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Amarna Age Chronology and the Identity of Nibḫurriya in the Light of a Newly Reconstructed Hittite Text¹

The Newly Reconstructed Text KUB 19.15+KBo 50.24²

At the 6th International Congress of Hittitology in Rome, 5–9 Sept. 2005, I presented a historical text of the Hittite Great King Mursili II that I had been able to reconstruct from seven fragments (Miller, in press a). In this text Mursili recounts the flight of Tette of Nuḫḫašše to Egypt and the interference of Egypt in Amurru, events which can likely be dated to Mursili's 7th and 9th years, respectively.³ In that presentation I only briefly touched on the text's chronological ramifications. The present paper will therefore discuss the document's implications for Amarna Age chronology and the long-debated question of the identity of Nibḫurriya, the deceased pharaoh whose widow, the *tahamunzu*,⁴ writes to Suppiluliuma asking him for a son to become her husband and king of Egypt.⁵ For the reader's convenience, a translation of KUB 19.15+KBo 50.24, which picks up at approximately the bottom half of obv. i, is provided here:

(⁵)Then [...] became hostile^(pl.) towards me, and Titti, [my servant] wrote [to] the ['ma]n^(?) of Egypt (saying): (⁷)'[Send] troops and chariots, [and] ... *shall* ... me forth, (⁸)and [I] will arise [and] come to the land of Egypt.' (⁹)Then the troops and chariots of the land of Eg[ypt] came, (¹⁰)and Titti arose (¹¹)and went to the land of [Eg]ypt. When, however, I

¹ This paper represents an expanded version of a lecture given at the 52^e Rencontre Assyriologique Internationale, Münster, 17–21 July, 2006. Abbreviations employed are those found in the standard reference works CHD (The Hittite Dictionary of the Oriental Institute of the University of Chicago), CAD (The Assyrian Dictionary of the Oriental Institute of the University of Chicago) and *Orientalia*. References to and quotes from the Amarna letters are based on Moran 1992 unless otherwise stated.

² I would like to express my heartfelt appreciation to Marc Gabolde (Montpellier), Dan'el Kahn (Haifa), Rolf Krauss (Berlin) and Nadav Na'aman (Tel Aviv) for reading and commenting on earlier drafts of this paper.

³ For more on the events of Mursili's 7th and 9th years, see now Miller (2007; in press b).

⁴ The Hittite cuneiform representation of Egyptian *t3-ḫ(.t)-nsw*, 'king's wife'; see Federn (1960); Campbell (1964: 54 and n. 73).

⁵ For summaries of this episode, see Klengel (1999: 161 ff.); Bryce (1998: 193 ff.; 2003: 187–198); Gabolde (1998: 187–212); Freu (2004: 33–47). The original sources have been conveniently collected by Parker (2002: 32–35) and Gabolde (1998: 187–194; 2001: 31–34).

wrote to ʾArmaʾa (saying): ⁽¹²⁾‘[Si]nce Tetti was m[y] servant, ⁽¹³⁾why then did you send your troops and chariots and [bring] him a[way]? ⁽¹⁵⁾Give my [serv]ant back to me!’ [ʾArmaʾa] ⁽¹⁶⁾did not give [him ba]ck [to me], nor did he [even wr]ite back to me. ⁽¹⁷⁾Then it came about that Zirtaya, [his] servant, wrote to me (saying): ⁽¹⁸⁾‘Sen[d] troops and chariots, ⁽¹⁹⁾and I will arise, and [come] to Ḫattusa.’ ⁽²⁰⁾So I sent troops and chariots, and they brought Zirt[aya, his servant], to Ḫattusa. ⁽²¹⁾Then ʾArmaʾa w[rote] to me (saying): ⁽²²⁾‘Since [Z]irtaya is my servant, [giv]e hi[m back to me]!’ ⁽²³⁾But I wrote back to him (saying): ⁽²⁴⁾‘An[d you]? Wh[y] did you [not g]ive Tetti back to me?’ ⁽²⁵⁾Then ʾArmaʾa remained totally quiet, ⁽²⁶⁾[and] said [nothing] at all! [So] we were [not] on good terms with one another. ⁽²⁸⁾We were [not] at all on [goo]d⁽²⁾ terms.

The upper half of obv. ii is missing, and the text resumes thus:

⁽¹⁾[... But when] ⁽²⁾[PN] sat [upon the throne of] ki[ngship], ⁽⁴⁾ʾArmaʾa began t[hereup]on to *take [ve]ngeance upon A[murru]*, ⁽⁶⁾and he sent troops and chariots to the land of [Amurru] to attack. ⁽⁸⁾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 me, ⁽¹²⁾and I [pu]rsued him. Thereafter I wrote right back to him (saying): ‘You are *taking [ve]ngeance upon* the land of Amurru. ⁽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? ⁽²⁰⁾It was the king of the land of Ḫanigalbat who took the land of Amurru away from the king of the land of Egypt, ⁽²²⁾and then my father defeated the king of the land of Amurru, ⁽²³⁾and [he took the land] of A[murru away] from the king of the Ḫurri land.⁶ [...] ⁽²⁷⁾grasped^(?) [...] land^(?) [...]’

Some few phrases are also preserved toward the top of rev. iii:⁷

‘[...] ⁽³⁾Where/When(ever) the [sto]rm-god thund[ers] ⁽⁴⁾[...] the message of the storm-god ⁽⁵⁾[...] is [...], it shall be the [border] of the Land of A[murru^(?)]. ⁽⁶⁾Whe[re/Wh[en(ever)], then, the storm-god [thun]ders, ⁽⁸⁾[it] shall be the border [of the Land of Amu]rru. [...]’ [...] someone/something^(acc.) [...] the/a word/affair^(acc.) [...]’

ʾArmaʾa = Haremhab, Egyptian Viceroy and Commander in Asia

As I argued in my initial presentation of this text (Miller, in press a), it seems likely that ʾArmaʾa is to be equated with Haremhab in his role as viceroy and commander of Egypt’s Asian territories, that is to say, before his reign as pharaoh. Since the chronological questions to be addressed in this paper are dependent on this identification of ʾArmaʾa and the assumption that he is not yet pharaoh, the argumentation will be very briefly repeated here.⁸

⁶ Note Redford (1992: 177): ‘By a curious sort of “revisionism” the Hittites could, in later generations, consider all the states north of the new line, even Kadesh and Amurru, as once having belonged to Mitanni, and therefore fair game; but Egypt was reluctant to accept this new political “fact”’; and the discussion by Altman (2003: 351 ff.).

⁷ In addition to my comments (Miller, in press a) on the storm-god being the one in this passage to set the boundaries of Amurru, and thus between Ḫatti and Egypt, see Altman (2001: 31 and n. 11).

⁸ This paper will essentially explore the interpretation according to which ʾArmaʾa/Haremhab is not yet pharaoh in KUB 19.15+KBo 50.24. For chronological schemes that allow for his being already pharaoh, see Miller (in press a, Fig. 3).

The plene writing of the name (*Ar-ma-a*, nom. *Ar-ma-a-aš*), presumably an attempt to portray H/H^l/armah/h^l/a, or similar, recalls Haremhab's name as found in excerpts from Manetho, i.e. Armais, Harmais, Armesis and Armaios. In addition to the excerpts from Manetho, all from the first to third centuries CE, briefly discussed in my original presentation of the text (Miller, in press a), three further attestations from two Demotic-Greek texts are of special interest,⁹ due to their certain dates and their occurrence in bilinguals. Here Haremhab is transliterated Harmais (and gen. Harmaios).¹⁰

The question thus arises whether the spellings in this Hittite historical text from the late 14th century and in much later Demotic-Greek bilinguals and excerpts of Manetho might approximate contemporary Egyptian pronunciation. Whatever the precise values of the consonants intended by *Ar-ma-a*, they likely would have been comparable to the first four in Haremhab, i.e. h/h^l-r-m-h/h^l vs. ḥ-r-m-ḥ(-b). In fact, Fecht (1960: § 136–145) demonstrated long ago that the last two elements of the name underwent the development *māḥ^zab > *māḥ^zab > *māḥ^za > *māḥ^a in the New Kingdom (and later to *māḥ), from which one may gather that Ḥarmaḥa would have been the reality behind the writings *Ar-ma-a(-aš)* and H/Armai(s).¹¹

If, however, one rejects the identity of Ḥarmaḥa with Haremhab, then one is forced to accept several disconcerting coincidences and incongruities: First, that both Ḥarmaḥa and Haremhab were 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; second, that this extremely important individual, Ḥarmaḥa, appears nowhere in the Egyptian literature, though he corresponded with a Great King shortly before the accession of Haremhab; and third, that the names of these two individuals whose careers overlap so conspicuously just happen to be practically identical in the Hittite sources on the one hand and the Demotic-Greek and classical texts on the other. To me, Occam's razor forces one to accept, at least until convincing evidence to the contrary becomes available, that Ḥarmaḥa is Haremhab.

As to Ḥarmaḥa's status in this newly won text, there are several points that suggest that he is not yet pharaoh.¹² First, Mursili never refers to him as LUGAL or LUGAL.GAL, as

⁹ Kindly pointed out to me by R. Krauss.

¹⁰ See Lüdeckens (1992: 812f.): Papyrus Zenon 19, 1 Bil, from 257/256 BCE (Spiegelberg 1929: 25); Papyrus Berlin 3116, II Bil., II 30 Bil., from 114/113 BCE (Erichsen 1952).

¹¹ Fecht (ibid.: 78, n. 237) predicted, 'Der Name des Königs „Haremhab“ würde demnach, wenn er in keilschriftlicher Umschreibung auftauchen sollte, *Ḥaramaḥab oder *Ḥaramaḥa lauten müssen.' Cf. already Stefanini (1964). A conventional spelling Ḥarmaḥa will be employed here.

¹² It is often assumed, e.g. by Redford (1973: 47), Murnane (1985: 39 and n. 52), Bryce (1998: 242 and n. 2) and Freu (2004: 135–138), that Mursili II and Haremhab were contemporary rulers, and that it would have been Haremhab who, as pharaoh, led the Egyptian intervention mentioned in the annals of Mursili's 7th and 9th years. This, however, is entirely dependent on the chronologies assumed by these authors, and more specifically, on the assumption that Nibḥururiya is to be identified with Tut^cankhamun, and in Murnane's case (1985: 39 and ns. 40 and 52), that Aya would have come to the throne at about the same time as Mursili. (Even assuming, however, with Murnane, that Aya and Mursili came to the throne at about the same time, and assigning Aya a three-year reign, Mursili's 7th year would have fallen in about Haremhab's 4th, not 10th, year, as estimated by Murnane.) KUB 19.15+KBo 50.24 now presents the first direct evidence, as opposed to chronological inferences, that might indicate whether Haremhab was pharaoh or not during Mursili's 7th and 9th years.

Hittite kings normally do with regard to other kings and Great Kings, but always by name, as Hittite kings habitually do when referring to subordinates. Second, 'Arma'a, if indeed relating to Haremhab, clearly represents his birth name, not his throne name, Djoserkheprure^c. And finally, as Schulman (1978: 46 and n. 17; 1988) has pointed out, the majority, if not all, of Haremhab's inferred military activity occurred during the reign of his predecessors, for the most part under Tut'ankhamun.¹³ These points would seem to outweigh the striking fact that Mursili would thus be corresponding with a subordinate to an enemy Great King, though such a circumstance would not be unparalleled.¹⁴ And though Haremhab would thus be nominally a subordinate to the reigning pharaoh, it is clear from the Egyptian documentation that he behaved and understood himself as essentially the de facto ruler, taking titles and epithets normally reserved exclusively for the pharaoh.¹⁵

If indeed 'Arma'a is to be equated with Haremhab, and if Haremhab is not yet pharaoh in this text, then it would yield the important *terminus a quo* of Mursili's 9th year for the accession of Haremhab. This *terminus a quo*, if valid, would exclude the identification of Nibḥururiya with Tut'ankhamun (see Fig. 1),¹⁶ and if so, then Nibḥururiya can only have been Akhenaten.¹⁷

If KUB 19.15+KBo 50.24 would appear to exclude the identity of Nibḥururiya with Tut'ankhamun and thus to cinch the identity of Nibḥururiya with Akhenaten, how can one reconcile it with the rest of the multifarious evidence relevant to the question? This paper will now turn to those issues which bear some import for the matter of the identity of Nibḥururiya and Amarna Age chronology associated with it.¹⁸ It does so with the realization that *no* current reconstruction seems to be able to account neatly for all the evidence.

¹³ Cf. Murnane (1985: 40–41); Klengel (1992: 115 and n. 139); Darnell (1991).

¹⁴ E. g. the letter from an Assyrian monarch, probably Tukultī-Ninurta I, to the king of Ugarit, for which see Lackenbacher (1982); Singer (1985).

¹⁵ See e.g. the Zizinia inscription from the tomb of Haremhab, for which see Martin (1989: pl. 91 and p. 80, text scene 69), kindly pointed out to me by M. Gabolde, who notes that 'the amazing (number) of formulae belonging to the "Königsnovelle" with specific royal phraseology clearly indicate that Horemheb consider(ed) himself as the actual (or) "virtual" ruler, even (though) Tutankhamun or Aÿ (is still) ruling. Such sentences are never used for non-royal individuals' (pers. comm.).

¹⁶ In the figure, 1 cm. = 4 years (scaling may have occurred for printing). Dates for the pharaohs essentially follow von Beckerath (1997), but reject a coregency between Akhenaten and Smenkhkare^c, as per Helck (1994: 20f.) and Murnane (2001: 12), and are taken for this period from Krauss (1978), according to whom dates for Smenkhkare^c are added. Some time should be left following Akhenaten (or Smenkhkare^c or Tut'ankhamun) for a short interregnum of the *tahamunzu* and/or the reign of 'Ankhetkheprure^c (see Section 6). The anchor for the Hittite Synchronism line is the placement of Mursili's years 7–9 shortly before Haremhab's accession. Cf. now the comparable dates of Hornung et. al. (2006: 492f.): Amenhotep III (1390–1353); Akhenaten (1353–1336); Smenkhkare^c/Nefernefruaten^c 'Ankhetkheprure^c (1336–1334); Nefernefruaten^c 'Ankhetkheprure^c (1334–?); Tut'ankhamun (?–1324).

¹⁷ It would allow the identity of Nibḥururiya with Smenkhkare^c proposed by Wilhelm and Boese (1987) only if one is willing to accept that Suppiluliuma outlived Nibḥururiya by only some two years, a scenario which would be excluded by precisely that analysis of the DS that leads Wilhelm and Boese to propose their identification; see discussion below, Section 7, and n. 91.

¹⁸ To which Freu (2004) and Gabolde (1998) have recently devoted entire monographs.

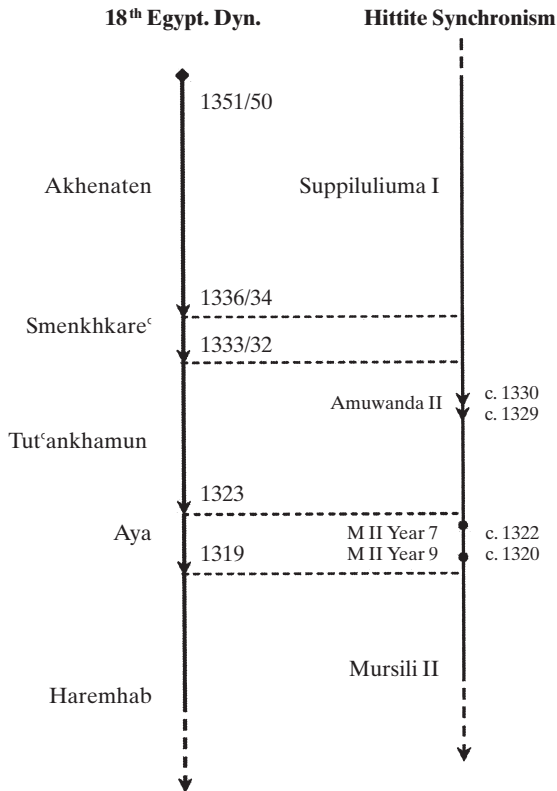


Fig. 1: Egyptian-Hittite Synchronisms in Light of KUB 19.15+KBo 50.24

Each must attempt to explain away at least a couple of uncomfortable issues, and hence, each remains less than entirely satisfactory.

Further, at the risk of overdoing the caveats, I would like to note what seems to be a valid maxim: The less certain a claim is, the more dogmatically that claim will be held. The tenacity and virulence with which many hold to their religious convictions is perhaps the best illustration. The issue of whether Nibḫururiya be Akhenaten, Smenkhkare^c or Tut'ankhamun is similar, in that some proponents of the one or the other hypothesis seem to claim with unerring certainty that their interpretation is correct, repeatedly emphasizing their conviction with (in)appropriate hyperbole. In recognition of this state of affairs, I wish to emphasize that I am fully aware of the fact that there are potentially serious objections to all three options, even if I will advocate one of them in this paper. I do not want to present this scheme as unequivocally proven, but rather, as the one that seems best to account for the new information of KUB 19.15+KBo 50.24 while attempting to allow for the other relevant evidence as well.

1. A Coregency between Akhenaten and Smenkhkare^c

First, it should be briefly noted that this paper assumes that there was no coregency between Akhenaten and Smenkhkare^c, which would likely be fatal to the identification of Nibḥururiya with Akhenaten,¹⁹ 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.

But a coregency is rejected not simply because the paradigm in this paper demands it. While the issue obviously cannot be decided in the present forum, it seems that any evidence for such a coregency is sorely lacking,²⁰ and thus, that it must be considered unlikely at best. Helck (1994: 20f., with refs.) regards the coregency as ‘schon lange widerlegt. ... Die alte Vorstellung einer Mitregentschaft von Semenkhkare mit Echnaton ist innerhalb der Ägyptologie schon seit längerem kein Thema mehr.’ Though this might overstate the case somewhat, von Beckerath (1994: 39) similarly concludes that, ‘... mit größter Wahrscheinlichkeit [folgt] Jahr 1 des Semenkhare^c auf das 17. Achenatens.’²¹ Still, as with most other questions relating to this mini dark age²² between the death of Akhenaten and the accession of Tut‘ankhamun, the final verdict on this issue remains unspoken.

2. Sons of Akhenaten

It is universally accepted that there were no male heirs to the Egyptian throne upon the death of Tut‘ankhamun, and indeed, non-royal military personalities, Aya, then Haremhab, succeeded him. Long debated and still far from enjoying any semblance of consensus, in contrast, is the status of the successors to Akhenaten, and hence, whether the claims of the *tahamunzu* that she had no son and the protestations of her messenger, Hani, that there was no son of the pharaoh, are amenable to the situation following the death of Akhenaten.

¹⁹ As noted, e.g. by von Beckerath (1994: 46). For the question of whether a wife of Akhenaten may have been raised to the status of coregent toward the end of his reign, see below, Section 6.

²⁰ Cf. recently Murnane (2001: 20), referring to a calcite jar from the tomb of Tut‘ankhamun with two sets of cartouches naming Akhenaten and Smenkhkare^c; Kitchen (1985: 43f.); and the contrasting interpretation by Allen (2007: 2–3 and n. 4).

²¹ See also von Beckerath (1994: 41, 45f. and 97ff.); Krauss (1997a; in press); Gabolde (2002: 36ff.); and most recently, Hornung (2006: 206ff.). Von Beckerath (1997: 113), however, substantially changed his view soon afterwards, in fact assuming a coregency of some three years, apparently largely since he feels that, ‘die Beinamen, die ihn (Smenkhkare^c) als von Ach-en-aten geliebt bezeichnen, deuten darauf hin, daß er damals dessen Mitregent war.’ That this evidence would be sufficient to posit a coregency seems quite doubtful to me, but the discussion will have to be carried out among Egyptologists; see also below, Section 6, and n. 35.

²² For the most recent, succinct discussion, see Allen (2007, with refs.), who sees Nefernefruaten – whom he identifies as the daughter of Nefertiti, Nefernefruaten ‘the younger’ – ruling as coregent alongside Akhenaten and for some time after his death.

The evidence for the origins of Smenkhkare⁶ and Tut⁶ankhamun, who may have been related,²³ is varied and contradictory. First is the series of inscriptions claiming that Amenhotep III was Tut⁶ankhamun's father.²⁴ Second are the texts indicating that Thutmose IV was his great-grandfather.²⁵ Third is the inscription, from before Tut⁶ankhamun's accession,²⁶ in which he bears the title 'bodily son of the king'.²⁷ Furthermore, Gabolde (1998; also 2002) has recently suggested, in a work that has been greeted with great respect even by those who do not share all of his conclusions,²⁸ that Tut⁶ankhamun would have been the seventh child and only son of Akhenaten and Nefertiti, born in the 13th year of that king's reign. This he has argued, *inter alia*, on the basis of: (1) the scene in Amarna tomb gamma portraying the mourning for Maketaton, which he interprets as picturing the infant Tut⁶ankhamun in the arms of his nurse;²⁹ (2) the fact that the 'bodily son of the king' inscription does not stand alone, but places Tut⁶ankhamun alongside a sister, perhaps [Ankhesenpa]aten;³⁰ (3) a further scene which he interprets as picturing Tut⁶ankhamun with his sisters; and (4) the Restoration Stele of Tut⁶ankhamun, which he understands as indicating that the accession of this king took place in two stages separated by several years. In recent reviews Murnane (2001) and Eaton-Krauss and Krauss (2001) have pointed to several important aspects of Gabolde's analysis that can or must be interpreted differently, and these issues will have to be determined among Egyptologists in the coming years.

If Gabolde's thesis that Tut⁶ankhamun was in fact the son of Akhenaten and Nefertiti turns out to be correct, then either (1) the paradigm in this paper and other studies in which the *tahamunzu* episode is placed between the death of Akhenaten and the accession of Tut⁶ankhamun collapses completely, since there would have been at this time a son of the pharaoh and his chief wife, essentially leaving only the death of Tut⁶ankhamun as the catalyst

²³ A conclusion – variously received and for a number of reasons, including the question of the identity of the mummy of KV 55, not beyond doubt – based primarily on examination of the mummies of these two young kings; see discussion and refs. in Allen (1994: 7f.; 2007: 6f., 16 and n. 67); Hornung (2006: 207); Gabolde (2007); cf. Gabolde (1998: 213–226), who suggests that Smenkhkare⁶ was none other than Suppiluliuma's son Zannanza, and the responses of Murnane (2001: 19f.) and Sadowska (2000: 73–77).

²⁴ Eaton-Krauss (1990); Gabolde (2002).

²⁵ Reeves (1982: 65ff.); Groddek (2002: 276); Allen (2007: 17 and n. 70).

²⁶ As with so many issues relating to this time period, who commissioned these reliefs and inscriptions and when, and therefore their precise import, are uncertain, i.e. during the reign of Akhenaten, during an interregnum of his spouse or during the reign of Smenkhkare⁶; Krauss (2007), for one assigns them to the reign of 'Ankhetkheprure⁶.

²⁷ Groddek (2002: 276 and n. 24); von Beckerath (1997: 114f. and n. 517); Gabolde (2002). In addition to these points, Helck (1994: 20; 1984: 167) has suggested that a lock of hair found in the miniature sarcophagus in the tomb of Tut⁶ankhamun might belong to Teye and that an unscribed royal figurine found with it would represent Amenhotep III, which, along with Smenkhkare⁶'s attested connections to Teye, leads Helck to suggest that Tut⁶ankhamun and Smenkhkare⁶ would have been grandsons of Amenhotep III and Teye. Allen (2007: 17), based in part on the same reasoning, has recently opted for Amenhotep III being the father of Smenkhkare⁶, in turn the father of Tut⁶ankhamun. As this is all quite speculative, however, it will not be further considered here.

²⁸ E. g. Murnane (2001); Eaton-Krauss and Krauss (2001).

²⁹ See already Eaton-Krauss (1990: 552f. and ns. 87f.); cf. Allen (2007: 11–13); van Dijk (2007).

³⁰ See Allen (2007: 7 and n. 29).

for the episode;³¹ or (2) Gabolde would also be correct in his assumption that the episode occurred following the death of Akhenaten despite the *tahamunzu*'s and Hani's claims that there was no son of the king and despite Suppiluliuma's sending an envoy to Egypt to confirm that this was indeed the case. If so, one would have to imagine a scenario in which the *tahamunzu* was able to very nearly succeed with her plan of marrying a Hittite prince (or indeed succeeding in doing so according to Gabolde) *despite* the presence of a biological son of the pharaoh *and* his queen. Further, one would either have to assume that the *tahamunzu* succeeded in duping Suppiluliuma into believing that no such son existed, or that the Hittite texts blatantly misrepresent the situation as one in which it was clear to all that there was no such son, when in fact it was obvious that there was.³² It would in fact suggest that the 12–13-years-old Merytaten³³ colluded with Suppiluliuma to deny the throne to her younger brother, and that the Hittites succeeded in a massive cover up once the plan derailed. While such a possibility should not be excluded a priori, it does not seem to be particularly amenable to the sources relevant to the *tahamunzu* episode (see *infra* and n. 104).

In any case, there are solid grounds for seeing at least the inscriptions designating Amenhotep III as Tut'ankhamun's father as no more than blatantly legitimizing propaganda.³⁴ It is of course out of the question that Amenhotep III was his father, since Tut'ankhamun died at ca. 16 years of age (Leek 1977), nearly 30 years after the death of Amenhotep III.³⁵ Further, the claim that Amenhotep III was his father conflicts crudely with the inscription in which he names Thutmose IV as his great-grandfather.³⁶ The most obvious explanation for this blatant falsehood would be an attempt on the part of Tut'ankhamun and/or his supporters to connect him with the king whom many considered to be the last legitimate pharaoh, Amenhotep III.³⁷ In any case, it is hardly credible that Tut'ankhamun's inscriptions regarding his ancestry can be taken literally if it is clear that at least some of them, for whatever reason, were fabricated. And if so, then it would seem not unlikely that the

³¹ In this case, either 'Arma'a in KUB 19.15+KBo 50.24 would not have been Haremhab, or he would already have been pharaoh.

³² For considerations on the reliability of the Hittite protestations of ignorance, but in relation to more distantly related male royals, see presently.

³³ Merytaten, since it would be quite unlikely that Nefertiti, as *tahamunzu*, would have colluded to deny her own son the throne in favour of a foreign prince; cf. n. 104.

³⁴ E.g. Eaton-Krauss (1990: 552 and n. 86); cf. Gabolde's explanation summarized below, n. 37.

³⁵ Unless, of course, one wishes to return to the idea of a coregency between Amenhotep III and Akhenaten. Gabolde (2002: 36) has summarized the Amarna coregency issues as well as anyone: 'la corégence entre Amenhotep IV et son prédécesseur est une fiction des égyptologues, et celle entre Akhenaton et son successeur est une fiction des Égyptiens eux-mêmes.' See also refs. in n. 65.

³⁶ As well as Aya's inscription calling Tut'ankhamun his son (see Gabolde 2002: 32; Allen 2007: 8f.; Eaton-Krauss 1990: 554 and n. 106), which in my view should not necessarily be rejected without consideration.

³⁷ Cf. Gabolde's (2002: 47f.) explanation, according to which Tut'ankhamun was regarded institutionally as the son of Akhenaten but religiously as the son of Amenhotep III. This he bases partly on the fact (*ibid.*: 32) that in those inscriptions in which Tut'ankhamun appears as the son of Amenhotep III, the latter is portrayed as a deity. (But must this indicate anything more than the mere fact that he was long since dead?) He also notes (*ibid.*: 47; cf. Murnane 2001: 17; Allen 2007: 9–11) the curious fact that the names of two of Tut'ankhamun's sisters, Ankhesenamun and Merytaten, are found without indication of their royal parentage.

‘bodily son of the king’ inscription should be understood in the same light, i.e. (1) as relating to Amenhotep III rather than Akhenaten, and/or (2) as thinly disguised manipulation.³⁸ At the very least it would seem that those who refer to the ‘bodily son of the king’ inscription as a central point in their identification of Nibḥururiya with Tut‘ankhamun are obliged to state clearly why they feel they can reject as obvious fabrication the inscriptions which claim that Tut‘ankhamun is the son of Amenhotep III, while simultaneously accepting as reliable the one which claims that he is the ‘bodily son of the king’, and further, upon what grounds this latter inscription should be seen as referring to Akhenaten rather than Amenhotep III, who is Tut‘ankhamun’s ‘father’ in all inscriptions (except Aya’s; see n. 36) in which the ‘father’ is named.

Still, many³⁹ have taken the ‘bodily son of the king’ inscription as literal and reliable. As has often been noted, however, the more general epithet ‘son of the king’ was taken by or given to numerous personages who certainly were not sons of any king, showing that it could be used honorifically. Helck (1981: 214), for example, notes that Aya gave the epithets ‘regent’ and ‘son of the king’ to the general Nḥt-Mjn.⁴⁰ Helck (1984: 166) suggests that the epithet may have been taken by Tut‘ankhamun as the brother of Smenkhkare’, and if such a possibility can be entertained, one might imagine that the epithet could have been taken by Tut‘ankhamun with reference to his great-grandfather Thutmose IV, if indeed he was such, and thus, that it should be understood as ‘descendent of the king’. Alternatively, since Tut‘ankhamun was a son-in-law of Akhenaten (as was Smenkhkare’), he may have used the epithet in light of his dynastic marriage.⁴¹

Von Beckerath (1994: 97 ff.; 1997: 112 ff.) suggests that Smenkhkare’ may have been a nephew of Akhenaten, the son of a sister, and since he seems to accept the conventional wisdom that Smenkhkare’ and Tut‘ankhamun were brothers (cf. n. 23, above), it seems that he also assumes that Tut‘ankhamun was no literal ‘bodily son of the king’, though he never states this explicitly. He settles for the likely intentionally vague (1997: 115 n. 523): ‘Die Situation, in der kein männlicher Nachkomme der Königsfamilie mehr vorhanden war, paßt nur zu der Zeit des Todes Tut-anch-amuns.’ Bryce (1990: 97), in his discussion, is similarly vague: ‘Although (Smenkhkare’s and Tut‘ankhamun’s) precise relationship with Akhenaten is uncertain, there can be no doubt about their royal lineage.’ In von Beckerath’s and Bryce’s formulations, though, there is a hint of the bait and switch technique. That is to say, they implicitly concede that neither Smenkhkare’ nor Tut‘ankhamun were sons of Akhenaten, and thus, that the ‘bodily son of the king’ epithet is fabricated, but still eke from it Tut‘ankhamun’s ‘royal lineage’. But considering the numerous offspring produced by the predecessors of Akhenaten, there would certainly have been ‘males of royal lineage’ alive at the time of the death of Tut‘ankhamun, too, if one broadens the inclusiveness of this

³⁸ One must also keep in mind the fact that it is often precisely illegitimate kings, from Šarru-kēn to Ḫattusili ‘III’ and many others, who feel compelled to fabricate and emphasize legitimacy, and one should not exclude the possibility that this might have been the case with Tut‘ankhamun as well.

³⁹ E. g. Groddek (2002: 276); Murnane (1983: 276 f.); Barta (1983: 17).

⁴⁰ Parker (2002: 47 and n. 59) notes instances of the more specific epithet ‘bodily son of the king’ being given to persons who were no sons of any king, though all his cases are from the Old Kingdom and thus not necessarily relevant for the present discussion. No examples are known from the 18th Dynasty.

⁴¹ Similarly Allen (2007: 7–8); cf. refs. in Eaton-Krauss (1990: 553 and ns. 89 f.).

vague phraseology sufficiently. The question is thus not whether there were any ‘males of royal lineage’, but (1) whether there was a son of the king, and failing that, (2) whether there were males extant who could be considered by those in power as legitimate contenders for the throne, and whether their claims could outweigh the claim (and clout) of the *taḥamunzu* that she and her husband had no son and that she therefore would rather marry a foreign prince than one of her ‘servants’.

Any dynastic connections that Smenkhkare⁴² and Tut⁴³ankhamun may have enjoyed (see n. 27) might explain why these youngsters, if not sons of Akhenaten, were married to his daughters and allowed to ascend (or placed on) the throne, despite their tender ages, and even though there were at least two ambitious generals waiting in the wings who indeed took over the reins of power once Tut⁴³ankhamun died. If so, the question would remain if these young royals, if such they be, might have been considered – and by whom – legitimate contenders for the throne or rather the ‘servants’ that the *taḥamunzu* so disdained to marry.

It is also sometimes claimed that it could hardly have escaped the attention of Suppiluliuma and his messenger, Ḫattusa-ziti, whom he sent to Egypt to investigate following his receipt of the *taḥamunzu*’s letter, that these young royals, Smenkhkare⁴² and Tut⁴³ankhamun, existed and that they would have had a claim to the throne, and hence, that their existence precludes the possibility of such a request being made of Suppiluliuma following the death of Akhenaten.⁴² While this is one possible scenario – granting for the moment their royal status – it must be remarked that it is nowhere explicitly supported in the textual sources, and that one could easily imagine quite another, no less plausible, scenario.

For example, one could reasonably suppose that it is precisely these great-grandsons of Thutmose IV – if indeed they be such rather than more distantly related to the royal family – that the *taḥamunzu* had in mind when she voiced her refusal to marry one of her ‘servants’ and to make him pharaoh. After all, ‘servant’ is likely used in a disparaging manner, rather than literally, and probably with reference to real persons who indeed were being put forth as candidates.⁴³ Further, though it is often assumed that Suppiluliuma would never have sent a son into such a precarious situation if he had known that there were male members of the royal family on the scene, such an assumption is not remotely convincing. Suppiluliuma may indeed have been willing to take such a gamble, betting on the *taḥamunzu*’s chances of being able to realize her plan in the face of the young contenders, who were, after all, certainly not her sons and likely not the sons of Akhenaten either. Indeed, history is replete with examples of queens going to great lengths to ensure that their own offspring come to the throne rather than those of another woman. And since when must one assume that a brutal, unscrupulous, uncompromising emperor, such as Suppiluliuma – who, after all, murdered his own brother to become king – would never consider offering up a single, expendable and replaceable son for the sake of coveted power? It may be, as is often assumed, that the *taḥamunzu* had the support of, e. g., Aya in her scheme, and that Suppiluliuma

⁴² E. g. Bryce (1990: 102); Groddek (2002: 276); von Beckerath (1994: 100); Barta (1983: 16f.).

⁴³ It seems rather more difficult to imagine that the *taḥamunzu*, in the person of Merytaten, would have used this term in reference to her younger brother and the rightful heir to the throne, Tut⁴³ankhamun, which would be a consequence of Gabolde’s paradigm.

liuma was willing to wager that the combination of the *tahamunzu*, Aya, the influential general, and Suppiluliuma's young and energetic son, Zannanza, could succeed in just such an exploit, despite the presence of Smenkhkare^c and Tut'ankhamun. After all, the stakes for Suppiluliuma were enticingly high.⁴⁴

Moreover, our only sources regarding this affair are the Hittite texts from many years after the events, written in full knowledge of the scheme's dismal failure, and one cannot dismiss the possibility that Mursili is revising history to some extent,⁴⁵ placing full responsibility for the fiasco on the Egyptians, absolving his father of any blame for his failed gamble,⁴⁶ giving the impression that he had done everything in his power to ensure that the way was free for Zannanza to take the Egyptian throne. But was it really?

Obviously, this scenario is largely imagined – and my intent is not to advocate it – but so is that envisioned by those who assume that the existence of 'males of the royal family' precludes the *tahamunzu*'s letter being written at this juncture. The point of the exercise is that one can almost always imagine a scenario which would fit one's interpretation of the available evidence, and that one should be careful in assuming that one's imagined scenario is fact. In the available sources we learn only that the *tahamunzu* claims to have no son, that her messenger Hani reinforces this claim by stating that there was no son of the pharaoh, and that the *tahamunzu* disdained to marry one of her 'servants', whoever he may have been.

Two other arguments have been put forth against Akhenaten's fatherhood of Smenkhkare^c and Tut'ankhamun, but even though they would, if valid, lend support to the paradigm advocated here, they do not seem entirely convincing. Parker (2002: 47ff.) has attempted to show that Smenkhkare^c could not possibly have been a son of Akhenaten, and thus, since he came to the throne upon the death of Akhenaten rather than Tut'ankhamun, Tut'ankhamun could not have been a son of Akhenaten, since if he were, he surely would have become pharaoh instead. His argument rests on the fact that as late as Akhenaten's 12th year he and his queen are shown in scenes with their six daughters, with no son. Smenkhkare^c, who was some 18–22 years old at the time of his death,⁴⁷ could not have been born to Akhenaten after his 12th year.⁴⁸ While this is a consideration which

⁴⁴ Liverani (2001: 193) even goes so far as to ask, 'Is it possible that he (Suppiluliuma) knew what would happen and deliberately sacrificed his son simply in order to establish his right to revenge?' Cf. Grodek (2002: 276, n. 27).

⁴⁵ That he does so is obvious, *inter alia*, from the fact that in the DS the Hittite attack on the Amqu is preceded by an Egyptian attack, which finds no reflection in the prayers, as noted, e.g. by Houwink ten Cate (1963: 274).

⁴⁶ Mursili of course finds blame with his father for some misdeeds, but this obviously need not prevent him from attempting to exonerate his father in other contexts.

⁴⁷ Based, of course, on the age of the mummy of KV 55 and assuming that Smenkhkare^c was its occupant, for which see most recently Murnane (2001: 20–22); Eaton-Krauss and Krauss (2001: 97; Gabolde (2007).

⁴⁸ Similarly, Helck (1994: 21) has emphasized that 'niemals wird ein Prinz unter Echnaton genannt,' and that there is no evidence that would suggest that Smenkhkare^c might be a son of Amenhotep III, 'da jede Nennung eines Prinzen (außer Jmn-ḥtp) in den ausgehenden Jahren der Regierung Amenophis' III. fehlt.' This, however, must be understood within the context of the 18th Dynasty as a whole, from which only two official representations of princes are known, indicating that they were kept behind the scenes until the death of the king, even in those cases in which they filled important offices (R. Krauss, pers. comm.).

should not be ignored, it does not seem to be conclusive, since one might counter that on such depictions only the children of the king and his principal wife would be represented, and hence, that if Smenkhkare^c were a son of a secondary wife he could have been born early in the reign of Akhenaten, and that at the time of the relief from Akhenaten's 12th year he would already have been a teenager.

Similarly, Krauss' explanation (1978: 79–83), which accepts that Smenkhkare^c and Tut^cankhamun were indeed sons of Akhenaten, of a secondary wife, but that they would not automatically be in line for the throne, allowing the *tahamunzu* and Hani to claim that Nibḥururiya had no son, does not entirely convince, since the legal arguments to which he refers would not necessarily be observed in precisely such an extraordinary situation as that reflected in the *tahamunzu* episode. Further, as Murnane (1983: 276; see also Barta 1983: 17) has pointed out, many sons of secondary wives indeed came to the throne with no difficulty, in some cases also marrying a daughter of their predecessor.

In sum, this obstacle to the identification of Nibḥururiya with Akhenaten, i.e. the possibility that Smenkhkare^c and/or Tut^cankhamun might have been sons of Akhenaten, and particularly the inscription naming the latter as the 'bodily son of the king', along with Gabolde's recent work, which is still in the process of being digested by the Egyptological community, should not be taken lightly. Still, there are considerations which may allow one to account for the facts within a paradigm equating Nibḥururiya with Akhenaten. If there was no son of Akhenaten upon his death, the way is clearly free for the *tahamunzu* episode to have taken place at this time. If (Smenkhkare^c or) Tut^cankhamun was in fact a son of Akhenaten, then one must either place the *tahamunzu* episode after the death of Tut^cankhamun and dispense with the understanding of KUB 19.15+KBo 50.24 suggested in this paper, or assume that Merytaten and Suppiluliuma conspired to deny Tut^cankhamun his rightful throne and that the Hittite texts expunged this fact from their version of history.

3. The Writing of the Name Nibḥururiya in the Amarna Letters and the DS⁴⁹

The second primary objection to the identification of Nibḥururiya with Akhenaten has been the spelling of the deceased pharaoh's name in the DS, Nibḥururiya,⁵⁰ with an /i/ in

⁴⁹ For a further summary of the discussion and refs., see Wilhelm and Boese (1987: 100f.).

⁵⁰ I.e. ^m*Ni-ib-ḥu-ru-ri-ia-aš* in KBo 14.12 iv 18, ^m*Bi-ib-ḥu-ru-ri-ia-aš* in KBo 5.6 iii 7. Groddek (2002: 273ff.) has recently suggested an explanation for the errant *Bi-* (i.e. *Bi-* as a misreading of a postulated *Ne-* [NE=*bi*]) in a letter from Egypt), and while initially attractive, it remains wholly speculative and not necessarily any more likely than an unmotivated scribal error or some other explanation. Moreover, there is little reason to assume, with Groddek, that a letter sent to Ḫatti from Egypt would have employed *Ne-*, as the value /ne/ for NE would hardly be expected in a letter from this period or from the Amarna archive (von Soden and Röllig, Syll., No. 122; see also the Amarna names beginning with *Ni-* in Hess 1993: 115–120, none of which are written with NE). Meyer (1992: 90f. and n. 29), in contrast, suggests that *Pip-* is an acceptable phonetic variant, and Klinger (2006: 318) writes, 'there is no means of knowing whether a Hittite copyist faced with an unfamiliar name written with what was at that time the quite alien sign /nap/ may not have chosen to divide the signs syllabically as NI-IB.' Wilhelm and Boese's (1987: 102) conclusion that Nibḥururiya is Smenkhkare^c of course forces them

the first syllable, which better reflects the throne name of Tutʿankhamun, Nebkheprure^c, than it does that of Akhenaten, Neferkheprure^c. Further, so it is claimed, spellings with an /i/ in the initial syllable are consistently found in the Amarna letters for the Neb- of Amenhotep III’s throne name, Nebmaʿatre^c, while all spellings of Akhenaten’s throne name, Neferkheprure^c exhibit an /a/ vowel. Thus, it is argued,⁵¹ it is not credible that the first element in Akhenaten’s throne name be represented in the DS as *Nib-*, where one would surely expect *Nab-*. This obstacle to the Akhenaten = Nibḥururiya hypothesis should not be taken lightly, but neither is the situation as unequivocal as is sometimes assumed, and the advocates of Nibḥururiya’s identification with Akhenaten have countered in one of several ways.

3.1. A Phonetic Explanation

First, they have referred to a phonetic development in Egyptian which might allow one to assume that *Nib-* in the DS represented a de-emphasized first syllable in which the *r* of *nfr-* had been dropped or assimilated, and thus, that Nibḥururiya could indeed represent the throne name of Akhenaten. Helck (1994: 19), for example, writes, ‘(m)an muß sich vor Augen halten, daß nap einem naf/nef- = nfr entspricht und nib/p einem niw/new- von nb. Der hörbare Unterschied zwischen *Nfr-ḥprw-R^c* und *Nb-ḥprw-R^c* war minimal – im Gegensatz zur deutlichen Verschiedenheit in der Schreibung.’ And Redford (1973: 49) maintains that ‘praenomina incorporating *ḥprw-r*’ were bound to give non-Egyptians trouble, especially since we know that these tended to be abbreviated in speech by the elision of the distinctive first element.’ Still, this phonetic argument has been variously received by Egyptologists,⁵² and whether valid or not, it does not explain the otherwise (nearly) consistent attested writings.

3.2. A Graphic Explanation

One might also point to the otherwise wide variation in the spelling of the names of the Amarna kings in general, even if the vowel of the first syllable remains constant for the throne name of Amenhotep III and nearly so⁵³ for that of Akhenaten: Nibmuaria, Nibmuariya, Nimuwariya, Nibm]uʿwariya, Nimmuriya, Nimmuariya, Nimmuriya, Mimuriya, Mimmuriya and Immuriya for Nebmaʿatre^c/Amenhotep III vs. Naphurriya, Naphuririya, Naphuriya, Naphu]ruriya, Naphuʿruri]ya, Naphu]rar[iy]a, Naphururia and Namḥurya for Neferkheprure^c/Akhenaten.⁵⁴

to assume that the Hittite scribes of this period no longer knew who was who among the last 3–4 Egyptian pharaohs, which is in my view the least likely of the possible explanations.

⁵¹ E. g. Murnane (1983: 277); Schulman (1978: 43); Freu (2004: 52f.); Kitchen (1985: 43f.); van den Hout (1984: 84f.).

⁵² See discussion and further refs. in Parker (2002: 36f. and n. 20); Groddek (2002: 275f. and n. 23).

⁵³ As shall be argued presently, the spelling Nibḥurriya in EA 9 should be added to the list of variants for Akhenaten, and Hess (1993: 116), among others, would add *Ni-ḥ]p-ḥu-ri-[ia* of EA 210 as well.

⁵⁴ For the complete list of attestations, see Hess (1993: 117–118 and 115–116).

In view of such markedly variant cuneiform spellings, one might be tempted – even if the attestations in EA 9 and 210 (see below and n. 53) and in the DS could not be attributed to Akhenaten – to see the consistency in the vocalization of the first syllable as a statistical fluke. Arguing such, however, would admittedly not be overly convincing, and indeed, does not seem to be necessary. In any case, Bryce's (1990: 97) assertion, according to which, 'Niphururiya/Nibhururiya is a precise rendering in cuneiform of Tutankhamun's prenomen, Nebkheprure,' which goes back to Edel's (1948: 14) claim that the writing is 'eine absolut einwandfreie Umschreibung des Namens Tutanchamons,' is at best an oversimplification.⁵⁵

3.3. *Hittite Confusion of Nebkheprure' and Neferkheprure'*

Perhaps the simplest explanation is that suggested by Meyer (1992: 91 ff.), who posits that Mursili or his scribe might simply have mixed up the very similar names of these two pharaohs, Nebkheprure' and Neferkheprure', whose reigns were separated by only some 2–4 years. She further speculates that such confusion might be considered especially plausible in light of the fact that the Hittite kings often bore names of illustrious ancestors, a practice that they might have projected onto the name-giving practices of the Egyptians.⁵⁶

3.4. *The Addressee of EA 9*

A fourth counterargument, however, would seem to be the weightiest, and that is the likelihood that EA 9, addressed to one *Ni-ip-ḫu-ur-ri-ri-ia*, was also intended for Akhenaten,⁵⁷ in which case it would provide a second instance,⁵⁸ in addition to that in the DS, in which this king's throne name, Neferkheprure', is spelled with an /i/ in the first syllable. Several points suggest that this letter from Burra-buriyaš of Babylon was indeed sent to Akhenaten rather than Tut'ankhamun.

First, as has become ever clearer with recent research, Tut'ankhamun left Akhet-aten, in Krauss' words (1997 a: 248), 'nach der Weinernte des 1. Jahres in ca. II Achet und vor der folgenden Weinernte.'⁵⁹ Thus, if the letter had been addressed to Tut'ankhamun, one would have to assume that the news of the death of Smenkhkare' and the accession of Tut'ankhamun had reached Babylon, and that Burra-buriyaš had sent his response to

⁵⁵ See also Edel (1994: 23 and n. 3); Freu (2002: 103).

⁵⁶ In any case, if one is forced to resort to suggesting Hittite confusion of pharaonic names, surely it is considerably more likely that Nebkheprure' was confused with Neferkheprure' than Nebkheprure' with 'Ankhkheprure', as Wilhelm and Boese (1987: 102) have suggested.

⁵⁷ Cf. e.g. Freu (2002: 102, with refs. in n. 57).

⁵⁸ For a likely third, see n. 53.

⁵⁹ See also Moran (1992: xxxiv): 'no later than the first year or so of Tutankhamun'; Helck (1994: 20); cf. Wilhelm and Boese (1987: 86).

Egypt, all within the few months during which Tut^cankhamun still resided in Akhet-aten. This might be possible, but would be rather speedy.⁶⁰

Second, though EA 9, if addressed to Tut^cankhamun, presumably would have been the first letter from Burra-buriyaš to him, there is nothing in it which congratulates him on or even alludes to his accession, which one might expect from an introductory epistle.⁶¹ And if one wishes to avoid this problem by suggesting that it may have been the second letter from Babylon,⁶² then one must assume that the news of the accession of Tut^cankhamun had reached Babylon, that Burra-buriyaš had responded with his best wishes, that Tut^cankhamun had written back, and that EA 9 thus represents the fourth trip between the two capitals, all within the span of time in which Tut^cankhamun resided at Akhet-aten, which can effectively be excluded.

Third, there is not a single Amarna letter that can be confidently attributed to any successor of Akhenaten, though debate continues concerning two or three cases.⁶³ It is important to note in this context that a key argument long used by some researchers,⁶⁴ according to which some of the vassal correspondence would have been addressed to a successor of Akhenaten, depends on the reading ‘12’ of the docket of EA 27, which must

⁶⁰ Further, Gabolde (1998: 89–90, 195) has recently made the seemingly plausible suggestion that Tut^cankhamun did not reside at Akhet-aten at all, which, if true, would make all attempted attributions of EA letters to him superfluous; cf. though Eaton-Krauss and Krauss (2001: 96), Krauss (2007).

⁶¹ Indeed, with the exception of a small number of letters addressed to Akhenaten (e.g. EA 41 and 116; see below and n. 147), nowhere in any of the Amarna correspondence does the writer congratulate a new pharaoh on his accession, which might reinforce the suggestion (see presently), that none of the Amarna correspondence can be attributed with much confidence to any successor of Akhenaten.

⁶² Kühne (1973: 73) in fact suggests that EA 9 might have been the second letter from Burra-buriyaš to Tut^cankhamun, but he must assume that Tut^cankhamun reigned into his third year at Akhet-aten, which can no longer be maintained.

⁶³ While this issue obviously cannot be settled here, a few discussions in recent literature might be briefly mentioned: (1) Parker (2002: 46) assumes that some Amarna letters would have been written to Smenkhkare^c, but he does not specify which these might be, and while I do not wish to categorically exclude the possibility (cf., e.g. the discussion below, Section 8.3, which leaves open the possibility that some of Aziru’s final letters could have been sent shortly after Akhenaten’s death), I know of none that can confidently be said to have been addressed to him. Similarly, Wilhelm and Boese (1987: 86f.) assume that, ‘Es ist zwar nicht nachweisbar, aber doch wahrscheinlich, daß ein Teil der Vasallenbriefe noch in die dreijährige Regierung Semenchkares fällt.’ Their assumption, of course, is based entirely on chronological considerations. Cf. their later comment (p. 87): ‘Bereits oben wurde auf Autoren verwiesen, die mit guten Gründen die gesamten Vasallenbriefe in die Regierungszeit Amenophis’ IV./Echnatons datieren.’ (2) The reading of the addressee’s name in EA 16 is debated, but can hardly be Aya, for which see Moran (1992: xxxv, n. 123 and 39, n. 1). (3) Freu (1992: 43) assumes that EA 210 was addressed to Tut^cankhamun, but this is also questionable. Moran (1992: xxix, n. 82) suggests the letter is likely not even to a king, while Hess (1993: 116) assumes it is addressed to Akhenaten. (4) For discussion of EA 41, see below. (5) For discussions of EA 139, 147, 155, 169, 171, see refs. in Moran (1992: xxxvi n. 129) and Krauss (1978: 71f.); cf. Houwink ten Cate (1963: 275), who dates all the Amarna letters dealing with the Amqu attack to the end of the reign of Tut^cankhamun, which can be categorically excluded.

⁶⁴ E.g. by Bryce (1989: 23), where the so-called First Syrian War is thereby dated to within the last 3 years of Akhenaten’s reign rather than to its first few years, as must be considered far more likely; cf. Section 8.2, below.

be abandoned in favour of '2',⁶⁵ and thus on a long coregency of Amenhotep III with Akhenaten. In short, it seems that, barring convincing evidence and argumentation to the contrary, the entire Amarna archive should be seen as that of Akhenaten, who, of course, brought with him to Akhet-aten some correspondence from the time of his father, Amenhotep III.⁶⁶

Fourth, if one maintains that the recipient of EA 9 was Tut'ankhamun, one must assume, in addition to all this, that the letter was accidentally left behind at Akhet-aten when Tut'ankhamun moved to Memphis. Current correspondence, as a rule, would have been transferred with the residence, and this letter, which refers to current and important diplomatic issues to be handled very delicately (see presently), would have been only weeks or months old at the time of the move.

Fifth, and perhaps most importantly, Burra-buriyaš seeks in the letter to nip in the bud the flowering political relations between Egypt and Assyria, which the Babylonian king claims as his vassal. This has been understood as an unlikely demand of Tut'ankhamun, since by the time of his reign Egypt had already been maintaining relations with Assyria since shortly after the fall of Mittanni and the ensuing independence of Assyria during the reign of Akhenaten.⁶⁷ The letter would thus seem to fit far better in about the middle or late in the reign of Akhenaten, when Assyria would first have sought political ties with Egypt, as seen in EA 15 and 16.⁶⁸

Finally, on a more generally tack, if EA 9 had been addressed to the ca. 6-year-old Tut'ankhamun within a year or so of his accession, might one not expect some hint in the letter that this might be the case? Or might the letter have in fact been addressed to a vice-regent?

While these arguments cannot be considered definitive, it does seem that strong counter-evidence would be needed to make likely the claim that EA 9 is indeed the single letter in the Amarna archive addressed to a successor of Akhenaten. Thus, the default assumption should be that EA 9 was addressed to Akhenaten, and hence, that the spelling of his name with an /i/ in the first syllable as found in the DS is not unique.

4. Suppiluliuma's Attack(s?) on Amqu

The proponents of an identification of Nibḥururiya with Tut'ankhamun are well aware of the fact that the equation necessitates that Suppiluliuma invaded Amqu once toward the end of the reign of Akhenaten, as attested in the Amarna letters, and again at the time of

⁶⁵ With Fritz (1991); see also Helck (1994: 16); Parker (2002: 57); Moran (1992: xxxvi and n. 129); Eaton-Krauss and Krauss (2001: 91 f.); Gabolde (2002: 36 f.), with a further argument against the coregency of Amenhotep III with Akhenaten, based on the docket of EA 26; Dorman (2007), with yet another argument against a coregency, based on the tomb of Kheruef; cf. Giles (1997; 2001), whose adherence, inter alia, to the reading '12' renders much of his scheme untenable, as per Rainey (1999) and Liverani (1999).

⁶⁶ See e.g. Moran (1992: xxxiv–xxxix).

⁶⁷ Cf. Kühne (1973: 73–75, with further refs.).

⁶⁸ See, e.g. Moran (1992: xxxv and n. 123); Kühne (1973: 77 ff.).

the death of Nibḥururiya (=Tutʿankhamun for these proponents) at least a dozen years later,⁶⁹ as attested in the Hittite texts. Being thus constrained, these researchers see in one account of the raid(s) on Amqu two separate attacks which they believe could indeed be separated by a dozen years or more.⁷⁰ The passage, from Mursili's Second Plague Prayer is the following:⁷¹

Since the men of Ḫatti and the men of Egypt were bound by the oath of the storm-god of Ḫatti, and the men of Ḫatti *proceeded to get the upper hand*, the men of Ḫatti thereby suddenly transgressed the oath of the gods. My father sent infantry and chariotry, and they attacked the borderland of Egypt, the land of Amqu. And again he sent, and again they attacked.⁷² When the men of Egypt became afraid they came and asked my father outright for his son for kingship. But when my father gave them his son, as they led him off, they murdered him. My father was appalled and he went to Egyptian territory, attacked the Egyptians, and destroyed the Egyptian infantry and chariotry.

While others may well have a different opinion,⁷³ it seems quite clear to me that Mursili is referring to a single episode of some duration and complexity, not two episodes separated by some dozen years or more. In fact, if it were not for the chronological necessity demanded by the equation of Nibḥururiya with Tutʿankhamun, no Hittitologist would assume in his treatment of the passage that *namma*, which means no more than 'then, again, further',⁷⁴ suggests any significant gap between the attacks. That is to say, it would never even occur to any Hittitologist without prior knowledge of the Nibḥururiya debate to translate the passage, paraphrasing slightly: 'My father sent infantry and chariotry, and they attacked the borderland of Egypt, the land of Amqu. And again (a

⁶⁹ I.e. the time between the last year of Akhenaten's reign in 1336/4, to the death of Tutʿankhamun in 1323, when the supporters of the Nibḥururiya=Tutʿankhamun equation must assume a second Amqu attack. It should be noted that Wilhelm and Boese's (1987: 101–102, 104) analysis also requires assuming two Amqu attacks several years apart, though separated by only some 3–4 years.

⁷⁰ Recent argumentation for seeing only one attack on Amqu can be found in Parker (2002). The main thrust of his thesis is not materially weakened by several errors or omissions that lead to some difficulties and confusion on pp. 44, 57 and 62, i.e.: (1) ignoring Izre'el's and Singer's (1990) redating of the Aziru correspondence (p. 44); cf. below, Section 8.3; (2) reliance on an older translation of Mursili II's treaty with Tuppi-Teššub in which the rebellion and subsequent repentance of Aziru is based on a faulty restoration (p. 57); cf. now Singer (2003a: 96ff. and n. 2); (3) ignoring Wilhelm and Boese's (1987) study of the DS, in which they actually come to similar conclusions regarding this issue as does Parker (p. 65f.).

⁷¹ A: KUB 14.8 i 16-23 // B: KUB 14.11+ // C: KUB 14.10+; translation based on Singer's (2002: 58).

⁷² Found only in A; B and C omit 'And again he sent, and again they attacked.' (More precisely, B omits 'And again he sent' and places 'they' sent again' following 'when the men of Egypt became afraid'.) Further, in A 'And again he sent' is written on the edge and rev. of the tablet, while the following 'and again' is written over an erasure (as is the preceding 'And they attacked the borderland of Egypt, the land of Amqu'). In light of these facts, one might speculate that the addition is simply an error, or perhaps that the scribe began to add some details regarding the multi-pronged attack but then decided to 'summarize' as he did. Cf. also Winkels (1978: 57), who discusses the relationship among the DS and prayer passages, suggesting that the scribe's Vorlage may have been unclear or damaged.

⁷³ E.g. Houwink ten Cate (1963: 275); Wilhelm and Boese (1987: 104); Schulman (1978: 43 and n. 3); cf. n. 84, below.

⁷⁴ For usage, see CHD L-N, 378ff.

dozen years or more later) he sent, and again they attacked.’ Rather, the most obvious understanding of the text would be, paraphrasing slightly: ‘My father sent infantry and chariotry, and they attacked the borderland of Egypt, the land of Amqu; and (thereupon) he sent further (troops), and they also attacked.’ Further, it is these attacks that result, according to Mursili, in the Egyptians asking for Suppiluliuma’s son out of fear, which is obviously a reference to the events of the *tahamunzu* affair. Moreover, the other descriptions of these events likewise give the distinct impression of a single episode, and none of them include the additional *namma* clause of KUB 14.8.⁷⁵

Further, the report in EA 170, 14ff. also describes the Amqu attack as a two-pronged affair, which could easily accommodate the *namma* clause of the Plague Prayer: ‘Moreover, troops of Ḫatti under Lupakku have captured cities of Amqu, and with the cities they captured Aaddumi. May our lord know (this). Moreover, we have heard the following: Zitana has come and there are 90,000 infantrymen that have come with him. We have, however, not confirmed the report, whether they are really there and have arrived in Nuḫḫašše, and so I am sending Bet-ili to him.’ Clearly, Suppiluliuma sent a two-pronged attack at this juncture,⁷⁶ one prong certainly into the Amqu. Where the other prong attacked is not specified, not in the Plague Prayer and not in EA 170, the author of which is uncertain about the route taken by Zita(na); there are apparently rumours that it was headed toward Nuḫḫašše. Thus, it may be suggested that the two- or multi-pronged attack, likely of some duration, suggested in the Plague Prayer fits best with the evidence of the single, two-pronged campaign of EA 170. In any case, this certainly seems more plausible than assuming a dozen years or more between the attacks separated by the *namma* clause in the Plague Prayer.

It has also been noted as support for the assertion that there was only one Amqu attack episode that the general Lupakki is mentioned both in Mursili’s Fifth Plague Prayer and the DS on the one hand and in EA 170, a letter from the brothers of Aziru to him in Egypt, on the other.⁷⁷ Further, the messenger who assures Suppiluliuma of the veracity of the *tahamunzu*’s assertions is Hani, who was also active during the period of the Amqu attack of the Amarna correspondence.⁷⁸ Obviously, these correspondences of and by themselves are not conclusive, as others have countered,⁷⁹ but they are suggestive. Of course, those who would like to see two Amqu attacks point to details in one account that are missing

⁷⁵ For convenient collections of the Hittite sources dealing with the Amqu episode, see Parker (2002: 38–46); Sürenhagen (1985: 40–51).

⁷⁶ One could take the account in the DS, in which it is stated that Suppiluliuma send Lupakki and Tarḫunta-zalma to the Amqu, as indicating a three-pronged attack in Syria; and further, one could extend it even to a four-pronged attack on the basis of EA 174–176, which add that Aitakkama, king of Qadeš, also assisted the Hittites, presumably leading a contingent into the Amqu. In short, there are no shortage of candidates for whom might have been sent following the *namma* clause of the Plague Prayer.

⁷⁷ The Amqu episode dates to Aziru’s stay in Egypt, and is the same attack as that in which Aitakkama and Aziru (from afar) were involved, as shown most recently by Parker (2002: 40–41, 43ff., with refs.), where, however, EA 170 is errantly described as written to the pharaoh.

⁷⁸ See list of refs. for Hani in Moran (1992: 381).

⁷⁹ E.g. Sturm (1933: 165ff.) in his early, influential article; Schulman (1978: 43f.); Freu (2002: 102).

from the other, but this is to be expected when differing vantage points on the mayhem of war are related.

Moreover, Krauss (1978: 36–40) has forwarded the following argument, which he claims excludes the equation of Nibḫururiya with Tutʿankhamun: From the DS it is clear that up until the Egyptian attack against Qadeš and the Hittite counterattack against Amqu, which occurred in the year of the death of Nibḫururiya, Ḫatti and Egypt had been bound by a peace treaty.⁸⁰ And since EA 170 reports on a Hittite attack on Egyptian territory, it is certain that conflict between the two powers had occurred before the reign of Tutʿankhamun, and hence, the first conflict cannot be dated to the time of death of Tutʿankhamun. This argument has been variously received and indeed, though suggestive and not to be discounted, does not seem to be as water tight as Krauss initially suggested, as Murnane (1983: 279) has argued.⁸¹ Still, it does seem that Mursili interpreted the Amqu incursions as the first outright abrogation of an official state of peace with Egypt,⁸² which would, according to Krauss, fit better at the end of Akhenaten’s reign than at the end of Tutʿankhamun’s (or Smenkhkare’s).

In short, it should be clear that assuming two attacks on the Amqu some dozen years apart is a wholly ad hoc solution necessary solely for those who advocate the Nibḫururiya=Tutʿankhamun identification. No Hittitologist would ever have assumed two such attacks separated by over a dozen years based on the sources dealing with them. The attacks separated by the *namma* clause in Version A of the Plague Prayer (and only there; see n. 72) are easily accommodated within a single, two- (or multi-) pronged campaign to Syria, indeed, as is reflected in the Amarna letters.

In fact, it seems prudent to abandon the attempt to find evidence in the Hittite sources for two attacks separated by some dozen years. Those who find it necessary to have two Amqu attacks separated by a dozen years should, in my view, assume that the first attack attested in the Amarna correspondence during the reign of Akhenaten finds no reflection in the Hittite sources, and that the attack related in the Hittite sources was a single, two-pronged campaign at the time of the death of Nibḫururiya, concerning which there

⁸⁰ The Kurustama treaty according to the traditional interpretation (Houwink ten Cate 1963: 274), ‘ein paritätischer Staatsvertrag zwischen Hatti und Ägypten, geschlossen vor dem Regierungsantritt Suppiluliumas,’ according to Sürenhagen (1985: 64). For the latest discussion of the Kurustama Treaty, see Singer (2004).

⁸¹ Murnane shows that Krauss’ attempt to fit the whole of Suppiluliuma’s Syrian campaigns into the 6 years of KUB 19.9 and his placement of the ‘First’ and ‘Second Syrian Wars’ in consecutive years are not tenable. One must spread these 6 years out over significantly more time and separate the two wars by at least a couple years. Cf. also Section 8.2, below; Wilhelm and Boese (1987: 104f.); Barta (1983: 19f.).

⁸² That this was also the case according to Egyptian historiography is argued by Schulman (1978: 44 and ns. 10–11), who even suggests that Suppiluliuma may have concluded a peace treaty with Amenhotep III. However, even if the Hittite and Egyptian ‘historians’ attempted to portray the period preceding the Qadeš and Amqu altercations as a period of peace between Egypt and Ḫatti, it would likely be a case of revisionist history, as it is clear from the Amarna corpus, regardless of whether Nibḫururiya of the DS be Akhenaten or Tutʿankhamun, that Egyptian-Hittite relations were in a state of DEFCON 2 for years before the death of Akhenaten; see also below, Section 7.3, and Murnane (1983: 279).

exists no Egyptian (or Amarna) documentation. That is to say, one is essentially forced to accept not only that the elements shared by the two attacks (e.g. the appearance of Lupakki and Hani in both episodes; see above) are coincidences that would have happened twice some dozen years apart, but also that there are extant two completely independent and mutually exclusive datasets concerning two entirely unrelated Amqu attacks: First, the Amarna letters, which relate abundant data concerning an Amqu attack that can certainly be dated toward the end of the reign of Akhenaten, an attack which finds no reflection whatsoever in the Hittite sources; and second, the Hittite material, which supplies a rich set of information concerning an entirely different Amqu attack some dozen years later at the time of the death of Tutʿankhamun, which naturally finds no mention in any Egyptian material, least of all the Amarna correspondence, since Tutʿankhamun had by this time resided at Memphis already for nearly ten years.⁸³ Thus, it seems that the only viable option for the supporters of the identification of Nibḫururiya with Tutʿankhamun is the two mutually exclusive datasets scheme.⁸⁴

5. The Death and Burial of Nibḫururiya

A further argument against the identification of Nibḫururiya with Tutʿankhamun, which will only be touched upon here, is the fact that Nibḫururiya is known to have died in late summer or autumn, as can be gathered from the Hittite sources, while Tutʿankhamun was buried in (or around) April, as can be gathered from details of his burial,⁸⁵ which would thus be long after the customary 70 days between death and burial. Hence, Tutʿankhamun would have died in late January or February,⁸⁶ some 3–4 months after the estimated time of death of Nibḫururiya. And of course, there was already a new pharaoh at the time of Tutʿankhamun’s burial, Aya, who is pictured as presiding over the burial rites.⁸⁷ The supporters of the Nibḫururiya=Tutʿankhamun hypothesis are thus forced to explain (1) why Tutʿankhamun would have been buried some six months after his death rather than 70 days; and (2) how the *taḫamunzu* could have claimed that she needed a new husband to become king though Aya apparently had already become king.

Bryce (1990: 104f.; 2003: 190ff.), for example, attempts to do so by suggesting that it was precisely during such an interregnum that the *taḫamunzu* would have carried on her

⁸³ Similarly Parker (2002: 46), who emphasizes that if one wishes to unite the two datasets, a large portion of the Amarna archive would have to be dated to the end of the reign of Tutʿankhamun (with e. g. Houwink ten Cate 1963: 275), which can be categorically excluded.

⁸⁴ Precisely this indeed seems to be embraced by Freu (2004: 38f.); cf. also Kitchen (1985: 44); Klengel (1964: 77); and Barta (1983: 20), who conveniently assumes that the Amqu incursion as related in the Amarna documents would have been related in another tablet of the DS.

⁸⁵ See Krauss (1996); Hornung (2006: 206ff.); Parker (2002: 48 and n. 62).

⁸⁶ According to Krauss (1996: 248), that he would have died in February finds support in the ostrakon inscription according to which Haremhab took the throne in March coupled with the assumption that the 4 years and 1 month assigned by (the excerpts of) Manetho to Harmais in fact are to be assigned to Aya, and thus, that Aya would have taken the throne in February.

⁸⁷ See Parker (2002: 49 and n. 64); Helck (1984: 165); cf. von Beckerath (1994: 102; 1997: 115).

negotiations for a son of Suppiluliuma, which amounts to a neat explanation of the abnormally long period between death and burial, and further, that Aya would have taken the throne and carried out the burial rites upon the failure of the *tahamunzu*'s attempt.⁸⁸ It must be emphasized, however, that there is no Egyptological evidence for an autumn death of Tut'ankhamun (nor any other information that would indicate when he died, except for the approximate date of his burial), and that such a proposal stems solely from the assumption of the identity of Nibhururiya and Tut'ankhamun. Thus, this issue remains a serious difficulty for those who advocate the identity of Nibhururiya and Tut'ankhamun, but ad hoc explanations can be (and have been) imagined.

6. The Identification of the *tahamunzu*

A further issue that should be noted is that of the identity of the *tahamunzu*, though this paper is obviously not the forum where this complex and oft-debated matter will be decided.⁸⁹ Perhaps it will suffice to mention just a few considerations in passing and to point to some recent literature.

As Hornung (2006: 207) has most recently noted, based on a wide array of evidence, from excerpts of Manetho to wine-jar sealings to finger rings, 'It is now certain that not only a man 'Ankhhkheprure', but also a woman 'Ankhetkheprure' ruled between Akhenaten and Tut'ankhamun,' and this seems to have found a fair degree of consensus. It is also generally accepted that Smenkhkare' was the man who ruled under the male version of the name. What is not yet certain is: (1) who ruled as pharaoh under the feminine name, 'Ankhetkheprure', though Merytaten seems to be the favoured candidate; (2) whether the reign of 'Ankhetkheprure' as pharaoh followed the death of Akhenaten or that of Smenkhkare', though the latter is generally preferred; (3) whether this reign of 'Ankhetkheprure' as pharaoh represents the period of the *tahamunzu* episode; (4) or if this episode should rather be assigned – excluding for the moment the period after the death of Tut'ankhamun – to an 'interregnum' between the death of Akhenaten and the accession of Smenkhkare'.

Placing the *tahamunzu* episode after the reign of Smenkhkare' would be difficult to reconcile with the paradigm presented in this paper, as it would allow a mere 2–3 years between this episode and the death of Suppiluliuma. That said, Klinger (2006: 319) supposes exactly this, i.e. that Suppiluliuma 'can only have lived for 2–4 years' after the death of

⁸⁸ Similarly Barta (1983: 18f.); Helck (1994: 18) does not accept such a proposition, but I do not find it as absurd as he does.

⁸⁹ For **(1a)** Nefertiti as the widow of Akhenaten, see Allen (1994); Murnane (1995: 205ff.); Krauss (1997a; 1997b; 2007); **(1b)** Merytaten as the widow of Akhenaten, Krauss (1978); Parker (2002: 49–52); Gabolde (1998; 2001; 2002); cf. Krauss (1997a); Murnane (2001: 18); Eaton-Krauss and Krauss (2001: 95); **(1c)** Kiye, a secondary wife of Akhenaten, Helck (1981; 1984; 1994: 18ff.); cf. Parker (2002: 51, n. 77); von Beckerath (1997: 112); Murnane (2001: 13); Krauss (1997b); **(2)** Merytaten as the widow of Smenkhkare', Wilhelm and Boese (1987: 98ff.); **(3)** Ankhesenamun, as the widow of Tut'ankhamun, Sturm (1933); Bryce (1990: 103); Barta (1983); cf. Helck (1994: 18).

Nibḥururiya (Tut‘ankhamun for him),⁹⁰ numbers which would also allow for the *taḥamunzu* episode to follow the death of Smenkhkare^c within the chronological paradigm of this paper.^{91,92} The present paper favours the hypothesis that Nefertiti is the most likely candidate for identification with the *taḥamunzu*, and that this episode occurred, likely during an interregnum, following the death of Akhenaton and before the accession of Smenkhkare^c, a position assumed, admittedly, more for the sake of clarity in the present paper than out of steely conviction.⁹³ This tentative hypothesis is favoured with the full realization that several vital issues within this complex web of evidence and deduction have yet to find consensus among Egyptologists, and that this section, perhaps more than any other, must remain open to revision. The evidence which leads me to this view is the following:⁹⁴

First, as Krauss (1997a: 226; 1997b) has endeavoured to show, there is actually no conclusive evidence for the long-standing assumption that Nefertiti died or was exiled

⁹⁰ Klinger’s assumption of only 2–4 years, however, seems to be based more on an effort to reconcile his scheme with the solar omen of Mursili II’s 10th year (cf. below, Section 8.4) than a consideration of the minimum number of years required for what is known of Suppiluliuma’s reign following the *taḥamunzu* episode. Determining how long Suppiluliuma lived after this episode, though, is essentially impossible on present evidence (cf. Wilhelm and Boese 1987), unless one accepts KUB 19.15+KBo 50.24 as yielding the solution.

⁹¹ Klinger’s 2–4 years would also allow for Wilhelm and Boese’s identity of Nibḥururiya with Smenkhkare^c, but, paradoxically, only if one rejects the very calculations – i.e. ca. 10 years of Suppiluliuma’s reign before the *taḥamunzu* episode, ca. 10 years after (see Section 7.1) – that led them to their conclusion in the first place. If all the events of KUB 19.15+KBo 50.24 were to be attributed to Mursili’s 7th year rather than to years 7 and 9 (Miller, in press b, n. 61), this would yield some 4–5 years between the deaths of Smenkhkare^c and Suppiluliuma, yielding a ratio of ca. 15 to 5, which would still conflict seriously with Wilhelm and Boese’s preferred ratio of ca. 10 before and 10 after.

⁹² If it is the reign of ‘Ankhetkheprure^c as pharaoh following the death of Smenkhkare^c that represents the *taḥamunzu* period, then one is left with at least three possibilities: (1) Wilhelm and Boese’s identification of Nibḥururiya with Smenkhkare^c (see Section 7 and n. 16); (2) that Merytaten, as ‘Ankhetkheprure^c, would have initiated the *taḥamunzu* episode only upon the death of her second husband, Smenkhkare^c, at which time she referred in her letter to Suppiluliuma to her first husband and father (Nibḥururiya/Akhenaton) as having died and leaving her with no son, without bothering to mention the short-lived and inconsequential Smenkhkare^c; (3) that Nefertiti, as ‘Ankhetkheprure^c, initiated the *taḥamunzu* episode after outliving Akhenaton and Smenkhkare^c.

⁹³ It would not surprise me at all, e.g., if it turns out that Nefertiti in fact predeceased Akhenaton and the *taḥamunzu* turns out to be Merytaten following the death of Akhenaton, as several researchers have suggested; cf. ns. 89 and 95.

⁹⁴ If it turns out that Nefertiti was in fact the *taḥamunzu*, then one might speculate further that part of her motivation for refusing to marry anyone from the Egyptian court might have stemmed from a realization that no such candidate was likely to continue with the exclusive Aten worship which may have been just as dear to her as it was to Akhenaton. If so, perhaps she sought a Hittite spouse and pharaoh – as opposed, e.g. to a Babylonian – not only on account of Ḫatti’s rising star on the international political scene, as is sometimes assumed, but precisely because of the lofty status of the sun-deity in Ḫatti, as expressed first and foremost in the title of the Hittite emperor, ^dUTU-ŠI, ‘My Sun’ (of which Nefertiti certainly would have been aware), but also in the high position of the sun-goddess of Arinna in the Hittite pantheon and in particular for Suppiluliuma (of which the *taḥamunzu* may perhaps have been aware), in whose Deeds she appears at the head of the list of gods who guide him, ahead of the storm-gods of Ḫatti and the Army and the Lady of the Field, an Ištar hypostasis.

before the death of her husband, Akhenaten. Similarly Murnane (2001: 17), in his review of Gabolde's work, states that 'No single piece of evidence, by itself or in combination with others, indicates that Nefertiti is dead, or that she predeceased her husband.'⁹⁵

Second, as laid out by Murnane (2001: 16ff.),⁹⁶ the attestations of the two throne names prefixed with 'Ankhkheprure' can be divided into a first group that (a) receives epithets, including 'beneficial for her husband',⁹⁷ (b) that is always associated with the personal name Nefernefruaten, and (c) is sometimes feminized; and a second group which (i) does not take epithets and (ii) is always associated with the name Smenkhkare'.⁹⁸ Murnane (*ibid.*), following Gabolde, asserts that these readings 'go a long way toward proving ... that the owner (of the first group) was both female **and** married to Akhenaton' (emphasis in original).⁹⁹

Third, it is known that Nefertiti is pictured on several media wearing the blue crown alongside her husband Akhenaten¹⁰⁰ or holding the royal sceptre. Krauss (2007) has interpreted these scenes as indicating either that Nefertiti was raised to this status, which would not have entailed a full coregency, toward the end of the reign of Akhenaten,¹⁰¹ or that she ruled during an interregnum of sorts, without assuming full pharaonic status, following his death and before the accession of Smenkhkare'.

⁹⁵ See also Eaton-Krauss and Krauss (2001: 95). It must be admitted, though, that while the evidence taken by Gabolde (1998: 170–174; 2001: 20f.) and Allen (2007: 15 and n. 64) as indicating that Nefertiti predeceased Akhenaten might not amount to mathematically certain proof, it is suggestive, and that pleading in such a situation that it is not 'proved' may be (conveniently) expecting too much.

⁹⁶ As originally noted by Allen (1994); see also Gabolde (1998: 147–157); Eaton-Krauss and Krauss (2001: 94); Hornung (2006: 207); von Beckerath (1997: 112); Allen (2007); Krauss (2007).

⁹⁷ I. e. Akhenaten. Or 'Ankhkheprure', with Hornung (2006: 207)? Since the other similar inscriptions, i. e. 'beloved of Neferkheprure/Waenre/Akhenaten' (von Beckerath 1997: 112), clearly relate to Akhenaten, it seems likely that this one could as well; cf. also n. 98, and Allen (2007: 5 and n. 18).

⁹⁸ Cf. also Allen's (2007: 1–2) summary, as well as Hornung's (2006: 207): 'The king is first known as 'Ankhkheprure' (throne name) Smenkhkare' *dsr-hprw* (personal name), later as 'Ankhkheprure' *mrjj/Neferkheprure/Waenre* and Nefernefruaton *mrjj Waenre*. The two names of the queen, i. e. 'Ankhetkheprure' *mrjj/Neferkheprure/Waenre* and Nefernefruaton *ꜣht n hj.s*, are nearly the same as the king's later set of names and epitheta. The "funerary" epitheton *ꜣht n hj.s* (beneficial for her husband) is hers alone and indicates that she succeeded her husband 'Ankhkheprure'.

⁹⁹ Cf. von Beckerath (1997: 113): 'Der Name Nefer-nefru-aten findet sich (außer als Beiname der Nofret-ete) stets nur mit männlichen Titeln und Epitheten.' While Hornung leaves open the identity of 'Ankhetkheprure', 'kommt Anchet-chepru-rê nur noch als Thronname der Nofret-ete in Betracht' according to von Beckerath (1997: 113), 'sie scheint von Ach-en-aten in den späteren Jahren seiner Regierung zur Mitregentin erhoben worden zu sein' (cf. n. 21, above). He (p. 112f.) argues that 'Ankhetkheprure' cannot have been Merytaten, due to the representations of her as the spouse of Smenkhkare' and since 'es ist nach ägyptischer Vorstellung unmöglich, daß eine Frau, die einmal "Pharaoh" mit den göttlichen Aspekten eines solchen war, später in den "irdischen" Stand einer Königsgemahlin zurückversetzt werden könnte' (cf. also Allen 2007: 14 and n. 61).

¹⁰⁰ Von Beckerath (1994: 100); Krauss (2007).

¹⁰¹ Cf. Allen (2007: 3 and n. 12), who interprets them as portraying living individuals, Akhenaten and Nefernefruaten, and thus that the reign of Nefernefruaten ('the younger' for him; see n. 22) 'was at least partly contemporary with that of Akhenaten.'

Fourth, as Krauss (1997a: 247; 2007) and Hornung (2006: 207) have argued, the post-Akhenaten wine-jar sealings from Amarna are most likely to be distributed so that the year 2–3 attestations are attributed to Smenkhkare^c, the year 1 attestations divided between Smenkhkare^c and his successor, either ‘Ankhetkheprure^c or Tut‘ankhamun. Krauss (1997a: 247) has also pointed out, though, that these same wine-jar sealings cannot rule out an interregnum following the death of Akhenaten and before the accession of Smenkhkare^c.

These indices, however, cannot be considered decisive and are open to various interpretations. Gabolde (1998; 2001; 2002; see also Parker 2002: 49–52), for one, has recently argued at length that Nefertiti died shortly before Akhenaten did,¹⁰² that Merytaten was raised to the status of coregent in her place, and that she reigned following her father’s death, first as the queen of Smenkhkare^c/Zannanza¹⁰³ and then alone as ‘Ankhetkheprure^c.¹⁰⁴ Murnane (2001) and Eaton-Krauss and Krauss (2001) have responded that there is no evidence for the passing of Nefertiti and that some of the evidence understood by Gabolde as indicating the identity of Merytaten with ‘Ankhetkheprure^c cannot be taken as such, though Eaton-Krauss and Krauss (2001: 95) admit that Merytaten indeed bore that name at least at some point.

In short, the question of the identity of the *tahamunzu* must remain open for the time being. If the *tahamunzu* episode in fact followed the death of Akhenaten, then Nefertiti and perhaps Merytaten must be considered the prime candidates, though still others have been suggested.

¹⁰² Cf. Murnane (2001: 17f.); Eaton-Krauss and Krauss (2001: 95).

¹⁰³ Pace some suggestions in recent literature (e.g. Gabolde 1998: 193), it seems to me that KUB 19.20+KBo 12.23 (see van den Hout 1994), (a draft or copy of) the letter sent by Suppiluliuma to Egypt upon being informed of the death of his son, offers no hint concerning whether Zannanza was killed before arriving in Egypt or in Egypt itself, and no evidence regarding whether there was already a pharaoh reigning in Egypt upon his (assumed) arrival, since restoring ‘[Daß du (aber)] / [auf dem Thron s]jaßest?, das eben [wußte ich] nich[t]’ in obv. 22’f. is wildly speculative. Further, nothing in this letter supports placing the *tahamunzu* episode following the reign of Tut‘ankhamun rather than that of Akhenaten, or vice versa. Moreover, contra van den Hout (ibid.: 85, n. 70), one can base no conclusions regarding the gender of the letter’s addressee on ŠEŠ-*tar*, ‘brotherhood’, in rev. 31’ and 33’, since (1) in l. 31’ it is Suppiluliuma quoting the pharaoh as writing concerning ‘brotherhood’, not Suppiluliuma referring to the pharaoh; and (2) the term is likely employed in the standard usage of international diplomacy, not as an indication of the receiver’s biological gender, so that one could hardly expect the letter to have employed NIN-*tar*, ‘sisterhood’, if the addressee were a woman.

¹⁰⁴ In addition to the comments of Murnane (2001) and Eaton-Krauss and Krauss (2001), two aspects of Gabolde’s reconstruction seem unlikely to me: (1) Since Merytaten, who would have been only 12–13 years old at the time of her father’s death, would presumably have needed the backing of some powerful supporter(s) to carry out such a scheme as the *tahamunzu* episode, one is left with the question of why this supporter would have chosen to throw his weight behind such a daring scheme as this if there was already a legitimate son and heir to the throne in the person of Tut‘ankhamun? (2) If, upon the death of Nefertiti, Akhenaten chose in the last year or so of his reign to raise one of his children to the status of coregent, why would he have chosen Merytaten rather than Tut‘ankhamun, his son from his first wife? This would essentially mean that it was Akhenaten who denied the throne to his son in favour of his daughter, as opposed to Merytaten pushing aside her brother, as Gabolde (2001: 38) assumes; see similarly Allen (2007: 9).

7. Wilhelm and Boese's Calculations on the Deeds of Suppiluliuma¹⁰⁵

7.1. A Summary of the Issues

In an influential article which appeared in 1987, Wilhelm and Boese published the results of their analysis of the highly fragmentary DS. Those relevant to the issues in the present paper, summarizing as succinctly as possible, are the following: (1) the distribution of the events of Suppiluliuma's reign in the surviving fragments of the DS require that the 40-odd years previously assumed for his reign be cut to roughly 20;¹⁰⁶ (2) since the *taḥamunzu* episode is recounted at about the midway point in the portion of the DS detailing Suppiluliuma's reign, approximately half of his reign (ca. 10 years) must precede the *taḥamunzu* episode, while about half must follow it;¹⁰⁷ (3) this leads them to conclude that neither Akhenaten nor Tut'ankhamun could have been Nibḥururiya, since: (a) those reconstructions according to which Nibḥururiya is equated with Akhenaten require at least some 20 years of Suppiluliuma's reign to be placed before the death of Akhenaten and only some 6–10 years following that point;¹⁰⁸ and (b) since placing Suppiluliuma's accession only 10 years before the death of Tut'ankhamun would not allow for the several years of Suppiluliuma's actions in Syria attested already during the reign of Akhenaten according to the Amarna letters.¹⁰⁹ Thus, they opt for an identification of Nibḥururiya with Smenkhkare'.

¹⁰⁵ This study's conclusions have been received variously: cf. de Martino (1993: 230–240), who accepts it fully (but cf. now de Martino 2006: 205 and n. 75); Bryce (1998: 410 and n. 2), who claims to accept the shortening of Suppiluliuma's reign, but retains the identification of Nibḥururiya with Tut'ankhamun, though one cannot do so and maintain his paradigm (see Wilhelm 1991, and further below, especially n. 109); Starke (2002: 312–315) and Freu (2000; 2002; 2004: 33–47), who essentially reject the revision; Na'aman (1996), who rejects at least the revised date of Suppiluliuma's accession; Kempinski (1993: 88), who rejects the study categorically; see also notes 16, 50, 56, 63, 69, 90, 91.

¹⁰⁶ I cannot agree with Groddek's (2002: 277, n. 29) assertion that the development of the palaeography of the Hittite texts from that of Suppiluliuma's era to that of Mursili's would be difficult to explain if one accepts a reduction of the reign of Suppiluliuma from ca. 40 to little more than 20 years. The palaeographic dating criteria, invaluable as they may be, are simply not this precise.

¹⁰⁷ I.e. ca. 4 tablets (of the short-columned version) are dedicated to the years between Suppiluliuma's accession and the *taḥamunzu* episode, while about 5 contain his deeds from that episode up to the end of the series (assuming the 12th and last tablet attested is indeed the last). Similarly Helck (1984: 165), though here still assuming a coregency between Amenhotep III and Akhenaten; cf. Helck (1994), where a coregency is unequivocally excluded.

¹⁰⁸ As far as I see however, there is actually nothing in Wilhelm and Boese's analysis (as it appeared in 1987, i.e. before the evidence of KUB 19.15+KBo 50.24 became known) that rules out the equation of Akhenaten with Nibḥururiya. They reject this identification primarily since the reconstructions that assume it require at least ca. 20 years of Suppiluliuma's reign to be placed before the death of Akhenaten and at least ca. 6–10 thereafter, which conflicts both with Wilhelm and Boese's reduced reign of ca. 20 years and their roughly even distribution of his reign before and after the *taḥamunzu* episode. Such reconstructions, however, assume that EA 41 was addressed to Akhenaten, but since Wilhelm and Boese argue that EA 41 would have been addressed to Akhenaten's successor, the identity must not conflict with their paradigm. That is to say, within the framework of their reconstruction (and ignoring KUB 19.15+KBo 50.24), Suppiluliuma could have ascended the throne some 10 years before the death of Akhenaten and the *taḥamunzu* episode, then outlived him by ca. 10 years.

¹⁰⁹ Wilhelm and Boese (1987: 95); cf. also Wilhelm (1991: 474–475; 2004: 73 and n. 13); see similar considerations already in Helck (1984: 165). Bryce's (1989; also 1998: 410 and n. 2) attempt to reconcile

Wilhelm and Boese's conclusions are indeed difficult to reconcile with an equation Nibḥururiya=Akhenaten and the understanding of KUB 19.15+KBo 50.24 advocated in the present paper. Even if, for the sake of argument, one were to allow for the attribution of EA 41 to a successor of Akhenaten (cf. Section 7.3 and n. 108), it would be of little help when trying to reconcile the new evidence of KUB 19.15+KBo 50.24 with their conclusions. That is to say, if one accepts the *terminus a quo* of Mursili's 9th year for the accession of Haremhab as derived from KUB 19.15+KBo 50.24 in this paper (see Fig. 1), then the length of time between the death of Akhenaten and that of Suppiluliuma can not have been more than ca. six years (cf. ns. 91, 117), which would leave some 14 years of Suppiluliuma's reign to be placed before Akhenaten's death, yielding a ratio of more than $\frac{2}{3}$ before to less than $\frac{1}{3}$ after. Alternatively, reducing the reign of Suppiluliuma to only some 12 years, in order to preserve Wilhelm and Boese's half before to half after ratio, would almost certainly leave his reign too short to accommodate all that is known of it.¹¹⁰ Hence, there does not seem to be at present any convincing way of reconciling the interpretation of KUB 19.15+KBo 50.24 suggested in this paper with Wilhelm and Boese's conclusions based on their analysis of the DS.

Thus, one must either find a way to undermine the suggested *terminus a quo* argued in this paper¹¹¹ or advance acceptable reasons to discard Wilhelm and Boese's conclusions. The present section, then, will explore potential grounds for doubting Wilhelm and Boese's paradigm.

7.2. Methodological Considerations

Just how risky are such calculations based on assumptions of how many years and events should be assigned to a missing portion of text is shown, for example, by pointing to the mere 11 lines dedicated to the second year on the 7th tablet, as noted already by Wilhelm

Wilhelm and Boese's conclusions with the equation of Nibḥururiya and Tut'ankhamun cannot be considered a success, since, in addition to the reasons repeated and emphasized by Wilhelm (1991), it rests (Bryce 1989: 23) on the now outdated reading '12' for the year of the hieratic docket of EA 27 (see n. 65) and thus accepts a coregency of Amenhotep III and Akhenaten, which is no longer credible; see Hornung (2006: 206), and further refs. pro and contra in Freu (2002: 92 and n. 24). Only with such a coregency can Bryce date the so-called 'First Syrian War' to a maximum of three years before the death of Akhenaten. The elimination of the coregency, would, by Bryce's own reasoning, date the First Syrian War to 10 years earlier than he assumes, unequivocally disallowing his chronological scheme. To maintain the equation of Tut'ankhamun with Nibḥururiya one must assume at least some 20 years for Suppiluliuma before the death of Tut'ankhamun and ca. 6–10 afterwards (Bryce [1989] assumes 17 before and 5 after), which can hardly be reconciled with Wilhelm and Boese's conclusions.

¹¹⁰ And of course, since it is assumed here that EA 41 was addressed to Akhenaten, this requires some 18–20 years from Suppiluliuma's accession until the *tahamunzu* episode vs. at most 6 years from the *tahamunzu* episode until the death of Suppiluliuma.

¹¹¹ As mentioned in my original presentation of the text (Miller, in press a, n. 61 and Fig. 3), one might advocate rejecting this *terminus a quo* by attempting to show that 'Arma'a was already pharaoh in KUB 19.15+KBo 50.24, which would allow an identification of Nibḥururiya with Smenkhkare' or Tut'ankhamun.

and Boese (1987: 88). It should further be noted that, while Wilhelm and Boese place the *taḥamunzu* episode at approximately col. i of the 6th tablet of the long-columned series, Güterbock (1956: 47b) suggested that the long-columned fragment which duplicates tablet 7 of the short-columned version might rather be tablet 4 of the series, showing that a considerable amount of leeway can legitimately be imagined, even if Güterbock's estimate must be considered too low. Moreover, Wilhelm and Boese seem to ignore in their calculations the fact that large portions of the first, second and third columns of the seventh tablet (KBo 5.6) are left uninscribed, which Güterbock (*ibid.*) understood as indicating that the tablet from which it was copied had been broken.¹¹² In any case, this would introduce an added factor of uncertainty into any such calculations, and whatever its explanation might be, it is difficult to imagine that this blank space has no relevance at all for Wilhelm and Boese's analysis.

Further, though as far as can be seen from the preserved portions of the DS, the versions of different lengths are merely duplicates of the same composition, it should perhaps not be excluded that they represent different versions along the lines of the 10-year Annals of Mursili vs. the Extensive Annals. And of course, it is known that the 10-year version is not only limited to Mursili's first 10 years, it also omits wholesale, for no apparent reason,¹¹³ some extremely important episodes found in the Extensive Annals, such as the *entire* Syrian episode of Year 7. (Perhaps because the 10-year Annals related only campaigns which Mursili personally led?) Can one be even reasonably certain, when undertaking an analysis of such lamentably fragmentary texts, that this or that version of the DS does not do something similar?¹¹⁴ And what if this or that campaign was perceived by the Hittites to have been a defeat? Perhaps it would have been simply omitted or hurriedly skimmed over.

The risk can also be demonstrated by briefly considering, e.g. the Black Obelisk of Šulmānu-ašarēd III, not so very far removed in genre, time and space from the DS.¹¹⁵ If the middle *palū* of the reign of Šulmānu-ašarēd from 10–23 were missing,¹¹⁶ one would calculate (ignoring the dedication, genealogy and accession) from *palū* 1–9 (58 lines) and 24–31 (80 lines) that these missing years must have taken up some 120 lines and that the

¹¹² Alternatively, one could speculate that the scribe intended to fill in the blanks with elaboration, which he never got around to doing.

¹¹³ Cf. Spalinger (1979: 56f.).

¹¹⁴ In fact no single fragment from the DS can be attributed with any confidence to the so-called 'First Syrian War', but considering the text's state of preservation this can obviously not be considered indicative.

¹¹⁵ Astour (1989: 8) similarly criticized Wilhelm and Boese's analysis by pointing to Lavis and Rambaud's history of 19th century France, criticism which Wilhelm (1991: 473) rejected as too far removed in time, space and genre from the textual material at hand. (Unfortunately, no Hittite annalistic text is sufficiently preserved for such a comparison, so the parallel with Šulmānu-ašarēd's annals will have to suffice; Kempinski [1993: 88] pointed to the Annals of Mursili.) Wilhelm (1991: 473) points out, in response to Astour's criticism, that no assumption of basically evenly distributed years underlies his and Boese's calculations. While this may be technically true, as they do not assume that each year would receive the same length description, their study does assume a great deal about how many years would occupy a given length of break, and it is exactly this assumption that is very risky.

¹¹⁶ And the *palū* numbering was not generously provided for those fortunate Assyriologists!

text of the *palû* years would thus be some 258 lines long. But they do not and it is not. They take up only 25 lines, only about 1/5 as many lines as expected, and the *palû* years together take up only 164 lines.

Thus, while the considerations published by Wilhelm and Boese should certainly not be dismissed,¹¹⁷ it does seem that one must be very cautious in using their calculations to unequivocally rule out this or that chronological reconstruction. It might be suggested that their considerations should not be taken as the primary litmus test, but rather as one reflection among many. Further, there are more than merely methodological difficulties with Wilhelm and Boese's study. As will be seen presently, it is very difficult to reconcile their assumption that Suppiluliuma came to the throne only some seven years before the death of Akhenaten with what is known of the Hittite-Egyptian confrontation as attested in the Amarna letters.

7.3. *The Addressee of EA 41*

As mentioned (n. 108), the question of the identity of the addressee of EA 41 is relevant both as an argument against Wilhelm and Boese's paradigm and also for purposes of attempting to ascertain the date of the beginning of Suppiluliuma's reign even apart from their considerations.¹¹⁸ If the addressee was Akhenaten, then the identification of Nibḫururiya with Akhenaten would necessitate a reign for Suppiluliuma of at least some 25 years.¹¹⁹ Equating Nibḫururiya with Tut'ankhamun would require a reign of at least ca. 40 years.¹²⁰ In fact, it seems considerably more likely that EA 41, from Suppilu-

¹¹⁷ If for no other reason than the fact that assuming that the missing portions were necessarily filled with succinctly described years would also be gratuitous. Indeed, it is disconcerting that the reconstruction presented in this paper would presumably fill approximately 3 tablets of the DS before the *taḫamunzu* episode with ca. 18–20 years and about the same number of tablets after that episode with ca. 6 years, and this should not be simply ignored. (If all the events of KUB 19.15+KBo 50.24 were to be attributed to Mursili's year 7 [Miller, in press b, n. 61], this would ameliorate the conflict with Wilhelm and Boese's ratio only slightly, yielding ca. 18–20 before and ca. 8 after.)

¹¹⁸ The chronological paradigm presented in this paper could actually accommodate EA 41 having been addressed either to Akhenaten or to one of his successors, and the issue will be discussed primarily because it so often figures into deliberations in the secondary literature. In fact, the question of the addressee of EA 41 only became an issue for the identification of Nibḫururiya once Wilhelm and Boese (1987) suggested a reduction in the length of Suppiluliuma's reign and a lowering of the date of his accession (see Section 7.1). Since for Wilhelm and Boese the beginning of Suppiluliuma's reign cannot reach back any further than about the middle of Akhenaten's reign, they are prohibited from seeing in EA 41 a letter addressed to Akhenaten. Of course, if one does not accept Wilhelm and Boese's conclusions, then EA 41 no longer bears any import for the discussion. Cf. also n. 108.

¹¹⁹ I.e. a couple years at the end of the reign of Amenhotep III, the entire ca. 17 years of the reign of Akhenaten, and some 6–10 years following his death, the numbers 6 and 10 being two estimates, based on the Hittite sources, of the length of time Suppiluliuma outlived Nibḫururiya. Assuming that EA 41 was addressed to Akhenaten and equating Nibḫururiya with Smenkhkare^c would yield a reign of some 30 years, but with a division (ca. $\frac{2}{3}$ of Suppiluliuma's reign before the *taḫamunzu* episode, some $\frac{1}{3}$ thereafter) that would fatally conflict with Wilhelm and Boese's (1987) analysis.

¹²⁰ I.e. the ca. 25 years of n. 119 plus the 2–3 year reign of Smenkhkare^c and the 10–11 year reign of Tut'ankhamun; cf. n. 109.

liuma to a defectively written Ḫuriya,¹²¹ was addressed to Akhenaten rather than one of his successors.

The first clue suggesting that EA 41 was addressed to Akhenaten is the fact that Suppiluliuma repeatedly refers to the addressee's 'father', and it seems likely that the default assumption, until convincing evidence is forwarded suggesting otherwise, should be that 'father' literally means 'father'. After all, it occurs not in a formulaic introduction,¹²² which indeed can appear at the beginning of a letter regardless of its applicability to the receiver, but in the body of the text, and it should not necessarily be carelessly explained away.¹²³ Thus, if this is to be taken literally, then at least Aya is eliminated, as he was certainly no son of a preceding king. If Smenkhkare^c and/or Tut'ankhamun were indeed sons of Akhenaten, then they would remain candidates, but this seems unlikely (see Section 2), leaving Akhenaten as the most likely addressee.

The second clue is Suppiluliuma's repeated references in EA 41 to the warm relations between himself and the addressee's father, and this is much more easily understood as referring to the last years of the reign of Amenhotep III than to the last years of the reign of Akhenaten, the principal feature of which was the struggle between Egypt, Ḫatti and Mittanni for domination of Syria (cf. also n. 82). Since the Syrian wars certainly began and enmity had long festered between Ḫatti and Egypt during the reign of Akhenaten, as is clear from the Amarna archive, it would be very difficult to explain such a friendly letter being written to the direct successors of Akhenaten, i.e. to Smenkhkare^c or Tut'ankhamun.

Bryce's (1990: 100–103) attempt nevertheless to see in EA 41 a letter to Smenkhkare^c is, in my view, unconvincing. He asserts (103 and n. 28) that the 'one possible exception' to the friendly relations between Suppiluliuma and Akhenaten would be the Amqu incursion mentioned in EA 170, as if such a retaliatory invasion alone would not suffice to put the

¹²¹ Wilhelm and Boese (1987: 97f.), seeing in Smenkhkare^c the addressee, have suggested an attractive explanation for the errant writing. As is the case with Groddek's explanation of the errant writing *Bi-ib-* in the DS (see n. 50), however, the solution is largely speculative, and not necessarily any more likely than a simple unmotivated scribal omission or some alternative explanation, such as the possibilities considered by Meyer (1992). As Groddek (2002: 275, n. 15) noted, though, Meyer's *DIŠ* for *ana* would hardly be expected from a Hittite scribe of Suppiluliuma's era; this, however, invalidates only this one of Meyer's several considerations, not the others.

¹²² E. g. Suppiluliuma refers to the addressee's 'sons' in the introduction, which is very standardized and is included as such even in cases in which it is known that the receiver has no sons. Neither can the usage be related to that in which a subordinate addresses a superior or an elder as 'father', and vice versa. See, e.g. Moran (1992: xxiii), who states with regard to the Amarna letters generally, 'The body of the letter is, naturally, much less stereotyped, and formal conventions are few and variable.'

¹²³ Bryce (1990: 103, n. 29), e.g. assumes that 'the term "father" in the letter is used essentially to designate the addressee's predecessor on the Egyptian throne.' This, of course – be it true or not – is an unwarranted ad hoc assumption, with no accompanying discussion, necessitated by his acceptance of the equation Nibḫururiya=Tut'ankhamun and his conclusion that Suppiluliuma ascended the throne during the reign of Akhenaten. Cf. Wilhelm and Boese (1987: 98): 'Als Einwand gegen eine Identifikation des Adressaten von EA 41 mit Semenckare hat dieser Sachverhalt wenig Gewicht, da die übliche diplomatische Verwandtschaftsterminologie nicht notwendig auf die realen genealogischen Bezüge Rücksicht nehmen muß.' This reasoning, however, cannot convince. It may not be necessary, but the default assumption must be that 'father' is used to denote 'father', and the burden of proof is on the one who claims that it should be understood otherwise.

two great powers at enmity. But even this is a radically minimalistic interpretation of the Amarna evidence certainly datable to Akhenaten's reign, and does not match Bryce's own assessment of the era. Suppiluliuma had, e.g. taken over all of the Syrian possessions of Egypt's principal ally, Mittanni, right up to the doorstep of Egyptian territory, which could hardly have been perceived by the Egyptians as neighbourly (but cf. n. 141). Further, Bryce neglects the attack of Suttarna of Qadeš on Suppiluliuma and the latter's retaliation and replacement of Suttarna with Aitakkama as a Hittite vassal, who, incidentally, intrudes even further into Egyptian vassal territory by skirmishing with Biryawaza of Upe,¹²⁴ all of which Bryce (1998: 176) himself acknowledges falls in the reign of Akhenaten – and for Bryce this must be late in Akhenaten's reign, i.e. only shortly before when he would like to date EA 41. And of course Bryce (1998: 182ff.) also admits that the Egyptian vassal 'Abdi-Aširta, also during the reign of Akhenaten for him, was able to get away with as much as he did at least partly owing to the Hittite threat, and that his successor, Aziru, constantly played the tensions between Egypt and Hittite to his advantage, even allying himself with the Hittite vassal, Aitakkama; and further (p. 188), that Akhenaten allowed Aziru to return to Amurru to prepare for a Hittite invasion, but that Aziru instead treasonously switched allegiance to the Hittites, and that he and Aitakkama thereupon used their Hittite backing to continue to prey upon neighbouring Egyptian vassals, including the conquest of Qatna (p. 189f.). Obviously, this (and more) does not square at all with Bryce's claim that the Amqu invasion of EA 170 would be the 'one possible exception' to the peaceful relations between Suppiluliuma and the father of the addressee of EA 41.¹²⁵ Quite to the contrary, the relations between the two superpowers must have been extremely tense, indeed on the verge of an all out conflagration.

Letters between kings who have in recent times been at war or had prolonged altercations of this nature generally do not completely ignore such issues, though they may well tend to interpret them in a positive light if they wish to allow for current improved relations.¹²⁶ Indeed, EA 42, from a Hittite king to an Egyptian king, seems to be particularly aggressive, placing great importance on the order in which the names of the respective

¹²⁴ See Murnane (1985: 11f.).

¹²⁵ Schulman (1978: 44) assumes – apparently without realizing the implications regarding the addressee of EA 41 and its implications, in turn, for the question of the spelling of Nibḫururiya in the DS – that 'such good relations were certainly broken off as a result of the Hittite attacks on the Amka and against other locales in and on the periphery of the Egyptian sphere of influence in West Asia, namely around Nuḫašše and along the coast from Ugarit to Byblos.' He also provides (ibid.: 44f.) a description of the drawn out conflict between Egypt and Ḫatti already before the death of Akhenaten based on the Amarna letters and on Egyptian inscriptions and reliefs (but cf. Singer 1990: 165, where it is stated that Schulman has retreated on some of his interpretations of the Egyptian scenes, and Darnell 1991). And he also suggests (ibid.: 46, with refs.) that Akhenaten planned a counterattack to the Amqu aggression in the last year of his reign (for which see also Gabolde 1998: 195–207; 2001: 34f.), which was presumably prevented by his death (Singer 1990: 164f.).

¹²⁶ Cf., e.g. from the so-called Tawagalawa Letter, iv 7–10 (Miller 2006: 246), where the Hittite king requests of his Aḫḫiyawan counterpart that he instruct Piyamaradu to stop attacking Ḫatti from Aḫḫiyawan territory in the following manner: 'In der Angelegenheit von Wilusa, über welche der König des Landes Ḫatti und ich uns feind waren, in der Angelegenheit konnte er mich überreden, und wir haben Frieden geschlossen; ... unter uns ist Feindschaft nicht angebracht.'

kings are written in their letters; EA 43,¹²⁷ also from a Hittite king to an Egyptian king, likewise speaks of difficulties and conflict (though what parties were involved is uncertain); while EA 44, from the Hittite prince Zita (presumably the son of Tudḫaliya 'III' and brother of Suppiluliuma) to an Egyptian king, appears to discuss only entirely peaceful matters of gift giving, and thus, may well belong to the pre-conflict correspondence.¹²⁸

One might counter that these conflicts do not represent an actual war between Ḫatti and Egypt themselves, but this would underestimate the fact that interference with or an attack on a vassal, then as today,¹²⁹ would have been considered a grave threat, essentially tantamount to a declaration of war.¹³⁰

A third clue is the fact that not a single Amarna letter has been convincingly attributed to any successor of Akhenaten (cf. discussion of EA 9, above, and n. 63), though several scholars have attempted to attribute this or that letter to Smenkhkare' or Tut'ankhamun (or even Aya). While this might in any given case be a possibility, it seems clear that any such suggestion would have to have rather clear evidence in its favour in order to justify its position as a unique outlier, and this is certainly not the case with EA 41 (or EA 9). Rather, the letter is unproblematic when read as addressed to Akhenaten, but must be heavily abused when read as intended for one of his successors.

In short, it seems highly unlikely that EA 41 should be attributed to a successor of Akhenaten. Until evidence to the contrary becomes available, it should be assumed that EA 41 was sent to Akhenaten, and thus, that Suppiluliuma came to the throne some time before the death of Amenhotep III.

8. Further Notes on the Chronology of the Reigns of Suppiluliuma and Mursili

8.1. *The Overall Length of Suppiluliuma's Reign*

Parker (2002: 53ff.) has recently (re)suggested a seemingly reasonable interpretation of KUB 19.9, in which Ḫattusili 'III' summarizes Suppiluliuma I's campaigns as having lasted 20 years in Anatolia and 6 in Syria. These summaries need not be, indeed can hardly be, understood as first 20 years that preceded a final 6,¹³¹ but as likely indicating that during his

¹²⁷ Without wishing to exclude the possibility, I share Murnane's (2001: 19) scepticism about claims that EA 43, as liberally restored by Artzi (1993), can be employed as Gabolde (1998: 206f.) does.

¹²⁸ For confirmation of the provenience of these letters, see now Goren et al. (2004: 31 ff.).

¹²⁹ Countless examples could be mentioned, but the Falklands War, fought between the UK and Argentina over an economically entirely worthless island group in the South Atlantic, indeed heavily subsidized and thus to the parties to the conflict less than worthless, should suffice.

¹³⁰ Cf. e.g. von Dassow and Greenwood (2006: 199f.): 'This scenario (i.e. Suppiluliuma writing the friendly letter, EA 41, to Smenkhkare' while in the midst of conquering Egypt's Asian holdings) should not be seen as self-contradictory: the Hittite ruler's position could only have been enhanced by his presenting a credible military threat to Egypt's empire, for this gave him an advantage in negotiating peace and friendship.'

¹³¹ Cf. more literal assessments of this passage by, e.g. Kitchen (1962: 3ff.) and Bryce (1990: 98 and 100), who assumes a full 20 years of war in Anatolia, and only then 6 years of war in Syria, which is impossible to reconcile with any identification of Nibḫururiya or any reading of the Amarna correspondence

reign he spent a total of 20 years campaigning in Anatolia¹³² and a total of 6 in Syria, the 6 presumably being interspersed among the 20. The figure of 26 would probably give the total years of Suppiluliuma's reign.

According to the paradigm suggested in this paper, then (see Fig. 1), the figure of 26 years would allow Suppiluliuma to have ascended the throne in ca. 1356, some 5 years before the death of Amenhotep III.¹³³ This would permit Tušratta's defeat of the Hittite's mentioned in EA 17, sent to Amenhotep III,¹³⁴ to refer to an early foray of Suppiluliuma (though this is not necessary for the present reconstruction), a razzia which has been related to that mentioned in the first lines of the treaty between Suppiluliuma and Šattiwaza.¹³⁵

and the Hittite sources. Bryce elsewhere (1989: 20) allows for the figure 20 to be understood more along the lines of Wilhelm and Boese's 'long time' or as Parker would understand them, i.e. as a summation of his years spent in Anatolia.

¹³² For a different interpretation of the number 20, i.e. as simply indicating a long time, and therefore 'weitgehend wertlos', see Wilhelm and Boese (1987: 90f.), followed by Bryce (1989: 20). For a diametrically opposite assessment of such figures, see van den Hout (1994: 85ff.), who takes literally even the mention of the plague raging in Ḫatti for 20 years.

¹³³ Note that this would indeed force one to accept the rather long average of ca. 40 years for the reigns of the three generations from Suppiluliuma I to Ḫattusili 'III' (this number 40 would actually include the years of the reigns of Arnuwanda II, Muwattalli II and Urḫi-Teššub, which are, however, irrelevant for the sake of generation counting); see Wilhelm and Boese (1987: 95–96), referring to von Beckerath's argument. This is not, however, unrealistic, especially considering the fact, as mentioned by Wilhelm and Boese, that Mursili II and Ḫattusili 'III' were late-born sons. If, for example, Suppiluliuma ascended the throne at age 35 in 1355, Mursili was born to him at age 40 in 1350, then Suppiluliuma would have died in 1330 at the age of 60 and Mursili would have been 20 at his accession (neglecting the inconsequential reign of Arnuwanda); if Ḫattusili was born to Mursili at age 50 in 1300 (and took the throne after the reigns of Muwattalli and Urḫi-Teššub in ca. 1270 at age 30), he would have been 63 in 1237; all well within the realm of credibility. The error in using this argument against the accession of Suppiluliuma during the reign of Amenhotep III is trying to apply generational statistics to the small number of just three reigns. If calculations yield a number of 40 years per reign for some 6 to 8 or more (father–son) kings, the number is very suspect. If for only three kings, the younger of whom are late-born sons, the statistical argument is lost, and hence, this is no credible argument against Suppiluliuma coming to the throne a short time before the death of Amenhotep III.

¹³⁴ See, e.g. Wilhelm and Boese (1987: 76, with refs.). That Tušratta's claimed victory over the Hittite's should be linked with a defeat of Tudḫaliya 'III' in Isuwa, as suggested by Wilhelm and Boese, and followed, e.g. by de Martino (1993: 230f. and n. 83), must now be seen as unlikely. First, the Sunaššura treaty and the Tudḫaliya annals in which the Isuwa conflict is mentioned are now dated to Tudḫaliya I (I/II) (Wilhelm 1988), not Suppiluliuma, and Tudḫaliya 'III', respectively, and thus cannot reflect a conflict with Tušratta. Second, the matter of Isuwa in the Šattiwaza treaty hardly suggests that the father of Suppiluliuma, Tudḫaliya 'III', fought and lost in Isuwa. Rather, Suppiluliuma says only that during his father's reign the people of Isuwa rebelled and that only in his own reign did he campaign there and bring them back into the Hittite fold. There is no mention at all of Tudḫaliya 'III' attempting to do anything about the situation. Third, Tudḫaliya 'III' campaigned only in Anatolia, as far as is known, primarily in the west and the north. Thus, while it cannot be excluded that Tušratta's claimed defeat of the Hittites during the reign of Amenhotep III refers to a predecessor of Suppiluliuma, there is nothing in the Hittite sources that can at present be connected to this event. Only during the reign of Suppiluliuma did events transpire which could easily be linked to Tušratta's claim; cf. also n. 142.

¹³⁵ See also Parker (2002: 61); Altman (2004: 83 and n. 87); cf. Wilhelm and Boese (1987: 84f., with further refs.), who doubt that the first lines of the treaty should be interpreted as mentioning a previous foray.

It would allow the Amarna evidence to be largely correlated with the Hittite data concerning Suppiluliuma's campaigns in Syria. Most or all of Suppiluliuma's Syrian campaigns prior to various mopping-up activities and his final (re)conquest of Mittanni would have taken place during (and perhaps shortly before) the reign of Akhenaten, and this is reflected in the Amarna letters. The death of Akhenaten in 1336/4 would thus fall some 4–6 years before Suppiluliuma's death in ca. 1330.

8.2. Evidence Placing Suppiluliuma's Activities Early in Akhenaten's Reign

That Suppiluliuma was the reigning king in Ḫatti already during the late years of Amenhotep III and the early years of Akhenaten's reign is apparent, however, not only from reckoning backwards from the death of Nibḫururiya and from the evidence provided by EA 41. At least two other strands of evidence are very difficult to understand in any other way.¹³⁶

The first strand is that which can be gleaned from EA 27 and 29, according to which it is likely that Mittanni fell, or at least suffered a major defeat, presumably to Suppiluliuma, already in the fifth or sixth year of Akhenaten's reign. This can be gathered from the docket of EA 27, which indicates that it was received in Akhenaten's second year, and from EA 29, probably the last letter sent to Egypt from Mittanni, which, based on internal evidence, was sent some 3–4 years after EA 27.¹³⁷ While the abrupt end of Mittanni's correspondence with Egypt could conceivably have been caused by any number of events, it seems not unlikely that it would have been connected to Hittite aggression at Mittanni's expense, whether this date is to be understood as the final fall of that empire as such and the death of Tušratta or not.¹³⁸

The second strand is what seems to be the most reasonable interpretation of the chronology of the correspondence of Rib-Adda, that hypergraphic Byblian potentate, a subject which is, admittedly, complex and intensely debated.¹³⁹ The interpretation favoured here echoes in large part that of Moran (1992: xxxiv ff.), Na'aman (1996: 254 ff.) and Liverani (1998),¹⁴⁰ according to which all of Rib-Adda's correspondence which mentions 'Abdi-Aširta as still alive, including EA 75 with its mention of the Hittites' seizure of Mitta<ni>'s

¹³⁶ See also Freu's (2002: 88 ff.) argumentation for the contemporaneity of Suppiluliuma and Amenhotep III.

¹³⁷ See Kühne (1973: 39–48); Murnane (1985: 197–199); Moran (1992: xxxv and xxxvii); in the latter two the value on the docket of EA 27 should be corrected from 12 years to 2; see n. 65.

¹³⁸ See Wilhelm (1994: 295).

¹³⁹ See e.g. Moran (1992: xxxv and n. 127); Freu (2002: 90 ff.).

¹⁴⁰ Although I find it difficult to accept the consequence that the reference in EA 85, in which 'Abdi-Aširta also appears, to 'your father's return from Sidon' (ll. 70f.) should relate to Thutmose IV, who by the end of the reign of Amenhotep III would have been dead for nearly 40 years. And though I am aware of some of the incongruities it might create, I therefore suspect that most of the letters in which 'Abdi-Aširta is Rib-Adda's primary enemy are indeed to be dated to the outgoing years of Amenhotep III, but some few, including EA 85 (and with it EA 83–84, 86), to the earliest years of Akhenaten, with e.g. Klengel (1969: 188 and 233, n. 29); cf. also Altman (2003: 366 ff. and refs. in ns. 57–59); Campbell (1964: 86, 93–96). According to this interpretation, the Hittites under Suppiluliuma would have been vying with Mittanni for supremacy in Syria already in the later years of Amenhotep III and/or the earliest years of Akhenaten.

Syrian holdings, must be dated to the outgoing years of the reign of Amenhotep III.¹⁴¹ This interpretation of the Rib-Adda correspondence, if correct, makes it likely that the Hittites, presumably led by Suppiluliuma,¹⁴² were vying with Mittanni for supremacy in Syria already during the last years of Amenhotep III and/or the earliest years of Akhenaten.

That the earliest of the three subdivisions of the correspondence of the Syrian vassals, i.e. that defined by the letters written by Rib-Adda when 'Abdi-Aširta was still alive,¹⁴³ should be dated to the outgoing years of Amenhotep III¹⁴⁴ is apparent from the following fact: In those letters in which the sons of 'Abdi-Aširta are Rib-Adda's main enemy he often reminds Akhenaten of how his father, Amenhotep III, had come and taken 'Abdi-Aširta captive,¹⁴⁵ whereas in those in which 'Abdi-Aširta is still his main enemy he never once refers to any such fatherly successes. This indeed would be difficult to explain if any significant portion of these letters had been directed to Akhenaten. In Moran's words (1992: xxxv–xxxvi, n. 127):

If, however, Rib-Hadda's letters fall [entirely in the reign of Akhenaten], it is very difficult, if not impossible, to explain why the Byblos ruler, when writing in the Aziru period, recalls (EA 108, 117, 131, 132, 362) the success the present king's father, certainly Amenophis III, had in an earlier action against 'Abdi-Aširta, but when writing in the 'Abdi-Aširta period, he does not refer to it even once. Why, when Aziru is the enemy, is Amenophis IV urged to do as his father did to 'Abdi-Aširta, but when the enemy is 'Abdi-Aširta, and therefore the example of his father even more pertinent, he hears not a word about his father?

A further remark of Rib-Adda's, found in EA 116 (a letter of the second group; see above and n. 143), might also be taken to support such an early dating of the first block of his correspondence. In this letter 'Abdi-Aširta is already a historical figure, his sons are carrying on his legacy, and Hittite intervention in Syria has already impressed Rib-Adda enough to cause him to add Ḫatti to the 'great powers list' with which he mockingly compares the rulers of Amurru.¹⁴⁶ It seems unlikely, however, that this letter is to be dated

¹⁴¹ It should be noted that this results in EA 41 being written from Suppiluliuma to Akhenaten shortly after he had dealt a significant blow to Egypt's principal ally; cf. above, Section 7.3.

¹⁴² It cannot, of course, be categorically excluded that Rib-Adda's description is a vastly exaggerated assessment of the presumably unremarkable raid in the mountainous area of north-western Syria that seems to have taken place during Suppiluliuma's military endeavours while his father was still alive, as reflected in KUB 19.12 iii, generally attributed to the DS (Güterbock 1956: 61 f.; Klengel 1999: 132), in which an expedition to Mount Nanni is found in connection with the grandfather (Tudḫaliya 'III') and father (Suppiluliuma) of the speaker (Mursili II); cf. also n. 134.

¹⁴³ The other two subdivisions are (2) those letters written by Rib-Adda after the death of 'Abdi-Aširta, during that period of time in which the latter's sons ruled in Amurru, and (3) those letters written after Rib-Adda had lost his throne in Byblos; see Moran (1992: xxxv).

¹⁴⁴ Cf. Singer (1991: 148); Wilhelm and Boese (1987: 85–87).

¹⁴⁵ And if an argument from absence may be allowed at this point, it might be remarked that there seems to be no indication that 'Abdi-Aširta returned to the scene in Amurru after his capture.

¹⁴⁶ This shift, if the distribution of the four attestations of this comparison in Rib-Adda's letters is to be taken at face value, must have happened after the death of 'Abdi-Aširta, since in one letter (EA 76) in which comparison is made only to Mittanni and Babylon only 'Abdi-Aširta is mentioned, while in one letter (EA 104) in which comparison is still made only to Mittanni and Babylon the sons of 'Abdi-Aširta are mentioned, while in the two others (EA 116, 129) comparison is made with Mittanni, Babylon and Ḫatti, and the sons of 'Abdi-Aširta are mentioned; see also Singer (1990: 126 n. 2) and Na'aman (1996: 255).

more than just a few years after the accession of Akhenaten, since Rib-Adda reminds that king (62ff.), ‘And now the gods and the Sun and the Lady of Gubla have granted that you be seated on the throne of your father’s house (to rule) your land.’ While such a statement could conceivably be included in a letter composed later in the reign of Akhenaten, the closer to his accession date it is placed the more timely and relevant it would seem to be.¹⁴⁷

Further, that Rib-Adda wrote to Egypt concerning the sons of ‘Abdi-Aširta, i.e. after their father’s death, already in the earlier part of the reign of Akhenaten, can be gathered from EA 108, 8ff., in which Rib-Adda praises the pharaoh as being ‘like Ba’al and Šamaš in the sky’, the former likeness of which he hardly would have used – and in fact no longer did use – in later letters. As von Beckerath (1997: 111 and n. 490) has stated, ‘In seinem 5. Jahr faßte der König den Entschluß, seine Residenz in die von ihm geplante „Sonnenstadt“ Achet-aten (el-Amarna) zu verlegen und änderte seinen Namen, der den Gottes Amun enthielt, in Ach-en-aten,’ and further, ‘der in Kartuschen gesetzte dogmatische Name seines Sonnengottes Aten wurde nach dem 8., jedoch vor dem 12. Jahr abgeändert, was für die Datierung mancher Denkmäler für uns von Nutzen ist.’ It thus seems likely that EA 108 should be dated to approximately the earlier half of Akhenaten’s reign, though a precise cut-off date would be difficult to specify.¹⁴⁸ The death of ‘Abdi-Aširta should therefore be dated to the first half of the reign of Akhenaten at the latest, and naturally, all the letters in which ‘Abdi-Aširta is Rib-Adda’s enemy should be dated before this point.

It thus seems quite likely that at least most, perhaps all, of Rib-Adda’s correspondence in which ‘Abdi-Aširta alone is his main enemy in Amurru must be dated to the outgoing years of the reign of Amenhotep III. And if this is the case, then EA 75, in which ‘Abdi-Aširta still figures, shows that Ḫatti, presumably under Suppiluliuma I, already during the outgoing years of Amenhotep III or perhaps in the early years of Akhenaten, dealt a significant blow (at least in Rib-Adda’s admittedly paranoiac view) to Mittanni’s suzerainty in Syria.¹⁴⁹ Moreover, it seems likely that EA 75 should be situated not all too late within Rib-Adda’s correspondence in which ‘Abdi-Aširta is still alive, since that Byblian ruler apparently wrote still more letters to Egypt following the Hittite strike

¹⁴⁷ With Klengel (1969: 199, 237 n. 77), Na’aman (1996: 254, n. 16) and Campbell (1964: 87), though, it is clear that EA 116 cannot be dated due to this passage to immediately after Akhenaten’s accession. The intent of the passage is not to congratulate the pharaoh on his recent accession, but to remind him that his duty to his vassals is the same as it had been for his father.

¹⁴⁸ Similarly Na’aman (1996: 254ff.). One must be careful, though, in assuming that this epithet would have necessarily been outdated immediately upon Akhenaten’s move to Akhet-aten. It may be, for example, that it would have been the beginning of Akhenaten’s attack on the cult of Amun that caused the change in Rib-Adda’s letters, but when exactly this attack began is uncertain. Further, Akhenaten’s ‘monotheism’ did not reach into every corner of Egypt. While it doggedly persecuted the gods of Thebes (Amun, Mut, Chonsu, Monthu) and the goddess Nechet of Necheb (Eileithyiaspolis), it left others in peace, such as Osiris in Abydos and Ptah in Memphis (e.g. Krauss 2000). See also Gabolde (1998: 24–30); Murnane (2001: 12).

¹⁴⁹ As seen in EA 85 the Mittannian king was soon after, but still during the lifetime of ‘Abdi-Aširta, able to campaign all the way to Šumur (see also EA 86, 90, 95, 101), presumably temporarily correcting the situation (on the Mittannian raid of Amurru in EA 85, see most recently Altman 2003). The mention in EA 17 of Tušratta’s defeat of the Hittites likely refers to some such early razzia, perhaps during the reign of Amenhotep III.

against Mittannian interests in Syria but while ʿAbdi-Aširta was still alive.¹⁵⁰ Thus, EA 75 was likely written at least many months if not some few years before the end of this earliest block of Rib-Adda's correspondence.¹⁵¹

8.3. *Izre'el and Singer's Reordering of the Aziru Correspondence*

Parker (2002: 42, 44, 56ff. and n. 105) does not take Izre'el and Singer's (1990) work into account and relies on an outdated and likely inaccurate restoration and interpretation of the treaty between Mursili and Tuppi-Tešsub (cf. above n. 70), according to which Aziru first submitted to Ḫatti, then rebelled, then repented,¹⁵² and thus has difficulty integrating Aziru's Amarna correspondence into his otherwise feasible reconstruction. Izre'el and Singer's reordering of the Aziru correspondence,¹⁵³ along with Singer's more plausible understanding of the treaty between Mursili and Tuppi-Tešsub, essentially eliminate the problems Parker has in reconciling these letters with his paradigm and demonstrate that Aziru became a Hittite vassal, for the first and only time, probably within a short period after his visit to Egypt toward the end of Akhenaten's reign.

Though this involved discussion cannot be repeated here, a brief sketch of how Aziru's Amarna letters, and Izre'el and Singer's revised interpretation of them, might fit with the reconstruction offered in this paper will be provided. It seems that all the events in question would have occurred during the last year or so of Akhenaten's reign (and perhaps some few weeks or months thereafter): The two- or multi-pronged Amqu attack(s) (see above) would have stretched over this year (or over the campaigning season of this year). Aziru would thus have been in Egypt during the initial phase of the Amqu episode when EA 170 was sent, whereupon (and perhaps because of which) he returned to Amurru and sent, within about a year's time at the most (Singer 1990: 155ff.), but perhaps still within the same campaigning season, his remaining letters EA 161, 164–167. Probably toward the end of this period the ominous letter EA 162 was prepared to be sent from Egypt to Aziru (but perhaps never sent?). At this point (or even before writing the last of his letters EA 161, 164–167) Aziru defected, and the Egyptians made initial preparations for war.¹⁵⁴ In the autumn of this year Akhenaten died, initiating the *tahamunzu* episode. The Hittite descriptions, some of which seem to imply that the *tahamunzu* episode occurred somehow at the same instant as the Amqu affair, would thus be telescoping to some degree, as would the Hittite story of Aziru defecting immediately upon his return from Egypt. The *tahamunzu* affair would have occurred toward the end of the same Syrian campaign in which Amqu was attacked, a campaign, or series of campaigns, which might have lasted an entire season, i.e. perhaps some 6–9 months, from spring to autumn.

¹⁵⁰ E.g. Parker (2002: 58 and n. 104); Freu (2002: 93f.); Altman (2001: 42f. and n. 48; 2004: 86, n. 100).

¹⁵¹ Cf. e.g. Wilhelm and Boese (1987: 87ff.); Bryce (1989: 22f.).

¹⁵² Cf. Singer (2003a: 96 and n. 2).

¹⁵³ See also Goren et al. (2004: 101ff. and esp. 123ff.).

¹⁵⁴ Singer (1990: 159) even suggests that the defection of Amurru might have been the catalyst.

8.4. *The Solar Omen in Mursili's 10th Year*

The beginning of Mursili's reign has often been dated to 1322 with reference to a solar omen that occurred in his 10th year, in turn linked to an eclipse datable to June 5, 1312.¹⁵⁵ As Huber (2001) has shown, however, a conclusive assessment of the solar omen of KUB 14.4 is extremely difficult to attain. His preferred date of 1339 and his second choice of 1334 would fit with no currently discussed chronological scheme, as this would place Mursili's 10th year toward or at the end of the reign of Akhenaten. Huber's other possibility for the eclipse, 1311, could seemingly only be reconcilable with the new information from KUB 19.15+KBo 50.24 if it is assumed that 'Arma'a is already pharaoh; Suppiluliuma's death would then have occurred some 12 years after that of Akhenaten (or some 10 years after that of Smenkhkare^c), allowing him (or Smenkhkare^c) to be equated with Nibḥururiya, even if 12 years between the two deaths seems high. In light of the myriad difficulties with the interpretation of the solar omen, it will not be considered further in this paper.

8.5. *Haremhab's Supposed Syrian Campaign in his 16th Year*

The new data from KUB 19.15+KBo 50.24, assuming the events of col. ii are indeed to be dated to Mursili's 9th year, would be amenable to Redford's (1973) interpretation of a text mentioning Haremhab's first foray into Syria in his 16th year, but only if 'Arma'a is already pharaoh in KUB 19.15+KBo 50.24. Redford (1973: 47ff.), who links the 16th year of Haremhab's reign as pharaoh to the mention of conflict with the Egyptians in the 7th year of Mursili's annals, felt this evidence could not fit chronologically with any identification for Nibḥururiya except Aya, and this led him to suggest that the texts must be emended, and indeed, to suggest a chronology that cannot be maintained.

Schulman (1978: 44–46 and n. 8, where he mentions those who doubt the authenticity of the inscription)¹⁵⁶, however, has called Redford's analysis into question, with sound argumentation. He criticizes Redford's attempt to force, by emendation, this evidence on all the rest, and suggests what might be a more likely interpretation of the inscription. He first notes that this campaign, if dated to the 16th year of Haremhab's reign as pharaoh, would have been his only foray into Syria as pharaoh. He then points out that Haremhab counted his own reign from the death of Amenhotep III, whom he apparently considered the last legitimate ruler of the 18th Dynasty, suggesting that the 16th year of Haremhab would thus be the 16th year after the death of Amenhotep III, and hence, that it likely would fall at about the last year of Akhenaten, when Egypt may have engaged the Hittites, perhaps with Haremhab playing the role of a leading military commander. This seems not unlikely and is less abusive to the other textual evidence.¹⁵⁷ In this case, it would have no direct

¹⁵⁵ E. g. von Beckerath (1994: 107f.); cf. Wilhelm and Boese (1987: 105ff.), who adhere to the date of 1308 for the solar omen, a date which might be deemed unlikely in light of Huber's study.

¹⁵⁶ Cf. Redford's (1992: 177 n. 250) attempt to save the inscription: 'because of the faulty writing of the personal name Horemheb, (it) has been declared to be a forgery. But the text itself shows such an extraordinary authenticity that it must have been excerpted by a modern forger.'

¹⁵⁷ Cf. Murnane (1985: 40 and n. 55).

relevance for the present discussion of KUB 19.15+KBo 50.24. In light of the inscription's murky background, however, it will not be considered further here.

Résumé

Obviously this paper cannot have given a final polish to each of the many facets relating to the question of the identity of Nibḫururiya and related Amarna Age chronology. Some issues would demand lengthy articles, others perhaps a monograph, while still others will remain indeterminate until further relevant data come to light. It is hoped, however, that the important new evidence provided by KUB 19.15+KBo 50.24 has been clearly presented and that it has been placed as well as currently possible within a reasonably credible paradigm, at least as far as the present forum permits.

It is asserted in this paper that the events of cols. i and ii of KUB 19.15+KBo 50.24 are to be dated to the 7th and 9th years, respectively, of the reign of Mursili II. 'Arma'a is equated with Haremhab of Egypt in his role of viceroy and commander in Asia, i.e. before his taking the throne. If these points are granted, then Haremhab would have become pharaoh some nine years after the beginning of Mursili's reign at the earliest, a synchronism which would exclude the equation of Nibḫururiya with Tut'ankhamun, and for all practical purposes, with Smenkhkare^c.

Since the question of the identity of Nibḫururiya has long and often been debated, the paper then reviews the other salient points relevant to the discussion, arguing that they too either suggest or at least allow an identification with Akhenaten. (1) A coregency between Akhenaten and Smenkhkare^c seems to enjoy no concrete evidence. (2) While it is certain that no male heirs to the Egyptian throne were available following the death of Tut'ankhamun, it is deemed likely that the same was the case following the death of Akhenaten, so that the situation of the *taḥamunzu* episode might fit either period. (3) Though it is sometimes asserted that the writing of the name Nibḫururiya in the Hittite sources can only be equated with the allegedly consistent writing of the throne name of Tut'ankhamun, it is shown that the writing is likely not the reliable indicator that it is often assumed to be, first and foremost since the spelling as found in the DS is indeed found in at least one Amarna letter (perhaps two) addressed to Akhenaten. (4) It is suggested that the Amqu attack reflected both in the Hittite and the Amarna sources be viewed as a single, multi-pronged episode and dated to the end of the reign of Akhenaten. (5) In passing, it is found that the identification of Nibḫururiya with Akhenaten is also more likely in light of what is known about the times of death and burial of Akhenaten and Tut'ankhamun, and that (6) the *taḥamunzu*, who solicited a son from Suppiluliuma upon the death of her husband, Akhenaten, probably would have been Nefertiti, perhaps Merytaten. (7) Wilhelm and Boese's calculations on the gaps in the DS, which they find exclude an identification with either Akhenaten or Tut'ankhamun, are found to be inconclusive on methodological grounds and since they do not seem to square with other data, such as the identification of the addressee of EA 41, likely Akhenaten. (8) Finally, it is found that the scheme suggested in this paper matches well (a) the suggested 26-year reign of Suppiluliuma, (b) the dating of the discontinuation of the Mittanni correspondence and the placement of Rib-Adda's

correspondence, which point to Suppiluliuma being active in Syria already during the first years of Akhenaten, (c) as well as the reordering of the Aziru correspondence, according to which he became a Hittite vassal within a short time of his return from Egypt and the Amqu episode. It would not, however, seem to be reconcilable with (d) any likely date of the solar omen of Mursili's 10th year.

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