from Amiens in France, combined a stylised look at the Wall with a list of military place-names' (Künzl 2008). In these instances an inscribed metal bowl served as a memento of military activity performed in a named location, or to be claimed as such, though, in contrast to ANKARA 2, associated with neither a specific conflict nor harking back to an earlier era.

A bowl with an inscription around the brim offers a particular advantage for display purposes. Its flank presents a visible surface, while its function as a container is unaffected by the text. A bowl is also ornamental and can be positioned round the house to catch the eye of anyone the owner would like to impress. A single HL word conveys simple ownership on the TRAGANA bronze bowl (Hawkins 2000: 569) and on the NIMRUD silver bowl (Hawkins 2000: 570), which each bear a name immediately below the brim.

An incidental observation at this point is that NIMRUD offers two further similarities with ANKARA 2, albeit circumstantial. Firstly, NIMRUD was excavated from a tomb, and, as mentioned above, ANKARA 2 may have lain in a tomb while SCC was reducing it to fragments. Secondly, the content of each seems to challenge known dating criteria. The difficulty in reconciling ANKARA 2's epigraphy with its narrative has led to much discussion and is the motive for this paper. The name on NIMRUD is probably Santasarma, but Hawkins goes on to observe that 'an attribution to Sandasarme of Hilakku would make this inscription almost half a century later than the latest datable Hieroglyphic inscriptions' (Hawkins 2000: 570).

The small, cup-like ŞARKIŞLA bronze bowl (summarised in Mora 2007: 515–16) resembles ANKARA 2 in having two HL texts just below the rim, but differs in that these texts are on the inside surface. This suggests that public display was not their purpose. It seems that both inscriptions consist of one word each and that at least one is a personal name. The other may also be a name, possibly a divine one, though lacking a determinant. There is no further wording to provide a narrative context. If both words are indeed names, and if both are human, then the roles played by each person in the finishing of ŞARKIŞLA may perhaps be compared to those played respectively by *A-sa-ma-i(a)* and the scribe *Pi²-t[i²]-[...]* in furnishing ANKARA 2. If, on the other hand, one of ŞARKIŞLA's names is a deity, then the other is likely to be the dedicator, as found on all the other bowls that bear more than one name.

The stone bowl BABYLON 2 (Hawkins 2000: 394–97) is dedicated to celestial Tarhunza as a thank-offering in a narrative of at least four clauses that also mentions the Karkamiš god Karhuha. Before slipping into the first person to give his reason for dedicating it, Runtiya, the bowl's donor, states that he 'made' it 'for Tarhunza', *Tarhunti izita*, with the god's name in the dative and using the same verb as ANKARA 2 has at the end of Inscription 1. This verb is not taken to imply that Runtiya was the stonemason who made the BABYLON 2 bowl with his own hands, nor even necessarily that he had it made, but that he did at least perform the acts of procuring the bowl somehow and then dedicating it. In this instance something like 'did' may be a better translation than 'made', and a similar consideration may turn out to apply to ANKARA 2's *izita* as well.

The KASTAMONU bronze bowl (Hawkins 1993; Mora 2007: 516) and the stone bowl BABYLON 3 (Hawkins 2000: 396–97) are both dedicated to a god with a single clause that uses the verb *tuwa-*, 'place'. KASTAMONU's donor is securely dated to the 13th century bce, and its dedication is written largely logographically. It is to the 'scribe god', without PRAE-*na* and with no case ending shown. The two stone bowls, BABYLON 2 and 3, are linearly incised and dated epigraphically to the ninth or eighth century bce, which may well have been the era to which ANKARA 2 also might have already been dated without controversy, were it not for its references to *Tudhaliya labarna* and the land of *Tara/i-wa/i-zi/a*.

Tara/i-wa/i-zi/a was discussed above. About the phrase *Tudhaliya labarna*, Hawkins summarises the usage of the term *labarna* (Hawkins 2005: 197, 200) and concludes that the phrase is not only without parallel in HL or cuneiform, but also could be of any date. Could *Tudhaliya labarna* have been an unofficial coining, the stock term for a semi-legendary figure, whose name and title had acquired a quasi-historical glamour and an unhistorical shape?

What's in a Hurrian name?

The grammatical subject of both clauses in Inscription 1 is *A-sa-ma-i(a)*, a name similar to the cuneiform *Aš-mi-ia*, witness to a contract from Emar (Mora 2007: 518) and which 'is now seen ... to be a hypocoristic of a Hurrian *asmu/i* name' (Hawkins 2005: 196). The subject of the single clause in Inscription 2 is read as *Pí⸢²⸣-t[i⸣²]-[...]*, 'which probably represents the Hurrian element *Benti(p)-*' (Hawkins 2005: 197). Thus the primary personages in both inscriptions are very likely to have borne Hurrian names.

Hurrians were present among the populations of the whole area from which the HL corpus comes, with the possible exception of western Anatolia. They were also present during all the centuries spanned by the corpus, though the Hurrian language itself appears to have declined from the middle of the second millennium bce and would have been effectively extinct as a living idiom well before the latest HL texts in the seventh century bce (Wilhelm 2004: 95). However, after a population has given up a spoken language it is common for personal names originating in that language to survive the transition and to persist in use for a very long time.

Hurrian names are therefore not a great surprise in the HL corpus and do little to help delimit the time and place of composition of ANKARA 2's inscriptions. There are nevertheless three observations which, though slight and perhaps coincidental, are worth bringing in. The first has already been mentioned, the similarity of the name *A-sa-ma-i(a)* to *Aš-mi-ia* from Emar. Emar, refounded by Mursili II, lies 100km due south of the next-door kingdom of Karkamiš, also on the west bank of the Euphrates. Does the similarity of two personal names indicate a local onomastic preference and point to an origin for ANKARA 2 in that region?

The second observation is that the Hurrians seem to have been particularly associated with technological skills. Kikkuli's instructions for training chariot horses are one well-known instance. Metalworking was another area of Hurrian expertise, and silver, a metal of symbolic purity in Anatolia and the subject of the Hurrian myth of the young god Silver, seems to have had special significance. If any such aura was attached to ANKARA 2, may one suppose that the bowl's being of silver may have added a stamp of veracity to the historical claim incised upon it? The mystery verb *273 *i(a)-sa₅-zi/a-* conceals *A-sa-ma-i(a)*'s role, which, as examined in this paper, may have been connected with the physical production of the bowl.

The third observation is that Tudhaliya I/II had to fight his way to the throne against a Hittite rival who had Hurrian support (Bryce 2005: 121–22). It is reasonable to suppose that knowledge of Hurrian opposition to the first Tudhaliya would have clung to the name when borne by later kings too, both during the Empire and among its successors. Consciousness of Hittite ancestry, both personal and cultural, stayed strong in Karkamiš, even centuries after the fall of Hattusa. Did a man with a Hurrian name feel it important to stress his loyalty to the ruling family by volunteering himself a man of Hatti?

The persistence of tradition may be glimpsed on the ALİŞAR ostracon graffito (Hawkins 2000: 568–69), whose first word reads *ha-tu-sà-mu-wa/i-s[a]*, the possessive form of the compound name *Hatusamuwa*. Both the stratification of the find and the form of the signs accord with an eighth-century bce date. Hawkins comments that, 'it is very surprising to find the toponym

Hattusa occurring as an onomastic element more than four centuries after the city's destruction' (Hawkins 2000: 569). Bronze Age and Iron Age societies remained primarily heroic in nature, despite the impression of unimaginative bureaucracy sometimes given to the modern eye by their excavated archives. Do we today underestimate the strength of historical or quasi-historical awareness among those who continued to write HL?

Alişar lies in the Hittite homeland, where Hatusamuwa's ostracon provides evidence of the continuation both of a post-Empire population and of something of their traditions. The fall of Hattusa may well have occurred during the reign of Ku(n)zi-Teshub, King of Karkamiš and hereditary viceroy of the region on behalf of the Great King in Hattusa. Ku(n)zi-Teshub's grandsons refer to him as 'Great King', whereas his father, Talmi-Teshub, had been simply 'King'. This suggests that Hattusa and its monarch had both passed away, allowing the newly vacant position of 'Great King' to be accorded retrospectively to Ku(n)zi-Teshub by his descendants. Whether he had assumed the title in his own lifetime we do not yet know.

Ku(n)zi-Teshub was the great-great-great-grandson of Suppiluliuma I and may well have found himself at that moment the only surviving member of the Hittite royal house who still had a kingdom. Pride in ancestry, under such circumstances, is likely to have been not only retained but positively encouraged.

What is *A-sa-ma-i(a)*'s role?

Inscription 2 includes the profession of the man named there, yet *A-sa-ma-i(a)* is labelled in Inscription 1, the main text, simply as a man of Hatti (or Hattusa), in a context where rank and status might be expected to be emphasised. If there was nothing else special to mention about him, was he an ordinary person with an extraordinary inheritance to boast about or was being a man of Hatti somehow a kind of professional identity in itself?

A-sa-ma-i(a) is described as REGIO.*HATTI* VIR₂, a phrase with no parallel in HL, although its equivalent is known in cuneiform. This cognomen-like term is curiously reminiscent of that of Uriah the Hittite in the Hebrew Bible, who lived in the tenth century bce and to the south of the area where the HL Late corpus was still actively growing. Much has been written about what is meant by 'Hittite' in relation to Uriah, but there is perhaps little to be made of the similarity in an era when an ethnic designation was quite usual.

Inserting *A-sa-ma-i(a)*'s affiliation could be taken as suggesting that a Hittite lineage was itself a rare and treasured attribute, which would be appropriate for a

context after the fall of Hattusa. Although Hurrians and Hittites had long been intermingled, it may have been felt important for a man with a Hurrian name to reaffirm explicitly that he was either ethnically a Hittite or loyal to a Hittite tradition.

As an alternative explanation it is conceivable that *A-sa-ma-i(a)* was intended to be defined as 'a man *from* Hatti', i.e., whether ethnically Hurrian or not, someone who has relocated from Hatti to Karkamiš. This migration by *A-sa-ma-i(a)* or his forebears would have occurred before Suppiluliuma I's conquests in north Syria, at or before a time when a king named *Ma-zi/a-Karhuha* could have existed there. The citing of one of Tudhaliya I/II's conquests in the opposite direction, into western Anatolia, would then have magnified that king's reputation, while being a man of Hatti may indicate that *A-sa-ma-i(a)* was a proud early settler, not a carpetbagger in Karkamiš in Suppiluliuma I's wake.

The foregoing speculations do not require ancient Hittite identity to have persisted for centuries into the Iron Age solely through legend or historical memory. It is sufficient only to remember that Hatti was the normal name for the area of Syria and western Upper Mesopotamia from the late 13th century bce (Singer 1999: 655, n. 150, 658–59, n. 170) through to the last of the HL corpus. This makes 'A-sa-ma-i(a) the man of Hatti' in a document from first millennium Karkamiš less remarkable than if ANKARA 2's inscriptions were to have come from somewhere else and with a much earlier date.

The key to the role of ANKARA 2's reputed provider lies with the mystery verb *273 *i(a)-sa₅-zi/a-*. Hawkins 2005 proposes a verb of dedication, as on the KASTAMONU and the BABYLON 3 bowls, while wondering whether there is a connection with *i(ya)sa-*, 'buy'. As already described, I am inclined towards a verb of making or possibly of commissioning, as this seems the best fit for the various other contexts in which *273 makes an appearance.

The Franks casket, a comparable document

The oldest piece of Anglo-Saxon poetry so far known is carved in runic script on a rectangular casket of roughly similar size to ANKARA 2 and now in the British Museum (Room 41: 'Europe AD 300–1100'). The casket was discovered in France in the 19th century and donated to the museum by Sir Augustus Franks, from whom it takes it name, but its previous history is unknown. As with ANKARA 2, the casket's origin has had to be deduced from the object itself. On the evidence of script, language and pictorial style it is now agreed to be from the north of England and dating from the first half of the seventh century ce.

Just as ANKARA 2 appears to refer to its own creation, so the casket's front panel tells us that it was carved from bone taken from a beached whale. Its other panels depict scenes from Roman, Germanic and Christian stories, each surrounded by runic text. The casket was evidently a prestige object and easy for its owner to display.

The left panel shows four armed men observing two children being suckled by a wolf, and the accompanying runic inscription contains the words *Romwalus and Reumwalus ... in Romæcœstri*, 'Romulus and Remus ... in Rome'. Here one sees an allusion to legendary events from centuries before, in which two personal names and one place name are written with non-standard spellings. However rigorously we might wish to analyse the evidence that ANKARA 2 offers about itself, we must allow that those who inscribed it may not have had a very formal source and that the names there may diverge in form from those in the other sources known to us.

Other HL documents showing contradictory dating criteria

In any literate culture one may find the occasional text claiming a surprising provenance for itself or for some artefact associated with it. Some of these texts may in fact be taken at face value for centuries before their authenticity is challenged. Indeed, is not every document potentially a candidate for this fate?

Attempted archaism is a familiar tactic when the author of a text seeks to convey the impression of antiquity, whether justified, merely presumed or deliberately feigned. Once scholarship has learnt enough to detect it, erroneous archaism reveals itself and gives the game away. The HL corpus is not without instances. For example, in the TOPADA inscription, 'The unusual sign forms suggest a deliberate attempt at archaism with varying degrees of success' (Hawkins 2000: 460).

A converse example is supplied by the group of three KIZILDAĞ inscriptions. The carved figure of king Hartapu is associated with KIZILDAĞ 1 and is dated on stylistic grounds to perhaps the eighth century bce. However, the palaeography of all three texts,

shows only archaic features with no traces of innovations and ... must reinforce the arguments for a high dating, probably to the period shortly after the fall of the Hittite empire. It may be necessary to assign the inscriptions and the figure to the two widely differing dates (Hawkins 2000: 434).

In short, the inscribed rock face was pictorially upgraded a few centuries after the texts were cut, perhaps for reasons of prestige. Among the motives for adding

the figure of Hartapu may perhaps have been the desire to hark back to a past era of greatness. If so, much the same may also be said of the texts added to ANKARA 2.

A speculative interpretation

A bold and idiomatic translation of Inscription 1 might be, '*A-sa-ma-i(a)* the Hittite forged this bowl for himself during the reign of king *Ma-zi/a-Karhuha*. He made it in the year that Tudhaliya *labarna* smote the land of *Tara/i-wa/i-zi/a*'. There is no hint of donation or dedication, which makes ANKARA 2 different from other inscribed bowls.

Is ANKARA 2 an ancient 'folly', a faux relic, entirely constructed long after the fall of Hattusa so as to add constructed prestige to its owner by harking back to a legendary event? Alternatively, is it a genuine relic from more ancient times, later adorned with a description of its purported origin? Or is it, as I am disinclined to believe, to be taken at face value as a historical document contemporary with a notable campaign of a Tudhaliya of the Hittite Empire?

I wish to offer the hypothesis that ANKARA 2 was an heirloom, retained in the area of Karkamiš by the descendants or heirs of its supposed maker, *A-sa-ma-i(a)*, whose Hittite identity was unexceptional in the Karkamiš area in post-Empire centuries, although it could well have its origin long before and elsewhere. His family, or perhaps even a craft guild of scribes or silversmiths, retained ownership of the artefact and were able to add the inscriptions to record the story handed down to them. The fact that they did this may indicate that traditions were already starting to fade.

The true date and place of manufacture of ANKARA 2's bowl cannot be deduced from its inscriptions. It could indeed predate both texts by centuries, and the association with *Tudhaliya labarna* and *Tara/i-wa/i-zi/a* could be a true allusion or a false ascription or mere wishful thinking If one allows for deliberate archaisms in spelling, such as would have been visible on public monuments and other documents of known antiquity, then the script used could be very late.

The untypical phrases *A-sa-ma-i(a)* REGIO.*HATTI* VIR₂, *tara/i-wa/i-zi/a-wa/i* (REGIO) and MONS.[*tu*] LABARNA+*la* have the look of stock terms, parroted in legend from one generation to the next. If found in a verse context, these might be the stuff of what could be called formulae, as found in Homeric epic and also suspected in Hittite literature (McNeill 1963; Durnford 1971).

It remains to try to account for Inscription 2, written in a different hand and naming a scribe. A family or guild of silversmiths or scribes would have been in a position to add both texts, and we can only surmise what

is in Inscription 2's lacuna, but it probably has little or nothing to contribute to the story told in Inscription 1. If the missing fragment were to be found, it is likely to say more about scribes than about great events.

One possibility is that Inscription 2 was the first to be incised, telling not very much, and another hand added Inscription 1 later so as to give the world a fuller and more impressive narrative. Whether the two texts were added only minutes apart or separated by many years is one question that has to be left hanging in the air, but both appear to belong to the Late corpus, perhaps even to the latest phase of it.

Nor can we tell whether the bowl was new when first inscribed or already in existence for a theoretical maximum of some seven centuries. The first of these extremes would mean that it was deliberately created as a faux relic. The second extreme would mean that ANKARA 2 was a genuine heirloom, but we cannot tell whether the narrative on it truly belonged to it or was a fanciful ascription. Even if the bowl were not to belong to the story told in Inscription 1, it would matter little, as it is really only the story that concerns us.

Treating the persons and apparent date formula in Inscription 1 as a form of literature prompts other kinds of question. However accurately recorded, and whoever is meant, the conjunction of names in the second sentence of Inscription 1 nevertheless offers an apparent synchronism from the days of Empire which has to be considered seriously. Our questioning then becomes less focused on the bowl and more on its cultural context.

Hittite control over parts of north Syria had been reversed by the expansion of Mitanni, which had fought against Hatti early in Tudhaliya I/II's reign and had perhaps backed the faction that had sought previously to oppose his accession. The bowl, it is suggested, was inscribed in or near Karkamiš in an overtly archaising Late corpus style and at a time when that area had for centuries been back under Hittite control or influence, firstly as part of the Empire and then as a successor state.

If there was a ruler of Karkamiš named *Ma-zi/a-Karhuha* in the early 14th century bce, when Tudhaliya I/II may have conquered *Tara/i-wa/i-zi/a*, what was *Ma-zi/a-Karhuha*'s relationship with the contemporary king of Mitanni, who probably held overlordship of that area at that time? Could *A-sa-ma-i(a)*'s existence during a time of Mitannian domination be why his persisting allegiance to Hatti is proclaimed?

The capture of Troy by the Mycenaeans resonates down to the present day. If the city of Troy is indeed the same place as that named *Tara/i-wa/i-zi/a*, then did Tudhaliya's capture of it become the subject of legend in Anatolia several generations earlier than the conflict enshrined in the *Iliad*?

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Patterns of Iron Age interaction in central Anatolia: three sites in Yozgat province

Lisa Kealhofer,[1] Peter Grave,[2] Ben Marsh,[3] Sharon Steadman,[4] Ronald L. Gorny[5] and Geoffrey D. Summers[6]

[1]Santa Clara University, [2]University of New England, [3]Bucknell University, [4]SUNY Cortland, [5]University of Chicago and [6]Middle East Technical University

Abstract

The cultural and political changes that happened in Anatolia after the collapse of the Hittite Empire have only recently been recognised as a significant, but as yet unexplained, phenomenon. Here we present the results of analyses of ceramics from three sites south and southwest of the present-day town of Sorgun – Çadır Höyük, Kerkenes Dağ and Tilkigediği Tepe – to identity how regional groups within the Hittite core area regrouped in the aftermath of the collapse. Ceramic analyses provide a means to assess both cultural continuity and the scale and nature of interaction in a region. Results suggest some evidence of cultural continuity at Çadır Höyük from the Late Bronze Age into the Middle Iron Age, and highlight the variable local responses in the aftermath of Hittite collapse.

Özet

Anadolu'da, Hitit İmparatorluğu'nun yıkılmasından sonra ortaya çıkan kültürel ve siyasal değişiklikler ancak son zamanlarda önemli sayılmaya başlanmış, fakat henüz tam olarak açıklanamamış bir olgudur. Bu makalede, imparatorluğun yıkılması sonucunda Hitit merkez alanı içindeki bir bölgenin yeniden birleşme çabalarının tespiti için, günümüz Sorgun ilçesinin güney ve güneybatısındaki üç yerleşimden; Çadır Höyük, Kerkenes Dağı ve Tilkigediği Tepe'den seramik örneklerinin analiz sonuçlarını sunmaktayız. Seramik analizleri, bize bir bölgedeki kültürel devamlılığı, onun derecesini ve aynı zamanda o bölgedeki etkileşimin kökenini belirleme olanağı sağlamaktadır. Sonuçlar, Çadır Höyük'de Geç Tunç Çağı'ndan Orta Demir Çağı'na kadar uzanan kültürel devamlılığa işaret etmekte ve Hitit İmparatorluğu'nun yıkılması sonucunda ortaya çıkan değişken yerel tepkileri vurgulamaktadır.

The region circumscribed by the large bend of the Kızılırmak river in central Anatolia has long been a focus for Bronze Age archaeologists interested in the Assyrian Colony period and the development of the Hittite Empire. The reformation of societies following the 12th-century BC collapse of the Hittites has received comparatively little attention beyond von der Osten's 1937 publication of Alişar IV and V. Consequently, our understanding of societal responses to the collapse of Hittite political control in this region and the dynamics of polity building during the first millennium BC has remained largely descriptive and limited to relatively few sites.

The present study focuses on a cluster of three Iron Age sites in Yozgat province, central Turkey, that provide an unusual opportunity to look more closely at how

regional interaction was reconfigured during the first millennium BC inside the bend of the Kızılırmak. The three sites, south and southwest of Sorgun, are: Çadır Höyük, Kerkenes Dağ and Tilkigediği Tepe (fig. 1). Çadır Höyük is a mounded site with evidence of occupation from the Chalcolithic to the Byzantine period. Kerkenes Dağ is a large fortified (late) Middle Iron Age hilltop city with a relatively short occupation (<150 years?). Tilkigediği is a small, hilltop Iron Age site 3km northeast of Kerkenes.

This paper is part of a larger study of Iron Age contexts across western and central Anatolia (the Anatolian Iron Age Project). The over-arching goal of the broader project is to move beyond site-based studies to understand how exchange and interaction patterns