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Cover: Tall-shouldered jar with an inscription of the Middle Kingdom princess Itakayet from Tomb VII at Tell Mishrife/Qatna (photo: M. Steinmetz; drawing: A. Gubisch, © Qatna excavation project, University of Tübingen); Cylinder seal depicting the seated Šamaš with two attendants, Keel (2007: 278, no.154), Akkadian style, found in a tomb from the 7th century B.C.E., Jerusalem, © Bible+Orient Museum, Fribourg/Foundation Bible+ Orient

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Hittite-Egyptian Synchronisms and their Consequences for Ancient Near Eastern Chronology

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Two phases of the relationships between Ḫatti and Egypt have long enjoyed special attention from both Hittitologists and Egyptologists: the Amarna age in the second half of the 14th century B.C.E. and the so called Pax Hethitica, namely that phase of peaceful relations between the two kingdoms that begins with the treaty ratified by Ḫattušili III and Ramesse II and continues until the collapse of the Hittite empire. The time span between these two phases has been studied much less frequently, with the exception of one event: the battle of Qadesh, which can probably be regarded as one of if not *the* most often discussed event in the common history of the two realms. This paper attempts to contribute toward filling this gap by investigating some of the other historical events that took place during this phase as well as their chronological setting and their consequences for Ancient Near Eastern chronology.

HITTITE-EGYPTIAN RELATIONS AT THE END OF THE 14th CENTURY B.C.E.

The combined analysis of old and new Hittite sources seems to suggest that at this time the relations between Ḫatti and Egypt might have enjoyed a phase of *détente* and were not always as hostile as one might assume.

An important new contribution to this question is represented by the recently expanded Hittite tablet KBo 3.3++ (CTH 63.A), which hands down the text of a Dictate of Muršili II to Tuppi-Teššub's Syrian Antagonists (see Miller 2007b). Within this text, the Hittite king mentions the possibility of concluding a peace treaty with Egypt:

CTH 63.A – KBo 3.3++ ii 55–61

So I, My Majesty, made the [fo]llowing decree concerning these civilian captives of Kinahḫa: "If the king of Egypt (58ff.) and I conclude peace, and if the king of Egypt [de]mands from me the [civilian captiv]es of Kinahḫa, they [will ...] to him; I will [*not*] give them to someone (else)."

This new piece of information on the relations between Ḫatti and Egypt raises several questions, the most relevant for the topic at hand being: When did Muršili II consider the possibility of concluding peace with Egypt?

CTH 63 offers only one sure *terminus post quem* for dating this episode, i.e. Tuppi-Teššub's ascension to the throne of Amurru, an event that can be dated on the basis of two pieces of evidence. The first is found in the historical prologue of the Akkadian version of the subjugation treaty imposed by Muršili on Tuppi-Teššub, where we read:

CTH 62.I – KUB 3.14 obv. 12–16

After my father went to his fate, I sat o[n] the thr[one] of my father. As your grandfather was loyal towards my father, in the same way he was loyal towards me. [When] for the second time the kings of Nuḫḫašši and the king of Kinza [became hostile] to [m]e, Aziru, your grandfather, and Ari-Teššub, your father, [] against the kings of [Nuḫḫašši and the king of Kinza]. As they respected the authority of my [father], in the same way they respected my authority.

This passage is a somewhat fragmentary, but nevertheless unequivocal, reference to a rebellion of Nuḫḫašše and Kinza, which I suggest should be identified with the rebellion that took place in the 9th year of Muršili's reign, rather than that of Muršili's 7th year, because in Muršili's Annals Kinza is mentioned only in relationship with the later of the two rebellions.¹ At this time both Aziru and his son, Ari-Teššub, were still alive; thus Tuppi-Teššub was surely not yet king of Amurru.² Therefore, Muršili's 9th year is a *terminus post quem* for Tuppi-Teššub's ascension to the throne of Amurru.

Another *terminus post quem* for Tuppi-Teššub's enthronement may perhaps be gained from a passage of a letter sent by Bentešina to Ḫattušili III, in which the king of Amurru quotes a message of the Hittite king:

CTH 193 – KBo 8.16 rev. 2'–4'

As my lord wrote thus: "When [] your father came to Arzawa and [], he established?"] (diplomatic) relations with His Majesty in the military camp."³

¹ Klengel (1969: 168–169), whose reconstruction of this set of events has served as the basis for all later discussions, identifies this rebellion of Nuḫḫašše and Kinza with that in Muršili's 7th year and uses this event as a *terminus post quem* for reconstructing the chronology of succession to the throne of Amurru. He acknowledges that in Muršili's Annals Kinza is not mentioned in connection with the rebellion of year 7, but resolves the problem by assuming that it was part of an implied collective "lands of Nuḫḫašše". This cannot be categorically excluded, but as a matter of fact we do not know which political and geographical entity was meant with the expression "the lands of Nuḫḫašše". Therefore it still seems more likely to identify this rebellion with that of Muršili's 9th year. Further, the fact that Nuḫḫašše and Kinza rebelled "for the second time" should not be understood as a second time within the reign of Muršili, but as a second time within the events narrated in the prologue of Tuppi-Teššub's treaty. It follows that the first time would correspond to the revolt of Nuḫḫašše and Kinza that took place during Šuppiluliuma's reign mentioned earlier in the prologue of the treaty (KUB 3.14 obv. 3–4) and possibly also in Šuppiluliuma's Deeds (cf. KUB 19.7 + KBo 19.48).

² Cf. n 1. Pace Klengel (1969: 293; 1992: 166 and 1999: 198 n. 274), and Singer (1991: 162).

³ See Hagenbuchner (1989: 371–372) and Izre'el (1991, II: 103–104).

Despite their admittedly fragmentary state, these lines seem to refer quite clearly to a visit paid by the father of Bentešina, i.e. Tuppi-Teššub, to a Hittite king, most likely to be identified with Muršili II,⁴ when the latter was in Arzawa. On this occasion, Tuppi-Teššub would have established “(diplomatic) relations” (Akk. *īemu*)⁵ with Muršili, a sentence that is surely to be understood as the issuing of the subjugation treaty imposed by Muršili upon Tuppi-Teššub. On the basis of this passage and of the report of Muršili’s campaigns in his Annals, Singer concluded that “this would date Tuppi-Teššub’s ascent to Muršili’s 3rd or 4th year, when he (= Muršili) is known to have campaigned in Arzawa”. However, since these conflicts with the 9th year of Muršili established as a *terminus post quem* by the treaty, Singer (1991: 163) concluded that “one of the two clues for dating Tuppi-Teššub’s ascent is misleading.” There is, however, another possibility, one which would solve this apparent contradiction between the letter and the historical prologue of CTH 62. Muršili’s Extensive Annals attests troubles in Arzawa (more precisely in Mira) also during his 12th and 13th years. While there is no explicit mention of Muršili’s presence in Arzawa at this time, the lengthy gaps in this section of the Annals hardly allow one to argue against his presence, and it seems likely that he would have been involved. This would yield a date for Tuppi-Teššub’s appearance before Muršili, and the issuing of the treaty at this point would fit nicely the fact that Tuppi-Teššub ascended the throne of Amurru sometime after the rebellion of Muršili’s 9th year.

In this light it is interesting that in CTH 63 one finds some hints perhaps suggesting that also this edict might have been issued on the occasion of a personal meeting between Tuppi-Teššub and Muršili. One element supporting this hypothesis is the dialogue between Tuppi-Teššub and Muršili quoted at the beginning of the text. Miller (2007b: 135) already noted that this might reflect “a face to face conversation between the two, either when Muršili was in Syria during his 9th year or perhaps during some prescribed visit of Tuppi-Teššub in Ḫattuša.” The meeting in Arzawa provides a more concrete occasion when this conversation could have taken place.

⁴ It is not known how long Tuppi-Teššub reigned, and it therefore cannot be excluded that his reign overlapped that of Muwattalli, and that he was the Hittite king to whom Tuppi-Teššub paid visit in Arzawa. It is certain, though, that Bentešina ascended the throne of Amurru at some point during the reign of Muwattalli. Lacking any direct evidence witnessing the contemporaneity of Tuppi-Teššub and Muwattalli, it seems quite safe to identify Muršili II with the Hittite king behind the title ^dUTU-šī in KBo 8.16 rev. 4’.

⁵ For the use of *īemu* with a similar meaning cf. e.g. KBo 1.24+ l. 10 (CTH 166: Ramesse II to Kupanta-Kurunta); KUB 3.63:19 (CTH 159: Ramesse II to Ḫattušili III and Puduḫepa); KUB 3.69: rev. 5, 6 (CTH 164: Ramesse II to Puduḫepa); KUB 3.65:9 (CTH 170: fragm. of Hittite-Egyptian correspondence). Among the several meanings of *īemu* (CAD Ṭ, *īemu*: 85ff.; AHW III, *īemu(m)*: 1385ff.), the only other possible candidate that could theoretically fit the context of a visit of Tuppi-Teššub to the Hittite king is “message, news, information”, but I would exclude it because in that case one would expect *ana* ^dUTU-šī instead of *itti* ^dUTU-šī, and also because it seems quite unlikely that Tuppi-Teššub himself would have travelled from Amurru to Arzawa just to deliver a message to the Hittite king.

The second element that might suggest a peripheral location for the composition of the edict is found at its end, where it is noted that the tablet had not yet been sealed because the other parties involved in the dispute (the king of Carchemish, Tuḫaliya and Ḫalpaḫi) had not yet appeared in his presence (KBo 3.3++ iv 2'–13'). This would all but rule out the possibility that the meeting between Tuppi-Teššub and Muršili took place in Syria, and makes it more likely that the two met in Anatolia.⁶ Since there is no evidence for a visit of Tuppi-Teššub in Ḫattuša, while the letter cited attests him going to see the Hittite king in Arzawa, one could reasonably reconstruct the following scenario: Tuppi-Teššub would have travelled to Arzawa in Muršili's 12th–13th year in order to conclude a new treaty with his overlord upon the death of his father, Ari-Teššub, and would have used the occasion to mention to Muršili II the still festering refugee problem at the heart of his complaint as preserved in CTH 63. This edict would thus have been issued roughly contemporaneously with Tuppi-Teššub's vassal treaty, and accordingly, Muršili's 12th–13th years would also be the time frame in which the Hittite king was considering the possibility of coming to an agreement with Egypt regarding the refugees of Kinahḫa. At this time Egypt was still regarded as an enemy and a potential threat to Hittite territories in Syria, as shown by a passage in Tuppi-Teššub's subjugation treaty where he is explicitly forbidden to establish secret contacts with Egypt or to leave the Hittite camp for the Egyptian side.⁷

Thus the next question one should ask is: Was such an agreement between Muršili and the pharaoh ever ratified? It is tempting to find an indirect positive answer to this question in a passage of the later and more famous peace treaty, the one concluded between Ḫattušili III and Ramesse II. In the hieroglyphic version of the treaty, which constitutes the Egyptian translation of the official copy sent by Ḫattušili to Ramesse, one finds an intriguing reference to previous official agreements between Ḫatti and Egypt.

Edel (1997: 27 §5A)

As for the *legal provision*⁸ which was current (lit. here) in the time of Šuppiluliuma, the Great Prince of Ḫatti, as well as the lawful regulation in the time of Muwatalli, the Great Prince of Ḫatti, my father(sic!), I seize hold of it / I hold it firm.

⁶ A further element supporting this hypothesis is provided by the other edict handed down by the same tablet, which is styled as the result of the pleading of its beneficiary, Abiradda, during a visit to Ḫatti (see in particular KBo 3.3+ i 9–13).

⁷ On this see already del Monte (1985).

⁸ Scholars have offered different translations of the Egyptian *nt-ḫmtiy*, e.g. “traditional regulation” (Wilson 1969: 200), “trattato personale” (Bresciani 1990³: 288), “standing treaty” (Kitchen 1996: 81), “festgelegten Vertrag” (Edel 1997: 29), “l’immuable trattato” (Pernigotti 2010: 99). According to Friedhelm Hoffmann (pers. comm.), in this context the best translation for *nt-ḫmtiy* would be “rechtmässige Bestimmung”.

Interestingly enough, this passage and its historical implications have received unequal consideration from Egyptologists and Hittitologists: the former having discussed it quite often, while it has not found the same echo in the Hittitological literature. In fact, even though the hieroglyphic version hands down the Hittite “point of view”, in their discussions of the treaty Hittitologists tend to refer to the cuneiform version found at Ḫattuša, where this passage is lacking, and this information has therefore been overlooked in the main works devoted to Hittite history in general,⁹ but also in many studies devoted to Hittite-Egyptian relations in particular.¹⁰ Only Klengel (2002: 87), in his most recent treatment of the topic, discusses this passage and offers an interpretation in line with that provided by the Egyptologists. Moreover, it is interesting that those few who have examined this passage from the Hittite point of view have denied the existence of a second peace treaty. Zaccagnini (1990: 49–50), for instance, regards these lines as an “obscure reference to a treaty/agreement stipulated at the time of Muwatalli” and hypothesises that the data is “deprived of historical grounds (...), being a cleverly invented artefact for the present stipulation of peace, after some fifty years of contrasts and wars”.

Admittedly, the text is not unequivocal, the main problem lying in the apparent contradiction between the mention of two agreements (one current at the time of Šuppiluliuma, the other in the time of Muwatalli) and the third person singular masculine pronoun in the sentence “I seize hold of it/I hold it firm” (*im.f*). Some scholars have interpreted this singular pronoun as an indication that there was only one previous treaty, which was valid through the reigns of Šuppiluliuma and Muwatalli,¹¹ while others, the majority in fact, favour the existence of two earlier treaties.¹² This apparent contradiction, though, can be explained on the linguistic level. For instance, Murnane proposed that the singular pronoun *.f* “could refer to them individually instead of as a collective (i.e., ‘I seize hold it [= each one of them]’)”.¹³ Alternatively, the suffix of the third person singular masculine could be interpreted as a reference to the general state of affairs current at the time when the agreements were in force.¹⁴ These possible explanations are convincing enough for us to follow the most widely accepted interpretation of this passage among Egyptologists, i.e. as an indication that *two* treaties between Ḫatti and Egypt had been ratified before the time of Ḫattušili III and Ramesse II.

⁹ Cf. e.g. Klengel (1999) and Bryce (2005).

¹⁰ See e.g. Archi (1997); Sürenhagen (2006); Miller (2007b).

¹¹ Sürenhagen (1985: 85–86); Davis (1990); Edel (1997: 29); Roth (2005: 186–187); Pernigotti (2010: 96 n. 3 and 98 n. 3).

¹² Wilson (1969: 200); Helck (1971²: 215); Spalinger (1979: 87); Kitchen (1982: 25 and 1996: 81); Murnane (1990²: 31ff.); Brand (2007: 30).

¹³ Murnane (1990²: 34 n. 169).

¹⁴ Cf. Junge (1996: 54): “Man beachte, daß im Neuägyptischen für den Rückweis auf einem allgemeinen Sachverhalt nicht selten das Suffix der 3.s.m. gebraucht wird (Neutrum).”

As for the dating of these two treaties, Murnane (1990²: 34ff.) demonstrated clearly that the passage does not refer to treaties that *were concluded* by Šuppiluliuma and Muwattalli, but that *were still valid* at the time of those two kings, who then broke them. The first treaty is generally identified with the so-called Kuruštama treaty,¹⁵ known from some fragments of the treaty itself found at Ḫattuša and from references to it in the Deeds of Šuppiluliuma and Muršili's Prayers. The same Hittite sources also relate that this treaty was broken by Šuppiluliuma at the end of the Amarna age, after the famous *taḫamunzu* episode. This is still the most likely hypothesis, but two other possibilities should be considered. Helck (1971²: 182 and 215) identifies the agreement valid at the time of Šuppiluliuma with a treaty that the Hittite king would have concluded with Egypt after the *taḫamunzu* episode: Presumably Helck interprets as evidence for a renewal of the Kuruštama treaty the passage of Šuppiluliuma's Deeds where one finds "Of old, Ḫattuša and Egypt were friendly with each other, and now this, too, on our behalf, has taken place between t[hem]! Thus Ḫatti and Egypt will continuously be friendly with each other!" (KBo 14.12++ iv 35–39, after Güterbock 1956: 98). The phrasing of this passage alone does not allow one to confirm or exclude the possibility that Šuppiluliuma renewed the terms of the Kuruštama treaty, but if this had been the case there would probably be some hint of this new agreement in the retrospective narration of these events in Muršili's Prayers, where one finds instead only reference to the old Kuruštama treaty. Another option is suggested by a passage of EA 41, a letter sent by Šuppiluliuma to Ḫuriya, in which the Hittite king states that "neither my messengers, whom I sent to your father, nor the request that your father made, saying 'Let us establish only the most friendly relations between us,' did I indeed re[fus]e" (obv. 7–10, after Moran 1992: 114).¹⁶ Assuming that the addressee of EA 41 was Amenhotep IV,¹⁷ the request of "establishing friendly relations" (Akk. *aterrutta epēšu*), which could have implied the ratification of a treaty between Ḫatti and Egypt, must have come from Amenhotep III: Was this treaty actually ever signed and could it thus be the agreement valid at the time of Šuppiluliuma referred to in the Silver Tablet? I would exclude such a possibility because, even apart from the absence of any further textual evidence supporting the existence of such a document, if such a treaty would have existed one would expect Šuppiluliuma to have asked for it instead of the tablet of the older Kuruštama treaty when obliged to verify what kind of relations the two kingdoms had entertained until that moment. As in the case of the previous hypothesis, I find it unlikely that Muršili and his scribes would have been unaware of the existence of such an important agreement and would have neglected to mention it in the texts where the *taḫamunzu* episode and the events that followed it are narrated.

¹⁵ Murnane (1990²: 34–35); Klengel (2002: 87). On the possible dating of the Kuruštama treaty see most recently Singer (2004) and Simon (2007).

¹⁶ I would like to thank Boaz Stavi (Tel Aviv) for having drawn my attention to this passage.

¹⁷ See Miller (2007a: 279–282) with further literature and discussion.

As far as the date of the second treaty is concerned, some ascribe it to Muwatalli II and Seti I (Spalinger 1979: 88; Kitchen 1982: 25), some to Muwatalli II and Ramesse II (Helck 1971²: 215), while others prefer to date it to the reigns of Muršili II and Horemheb (Murnane 1990²: 37–38, Brand 2007: 18 n. 19).

The new evidence provided by the reconstruction of Muršili's edict (CTH 63) makes it quite plausible that one should date it to some time after Muršili II's 12th–13th years, according to the *terminus post quem* discussed a moment ago. But with whom might Muršili have signed this treaty? According to the chronological scheme followed here (see below), Horemheb would have been pharaoh at this time, and thus, the partner with whom Muršili would have concluded the treaty.

In this context one should mention an object recovered at Ugarit, a travertine vase bearing the name of Horemheb (RS 17.420+17.421), which was recently “rediscovered” in the storage facilities of the National Museum of Damascus and subsequently published by Lagarce in 2008.¹⁸ Lagarce regards the vase as a gift sent when Horemheb was already king, but she does not discuss the fact that, among the many vases sent by Egyptian kings to Ugarit, this is the only one on which the name of the pharaoh is not framed by a cartouche or accompanied by a throne name. Should one nonetheless regard this vase as an object dating to the reign of Horemheb? Or should one consider the possibility that it might have been sent by the general Horemheb? While no definitive answer can be offered, the different scenarios that the presence of this object at Ugarit might suggest can be reviewed. Traditionally, the vase has been interpreted as evidence that the coastal kingdom had been plotting with Egypt and the Syrian rebels against Muršili. This situation was dated to the time of Ar-Ḫalba, whose presumably very short reign was interpreted as confirmation of this hypothesis: He would have been replaced on the throne of Ugarit relatively soon after his ascension, as punishment for having sided with the rebels and Egypt. However, as Singer has pointed out, there is no conclusive evidence supporting this hypothesis, and “Ar-Ḫalba's prompt disappearance could simply have been caused by non-political circumstances, such as sudden illness and death.”¹⁹ Furthermore, the

¹⁸ Lagarce (2008: 268–269, 274–275). The very existence of this object has been shrouded in mystery for a long time: it was first mentioned in 1954 by Schaeffer (1954: 41; also 1955: XXIII) among the Egyptian objects recovered from the Central Archives of the Royal Palace, but no picture or drawing was published at that time. Thereafter it seems to have “disappeared”; in their *Trouvailles Epigraphique de l'Ougarit* Bordreuil and Pardee (1989: 152 under entry RS 17.[477]) mention it as known only from a letter kept in the archives of the archaeological mission. According to Lagarce (2008: 269 with n. 24), the vase comes from the residential area, more precisely from the House of Rašapabu (see Yon 2006: 65 and 72 for plan and description), while Schaeffer in his excavation report indicates Rooms 30 and 31 of the Central Archives as the find spot for this vase and other Egyptian objects dated from Amenhotep III to Ramesse II (cf. Schaeffer 1955: XXIII and fold-out plan at the beginning of the volume). Thus, there are either two Horemheb's vases, one from the House of Rašapabu and the other from the Central Archives, or there is only one vase and thus only one of the two find spots can be correct. It should be noted that all but two vessels with cartouches were found in the royal palace (Caubet 1991: 214).

¹⁹ See Singer (1999: 637–638) for an overview on Ar-Ḫalba's reign and previous literature.

presence of Horemheb's vase at Ugarit should not necessarily be regarded as an indication that at that time Ugarit was either hostile or lost to the Hittites. Quite to the contrary: The presence at Ugarit of Egyptian objects indicates clearly that the Syrian kingdom was openly cultivating diplomatic contacts with Egypt at a time when it was doubtlessly under Hittite control. Examples include objects bearing the royal cartouche of Ramesse II²⁰ and the recently published letter to the king of Ugarit from Egypt dated to Merneptah, both from the period when Ugarit was a Hittite vassal. Since vassals could not entertain diplomatic relations with enemies of their own masters, one may assume that the presence of these objects at Ugarit attests to peaceful relations not only between Egypt and Ugarit, but also between Egypt and Ḫatti. Now, if this holds true in the case of this later evidence, why should it not be the case with regard to Horemheb's vase? Thus, far from being evidence of conflict between Ḫatti and Egypt, this object could rather be an indication that the two kingdoms were at peace during at least some period of Horemheb's reign. This would then provide further support for the possibility that Muršili actually concluded a peace treaty with Egypt and that the second treaty mentioned in the Silver Tablet should be dated to his and Horemheb's time, instead of to the reigns of Muwatalli and Seti I.

HITTITE-EGYPTIAN SYNCHRONISMS AND ANCIENT NEAR EASTERN CHRONOLOGY

Several issues regarding the relative and absolute chronologies of Muršili II's and Horemheb's reigns are now being reassessed in the face of new data. This evidence consists on the one hand of a recently reconstructed Hittite historical text,²¹ which has been interpreted as demonstrating a synchronism between Muršili's 9th year and Horemheb before his ascension to the throne, an interpretation which has not gone unchallenged, and on the other hand the shortening of Horemheb's reign.

The Suggested Hittite-Egyptian Synchronism in KUB 19.15+

The Hittite text in question is a narrative composition of Muršili II,²² in which he relates his surly correspondence with an Egyptian named *Ar-ma-a*. In the first column the pair trade angry letters concerning two Syrian vassals who had changed sides in

²⁰ In this regard it should be pointed out that the fashion in which the birth name of Ramesse II is written on these vases provides an important *terminus post quem*, namely his 16th year, for the shipping of these gifts (Lagarce 2008: 275–276).

²¹ KUB 19.15+KBo 50.24; see Miller (2007a; 2008); Groddek (2007a); Stipich (2008); Taracha (2008); Simon (2009); Wilhelm (2009); Lebrun – Degréve (2008: 127–129).

²² Freu (2008: 46f.) maintains that the text “se présente comme le message(?) d'un roi(?) ou d'un grand personnage hittite, chargé de surveiller les vassaux syriens (le roi de Karkemiš?), à un dignitaire ou au roi de Ḫatti. Malgré certaines des expressions employées il semble difficile de faire du Grand Roi son auteur,” but adds “ce qui cependant n'est pas exclu et pourrait expliquer certaines particularités du texte.” For my argumentation that the text is a historical narrative of Muršili II, see Miller (2008: 539ff.); already Hagenbuchner (1989: 479); Singer (*in press*), who suggests that it may belong to a narrative section of a prayer.

the big powers' conflict. In the second column Muršili complains about Egyptian intervention in Amurru, which is said to have taken place when a certain person ascended a throne; the new king's name and country are broken away. This intervention prompts Muršili to berate *Ar-ma-a*, informing him that it was not he, Muršili, who had taken Amurru from Egypt, but Mittani, from which Muršili's father, Šuppiluliuma, had then captured it. Comparison with Muršili's Annals and other texts suggests that these events likely occurred in his 7th and 9th years, respectively. In the initial publication of this new text (Miller 2008) the Egyptian *Ar-ma-a* was identified with Horemheb, and for a number of reasons it was suggested that he was at this time not yet pharaoh, but still a military officer and/or governor in the Southern Levant. The resulting synchronism of an accession of Horemheb in or after Muršili's Year 9 was interpreted as supporting an identification of Akhenaten with Nibḫururiya, the pharaoh who had died leaving the Egyptian queen to write to Šuppiluliuma to beg for a son in the famous *tahamunzu*²³ episode. An identification of Nibḫururiya with Tutankhamun was thus ruled out on chronological grounds, an identification with Smenkhkare²⁴ found to be very unlikely.

Since the initial publication of this text a number of researchers have published insightful papers commenting on various issues, some raising challenging new questions. On this occasion some of these considerations will be assessed anew, attempting to analyse their strengths and weaknesses.²⁵

²³ Groddek (2002: 275 n. 16) suggested, pending collation, that a reading ^{MUNUS}tá-ḫa-mu-un-zu-r[a-a]š (KBo 5.6 iii 8) should be considered. From what one can see on the photos now available at www.hethiter.net, it does not seem that such a reading is possible, and -₁zu-uš₁ remains the most likely, and indeed quite convincing, reading.

²⁴ In a previous article I wrote (Miller 2007a: 257), "this paper assumes that there was no coregency between Akhenaten and Smenkhkare", which would likely be fatal to the identification of Nibḫururiya with Akhenaten, since it is hardly credible that a situation in which a male successor already designated and reigning alongside Akhenaten during the last few months or years of his tenure could allow for the *tahamunzu* episode." Naturally, this would be the case only if Smenkhkare outlived Akhenaten for a significant period of time. If, on the other hand, Hachmann (2001: 139–141) is correct in assuming that Smenkhkare did not outlive Akhenaten for any substantial length of time, then the *tahamunzu* affair could have taken place following the roughly contemporaneous deaths of Akhenaten and his co-regent and before the reign of Tutankhamun. For the most recent treatment of the issue, see Krauss (2007a), who sees Smenkhkare and Meritaten ruling following the death of Akhenaten, followed by a brief sole reign of Meritaten, whereby he leaves open the possibility of a short interlude between Akhenaten and Smenkhkare for the rule of Nefertiti alone.

²⁵ In addition to the papers discussed here, Freu (2008: 47f.) has rejected wholesale the identification of 'Arma'a with Horemheb and maintains his previously argued chronology in which Nibḫururiya is equated with Tutankhamun. To do so Freu (2008: 48 n. 104), who presents no argumentation against the new interpretation but simply refers to his previous works, must, *inter alia*, flout the Egyptological consensus that Akhetaten was abandoned during the first months of the reign of Tutankhamun at the latest (Freu 2007: 281f.); maintain that EA 16 was addressed to Smenkhkare, misconstruing Moran's (1992: xxxv and n. 123; 39 n. 1) presentation of the facts and implying that Wilhelm and Boese's attribution of EA 41 to Smenkhkare on the assumption that Ḫuriya is an error for <Ana>ḫuriya somehow supports attributing EA 16 — addressed to what Freu (2007: 282f.), following (but distorting) Gordon's unpublished collations, reads ḫu-ru/ra-ḫu-ri-ya] — to the same pharaoh. For further arguments against Freu's reconstruction, see Wilhelm (*in press*, §3.2); Miller (2007a).

The Identity of Ar-ma-a in KUB 19.15+KBo 50.24

Perhaps the most serious challenge to the hypothesis just outlined consists in the reservations expressed by Simon (2009). He convincingly shows that for the likely contemporary pronunciation of Horemheb's name, i.e. *Ḥarmaḥa, one would expect a cuneiform representation *Ḥarmaḥa rather than *Ar-ma-a* as found in Muršili's narrative. He thus concludes that *Ar-ma-a* cannot have been Horemheb at all, which would of course moot any thought of a chronological link. In fact, as can be seen from Muchiki's work (1999), Egyptian ḥ is consistently represented by ḥ in Phoenician/Punic, Aramaic, Hebrew and Ugaritic and by ḥ in Akkadian, while ḥ is represented by ḥ and k in Phoenician/Punic, Aramaic and Hebrew, and by ḥ in Ugaritic and Akkadian.²⁶

Simon's strongest argument is summarised in a table in which seven Egyptian names and terms containing ḥ are represented in Hittite cuneiform sources with signs containing ḥ.²⁷ The distribution of the seven examples in Simon's table, however, is pertinent to the question of their statistical robustness. The first two entries are from a tablet of the Deeds of Šuppiliuma, while four (the 3rd, 5th, 6th and 7th entries) are from letters of Ramesse to Ḥattušili or Puduḥepa. The 4th entry is too fragmentary to be certain.²⁸ At least six of the seven entries therefore stem from two scribes or scribal groups, the one perhaps dependent on the other, since the compiler of the Deeds may well have referred to Egyptian correspondence to complete his work. Whether such is statistically relevant can thus be doubted. As mentioned (n. 27), however, further attestations actually render Simon's counterargument more statistically robust than his table shows.

On the other hand Simon discusses (2009: 345 n. 36) the writing *Šu-ta-ḥa-ap-ša-ap* in KUB 3.70 for *sth-ḥr-ḥpš.f*, in which the expected ḥ is omitted. This is perhaps a telling example, as the letter is from a son of Ramesse II, so that one must assume, if one opts to fall back on scribal error as an explanation, that a royal scribe botched the prince's name, which, though striking, is not impossible. Otherwise it would clearly show that even Egyptians could omit the ḥ.

²⁶ Cf. already Groddek (2007a: 97 n. 13), who makes the "Verschriftlichungsprozess in Boğazköy für die Namenslautung verantwortlich," suggesting that Muršili "bei der Abfassung von KBo 50.24+ also offensichtlich keine Originalbriefe Haremhab's eingesehen, sondern die Ereignisse einschließlich der Kurzzitate der Briefe aus der Erinnerung gegeben [hat]." To avoid further prejudicing the issue, I will henceforth employ the neutral *Ar-ma-a* rather than the interpretive 'Arma'a I have used elsewhere.

²⁷ Cf. also Edel (1980: Nos. 4, 5, 7, 9 and 14–16), from various cuneiform traditions. In correspondence of March 17, 2008, Marinus van der Sluijs and Peter James kindly pointed out to me the following additional examples of Egyptian ḥ preserved in cuneiform as ḥ or (alphabetic) ḥ: the PN Ḥaramassa/i (i.e. *ḥry-mš[yw]*; EA 20: 33, 36; Hess 1993:73f.); the Levantine rulers of the Neo-Assyrian period Ḥursiešu, Naḥtiḥuruansini and Pišanḥuru (PNA 2, 481, 922; 3, 997); and the Ugaritic PNs [Bn]-Ḥr (UM 146: 8), Bn-' / Abdḥr (UM 64: 36; 83: 11). I thank them for sharing this information.

²⁸ It is judged by Edel (1994: I, 211; II, 318f.) to be from Ḥattušili or Puduḥepa to Ramesse, but the evidence is hardly conclusive.

There is also at least one spelling of the name Merenptah-ḥotḫimā'e (*hotepher-maat*) as *mar-ni-ip-ṛtaḫ¹ ḫa-at-pa-mu-a* without the expected third ḫ in a letter from Egypt discovered at Ugarit (RS 88.2158), which in turn led Lackenbacher (1994) to suggest the plausible reading *ḫa¹-ṛat¹-pa-mu-a* in a further letter from Ḫattuša (KBo 3.38).²⁹ Also the name *Ia-ma-a-ia* in EA 62: 42, 45 has been interpreted as a spelling of Egyptian *i^ḥ-mś* (Hess 1993: 81). Simon's case thus remains relevant and strong, and one could even argue that these cases suggest that the elision of ḫ is seen only in proximity to a consonant and therefore not relevant for the question of the writing of *Ar-ma-a* for *Ḫarmaḫa; still, significant exceptions to the clear pattern are attested, so that one can feasibly maintain the identification of *Ar-ma-a* with Horemheb without overly egregious special pleading.

Further, as Simon himself details (2009: 344 and ns. 16 and 26), there are perhaps plausible phonetic explanations for such a writing, though they cannot be considered the most straightforward or obvious interpretations, nor do they represent the current *communis opinio*, and Simon argues capably against them. Nonetheless, even he must admit the possibility, which he considers highly unlikely, that the writing *Ar-ma-a* might represent something akin to 'Arma'a. Even if this were the case, however, it would not diminish the strength of Simon's primary argument: one would expect a cuneiform writing *Ḫarmaḫa. If *Ar-ma-a* should nevertheless turn out to be a representation of *Ḫarmaḫa, then it would presumably have to be explained with reference to a speaker of an Indo-European language not knowing what to do with an Egyptian phoneme that he had heard and with which he was not familiar, indeed, as is well attested in the (later) Greek sources (Miller 2007a: 254).³⁰ Against this background, Simon's claim, in response to the suggestion that the names are practically identical (Miller 2007a: 254), that they are on the contrary "nicht praktisch identisch, sondern auffällig unterschiedlich" (347, n. 43) can perhaps be considered overstated. The likely contemporary pronunciation *Ḫarmaḫa and the name *Ar-ma-a*, however one interprets the cuneiform spelling, differ only in the precise realisation of their first and penultimate phones.

²⁹ The appearance of Merneptah's birth name would at first glance seem to speak against the claim discussed below, i.e. that the use of Horemheb's birth name in KUB 19.15+ suggests that he was not yet pharaoh. This letter, however, seems to imply that the enthronement of Merneptah was in progress (Lackenbacher 2001: 243f.) – or, alternatively, that he had been somehow elevated during the reign of his father – so that he may not yet have taken his throne name. This would presumably not apply, though, to the name as reportedly found in an unpublished letter (Lackenbacher 2001: 242), where ^m*Mar-ni-ip-ta-aḫ* lugal.gal lugal kur *Mi-iṣ-ri-i* is said to appear. Further analysis of the passage must await the publication of the Urtenu archive. In any case, as Müller (2006: 206) affirms, "Der Geburtsname tritt in den zeitgenössischen Quellen zwar auch in Dokumenten des Alltags und bei verstorbenen Königen auf, nicht aber in den offiziellen, die Fülle der erhaltenen Inschriften bildenden Dokumente und Denkmäler, wo nahezu ausschließlich der Thronname verzeichnet ist." It should be noted that the exceptions Merneptah and *stḫ-ḫr-ḫpš.f* are not found in Muchiki's work (1999).

³⁰ It is, of course, well known that Hittite scribes were prone to have trouble with the Semitic laryngeals (e.g. Labat 1932: 8; Devecchi *in press*: §3 and n. 32), but it is not clear that this fact should be used to explain the absence of the consonant in question.

While the spelling is clearly problematic, the writing is only one of several considerations, which can be reviewed briefly now. In a footnote Simon addresses what had been presented as three disconcerting coincidences or incongruities that would result from rejecting the identification. The first is that “both ’Arma’a and Horemheb [would thus have been] high Egyptian officials during approximately the same period and both for some time apparently a kind of governor or commander in charge of the Asian realm, and that both acted in stead of the king” (Miller 2008: 546). To this Simon (2009: 347 n. 43) replies rather succinctly, perhaps without considering the import of the rejoinder, “Laut der traditionellen Chronologie sind sie tatsächlich zu gleicher Zeit tätig – aber einer als Pharao, der andere als Befehlshaber.” Such a scenario, however, envisions Muršili carrying on intensive international correspondence with this “Befehlshaber” named *Ar-ma-a* stationed in Canaan instead of his commander-in-chief and pharaoh named *Ḫarmaḫa in Egypt. That the Great King Muršili II would correspond with a non-royal subordinate of Horemheb concerning matters of insubordinate vassals while this powerful and active pharaoh sat idly by in Egypt is historically highly unexpected;³¹ indeed, it may be suggested, at least as unexpected as the writing *Ar-ma-a* for *Ḫarmaḫa.

The second coincidence noted is that “this extremely important individual, ’Arma’a, appears nowhere in the Egyptian literature, though he corresponded with a Great King shortly before the accession of Horemheb” (Miller 2007a: 254). To this Simon responds: “dass ein *argumentum e silentio* bei einer fragmentarischen Quellenlage nicht glücklich ist, wird auch dadurch gezeigt, dass die ägyptischen Statthalter von Upi und Kinahhi (ÄHK 55), die wiederum wichtige Persönlichkeiten ihrer Zeit [gewesen] sein mussten, ebenfalls unbekannt aus den ägyptischen Quellen sind.” Though it is naturally the case that an *argumentum e silentio* can only be considered more or less suggestive, not conclusive, the comparison with the governors of Upi and Kinahhi is hardly an appropriate one.³² They were clearly not in a position to correspond with the Great King of Ḫatti concerning renegade subordinates, demanding extradition from a Great King, or to challenge that Great King’s rule over Amurru. *Ar-ma-a* was. Indeed he seems to have been responsible for Egypt’s Asian possessions and international relations. That this personage should be otherwise unattested is appreciably more disquieting than the anonymity of the governors of Upi and Kinahhi. Moreover, the names of several

³¹ Moreover, the “traditionelle Chronologie” of which Simon speaks is hardly traditional in the sense of “established” or “widely accepted”. It is based entirely on the assumption that Nibḫurriya is to be equated with Tutankhamun, which is precisely the question at hand, and thus but one of two alternative and oft-debated chronologies.

³² Freu (2008: 49) similarly assumes that *Ar-ma-a* “ait été alors un haut dignitaire de ce roi d’Égypte [i.e. Aya] et son représentant en Syrie. Il faut plutôt supposer que le dit Armâ est le gouverneur ou le vice-roi (*rabiṣu*) de la province égyptienne de Canaan (ou de celle d’Ube).” As an alternative Freu (ibid.: 49) allows the possibility that Muršili “ait nommé le roi d’Égypte qui était «d’origine roturière» sans lui donner son titre et l’ai traité de façon peu protocolaire.”

governors of Upi and Kinahḫi are indeed known from (roughly) this period as well as from the Amarna era, as Itamar Singer kindly pointed out to me (Singer 1983: 18ff.).

To the third incongruity, that the careers “of these two individuals ... overlap so conspicuously” (Miller 2007a: 254), Simon replies, “Da man über Armā übrigens nichts weiß (vgl. Anm. 9), kann man über ähnliche Karrieren nicht sprechen.” However, the text of KUB 19.15+ alone suffices to warrant the statement that the careers of *Ar-ma-a* and Horemheb overlap remarkably. It alone provides information on name, date, status, function and even hints of *Ar-ma-a*’s character.

Simon’s study is thus clearly successful in showing that the cuneiform writing *Ar-ma-a*, if indeed the name is to represent contemporary *Ḫarmaha, must be considered unexpected, though not entirely unparalleled. His paper would seem to overstate the case, however, when it comes to the more general historical and political scene. His point must therefore be taken seriously, and the identification of *Ar-ma-a* and Horemheb should henceforth be approached with due caution, but this one point alone does not seem to warrant jettisoning the equation entirely.

Interpretations of KUB 19.15+KBo 50.24 and Ar-ma-a’s Status therein

In the first paper to appear following the publication of Muršili’s narrative, Groddek (2007a) reached quite different conclusions. This text, Groddek asserts, does not place the *tahamunzu* episode after the death of Akhenaten; on the contrary, it completely excludes the option.³³ He thus affirms his previous identification of Nibḫururiya with Tutankhamun (Groddek 2002: 273–277), arguing that the identification with Akhenaten is incompatible with the chronologies of Egypt and Ḫatti.

Groddek (2007a: 96) argues against the proposed chronological scheme based primarily on an ironic comment of Ramesse II in response to Ḫattušili III’s request for a doctor who could help his sister, Massanauzzi, to conceive a child. Ramesse responded rather flippantly that she was already 50 or 60 years old, and in such a case one should not expect miracles. Groddek, despite the rather whimsical context, takes the number 60 quite literally and measures it against known chronological points.³⁴ He shows that Ḫattušili’s sister is likely to have been born not later than the 7th year of their father’s, Muršili’s, reign, and that the letter in question must have been written after the peace between Egypt and Ḫatti in Ramesse’s

³³ He arrives at the synchronisms “Muršili II. Jahr 7 = Jahr x Haremhab, sowie Jahr z (vorletztes Jahr) Šuppiliumas I. = Jahr y (=1) Haremhab” (Groddek 2007a: 100).

³⁴ Groddek interprets the pharaoh’s response as indicating that Ramesse rejected Ḫattušili’s assertion that Massanauzzi was 50, insisting instead that she was 60. This is not the case, however. Ramesse writes merely that she is 50 or 60, and that for a 50- or 60-year-old woman one cannot expect too much. Cf. Edel (1994: 178–181); Beckman (1999²: 137–138); Wilhelm (2006: 236–237).

21st Year (see also Groddek 2007b: 43 and 60 n. 58). Given a synchronism between Muršili and an Egyptian pharaoh, one can calculate Massanauzzi's age on the basis of the reigns of Horemheb, Ramesse I, Seti I and the first 21 years of Ramesse II. If one places Muršili's 7th year in the reign of Ay, as was suggested in the original publication of the text, then, according to Groddek, Ḫattušili's sister would have been 62 to 66 years old by the time Ramesse sent his letter, far too old for the 60 years she is given by Ramesse.

Even if Groddek's calculations might have seemed suggestive at the time his paper was written, the new evidence for the length of Horemheb's reign, to be discussed presently, which now must be set at some 14 years, is fatal to his argument. Updating Groddek's calculations accordingly would yield a minimum of some 48 to 52 years for Massanauzzi's age, which fits quite nicely the number Groddek would demand, and a number easily capable of absorbing the variables he notes in passing. This argument against the suggested chronology, fundamentally weak because of the context and tone of Ramesse's reply, can thus be discarded.

Also fragile is Groddek's assumption (2007a: 97, 98 and n. 15; similarly Wilhelm 2009: 113), referring to col. i 6' of the text in question, that "laut Text Tette an den König Ägyptens geschrieben hatte." It must be emphasised, on the contrary,³⁵ that LUGAL here must be almost completely restored, and that other restorations are entirely possible. Only slight traces of what seem to be horizontals, perhaps somewhat slanted, are preserved, which would allow, among other things, [L]Ú or [UGUL]A. Further, even if [LUGA]L were to be restored here, it is not at all certain that this would necessarily make *Ar-ma-a* already pharaoh in the text. As has been pointed out (Miller 2008: 551 and n. 58),³⁶ even if Tette did write to the pharaoh, nothing requires that it was the same person who appears in subsequent lines. It is entirely possible that Tette wrote, e.g., to Ay and that *Ar-ma-a*, either as Ay's vice or as the one who de facto held the reins of power instead of an aging and enfeebled pharaoh, took matters into his own hands.

The other major difference in Groddek's understanding of the text is his placement of the events of col. ii within the reigns of Šuppiluliuma I and Arnuwanda II, along with his restoration of Arnuwanda in l. 2'. Wilhelm (2009: 112) has recently shown convincingly why this reconstruction of events must be considered quite unlikely. That the events of col. ii are not to be placed in the reigns of Muršili's predecessors is shown clearly, *inter alia*, by Muršili's reaction to the Egyptian moves, narrated in the first person, "But when I heard (about it), ⁽⁹⁾I came to the rescue, ⁽¹⁰⁾and the troops and chariots of the Land of Egypt fled before

³⁵ See already Miller (2008: 534, 551 and n. 58).

³⁶ In the footnote should have appeared "It could be that Tette actually wrote to the king of Egypt, Ay according to the reconstruction suggested in this paper, but that 'Arma'a responded and was in fact the one to be dealt with at this time." Similarly Wilhelm (2009: 113).

me,¹²⁾ and I [pu]rsued him.” Groddek’s interpretation therefore rather roughly distorts the relatively clear text, and as Wilhelm (2009: 112, n. 20) rightly emphasises, “Nur auf Grund dieser Interpretation kann Groddek ... daran festhalten, dass der verstorbene Pharaο der *res gestae* Tutanchamun ist.”

In a further effort to argue that *Ar-ma-a* in the text at hand could not have been acting in place of the reigning pharaoh, Groddek writes (2007a: 97 n. 14) that Horemheb in his monuments and tomb claimed “keine überzogene Sonderstellung” vis-à-vis the pharaoh, though in the same sentence he paradoxically speaks of phraseology belonging to the “sonst nur dem König vorbehaltenen Feld.” In fact, it is quite clear that Horemheb’s authority, position and titles before ascending the throne far exceeded the norm and that he had appropriated much of the royal symbolic, as emphasised e.g. by Gabolde (refs. in Miller 2007a: 255 and n. 15), the details of which need not be repeated now.³⁷

Along the same lines Groddek highlights the first column’s parallel structure, with Tette defecting to Egypt while Zirtaya defects to Ḫatti, from which he concludes that *Ar-ma-a* must have been pharaoh already, writing “Es fällt schwer, hier plausibel von zweierlei Ebenen auszugehen, einmal, im Falle Tettes, dem Diener/Vasallen eines Großkönigs, andererseits, im Falle Zirtias, einem (Kammer)diener beispielsweise Armaas, denn, wenn Armaa nicht der regierende ägyptische König sein soll, kann Zirtia auch kein Vasall Ägyptens sein und mit Tette nicht auf einer Stufe stehen.” Such a conclusion, it seems, is based on an overly literal reading of the text. One need not conclude that Zirtaya could not have been a vassal of Egypt if *Ar-ma-a* was acting in place of the pharaoh or as de facto ruler, placing him for all intents and purposes on a plane with Muršili; and it is hardly outlandish to suggest that Muršili in his word choice was reconciled with Horemheb’s de facto status. Similarly unconvincing is Groddek’s insistence that if *Ar-ma-a* were not pharaoh he surely would have replied to Muršili’s question concerning why he had given refuge to Tette with (2007a: 98) “Ich habe Truppen geschickt, weil es mir Pharaο - LHG - so befohlen hat, frag doch bitte meinen Herrn. Der macht die Außenpolitik!” This is simply not in keeping with Horemheb’s attested character and status, and it naively assumes that every subordinate military officer humbly accepts his subordinate position.³⁸

³⁷ According to Forbes (1998: 34), Horemheb “was the central figure in the administration of Tutankhamen’s reign, serving as the commander-in-chief of Egypt’s armies, overseeing all of the building works of the juvenile king and, presumably, functioning in the capacity of the latter’s regent, in fact if not in name. Due to the extensive representation of foreigners in the tomb reliefs, it is reasonable to conclude that Horemheb also was in charge of directing Tutankhamen’s foreign policy.” There is little reason to suspect that Horemheb relinquished any status or power during the short reign of the young pharaoh’s successor.

³⁸ Equally unconvincing is Groddek’s comparison with the letter from a Hittite Great King to an Assyrian ruler that includes a note to the official Baba-aḫa-iddina and others, for which an entirely different situation must be envisioned. In Miller (2007a: 255 n. 14) was discussed already a letter from a Great King, likely Tukulti-Ninurta I, to a subordinate of another Great King, a king of Ugarit, that shows an entirely different tone, due to yet another situation (see now Schwemer 2006: 254–256).

Overly literal is also Groddek's (2007a: 99) extraction from col. ii 16'–19' of a synchronism between Šuppiluliuma and Horemheb (see n. 33, above) based on Muršili's rhetorical question concerning who had taken Amurru from Egypt, in which Muršili employs the 2 sg.: "(16') But was it I who took the [Land] of Amurru away from you, (18') or was it rather my father who took it away from you?" It is hardly a stretch of the imagination – indeed the context rather suggests – that "you" is used here *pars pro toto* for Egypt. In fact, Muršili answers his own rhetorical question a few lines later by explaining that neither he himself nor his father had taken Amurru from Horemheb or Egypt, but from the Land of Hurri, which had taken it from Egypt. Muršili's point is that Ḫatti had *not* taken Amurru "from you (scil. *Ar-ma-a*)", but from someone else, and that *Ar-ma-a* therefore had no right meddling in Amurru. In other words, Muršili states explicitly that Šuppiluliuma didn't even take Amurru from Egypt, let alone from *Ar-ma-a*.

Groddek (2007a: 99, n. 17) also rejects the argument according to which Horemheb's birth name, *Ar-ma-a*, as found in Muršili's narrative (Miller 2008: 551), rather than his throne name can be seen as evidence against his already being pharaoh. In rejecting the suggestion Groddek points to the occurrence of the birth name Merneptah in KUB 3.38 obv. 7'. However, this counterargument cannot be accepted, since in all likelihood Merneptah was not pharaoh at all at this point and is being referred to as a son of Ramesse II (cf. n. 29).³⁹

In short, it still seems considerably more likely that *Ar-ma-a*, identified with Horemheb, was not yet pharaoh in the text in question and that the events of col. i and ii are to be dated to Muršili II's 7th and 9th years, respectively. The suggested synchronism would thus remain valid, excluding the equation of Tutankhamun with Nibḫururiya of the *taḫamunzu* episode.⁴⁰

A New King Ascends the Throne

Further important considerations concerning Muršili's narrative have been expressed by Wilhelm (2009), who, it should be noted, has long argued for the

³⁹ Of interest in this context is the vase from Ugarit bearing the name of Horemheb touched on above. Although the piece shows his birth rather than his throne name, placing it in a decorative motif rather than a cartouche, Lagarde (2008: 275) states that it obviously dates to Horemheb's reign as pharaoh. One wonders, however, if it might rather point to Horemheb's significant influence in Syria already before his ascent to the throne. If so, it would fit well with the suggestion that *Ar-ma-a* in Muršili's narrative could be Horemheb.

⁴⁰ Incidentally, as far as I understand, being neither a geneticist nor an Egyptologist, the recent DNA testing of Tutankhamun's and related mummies has not entirely clarified this pharaoh's parentage. As far as I have been able to gather from Hawass *et al.* (2010) and various internet and journal articles (Moisse 2010; Baker 2010; Phizackerley 2010; Bickerstaffe 2010), it is clear that Tutankhamun's grandmother was Tiye (KV35EL) and that his mother was the mummy of KV35YL, apparently not Nefertiti. It is further clear that his father was the mummy of KV55, the identity of which has long been debated (see most recently Gabolde 2009). The question of the appropriateness of the *taḫamunzu*'s claim that she and her late husband had no children thus remains unresolved (cf. Miller 2007a: 257–263).

identity of Nibḫururiya of the *taḫamunzu*-episode with Smenkhkare rather than Akhenaten or Tutankhamun (Wilhelm – Boese 1987).

Wilhelm's first point pertains to the largely missing royal name in obv. ii 2',⁴¹ where he would restore ^mA[r-*ma-a* as the individual who thus ascended the Egyptian throne.⁴² As he notes, this otherwise attractive possibility was deemed unlikely in the original publication, as it was argued that if l. 2' had contained the same name, *Ar-ma-a*, then one would not expect explicit resumption of the subject in l. 4'.⁴³ This consideration was based, however, not merely on the "kurzem Abstand" (Wilhelm 2009: 112) between ll. 2' and 4', nor on the assumption that it should be understood as a temporal relative clause (ibid.), but primarily on the syntax resulting from the restoration of ^rú¹-[i]t before the name in l. 4'. As noted in the commentary to the line (Miller 2008: 538), this restoration results in a periphrastic, the so-called phraseological, construction, a syntactical arrangement which relates the clause immediately to the preceding one, as van den Hout (2003) has so capably demonstrated.⁴⁴ In no clause (among those treated by van den Hout and GrHL) linked by the phraseological construction to the previous clause is a named subject resumed explicitly; it is, when the subject remains the same, always resumed by a pronoun.⁴⁵

That said, it must be granted that the restoration of ^rú¹-[i]t is uncertain, and one might even argue that it would be just a bit too long. The only other sensible restoration that comes to mind that fits the space, traces and context would be the slightly shorter E[GI]R, which would link in a similar manner the content of the clause to the taking of the throne in the previous one. If one were to restore E[GI]R one would want to translate, "[... But when] ^(2')[PN] sat [upon the throne of] ki[ngship], ^(4')*Ar-ma-a* a[gai]n/th[ereaf]ter began to *take [ve]ngeance upon A[murru]*." Indeed the very purpose of including the reference to the accession to the throne in ll. 1'–3' seems surely to have been to provide the chronological setting for *Ar-ma-a*'s attack upon Amurru, so that divorcing the two clauses to any significant degree

⁴¹ For ease of reference, here the first lines of col. ii as they appeared in Miller (2008: 535): ^(1')x[... *ma-aḫ-ḫa-an-ma-za-kán*] ^(2')m_x[...] ^rA⁷¹ -[NA G¹S¹Ú.A/GU.ZA] ^(3')LUG[AL-UT-TI] ^re-ša¹-at [...] ^(4')nu ^rú¹-[i]t ^mAr-ma-a-aš KUR ^{URU}rA¹-[mur-ri EGIR-an] ^(5')rša¹-an-ḫi-iš-ki-u-wa-an ti-i[a-at].

⁴² Wilhelm (2009: 111f.) states that I assume an Egyptian name should be restored here rather than a Hittite one, but this is not the case. In my papers (Miller 2008: 538, 552 n. 64) I mentioned that perhaps Ari-Teššub of Amurru or Ar-Ḫalba of Ugarit might be candidates for the restoration, without committing to either.

⁴³ In my previous paper I wrote that one would expect a pronominal resumption, but of course, since we are dealing with a transitive clause, one would expect no pronoun at all.

⁴⁴ See now Rieken (2010) and van den Hout (2010), which I have not yet been able to assess for present purposes.

⁴⁵ Incidentally, Wilhelm's counterexample (2009: 113 and n. 23) is not immediately relevant, since in it Uḫḫa-ziti appears as the object of the preceding phrases, in which a thunderbolt had terrified him and made him ill. This is followed by a sentence in which Uḫḫa-ziti is subject – though unnamed and resumed as a pronoun – of an intransitive med.-pass. verb. That Uḫḫa-ziti is explicitly named in the following sentence, which begins a separate relative clause, is thus not strictly comparable to the present situation.

seems highly inadvisable. Even with Wilhelm's translation (2009: 112), "[Im folgenden Jahr aber] setzte [sich] P[N] au[f den Thron] des Königtums," one would surely want to continue with "w[orauh]n/w[onac]h *Ar-ma-a* begann sich um A[murru] zu [kü]mmern/für A[murru] zu [rä]chen," so that also in this case one would expect no explicit resumption in l. 4' if *A[r-ma-a]* were to be restored in l. 2'. The contextual proximity of the two clauses is thus not dependent on the first being a temporal relative clause (cf. Wilhelm 2009: 112),⁴⁶ but on the narrative content and on the restoration of ^rú¹-[i]t (or E[GI]R). If the clauses are not to be linked in this manner, then one is left with the narration of events in the now lost lines preceding l. 1', followed by the statement, "[Im folgenden Jahr aber] setzte [sich] P[N] au[f den Thron] des Königtums," which would thus end the section and be left dangling there with no function. This in turn would be followed by "(4) And *Ar-ma-a* began [...] to take [ve]ngeance upon A[murru], (6) and he sent troops and chariots to the Land of [Amurru] to attack," without any temporal framing. This would yield a very odd narrative structure. Thus, while it cannot be categorically excluded that *A[r-ma-a]* could be restored in l. 2', it still seems quite unlikely.

Another of Wilhelm's considerations (2009: 112), however, might allow one to avoid this question altogether. As he has rightly seen, if Ay was the person who ascended the throne in l. 2', one would not expect the writing *A-ia*, which would not fit the visible trace of a wedge, but some spelling of Ay's throne name, *heper-hepru-re* (*iry m3't*). Wilhelm then assumes that one would expect "nach Analogie der bezeugten Thronnamen auf -chepru-re" the sign H_U, and he correctly notes that the trace in l. 2' is not amenable to the restoration ^mH[*u-riya*]. It is, however, perfectly amenable to the restoration ^mH[*ur-riya*], a writing which is amply enough attested in comparable names (Hess 1993: 221f.), so that Ay ascending the throne at this juncture would be perfectly amenable to Wilhelm's suppositions.

It is not entirely certain, though, that one would expect the name *heper-hepru-re*, should it appear in cuneiform, to begin with *hu-ur-* or *hur-*. Wilhelm in the passage just quoted refers to writings analogous to the **second** part of Ay's throne name, *hepru-re*, which one would indeed expect to begin with *hu-ur-* or *hur-*; but naturally the relevant question is how the **first** element of the name, *heper-*, might be expected to appear. They are, of course, not the same word. The first element, *heper-*, is generally translated "everlasting", the second, *hepru*, as "forms, manifestations". Unfortunately, I was not able to find any cuneiform representations

⁴⁶ On a minor note, Wilhelm (2009: 112) suggests that "die Ergänzung zu einem Temporalsatzgefüge ist, wie soeben festgestellt, eher unwahrscheinlich." The legitimate considerations of Wilhelm's two preceding paragraphs do not, however, make it *unwahrscheinlich* that the first lines of col. ii are to be restored as a temporal relative sentence; they merely offer a plausible alternative. I would still hold the temporal relative sentence introduced with *mahhan* for significantly more likely, as it seems clear that the reference is intended to create the temporal frame for the following events and since phrases in which a king takes a throne are commonly temporal sentences in Hittite historical narratives.

of names beginning with *heper-*. I suspect, however, that the name might begin with a *he/i-* or *ha-* (M. Gabolde [email 28. 3. 2011] kindly informs me that he would tentatively expect “perhaps Xapxururia or Xipxururia”, but “as the ‘p’ of xpr generally disappears, Xaruxururia and Xiruxururia could also occur.”). The trace of a wedge in l. 2’ of the text in question would of course fit a *he/i-* quite well, but not a *ha-*. The trace in l. 2’ could therefore, purely graphically at least, allow for Groddek’s suggestion of Arnuwanda, Wilhelm’s suggestion of *Ar-ma-a*, my previous suggestions of Ar-Ḫalba or Ar(i)-Teššub (Miller 2008: 538, 552 n. 64) or Ay’s throne name Ḫeper-ḫepuru-re. Historical, chronological and contextual considerations, however, render Groddek’s suggestion very unlikely, while contextual and syntactical considerations militate against Wilhelm’s. Among the remaining possibilities, perhaps Ay’s throne name or, surely less likely, Ar(i)-Teššub’s name might be considered the most likely.⁴⁷

What name is to be restored in l. 2’ of KUB 19.15+KBo 50.24 thus remains a matter of speculation. Of the suggestions put forth thus far, Ay’s throne name Ḫeper-ḫepuru-re can be considered a leading candidate. The events of col. ii, likely to be dated to Muršili’s 9th year, would thus have taken place while Horemheb was not yet pharaoh, perhaps shortly after Ay ascended the throne of Egypt.

The Chronology of Rib-Hadda’s Correspondence

Another of Wilhelm’s arguments concerns the placement of Rib-Hadda’s earliest letters – including EA 75 in which he refers to the Hittite conquest of Syria – within the reigns of Amenhotep III and Akhenaten. It will be remembered that Rib-Hadda, the ruler of Byblos, wrote dozens of letters to Egypt complaining mostly about the behaviour of his archrivals ‘Abdi-Aširta and later his sons, primarily Aziru, rulers of Amurru. The placement of EA 75 in relation to Amenhotep III and Akhenaten is important for the questions at hand because it is the most tangible clue available concerning the dating of Šuppiluliuma’s first Syrian campaign and thus the date of his ascension to the throne.

While I have followed Moran and others in placing Rib-Hadda’s earliest letters in the late years of Amenhotep III (Miller 2007a: 284f.; cf. similarly Hachmann 2001: 143ff.), Wilhelm (*in press*, §4.1.1) would like to place them toward the middle of the reign of Akhenaten. Moran’s (1992: xxxv–xxxvi, n. 127) primary argument is based on the fact that Rib-Hadda, when writing to Akhenaten, repeatedly encourages the pharaoh to act toward his archenemy, Aziru, like the pharaoh’s father (Amenhotep III) had done with regard to ‘Abdi-Aširta. In Rib-Hadda’s earlier letters, however, in which ‘Abdi-Aširta is still his primary nemesis never does he remind the pharaoh of his father’s previous action against his nemesis. This would

⁴⁷ Without even discussing what writing of the name might fit the traces, Freu (2008: 47f.) suggests Tuppi-Teššub, whose reign indeed would have begun at approximately this time.

be difficult to explain, so the argument goes, if these earlier letters had also been addressed to Akhenaten – why not urge Akhenaten to rein in ‘Abdi-Aširta like his father had done? – suggesting that the letters in which ‘Abdi-Aširta appears are to be dated to the reign of Amenhotep III, not to that of Akhenaten.

Wilhelm (*in press*: §4.1.1) counters by discrediting the “widespread, though unfounded assumption that at the end of his career ‘Abdi-Aširta was brought to Egypt and executed there”. In this point Wilhelm is surely correct, but Moran’s argument is not based on ‘Abdi-Aširta having been executed in Egypt. The thrust of Moran’s argument, which remains valid, is that in none of Rib-Hadda’s numerous letters to Akhenaten does one find him encouraging the pharaoh to discipline ‘Abdi-Aširta as his father had disciplined ‘Abdi-Aširta; we only find him encouraging the pharaoh to discipline *Aziru* as his father had disciplined ‘Abdi-Aširta. In other words, had ‘Abdi-Aširta lived for any significant length of time into the reign of Akhenaten, we would have expected Rib-Hadda to encourage that pharaoh to discipline ‘Abdi-Aširta as his father had disciplined the same ‘Abdi-Aširta. The argument is thus not dependent on ‘Abdi-Aširta having been executed in Egypt; it is predicated upon ‘Abdi-Aširta not having outlived Amenhotep III at all or by very much.

Wilhelm (*ibid.*) also seeks to weaken the argument by suggesting that ‘Abdi-Aširta’s capture and reprimand in Egypt may have taken place before the date of Rib-Hadda’s earliest letters, and that he henceforth behaved as a proper Egyptian vassal. Again, while true, this also does nothing to weaken Moran’s argument. Regardless of when Egypt’s bridling of ‘Abdi-Aširta’s took place within the reign of Amenhotep III, Rib-Hadda encouraged Akhenaten to act likewise, but always with regard to *Aziru*, never with regard to ‘Abdi-Aširta.⁴⁸

The Reduction of Horemheb’s Reign and Synchronisms with Babylonia

Recent work by van Dijk (2008) has strongly reinforced the likelihood that Horemheb reigned only some 14 years, perhaps beginning his 15th year at the most, rather than ca. 27/28 years as long assumed by most researchers (e.g. von Beckerath 1995; Kitchen 2000: 43; cf. e.g. Helck 1983: 47ff.; 1987: 19f.; Krauss 1994). The importance of these findings can hardly be overemphasised. Indeed, as von Beckerath has written (1995: 37), “Die absolute zeitliche Festlegung der XVIII. Dynastie hängt weitgehend von der Frage der Regierungsdauer des Horemheb, ihres letzten Herrschers, ab.” Van Dijk’s findings have thus caused Egyptologists to scramble to attempt to reconcile Horemheb’s reduced reign with what has generally been considered the astronomically fixed period of 200 years between the accession of Thutmose III and that of Ramesse II, 1479 to 1279 B.C.E. according to the low chronology.⁴⁹ Further, a truncated reign for Horemheb has long been understood to lead to serious difficulties in reconciling the Egyptian and the Babylonian chronologies (e.g. Groddek 2007a: 96, n. 8). The present section considers these issues, examining whether the reduction of the reign of Horemheb might be compatible with the Babylonian evidence.

The Babylonian chronology has often been linked to the Egyptian at two points in the following manner (Fig. 1):⁵⁰ At the upper end Kadašman-Enlil I wrote a number of letters to Amenhotep III (EA 2–3, perhaps 4; responses in EA 1, 5), in one of them rebuking the pharaoh for not having informed him of his “great festival” (EA 3: 19–22), generally interpreted to be one of the *heb-sed* festivals celebrated in his 30th, 34th and 37th years (e.g. Kühne 1973: 54). Kadašman-Enlil’s son and successor, Burna-Buriyaš II, is also attested as having written to Amenhotep III (EA 6; Kühne 1973: 129, n. 642; Boese 1982: 16f.), so that his accession to the throne must have occurred between the pharaoh’s 30th/31st regnal years and the end of his 37/38-year reign. As Amenhotep III reigned from 1391/90 to 1353/2 B.C.E., again according to the low chronology, Burna-Buriyaš cannot have ascended the throne earlier than about 1359 B.C.E., shortly after Amenhotep’s 30-year jubilee, and not later than ca. 1353/2 B.C.E., the year of the pharaoh’s death. In other words, Burna-Buriyaš Year 1 must fall between approximately Amenhotep’s 31st and 37th years.

The lower end of the time span in question is based on a letter (KBo 1.10+KUB 3.72)⁵¹ from Ḫattušili III to Kadašman-Enlil II (r. 1263–1255 B.C.E.)⁵²

⁴⁸ Also of note is Wilhelm’s comment (2009: 113), “Konfrontieren wir unter dieser Vorgabe abermals die ägyptischen mit den hethitischen Regierungslängen, so ergibt sich bei der Annahme, der Abstand zwischen der *taḫamunzu*-Affäre und Šuppiluliumas Tod betrage 6 Jahre, dass Semenchkare der verstorbene Pharao ist, während die These, dass es sich um Achenaten handele, den betreffenden Zeitraum um drei Jahre verlängern muss.” In other words, identifying Smenkhkare with the deceased pharaoh of the *taḫamunzu*-episode would result in some 14 years of Šuppiluliuma’s reign falling before the *taḫamunzu*-episode, about six years of his reign thereafter. This must be considered in light of Wilhelm – Boese’s (1987) re-examination of the Deeds, the central conclusions of which were that (1) Šuppiluliuma’s reign must be reduced to some 20 years, and that (2) ca. half of his reign must fall before the *taḫamunzu*-episode, ca. half of it thereafter. It is thus clear that Wilhelm’s retention of the equation of Smenkhkare with the deceased pharaoh produces numbers (14 before, 6 after) that led him and Boese to reject the alternatives (i.e. Akhenaten or Tutankhamun) in the first place. That Wilhelm has de facto abandoned the scheme that originally led him and Boese to equate Smenkhkare with Nibḫururiya is similarly revealed when he writes (*in press*, §3.5.3), “During the first years of Amenophis IV’s reign the close alliance between Mittani and Egypt deteriorated and eventually broke off. This was the situation in which the Hittite King Suppiluliuma, who had ascended the throne a few years earlier, decided to lead a fatal strike against the Mittanian Kingdom.” Since it is clear that the breaking off of Mittanian-Egyptian correspondence occurred in about the 5th or 6th year of Akhenaten’s reign, placing the accession of Šuppiluliuma “a few years earlier” places it roughly contemporary with or shortly after that of Akhenaten. Either way, he will have reigned at least some 12–15 years or so before the death of Akhenaten, ca. 15–18 years before that of Smenkhkare (i.e. 15–18 before, 2–5 after). Wilhelm thereby implicitly recognises that an accession for Šuppiluliuma no earlier than the middle of Akhenaten’s reign, demanded by Wilhelm – Boese’s reconstruction (1987), is untenable (cf. Miller 2007a: 276–282).

⁴⁹ See most recently Krauss (2007b: 180–183).

⁵⁰ See e.g. von Beckerath (1995: 39–41); Kitchen (2007: 168f). For recent reviews of Babylonian chronology, see Sassmannshausen (2006); Pruzsinszky (2009). Connecting the 2nd year of Melišipak with the 8th year of Ramesse III based on the dated document from a destruction layer at Emar (e.g. Krauss 1994: 78ff.) will not be further considered here.

⁵¹ For the most recent translations, with refs. to earlier literature, see Hoffner (2002) and Mineck (2006).

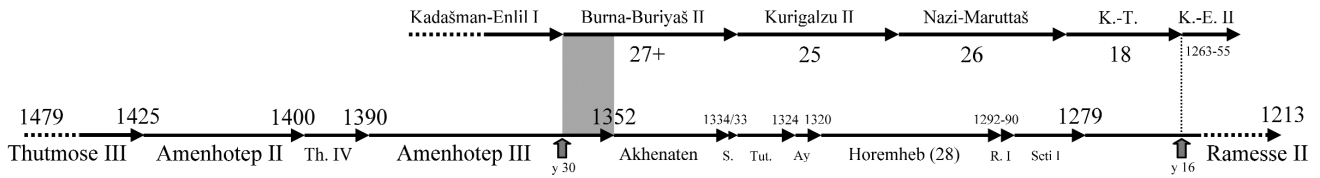


Fig. 1
Babylonian vis-à-vis
Egyptian Chronology
from Burna-Buriyaš II
to Kadašman-Enlil II

in which the Hittite king mentions that its addressee's father, Kadašman-Turgu, had once offered him military support against Egypt. Assuming that this letter must date to the Hittite-Egyptian peace treaty in Ramesse II's 21st year at the latest, one can then place the ascension of Kadašman-Enlil II between Ramesse's 14th and 18th years.⁵³

Between these two poles, i.e. the accession dates of Burna-Buriyaš II and Kadašman-Enlil II, one can calculate a minimum number of years based on the Babylonian Kings List and year names from administrative documents, 96 in total.⁵⁴ These 96+ years must be distributed over a period stretching from the last seven years or so of Amenhotep III (at the most) through the reigns of Akhenaten (ca. 18 years), Smenkhkare (ca. 3[?]), perhaps the *tahamunzu* (ca. 1[?]), Tutankhamun (ca. 10), Ay (ca. 4), Horemheb (ca. 15), Ramesse I (ca. 2), Seti I (ca. 11) and the first 16 years of Ramesse II.⁵⁵ Adding these reigns together according to the traditional chronology, which gave Horemheb some 27/28 years, yields 99 years, just enough to match the Babylonian kings. Once one subtracts some 13/14 years from Horemheb's reign, however, only some 87 years at the most remain, nearly 10 years less than the minimum attested for contemporaneous Babylonian kings.

⁵² (1) This is the date that results from Wilhelm – Boese's (1987) short Assyrian chronology, which I retain here because it is commonly accepted, despite reservations (see n. 58). (2) Neither the uncertainty inherent at this point in the Assyrian chronology (e.g. Pruzsinszky 2009: 32) nor that in the synchronisms that link the Babylonian to it (e.g. Sassmannshausen 2004) affects the calculations here, since the 96+ years for the four Babylonian kings in question and the synchronism Burna-Buriyaš I = Amenhotep III 31–37 remain unaffected, so that sliding the ascension of Kadašman-Enlil II to a later year of Ramesse II and adding x years to the number of years for the Egyptian kings on the lower end would require subtracting the same number of x years from some pharaoh(s) between Amenhotep III and Seti I toward the higher end.

⁵³ Similarly Kitchen (2007: 168f.); von Beckerath (1978: 48f.; 1995: 40).

⁵⁴ I.e. 27 years attested in administrative texts for Burna-Buriyaš II; 24 in administrative texts for Kurigalzu II, with 25 the most likely reading in the Babylonian Kings List; 26 in the BKL for Nazi-Maruttaš, 24 from administrative texts; 18 (or 17) in BKL for Kadašman-Turgu, 17 in administrative texts (Brinkman 1976; Grayson 1980–83: 90ff.; Sassmannshausen 2004; Pruzsinszky 2009: 83–101); for the ephemeral rulers Kara-ḥardaš and Nazi-Bugaš following Burna-Buriyaš II and for Kadašman-Enlil "IIa" following Nazi-Maruttaš see most recently Boese (2009), who also considers shortening the reigns of Kurigalzu II, Nazi-Maruttaš, Kadašman-Turgu and Kadašman-Enlil II by one year each.

⁵⁵ Here I have generally assumed a maximum number of years so as to give the calculations a chance of success. Aston (*in press*), e.g. allows 17 for Akhenaten, 4 for Smenkhkare/Neferneferuaten, 1 for Ankhethkeperure, 9 for Tutankhamun, 3 for Ay, 14 for Horemheb, one for Ramesse I and 8 for Seti I. My numbers are thus some seven years more generous than Aston's (cf. n. 56); cf. ranges in Müller (2006: 212).

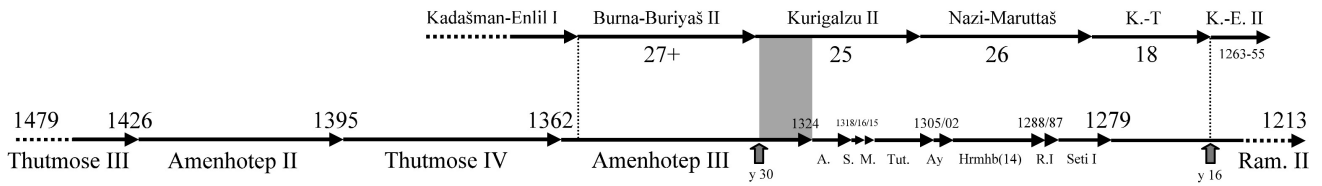


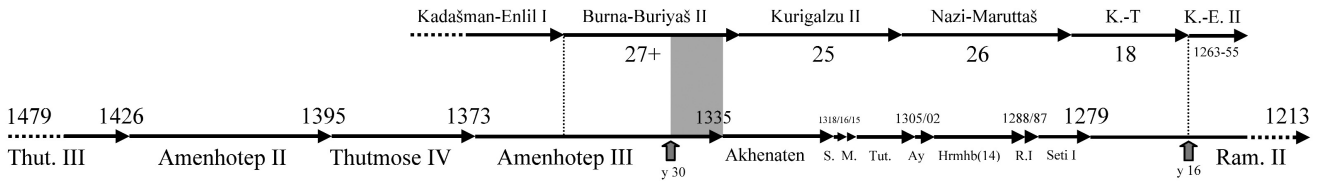
Fig. 2
Babylonian vis-à-vis
Egyptian Chronology
from Burna-Buriyaš II
to Kadašman-Enlil II
with Aston's revisions

Recently, Aston (*in press*) has suggested a revision of the chronology of the 18th and early 19th Dynasties in light of the reduction of Horemheb's reign. His major adjustments are an increase of Amenhotep II's reign by about 7 years and Thutmose IV's by some 23 years. Further, he assumes a ca. 10-year coregency of Amenhotep III and Akhenaten. As can be seen in Fig. 2, however, Aston's adjustments push the reign of Amenhotep III down some 20 to 30 years, so that between Amenhotep III's Year 30 and Ramesse II's Year 16 fall only some 69 years, far fewer than the 96+ years attested for the Babylonian kings. His revision thus conflicts dramatically with the common understanding of Egyptian-Babylonian chronological links.

Aston does offer an alternative, the major difference in which is the rejection of the coregency between Amenhotep III and Akhenaten and the subtraction of the resulting years from the reign of Thutmose IV. It is evident, however (Fig. 3), that this does little to remedy the stark discrepancy between the Egyptian and Babylonian chronologies, giving the Egyptian kings only ca. 80 years.⁵⁶

From the perspective of these Babylonian synchronisms, then, the 13/14 years that should now be subtracted from the reign of Horemheb would have to be added to the reigns of the pharaohs between Amenhotep III and Ramesse II, not to those of earlier pharaohs, assuming that reign-lengthening is the adjustment that one should resort to in order to restore the synergy with the Babylonian kings. Otherwise one is forced to find an alternative escape route. One possibility would be abandoning the assumption that the festival mentioned in Kadašman-Enlil II's letter to Amenhotep III was the *heb-sed* festival of his

⁵⁶ These considerations on Aston's revised chronology were written on the basis of a draft version of his paper from Jan. 2010. At the conference in Prague Aston was kind enough to give me an updated version, into which he had incorporated recent research that had led him to suggest four further alternative chronologies for a total of six, which he refers to as E (i-vi). If I am not mistaken, however, none of these alternatives are compatible with the synchronisms with Babylonian chronology assumed here. Aston's E (i) results in 69 years between Amenhotep III's 30th and Ramesse II's 16th year according to the Egyptian low chronology, 80 according to the high; his E (ii) in 80 years for the low, 91 for the high; E (iii) in 80 or 91; E (iv) in 78 or 89; E (v) in 78 or 89; E (vi) in 78 or 89. None of these figures quite arrives at the required 96+ years, though E (ii-iii) come fairly close, whereby one must remain conscious of the fact that these (near) matches would require *every* variable on the Egyptian side to yield a maximum (cf. also n. 55) and *every* variable on the Babylonian side to yield a minimum, which cannot be considered likely.



30th jubilee⁵⁷ and seeking another noteworthy occasion. On the Babylonian side, one could reconsider the seemingly strong linkage between the Assyrian and Babylonian chronologies that re-establishes their synchronicity following the break in the Babylonian lists.⁵⁸ One might even reassess the 200-year time span between the accessions of Thutmose III and Ramesse II.⁵⁹ One cannot, more-

Fig. 3

Babylonian vis-à-vis Egyptian Chronology from Burna-Buriyaš II to Kadašman-Enlil II with Aston's alternative

⁵⁷ Indeed this is the option that Greenwood (2006: 193) favours, apparently not based on any chronological considerations, but essentially because, "According to one Egyptian papyrus, goodwill gifts were typically received on a specific day each year. Therefore, the festival may be in reference to the Egyptian New Year's festival, which has been linked to this annual day of diplomatic gift exchange." It seems much less likely, though, that Kadašman-Enlil would refer to the New Year's festival as a "great festival" (*isinna rabâ*) than, e.g., a *heb-sed* festival, first, because one might expect Kadašman-Enlil to have used one of the common terms in Akkadian for New Year's festivals (Pongratz-Leisten 1999), second, because with "great" Kadašman-Enlil seems to be implying that it was something extraordinary, not ordinary or periodical (with e.g. von Beckerath 1995: 39), and third, because one would surely see far more such invitations in the Amarna (and other) letters if every great king expected to be invited to every New Year's festival, which seems unlikely. For the reference to Greenwood's remark I would like to thank Boaz Stavi.

⁵⁸ For some of the potential vagaries with regard to the Babylonian reconstruction, see Brinkman (1976: 32, n. 89). In my view 46 years for Aššur-dān I and 13 for Ninurta-apil-ekur (see Grayson 1980–83: 111) in the Assyrian Kings List are by far and away the better working hypotheses, pace Boese – Wilhelm (1979) and Pruzsinszky (2009: 60). First, in principle, if one has two versions in which 46 is clearly legible and one damaged version that allows various readings, including 36 and 46, it would be ill advised to opt for 36. Second, even in the Nassouhi list (rev. iii 42'), 46 is the much more likely reading, as the two clearly visible wedges are set high in the line, suggesting that one was also placed below them; since a third wedge in the upper row is clearly to be expected – and is indeed visible on Nassouhi's drawing (1927: 5) – as can be seen if one extends the preserved column divider from above and below the line in question; and because in the published photo (Nassouhi 1927: Taf. 6) traces of what seem likely to be the lower wedge are in fact visible (similarly Sassmannshausen 2006: 165). Cf. Weidner (1945–51: 88, n. 16): "Ich habe Nassouhis Autographie noch einmal genau mit der mir vorliegenden Originalphotographie des Textes, die wesentlich deutlicher ist als die Wiedergabe in AfO 4, Tf. I–II, verglichen und ... Rs. I, 42 ist 46(!) zu lesen, der unter den drei oberen stehende vierte Winkelhaken ist noch ziemlich deutlich zu erkennen"; and Brinkman (1973: 309, n. 15), who reads 26[+x]: "Weidner's reading of 46! (based on a photo) ... seems to me excessively optimistic, considering both the tablet and the old excavation photos." Regarding Ninurta-apil-ekur, Freydank (1991: 73–78) was able to show that some five or six eponyms should likely be attributed to his reign, and Llop (2008) has recently strengthened the case (see also Pruzsinszky 2009: 61), suggesting that 13 should be preferred to 3. A further long-standing difficulty was recently all but resolved by Heather Baker, who in her paper at the Rencontre in Barcelona was able to demonstrate that *tuppīšu* should indeed be interpreted as "one year"; cf. Freydank (2007); Janssen (2007).

⁵⁹ As, e.g., Helck (1983) and Krauss (1994; cf. Krauss 2007b: 181ff.) have done or have considered doing. For a recent assessment of the weak links in New Kingdom chronology in general, see Müller (2006).

over, exclude the possibility that there are errors in the Kings Lists that we are presently unable to recognise and correct.⁶⁰

Since the reigns of the four Babylonian kings from Burna-Buriyaš II to Kadašman-Turgu cannot be reduced appreciably (see ns. 54, 60); since it seems ill advised to abandon the synchronism between Kadašman-Enlil I and Burna-Buriyaš II with Amenhotep III in the latter's last years (see n. 57); since the lengths of the reigns of Amenhotep III, Akhenaten, Tutankhamun and Horemheb are quite secure, while it seems very unlikely that one should add a significant number of years to the reigns of Ankhethkeperure, Smenkhkare, Ay, Ramesse I or Seti I (Aston *in press*; von Beckerath 1995: 41); it appears unavoidable that (1) the supposed synchronism between the ascension of Kadašman-Enlil II and Ramesse's Year ca. 16 and (2) the year 1279 B.C.E. for the ascension of Ramesse II will have to be abandoned.⁶¹ It seems likely that – assuming for the moment that the dates for the Babylonian kings are not too far off the mark – the reigns of Horemheb's successors will have to be pulled upwards.

Indeed, the synchronism between Kadašman-Enlil II Year 1 and Ramesse Year ca. 16 has long been recognised, by some at least, as extremely tenuous at best.⁶² It makes a number of assumptions, some based on questionable text restorations in KBo 1.10+, that are either dubious or now known to be incorrect: (1) It assumes, first and foremost, that KBo 1.10+ was composed before the Hittite-Egyptian peace treaty in the 21st year of Ramesse's reign, and thus, that Kadašman-Enlil's ascension to the throne can be used as a *terminus post quem* for Ramesse's 21st year or, vice versa, that Ramesse's 21st year can be used as a *terminus ante quem* for Kadašman-Enlil's enthronisation;⁶³ (2) it assumes that the quarrel between Ḫattušili and Egypt during the time of the addressee's father, Kadašman-Turgu, which prompted the latter to offer Ḫattušili his military support should he decide to go to war against Egypt, could only have occurred during the period preceding the Hittite-Egyptian peace treaty; (3) it generally assumes that the Urḫi-Teššub affair

⁶⁰ Indeed, this is quite likely. One can only assume concerning those sections of text for which no duplicates or other correctives are available that they would contain on average a similar number of errors per corresponding length of text as those sections in which errors have been found and corrected by duplicates or other means. It is, however, essentially impossible to reduce the reigns of the five kings under discussion by more than two–three years (cf. n. 54).

⁶¹ The year 1279 B.C.E., of course, has long been regarded as the most secure date of the New Kingdom, significantly more reliable than that of 1479 B.C.E.; see e.g. Helck (1983: 47–49; 1987: 18); Müller (2006: 208).

⁶² E.g. Rowton (1960; 1966: 243–252); Brinkman (1976: 6f. n. 1); Pruzsinszky (2009: 88).

⁶³ E.g. Edel (1958); Tadmor (1958: 139f.); Rowton (1959: 6 and n. 31); von Beckerath (1978: 48f.; 1995: 40); Kitchen (2007: 168). More precisely, these authors place Kadašman-Enlil's ascension some two–three years before the peace treaty, in order to allow for various events of indeterminate length between his ascension and the treaty. Von Beckerath seems to allow for the possibility that KBo 1.10+ might have been written shortly after the peace treaty, placing Kadašman-Enlil's ascension some two–three years before it, as he locates the writing/sending of the letter “um die Zeit” (1995: 40) or “zur Zeit des Friedenvertrages” (1978: 48).

must have been resolved before the peace treaty would have been ratified (e.g. Kitchen 2007: 168); (4) it sometimes assumes that KBo 1.10+ was written to Kadašman-Enlil as soon as he was enthroned (e.g. Liverani 1990: 132; Bryce 2003: 216); (5) it often assumes that Urḫi-Teššub was indeed granted exile in Egypt, taking for granted that Ramesse would hardly have done so after having ratified a peace treaty with Ḫatti (e.g. Bryce 2003: 216).

Against assumption (1) militate, *inter alia*, (i) the fact that Ḫattušili explains to Kadašman-Enlil that he has no objections to Babylonia resuming its friendly exchange of envoys with Egypt (Rowton 1960: 16f.); (ii) the intimation in §7 that Ḫattušili and Egypt had already made peace during the reign of Kadašman-Turgu, and that already during his reign Kadašman-Turgu had offered Ḫattušili military support in case he decided to go to war over Egypt's purported harbouring of Ḫattušili's "enemy", now generally assumed to have been Urḫi-Teššub.

Against (2) one need only counter that it is hardly an outlandish suggestion that Kadašman-Turgu may have offered his military support in response to Ḫattušili expressing his grave aggravation with Egypt's harbouring – or so he assumes – of Urḫi-Teššub (e.g. Freu 2008: 263). The issue was clearly terribly important to Ḫattušili, and it is not difficult to imagine that Kadašman-Turgu understood his vehemence as leading him to the brink of war. Moreover, even if this offer of military aid were to be placed before the peace treaty, the ascension of Kadašman-Enlil could still end up falling considerably later, as seen in Fig. 4, in which it would fall in Ramesse's 35th year.⁶⁴

Against (3) it is now abundantly clear that the Urḫi-Teššub affair continued long into the period of "eternal peace" between Ḫatti and Egypt.⁶⁵ In fact, Bányai (2010: 2f.) argues that most if not all of the 13 Hittite-Egyptian letters that refer to the Urḫi-Teššub affair date to after the peace treaty. It is also known that Urḫi-Teššub first ruled a land in north-western Syria for some time (Bányai 2010), during which he negotiated with Babylon and Assyria about regaining his throne, and thereafter was banished "to the seaside", after which he fled, to Egypt according to Ḫattušili (Bryce 2003: 214ff.). Some time before the wedding of Ḫattušili's and Puduḫepa's daughter to Ramesse II in the latter's 34th year Puduḫepa still accuses Ramesse of hosting Urḫi-Teššub (Bryce 2003: 219).

⁶⁴ The graphic assumes that Muwattalli died two years after the Battle of Qadesh (for arguments that it should be more, see Rowton 1966: 244); that Urḫi-Teššub reigned ca. seven years, despite Klinger's (2006: 322) reservations, which, though justifiable, are surely too stringent for present purposes; that Kadašman-Turgu's offer of military aid occurred half way through the first four years of his reign; that Itti-Marduk-balātu held the reins of power for the first three years of Kadašman-Enlil's reign; that KBo 1.10+ dates to the mid-point of the remainder of Kadašman-Enlil's eight-year reign. All these assumptions are made merely for the purposes of the graphic representation and contain more or less leeway.

⁶⁵ See e.g. Bryce (2003: 213–222); Singer (2006); Bányai (2010).

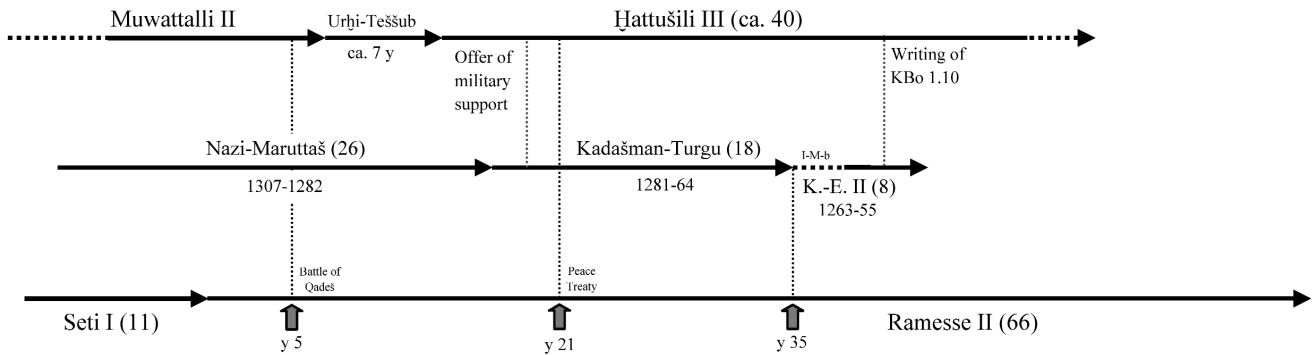


Fig. 4
Chronologies with
Kadašman-Turgu's offer
placed before the peace
treaty, the writing of KBo
1.10+ thereafter

Contrary to the assumption in (4) there is, apart from the reference to his coming of age, no clear evidence in KBo 1.10+ concerning when it was composed during Kadašman-Enlil's ca. eight-year reign. It is not known how long Itti-Marduk-balāṭu held the reins of power while the young king matured, and it is not known how long after his assumption of duties he received Ḫattušili's letter. Indeed, one might just as well assume that Itti-Marduk-balāṭu might well have kept the young king under control for some four–five years and that KBo 1.10+ witnesses enough correspondence between Ḫattušili and Kadašman-Enlil to fill up most of the rest of his reign, placing KBo 1.10+ toward the end of his eight-year rule.

Against assumption (5) Singer (2006) has shown that Ḫattušili's and Puduḫepa's repeated claims that Urḫi-Teššub was granted asylum in Egypt remain entirely unsubstantiated. In fact, while Ḫattušili's and Puduḫepa's letters at times accuse Ramesses of harbouring Urḫi-Teššub, they also implore him to capture Urḫi-Teššub and to hold him in Egypt (Bryce 2003: 217).⁶⁶

Furthermore, the synchronism suffers from a deadly case of confirmation bias, in that it recognises those hints that might suggest the letter was written very early, but ignores those hinting it was written quite late, such as (a) Ḫattušili's emphasis that Itti-Marduk-balāṭu had lived to a great age (§4), which, though clearly hyperbole (de Martino – Imparati 1995: 105), surely suggests that this vizier was, if not quite primeval, at least aggravatingly long-lived; (b) the Hittite king's complaints concerning Kadašman-Enlil himself discontinuing his correspondence (§6), i.e. following the period of Itti-Marduk-balāṭu's control of diplomacy and his own ascension, while Ḫattušili claims to have written repeatedly; (c) the long – from Ḫattušili's perspective – cessation of correspondence, after which Kadašman-Enlil had indeed corresponded with Ḫattušili for at least some period of time, since KBo 1.10+ itself responds

⁶⁶ If Bányai's (2010: 5ff.) recent plausible suggestion that Ramesses had married a daughter of Urḫi-Teššub proves to be correct, it would provide an obvious reason for Ḫattušili's suspicions, though still no evidence for his whereabouts.

to numerous issues raised by him (§§6–7, 9ff.), and since Ḫattušili comments extensively on the gifts he had received from Kadašman-Enlil (§§17ff.);⁶⁷ (d) Ḫattušili's narrative concerning the medicine man that Kadašman-Enlil had sent him, who had subsequently become ill and, despite Ḫattušili's concentrated efforts, died (§12).

If one therefore abandons this wholly untenable synchronism, it becomes immediately clear, given the present state of knowledge, that the best estimate for the chronological relation between Kadašman-Enlil II's and Ramesse's reigns will be given by adding together the lengths of the reigns of their predecessors up to the synchronism Burna-Buriyaš 1 = Amenhotep 31–37. As seen above, from the ascension of Burna-Buriyaš II to that of Kadašman-Enlil II one must reckon with at least 96 years. For the Egyptian side one can assume 17 years for Akhenaten, 3⁷ for Smenkhkare, 1⁷ for the *taḫamunzu*, 9 for Tutankhamun, 4 for Ay, 14 for Horemheb, 2 for Ramesse I and 11 for Seti I, for a total of 61;⁶⁸ to this one could add, for present purposes, say 3 years, if we conveniently allow Kadašman-Enlil I to die half way through the last 7–8 years of the reign of Amenhotep III, giving us 64 years. In order to reach the 96+ years on the Babylonian side, we must add to these 64 years the first 32 years of Ramesse II's reign as well, suggesting that Kadašman-Enlil II is likely to have ascended the throne sometime around the 32nd year of Ramesse II (Fig. 5).⁶⁹ Placing Ramesse's 32nd year in ca. 1263 B.C.E. yields an ascension date of ca. 1295 B.C.E., i.e. some five years higher than the previously favoured 1290 B.C.E., some nine years lower than the once preferred 1304 B.C.E.⁷⁰

⁶⁷ Alone the issue addressed in §11, Bentešina's cursing of Babylonia, requires some length of time in which correspondence between Ḫattušili and Kadašman-Enlil had obtained: Kadašman-Enlil had written Ḫattušili about the problem, Ḫattušili had written to Bentešina about it, Bentešina had replied to Ḫattušili, and Ḫattušili had taken action on it, forcing Bentešina to take an oath on the matter and to send a diplomat to Babylon to make amends; and since a messenger of Kadašman-Enlil had gone to hear Bentešina swear this oath, one can assume that Ḫattušili had already told Kadašman-Enlil to send this messenger to witness Bentešina's contrition. Cf. Na'aman (1998: 63 and n. 5).

⁶⁸ Here I use the most likely and generally accepted figures (Hornung 2006; Kitchen 2000; Müller 2006); cf. n. 55.

⁶⁹ For the assumptions in the graphic see n. 64. The potential error – in addition to that inherent in the Babylonian/Assyrian chronology at this point – would be ca. $\pm 5/6$ years, i.e. ± 3 for the range within the last 7–8 years of the reign of Amenhotep III and some $\pm 2-3$ years for the troublesome period between Akhenaten and Tutankhamun, even apart from the not yet entirely settled reign lengths of e.g. Seti I, thus easily allowing 1290 B.C.E., perhaps also 1304 B.C.E., for Ramesse II's ascension. It is also worth noting that this graphic, if one assumes the synchronism Muršili 9 = Horemheb before becoming pharaoh, yields a combined 43+ years to Muršili II and Muwatalli II, which accords well enough with the numbers generally attributed to them, i.e. ca. 26+ for the former, ca. 23± for the latter (e.g. Bryce 2005: xv). Naturally, since the reign lengths of these Hittite kings are only very approximately known, this can be used neither as support nor as counterargument for these chronological reconstructions.

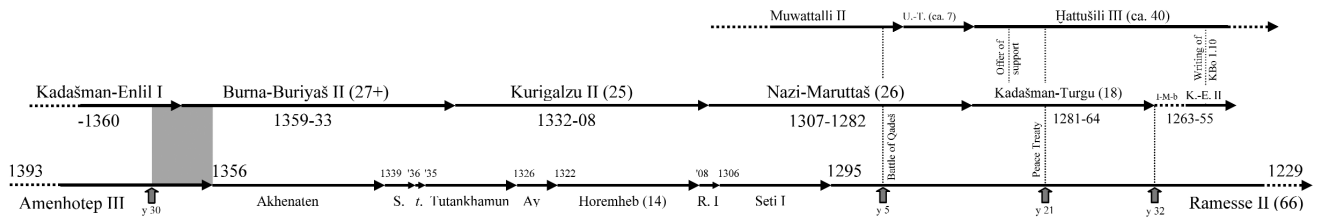


Fig. 5

Babylonian vis-à-vis
Egyptian Chronology
anchored at Burna-
Buriyaš II's 1st Year =
Amenhotep III's 34th year

The Solar Omen of Muršili II's 10th Year

A final point to be briefly touched upon is the much-discussed solar omen of Muršili II's 10th year. In my initial presentation of the chronology of the period I chose to explicitly ignore the issue (Miller 2007a: 288), because it is my view that, since we are not yet able with confidence to determine Muršili's reign with a precision of less than some 20, 30 or 40 years, it makes little sense to attempt to nail his 10th year to one of some six to eight eclipses falling within the ca. 40- or 50-year period in which we would search for an appropriate event,⁷¹ and this remains my approach. Following the publication of my paper Taracha (2008) suggested that the eclipse of the 4th of June 1321 B.C.E.⁷² would fit the scheme suggested in my papers quite well. It is not entirely clear to me, however, that this eclipse would be visible in central Anatolia. On the maps of the Five Millennium Canon of Solar Eclipses it appears to me to lie just outside the maximum extent of visibility. Wilhelm (2009: 115) has recently opted for the eclipse of the 24th of June 1312 B.C.E., abandoning his earlier preference for that of the 17th of October 1309 B.C.E. (cf. Groddek 2007b: 59 n. 51). The suitable eclipse that would lie closest to Muršili's 10th year in my chronological reconstruction would be that of the 26th of August 1315 B.C.E., but for the reasons mentioned a moment ago I would rather not plead ardently for this date.

⁷⁰ The same number is reached, naturally, by subtracting from 1356 B.C.E. (the midpoint of the last seven-eight years of Amenhotep III) the total of these same reign lengths, i.e. 1356-61=1295 B.C.E. As mentioned (n. 58), these dates assume the short Assyrian chronology favoured by most, though I would prefer adding 10 years to it, resulting in Ramesse's ascension in 1305 B.C.E., which would obviously be quite amenable to the once favoured date of 1304 B.C.E. Nemirovskii (2007), judging from his English summary, seems to have reached some similar conclusions, placing Kadašman-Turgu's death between the 24th and 29th years of Ramesse II.

⁷¹ As far as I can see from the Five Millennium Canon of Solar Eclipses (<http://eclipse.gsfc.nasa.gov/5MCSE/5MCSE-Maps-02.pdf>), the potential events between 1340 and 1275 B.C.E. would be those of 14th of May 1338 B.C.E., 26th of August 1315 B.C.E., 24th of June 1312 B.C.E., 17th of August 1306 B.C.E., 24th of April 1290 B.C.E., 15th of June 1284 B.C.E. and 14th of April 1281 B.C.E.; perhaps also 13th of March 1335 B.C.E. (too early in the year?), 15th of August 1333 B.C.E. (sufficient visibility?), 17th of October 1328 B.C.E. (too late?), 17th of October 1309 B.C.E. (visibility? too late? cf. Wilhelm 2009: 115 and n. 34), 5th of August 1305 B.C.E. (visibility?), 24th of April 1290 B.C.E. (visibility?), 17th of September 1279 B.C.E. (visibility?), 6th of July 1275 B.C.E. (visibility?).

⁷² Taracha speaks of the astronomical date, 1320, which would be 1321 B.C.E.

Summary and Conclusion

Of the various arguments against identifying *Ar-ma-a* of KUB 19.15+ with Horemheb before his ascension to the throne, two, it is suggested here, remain serious hurdles. First, as noted in my previous publications (e.g. Miller 2007a: 279 n. 117), the distribution of Šuppiluliuma's regnal years among the tablets of the Deeds as outlined by Wilhelm and Boese would be quite unexpected.⁷³ Second, Simon has shown clearly that one would anticipate a cuneiform writing containing ḥ for the likely contemporary pronunciation of Horemheb's name, *Ḥarmaḥa, which constitutes a serious challenge to the identification of *Ar-ma-a* with Horemheb. While these two observations speak against the identification, it is argued here that they do not tip the scales against it, since multiple strands of further relevant evidence either support it or cannot be deemed, as has been asserted, to constitute significant arguments against it. Further attempts at reconstructing this period in history should thus take into account the likelihood of the suggested synchronism between Muršili Year 9 = Horemheb before becoming pharaoh.

The likely reduction of the length of Horemheb's reign from some 27 to ca. 14 years remains extremely difficult if not impossible to reconcile with current reconstructions of Egyptian and Babylonian chronologies if one chooses to retain the synchronism of Kadašman-Enlil II Year 1 with Ramesse II Year ca. 16. A seemingly more robust reconstruction can be reached by adding together and correlating the lengths of the reigns of the successors of Amenhotep III and Kadašman-Enlil I, which places the ascension of Kadašman-Enlil II at about the 32nd year of Ramesse II, to which one would have to sacrifice the long cherished date of 1279 B.C.E. for Ramesse's ascension.⁷⁴

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⁷³ The only immediately obvious way around this problem would be a lengthening of the reigns of Smenkhkare, Ankhethkeperure and/or Tutankhamun, which does not seem terribly likely.

⁷⁴ It was not possible in this study, which has already grown far beyond original intentions, to thoroughly examine the possible ripple effects of such a chronological scheme, though my initial impression is that it leads to no significant conflicts. Two issues raised by Boese – Wilhelm (1979: 36 n. 65), e.g., constitute no hurdle: (1) Assuming the short Assyrian chronology would place Ḥattušili III on the throne at ca. 1281, the long at ca. 1291, while Adad-nērārī I would reign ca. 1295-1264 with the short, 1305-1274 with the long, both allowing the former to write to the latter; (2) Šulmanu-ašarēdu I would have come to the throne some 22 years before Tutḫaliya IV, allowing correspondence between them as well. More thorough examination, however, will have to be left to further research.

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