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43) Augeas' Stables in Elis and the Tigris – Much effort has been devoted to single out connections between Heracles' Labours and the Ninurta's mythology (but why not Marduk's one as well, since the latter descends from the former (see W. G. Lambert, *Ninurta Mythology in the Babylonian Epic of Creation*, Berliner Beiträge zu Vorderer Orient 6, Berlin 1986: 57 ff.)).

J. Van Dijk (*Lugal ud me-lám-bi nir-gál*, tome I, Brill, Leiden 1983: 17 ff.), W. Burkert (*Oriental and Greek Mythology: The Meeting of Parallels*, in: J. Bremmer ed., *Interpretations of Greek Mythology*, London 1987: 10-40; *The Orientalizing Revolution. Near Eastern Influence on Greek Culture in the Early Archaic Age*, Cambridge, MA, 1992: 119-126) and most recently M. West (*East Face of Helicon*, Oxford 1997: 465-472) listed either presumed or possible correlations of Heracles' deeds with the tallies of defeated enemies Ninurta proudly exhibited in his way home or in his temples (these tallies are extensively treated by J. S. Cooper, *The Return of Ninurta to Nippur*, Analecta Orientalia 52, Roma 1978: 141-154 and by F. A. M. Wiggermann, *Mesopotamian Protective Spirits*, Styx & PP, Gronigen 1992: 145-187). Giovanni Casadio, *Ex Oriente Lux?*, in: Ch. Riedweg ed., *Atti del simposio in occasione del 75° compleanno di Walter Burkert*, Bibliotheca Helvetica Romana (forthcoming), reconsidered all the topic of the Greek borrowing from Near East with regard to the research of Burkert, highlighting the limits of present evidence.

As a matter of facts, parallels had been almost exclusively based on the resemblance of the Sumerian names or on the alleged nature of the Ninurta's trophies, because the mythology of those beings is almost unknown. The five poems *lugal-e*, *An-gin₇-dim₂-ma*, *Anzu*, *Ninurta and the Turtle* and *Ninurta's Journey to Eridu* are the only available part of that god's mythology, that must have been much wider but unfortunately lost to us.

Surprising enough, neither van Dijk, Burkert nor West considered the cleansing of the cattle stables of Augeas. That task had to be accomplished in a single day and Heracles, by removing the walls before the fundament of the stables, diverted the course of the rivers Alpheus and Peneus and let their water run through the stables. W. Heimpel (*The Natural History of the Tigris According to the Sumerian Literary Composition LUGAL*, *Journal of Near Eastern Studies* 46 (1987): 309-17) exhaustively treated how Ninurta created mountains and succeed in building a rock way of gorges for the waters of the Kur to flow down into the Tigris. Of the surviving mythology of Ninurta this topic lends itself to a comparison with the story of the stables of Augeas: both heroes acted by means of moving rocks or stones and both were able to use for their finalities the stream of the river water. Moreover, the cattle Ninurta brought with him to Nippur in his triumph, that West takes as a possible parallel to Geryon's cattle (*ibid.*: 469), could be instead referred to the bulls and cows of Augeas. By cleansing those stables, Heracles made Elis free from the choking miasmas, with an obvious improvement of wealth by herding, a positive outcome just as the adding of the captured cattle to the Nippur ranches.

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44) Ancora sul teonimo eblaita ^dGa-na-na: alcune osservazioni comparative – Come già proposto in precedenza da chi scrive,¹ il teonimo eblaita ^dGa-na-na, che si riferisce ad una figura divina femminile legata alla sfera infera, può essere ricondotto al sem. occidentale *gmn (e varr.), “circondare; proteggere”. Da questa radice derivano, nelle lingue che l’attestano, sostantivi indicanti il “giardino”, inteso soprattutto quale luogo sacro e di sepoltura.² La dea ^dGa-na-na può, quindi, interpretarsi come la dea del giardino, colei nel cui “grembo”, simbolo di morte e rinascita, i defunti trovano protezione.

Semanticamente è possibile stabilire un confronto con il greco κήπος, che significava appunto “giardino” nel senso di “recinto protetto; grembo”, e riuniva, pertanto, in sé i concetti di protezione e fecondità.³ Il termine greco dava anche il nome ad una località situata nelle vicinanze di Atene, dedicata al culto di una ipostasi di Aphrodite, che veniva pertanto definita ἐν κήποις.⁴ Secondo la descrizione del luogo fatta da Pausania (I, 2, 3), esisteva in questa area sacra un sentiero naturale che conduceva sottoterra (κάθοδος ὑπόγαιος ἀύτομάτη) attraverso una fenditura nella roccia. Questo sentiero veniva percorso durante una cerimonia legata ad un culto misterico officiato da alcune vergini, il cui significato rimane ancora per molti aspetti controverso.⁵

Nell’Antico Testamento vengono menzionati (e condannati) i culti praticati nei giardini (*gnnt*). Talora questi giardini sono citati, come, ad esempio, in *Isaia* 65, 3-4, in evidente connessione con sepolcri ricavati dalle fenditure delle rocce.⁶ Il giardino, come ad Ebla ed Ugarit, doveva essere il luogo di sepoltura anche per i sovrani di Israele. Sappiamo, ad esempio, che nel giardino di Uzza furono sepolti il re Manasse ed il figlio di costui Amon (2 *Re* 21, 18; e 26), mentre la versione greca del libro di Neemia menziona il giardino della tomba di David (2 *Esra* 13, 16 = *Neemia* 3, 16). Così, anche il sepolcro di Gesù era scavato nella roccia e localizzato in un giardino (*Giovanni* 19, 41), tanto che Maria Maddalena nel vedere il Cristo risorto, nell’episodio della *Noli me tangere*, lo scambia all’inizio per un giardiniere o custode del giardino, come viene ben documentato anche dalla tradizione iconografica, che arriva fino al nostro Rinascimento: basti ricordare, tra gli altri, il celebre dipinto ad affresco del Beato Angelico (XV sec.) nel convento di San Marco a Firenze, in cui Gesù, stante dinanzi alla Maddalena, reca una zappa da giardiniere.⁷



Il termine “giardino sacro” (*gnt’ lym*) ritorna anche nell’iscrizione bilingue CIS II, 3902, dove identifica uno dei quattro templi di Palmira, dedicato con probabilità a divinità legate in certa misura all’oltretomba.⁸ Nella versione greca l’espressione è tradotta come ἱερὸν ἄλσος, “bosco sacro”. Vale la pena ricordare a tale proposito che il greco ἄλσος è impiegato di norma nei LXX per tradurre l’ebraico *’srh*, che indica sia il nome della paredra di YHYH, sia l’“oggetto” che nel culto la rappresentava.⁹ Le versioni latine conformemente hanno *lucus* o *nemus*. È, quindi, possibile supporre che la dea *’srh* si identificasse con il giardino sacro, luogo di sepoltura degli antenati reali.¹⁰ Se così fosse, il parallelo con l’eblaita ^dGa-na-na, la dea del giardino inteso come sepolcreto dei dinasti defunti, custode del Mausoleo di NE-na-*áski*, risulterebbe assai significativa. Ad una camera ipogeica o ad un sepolcro scavato nella roccia potrebbe, inoltre, fare riferimento la parola eblaita *a-ba-i*, che, nei seguenti passi dei testi amministrativi:

ARET I 7 (= MEE 2 22) v. VIII:5-9: 1 kin siki / ἰ-giš-sag / ^dGa-na-na / in u₄ / a-ba-i;

ARET III 256 v. I:1’-4’: [1* (giš*-)kin* siki* (tur*) / ἰ*-giš*-sag*] / ^dGa-na-na / in u₄ / a-ba-i;

ARET IV 13 v. XIII :5-9: 1 giš-kin siki tur / ἰ-giš-sag / ^dGa-na-na / in u₄ / a-ba-i;

ARET XII 239, II’:1’-4’: [1* (giš*-)kin* siki* (tur*) / ἰ*-giš*-sag*] / ^dGa-na-na / in / a-ba-i,

identifica un’occasione culturale strettamente connessa con ^dGa-na-na e che deve essere messa in relazione con le grafie emariote *a-bi-i* ed *a-bi*,¹¹ riferite ad un’installazione legata al culto dei defunti.

1. Pasquali 1998; 2005, pp. 30 ss.; 2005b, pp. 168-169; Pasquali-Mangiarotti 2005.

2. Peri 2006, pp. 111 ss.

3. Come ben illustrano Venturi Ferriolo 1989, *passim*; Venturi Ferriolo 1998, pp. 35 ss.; Serci 2004, pp. 28 ss.

4. Langlotz 1954, pp. 7 ss.; Bérard 1974, pp. 60, 67 ss., 71, 124, n. 6, 159; Venturi Ferriolo 1989, pp. 130 ss.; Perenne-Delforge 1994, pp. 48 ss.

5. Burkert 1966, pp. 1 ss.

6. Per un’attenta valutazione critica di questi passi, Venturi Ferriolo 1989, pp. 61 ss.; Ackermann 1992, pp. 186 ss.; Peri 2006, pp. 107 ss.

7. Stessa iconografia nel dipinto di Lavinia Fontana (XVI sec.), raffigurante il medesimo soggetto, conservato nella Galleria degli Uffizi di Firenze.

8. Garbini 1998, pp. 34 ss.

9. Ackermann 1992, pp. 189 ss.; Binger 1997, pp. 130 ss.; Wilson 2001, pp. 129 ss.

10. Come propone convincentemente Peri 2006, p. 112.

11. Come proposto da Pasquali-Mangiarotti 2005. La parola potrebbe ricondursi ad una radice **p(H)*, “aprire; scavare”, da cui l'accadico *aptum*, “finestra”, ma anche il foro per la nidificazione ei colombi e l'entrata della tomba, e l'ugaritico *ap*, indicante genericamente un'entrata, ma anche una camera chiusa o corte. Meno probabile, sulla base del parallelo con Emar, una connessione, pur adeguata foneticamente, del termine eblaita *a-ba-i* con l'ebraico *'bl*, “pianto, lamento funebre”.

Ackermann 1992 = Ackermann, S., *Under Every Green Tree. Popular Religion in Sixth-Century Judah*, Atlanta.

Bérard 1974 = Bérard, C., *Anodoi. Essai sur l'imagerie des passages chthoniens*, Roma.

Binger 1997 = Binger, T., Asherah. *Goddesses in Ugarit, Israel and the Old Testament*, Sheffield.

Burkert 1966 = Burkert, W., Kekropidensage und Arrephoria. Vom Initiationsritus zum Panathenäenfest, *Hermes* 94, pp. 1-25.

Garbini 1998 = Garbini, G., Gli dei fenici di Palmira, *ANLR Ser. 9 v. 9*, pp. 23-57.

Langlotz 1954 = Langlotz, E., *Aphrodite in den Gärten*, Heidelberg.

Pasquali 1998 = Pasquali, J., Su ^dGa-na-na e ^dBAD Ga-na-na-im ad Ebla, *NABU* 1998/1.

Pasquali 2005a = Pasquali, J., *Il lessico dell'artigianato nei testi di Ebla*, Firenze.

Pasquali 2005b = Pasquali, J., Remarques comparatives sur la symbolique du vêtement à Ébla, in L. Kogan et alii (edd.), *Memoriae Igor M. Diakonoff*, Babel und Bibel 2, Winona Lake, pp. 165-184.

Pasquali – Mangiarotti 2005 = Pasquali, J., Mangiarotti, P., Il rito dell'“ipogeo”(a-ba-i) ad Ebla, *NABU*, 20.

Peri 2006 = Peri, C., “Seduti nei sepolcri... mangiando carne di maiale”: operatori cultuali illeciti nel libro di Isaia, *SEL* 23, pp. 107-119.

Perenne-Delforge 1994 = Perenne-Delforge, V., *L'Aphrodite grecque: contribution à l'étude de ses cultes et de sa personnalité dans le Panthéon archaïque et classique*, Liège.

Serci 2004 = Serci, F., *Un percorso simbolico a Firenze. Il giardino Stibbert*, Firenze.

Venturi Ferriolo 1989 = Venturi Ferriolo, M., *Nel grembo della vita. Le origini dell'idea del giardino*, Milano.

Venturi Ferriolo 1998 = Venturi Ferriolo, M., *Giardino e paesaggio dei Romantici*, Milano.

Wilson 2001 = Wilson, L. S., *The Serpent Symbol in the Ancient Near East*, Lanham-New York-Oxford.

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45) The Inception and Terminal Dates of the Reigns of Seleucus II, Seleucus III and Antiochus III – It is generally believed that Seleucus II Kallinicus ascended the throne in 246 BC, ruled for 20 years and died in 226 BC. He was succeeded by his elder son, Seleucus III Keraunos, who was assassinated in 223 BC after a 3-year reign. Antiochus III the Great followed his brother on the throne and, having reigned 35 years, was murdered in Elam in 187 BC.

Our primary references on these dates are *BRM 2 (Babylonian Records in the Library of J. Pierpont Morgan. 2. Legal Documents from Erech Dated in the Seleucid Era (312-65 B.C.)*, Clay, 1913) and the *BKL (A Babylonian King List of the Hellenistic Period*, Sachs and Wiseman, 1954). These are also cited in *PD (Babylonian Chronology, 626 B.C. – A.D. 75*, Parker and Dubberstein, 1956). According to Obv. 12-13 of *BKL*, Antiochus II Theos died in month V of 66 SEB (Aug. 246 BC). Although this King List omits the accession of Seleucus II, the colophon date of Diary -245A+B confirms that he immediately succeeded his father. Given that Seleucus is attested as early as month V of 66 SEB, the earliest record from his reign, i.e. *BRM 2,17* (dated 22.III.67 SEB = 11 Jul. 245 BC) in *PD*, must now be superseded by Diary -245A+B.

As for the terminal date of the reign of Seleucus II, *PD* restore [86] SEB in Obv. 15 of *BKL* and conclude that he ruled for 20 years.

To this we may add the *UKL* (“Uruk King List”, *Texte aus dem Rēš-Heiligtum in Uruk-Warka = BaM Beiheft 2*, Van Dijk and Mayer, 1980). The incomplete text in Obv. 9 of this King List is assumed to confirm the 20-year reign of Seleucus II. However, this is simply a guess. The anticipated number 20 is only partially preserved on the IM 65066 (W 20030,105) fragment and the remaining traces are not necessarily two “Winkelhakens” only. As shown below, this year-number will have to be restored as 21.

Regarding the inception and terminal dates of the reign of Seleucus III, *PD* respectively give year 87 SEB in Rev. 1 of *BKL* (beginning 11 Apr. 225 BC), and 24.III.89 SEB (10 Jul. 223 BC) from *BRM 2, 28* (= *BibMes* 24,19).

As for the regnal dates of Antiochus III, we have 21.IX.90 SEB (21 Dec. 222 BC) in BM 30120 (*Documents juridiques de l'Assyrie et de la Chaldée*, Oppert and Ménant, 1877), and 25.III.125 SEB (3 Jul. 187 BC) in *BKL*.

In this note, I will present a brief discussion of a series of dates in several cuneiform records that enable us to improve the currently accepted inception and terminal dates of the reigns of Seleucus II, Seleucus III and Antiochus III.

The first of these are found on the obverse and reverse of *BKL*. Even a cursory glance at the regnal dates in this record reveals that the scribe employed two different methods for recording the beginning and end of the reported Seleucid reigns. He allocated Obv. 6-8 to Seleucus I (311-281 BC), placing his accession in year 7 SEB and equating it with year 1 of Seleucus' kingship. He then gave, in Obv. 7 and 8 respectively, the

reign-length and assassination date of Seleucus I as 25 years and VI.31 SEB (26 Aug. – 24 Sep. 281 BC).

Omitting regnal year 1 of Antiochus I (281-261 BC), the scribe began in Obv. 9-10 of *BKL* with Antiochus' 2nd regnal year in 32 SEB, giving him a 20-year reign and reporting that he died on 16.II.51 SEB (2 Jun. 261 BC). Similarly, in Obv. 11-13 the scribe started with 52 SEB, i.e. regnal year 2 of Antiochus II (261-246 BC), son of Antiochus I, confirmed that he reigned for 15 years and that the news of his death reached Babylon in V.66 SEB (31 Jul. – 29 Aug. 246 BC). This last date can, however, be slightly improved. According to an incomplete statement from month V in Rev. 5 of Diary -245A, the Babylonian scribe received the news of Antiochus' death on 20.V.66 SEB (19 Aug. 246 BC) and thus recorded the change of reign to Seleucus II. Assuming that Antiochus II died in Ephesus in Asia Minor and that it took no more than a month for the news of his death to arrive at Babylon, it is possible that he actually died in the last 10-15 days of July 246 BC.

The colophon of Diary -245A+B indicates that Seleucus II succeeded his father before the end of month V of 66 SEB: Diary from month I to month VI, Antiochus (was) King, from month V to month VI, Seleucus, his son (is) King. Yet the scribe of *BKL* omitted Seleucus' regnal year 1 and instead began his reign in Obv. 14 with 67 SEB, that is, the 2nd year of Seleucus II.

It is difficult to explain why the scribe of *BKL* started off the reigns of Antiochus I, Antiochus II and Seleucus II with their respective regnal year 2 (designated fictitious "year 1" hereafter). We know that the practice of noting accession year had been abandoned immediately after the death of Alexander III of Macedon. According to Diary -322A, Alexander died on 29 Ayyāru in his 14th regnal year (11 Jun. 323 BC). The Lunar Text *ADRTB* 5, 36 (*Astronomical and Related Texts from Babylon*, Hunger, 2001) confirms that Phillip III Arrhidaeus immediately followed Alexander on 1 Šimānu "which was identical with the 30th of the previous month". Abandoning Philip's accession year, this date marked both his first and Alexander's 14th regnal year in Babylonia. It is, therefore, highly unlikely that the scribe of *BKL* took the accession years of Antiochus I, Antiochus II and Seleucus II as their "year 0" and began each regnal year 1 on 1 Nīsānu of the succeeding year. However, whatever the scribe's original intention might have been, his preserved reign-lengths on *BKL* are, with the exception of that of Seleucus I, quite close to the actual values: Seleucus I, Antiochus I, Antiochus II, Antiochus III, Seleucus IV, and Antiochus IV respectively reigned 24 years and *c.* 6 months, 19 years and 8-9 months, 15 years and 2.5 months, 35 years and *c.* 2 months, 12 years and *c.* 2 months, and 11 years and *c.* 3 months. *BKL* gives them 25, 20, 15, 35, 12 and 11 years respectively. Except for 25, the latter five figures would have been obtained by counting from 1 Nīsānu of the year following each accession on the obverse of *BKL* and subtracting the inception from the terminal date of each reign on its reverse.

Unfortunately, save for a few unintelligible traces of some signs, the final line of *BKL* is completely lost. However, since its Rev. 1 begins with the reign of Seleucus III, it is safe to assume that the missing line at the end of obverse recorded the death of Seleucus II including, at least, the corresponding year-date.

Turning to the reverse of *BKL*, there are, fortunately, enough traces left in line 1 to show that the king following Seleucus II was his eldest son Seleucus III. However, contrary to the obverse which consistently starts each new reign one year later than the death of the previous monarch, thus skipping the first regnal year as already noted above, the reverse of *BKL* clearly begins with the accession year of each king.

We may never know why the scribe of *BKL* employed two anomalous dating methods for noting the reigns of Seleucid rulers. We could speculate that with both the death-dates and 2nd regnal years present on the obverse, the subsequent users of *BKL* could have easily decided each accession as the year preceding the fictitious "year 1". However, since the death of Seleucus II occupied the last line of the obverse of *BKL*, the scribe had to either repeat it on the first line of the reverse to enable the future users to determine the accession date of Seleucus III, or simply begin the reign of the latter with his true accession year. Since the scribe indeed started off the reverse of *BKL* with the accession of Seleucus III, he had to, for the sake of consistency and to avoid confusion, treat the subsequent reigns similarly. Unfortunately, the loss of the last line or lines of the obverse of *BKL* prevents us from knowing whether the scribe actually alluded to this obvious chronographic change. We should, nevertheless, note that more often than not the first and last lines of Babylonian clay tablets were most likely to be knocked off in shelf storage before the rest. Therefore, by noting the accession date of Seleucus III on the first line of the reverse, the scribe of *BKL* ensured that even if the regnal dates in the last line of the obverse were subsequently destroyed, it would still be possible to work out the death-year of Seleucus II from the accession date of his son, Seleucus III. This hypothesis is supported by the statement *AŠ.TE TUŠ-ab*, "acceded", which is conspicuously absent on the obverse but appears three times on the reverse of *BKL*, heralding the accession of Antiochus III, Seleucus IV and Antiochus IV (there would, most probably, have been more cases had the reverse text of *BKL* remained undamaged). For example, in Rev. 2 of *BKL* we read: [Year] 90, King Antiochus (III) as[cended] the throne. Naturally, 90 SEB (beginning 9 Apr. 222 BC) was the accession year of Antiochus III, since, as confirmed below, Seleucus III died in that same year and left the crown to his younger brother. We may, therefore, confidently regard the incomplete date [60+]27 in Rev. 1 of *BKL* as the death-year of Seleucus II as well as the accession of Seleucus III in 87 SEB.

We then have, in Rev. 3 of *BKL*, the 35-year reign of Antiochus III and in Rev. 6-7 his death in Elam on 25.III.125 SEB (3 Jul. 187 BC). This is followed by the accession in the same year of Seleucus IV (187-175 BC), his 12 year reign-length, and death-date 10.VI.137 SEB (3 Sep. 175 BC) in Rev. 8-9, and those of Antiochus IV

(175-164 BC) in Rev. 10 and 14. The latter died probably in Media and, according to Rev. 14 of *BKL*, the news of his death reached Babylon in month IX of 148 SEB (20 Nov. – 18 Dec. 164 BC). As can be deduced from these dates too, the partial date 87 SEB in Rev. 1 of *BKL* is indeed the accession year of Seleucus III and thus his regnal year 1.

Taking 67 and 87 SEB as regnal year 2 and terminal year of Seleucus II respectively, it can be seen that he had 22 regnal years although his actual reign was a few months shorter (cf. below). This agrees with both Eusebius and Syncellus who give Seleucus II a 21-year reign. However, using the information in Rev. 8-9 of BM 32171 “The Seleucid Accessions Chronicle” (*BCHP* 10), whose preliminary re-edition by R. J. van der Spek and I. Finkel is available at <http://www.livius.org>, the end of the reign of Seleucus II may be refined. It is clear that the reference to “Seleucus son of Seleucus” in Rev. 8 of *BCHP* 10 concerns Seleucus III. According to Rev. 9 of this same Chronicle, upon the death of his father, Seleucus II (registered in Rev. 7 of *BCHP* 10), Seleucus III ascended the throne before month X (Tebētu) : [... *ina* ^{giš}GU.ZA TU]Š-*ab* ITU.AB [...]. Combined with the text in Rev. 1 of *BKL* and provided that the news of the death of Seleucus II did not take more than a month to reach Babylon, Rev. 7 of *BCHP* 10 strongly suggests that he died in month IX (Kislīmu) of 87 SEB (Dec. 225 BC). Naturally, this same date may be taken as the inception of Seleucus III.

As for the terminal date of the reign of Seleucus III, the colophon date of *BRM* 2, 28 (24.III.89 SEB) places it after 10 July 223 BC. However, the unpublished BM 116690 cuneiform tablet from Uruk, a contract “with payment in silver staters of Seleucus”, extends, for the first time, the reign of Seleucus III into year 90 SEB and confirms that he died in 222 BC. It registers:

7: UNUG.KI ITU.BÁR

8: [UD-x-KÁ]M MU-1,30-KÁM ^ISe-lu-ku LUGAL

I should add that on 25.1.2006, C. B. F. Walker of the British Museum kindly re-collated the above date-formula. He confirmed that at the beginning of Rev. 8, where the edge is broken off, given the position of the right edge a little further up, one would conclude that there is very little room for [UDKÁ]M. However, although the possibility for 10 as an alternative to 1 or 2 cannot be excluded, the existing small gap strongly indicates that the missing day-number at the beginning of Rev. 8 was probably very small. This suggests a compilation date in the period 9-18 Apr. 222 BC.

Yet, except for the fact that Seleucus III was acknowledged in Babylon in month Nisānu of 90 SEB (9 Apr. – 7 May 222 BC), the above date-formula fails to determine the terminal date of his reign. Equally, the previously available dated texts from the reign of Antiochus III are of little help, since the earliest is the BM 30120 tablet, dated 21.IX.90 SEB (21 Dec. 222 BC). Given the new date-formula in BM 116690, the date of BM 30120 still leaves an 8-month gap into which the end of the reign of Seleucus III and accession of Antiochus III could be placed.

However, an important incomplete date-formula in a recently edited Goal-Year Text (*GYT*) narrows down the interval between the death of Seleucus III and accession of Antiochus III considerably and thus vastly improves our knowledge of the regnal years of these two rulers. The relevant date is found in Obv. 1, (1st Saturn Paragraph) of the BM 45661 + BM 4610 fragments, published in *ADRTB* 6 (Hunger, 2006) as *GYT* 41 for 149-151 SEB. It provides the earliest attestation of Antiochus III as king in Babylon. However, although the year number in Obv. 1 is lost, the date formula in Obv. 5 (2nd Saturn Paragraph) gives: MU-1,31-KÁM ^IAn LUGAL. It thus enables us accurately to restore the date in Obv. 1 as: [MU-1,30-KÁM] ^IAn LUGAL GU₄ *ina* 22.... In other words, we now know that Antiochus III was recognised in Babylon as early as c. 22 Ayyāru 90 SEB (around 29 May 222 BC). It may, nevertheless, be objected that because the scribe of *GYT* 41 extracted the required planetary and lunar data from earlier Diaries, he may have mistakenly included the personal name Antiochus rather than Seleucus in the corresponding date-formula. Also, the fact that Antiochus IV was on the throne in 149 SEB, the compilation date of *GYT* 41, may have induced a scribal error. Luckily, however, in spite of a general dearth of material from Seleucid Babylonia wherein a change of reign occurred, we have just enough evidence to show that the scribes of *GYT*s took due care to identify the rulers correctly.

As explained above, Antiochus III was killed in Elam on 25.III.125 SEB (3 Jul. 187 BC). His death, followed by the accession of his elder son, Seleucus IV, is recorded in Rev. 6-8 of *BKL* as:

6: Year 125, (month) III, it was heard in Babylon

7: as follows : on the 25th day, King An(tiochus III) was killed in the land of Elam

8: The same year, Se(leucus IV), his son, ascended the throne. He reigned 12 years.

It should be noted that Seleucus IV was already on the throne by 11.IV.125 SEB (19 Jul. 187 BC), the date of *BRM* 1, 88.

According to Obv. 1 of Diary No. -188, the co-regency of Antiochus III and Seleucus IV is attested as early as 1.I.123 SEB (3 Apr. 189 BC). Later cuneiform documents show that the two monarchs continued to rule jointly until the death of Antiochus III. Currently, the latest date-formula mentioning the two Seleucid kings is 25.I.125 SEB (6 May 187 BC). This is found in Obv. 14 at the beginning of Mercury Paragraph in *GYT* 53. It

clearly demonstrates the insertion in the corresponding date-formula of the correct royal names and also confirms that the scribe did not confuse Antiochus III and Seleucus IV with the contemporary ruling monarch (either Demetrius II (145-138 BC, 1st reign) or Mithradates I (165-132 BC) of Parthia) under whom he compiled *GYT* 53 in 171 SEB.

A similar accuracy prevails in three further date-formulas in *GYT*s from 144 SEB and 201-202 SEB. The first is *GYT* 39 whose first Moon Paragraph begins in Rev. 1 with: [Year] 125, Se(leucus IV) (is) King, covering months VII–XII of the same year and correctly excluding Antiochus III who had died some three months earlier.

The second is in *GYT* 40. Unfortunately, the year number in Rev. 1 at the beginning of Col. I in this fragment, covering, once again, months VII–XII of 125 SEB, is broken off (months X and XII are incompletely and month XI is fully preserved in Rev. 9, 11, and 10 respectively in the same Col.). However, the partial and also complete dates in lines 14 of Col. I and 1 of Col. II (the latter reading: Year 126, Se(leucus IV) (is) King), enable us to reconstruct the lost year number in line 1 of Col. I in *GYT* 40 as [Year 125], Se(leucus IV) (is) King, again unaccompanied by Antiochus III.

The third date-formula is in Rev. 16 at the beginning of Saturn Paragraph of *GYT* 73 for 201-202 SEB, compiled under the Parthian ruler Mithradates II (121-91 BC). It reads: Year 142, Kings An(tiochus IV) and An(tiochus). (Month) I[I ...]. Given that Rev. 12 of *BKL* dates the execution of co-ruler Antiochus, on orders from his father Antiochus IV, to V.142 SEB, the scribe of *GYT* 73 correctly subscribed the corresponding date-formula to both kings in II.142 SEB and not to Antiochus IV alone.

The apparent consistency and accuracy of the above date-formulas in *GYT*s 39, 40, and 73 strongly indicate that the one in *GYT* 41, acknowledging Antiochus III in II.90 SEB, is, most probably, correct. This confirms that Seleucus III was not recognised at Babylon in Ayyāru 90 SEB and that his younger brother, Antiochus III, had taken over in that same month. Accordingly, I.II.90 SEB (8 May 222 BC) may now be taken as the *terminus ante quem* of the reign of Seleucus III and the *terminus post quem* of the kingship of Antiochus III. Hopefully, future discoveries will further enhance the regnal dates of these two monarchs.

It is now possible to correlate the contemporary and near contemporary Babylonian cuneiform evidence with references in the extant classical literature to decide the inception, duration and terminal dates of the reigns of Seleucus II and his sons Seleucus III and Antiochus III:

Seleucus II

- 1: Accession (regnal year 1) = immediately after 20.V.66 SEB (19 Aug. 246 BC). Eusebius gives Ol. 133.3 (246/245 BC).
- 2: Regnal year 2 = 67 SEB (22 Apr. 245 BC – 10 Apr. 244 BC) = fictitious “year 1” in *BKL*.
- 3: Terminal date = IX.87 SEB (4 Dec. 225 BC – 1 Jan. 224 BC). Eusebius gives Ol. 138.2 (227/226 BC).
- 4: Number of regnal years = 22.
- 5: Duration of reign = 21 years and roughly 4.5 months. Both Eusebius and Syncellus report that Seleucus II reigned for 21 years (rounded down reign-length).

Seleucus III

- 1: Accession (regnal year 1) = terminal date of Seleucus II.
- 2: Regnal year 2 = 88 SEB (31 Mar. 224 BC – 18 Apr. 223 BC).
- 3: Terminal date = sometime in I.90 SEB (9 Apr. – 7 May 222 BC). Eusebius gives about Ol. 139.1 (224/223 BC).
- 4: Number of regnal years = 4.
- 5: Duration of reign = 2 years and 3-5 months. Both Eusebius and Syncellus register a 3-year reign for Seleucus III (rounded up reign-length).

Antiochus III

- 1: Accession (regnal year 1) = shortly before I.II.90 SEB (8 May 222 BC). Eusebius gives Ol. 139.2 (223/222 BC).
- 2: Regnal year 2 = 91 SEB (28 Mar. 221 BC – 15 Apr. 220 BC).
- 3: Terminal date = 25.III.125 SEB (3 Jul. 187 BC). Eusebius gives Ol. 148.2 (187/186 BC).
- 4: Number of regnal years = 36.
- 5: Duration of reign = 35 years and approximately 2 months. Both Eusebius and Syncellus give 36 years (rounded up reign-length).

It should be pointed out that Polybius (4.47.6-7) implicitly reports that Seleucus III reigned for about 2 years and Appian (*Syr.* 66) gives him a 2-year reign. These figures agree well with the Babylonian evidence. In spite of having 4 regnal years, Seleucus III actually reigned approximately 3 months in year 87 SEB, 2 full years covering 88 and 89 SEB, and less than 1 month in 90 SEB. These add up to 2 years and roughly 4 months, far short of the 3 full years quoted by Eusebius and Syncellus. It is, therefore, clear that Eusebius and Syncellus rounded up the duration of the reign of Seleucus III, as they did that of Antiochus III (giving 36 years rather than 35 years and c. 2 months) while Polybius and Appian respectively reported the approximate and rounded down reign-length of Seleucus III. In short, the following regnal dates may be proposed for the above-named

three Seleucid rulers:

Seleucus II (246 – 225 BC)

Seleucus III (225 – 222 BC)

Antiochus III (222 – 187 BC)

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46) Notes de lecture : la Malédiction d'Agadé – Ayant été amené, dans le cadre de mon enseignement, à retravailler la *Malédiction d'Agadé*, je livre dans cette note les principales modifications à la traduction que j'ai publiée voilà plus de vingt ans (RA 78 [1984] 99-121). Même si je ne le précise normalement pas dans les pages qui suivent, j'aimerais relever ici que je suis revenu plus d'une fois à l'interprétation de J.S. Cooper (*The Curse of Agade*, 1983 ; désormais Cooper, CA), que j'avais rejetée à tort.

L.1. « tel le taureau céleste ». **L. 8.** « immense "gynécée" ». **L. 10.** « à neuf » (gibil₍₄₎-bi). **L. 17.** « pour faire affluer les gens dans les places de fête » soulève deux problèmes : 1) SIG₇ (B pl. de til₃, probabl. še_x ou si₁₂) n'a pas de finale en -g (cf. le fréquent sila-a SIG₇-a à Ur III)¹. 2) ki izim-ma est une relation génitive (IS 3:39 ; ITT 3, 5280 i 10 ; NATN 695:3 ; nom de champ dans AnOr. 1, 309:9 ; MVN 14, 514 rev. 7 ; NATU 1, 32 ii 11 et iii 1 ; Gomi, *Orient* 21, 1 sq., BM 105334 ii 12 et iii 1 ; Langdon, RA 19, 192 n° 3:5 ; SACT 2, 130:2 ; TENS 226:3²). La seule traduction respectant la grammaire est « pour faire verdoyer les places de fête, (tout) le pays », mais elle n'est sémantiquement pas très convaincante. **L. 18.** « pour que les connaissances mangent en commun » (interprétation traditionnelle) ; lu₂ zu-k pourrait être un génitif sans régent (« celui des connaissances » = « celui qui a des connaissances »). **L. 19.** « tels des oiseaux rares » (mušen nu-zu-gen₇) ; comp. M. Civil, *AulOr.-S* 22 (2006) 126 sq. B₂:6' et comm. p. 128. **L. 21.** « immenses aurochs cornus » ou « immenses éléphants ». **L. 23.** « à la longue laine » (interprétation traditionnelle) ; contrairement à ce que j'affirmais dans *ELS* 203, les participes intrans. en -Ø sont loin d'être rares. **L. 24.** « ne dormait pas ». **L. 28.** « Ses piles de grain, c'est dehors qu'elles furent "enduites d'argile" » (comp. T. Jacobsen, *The Harps* 362 et n. 6) ; sur kuru₁₃-a im ur₃, v. en dernier lieu C. Huber, *Mél. Cagni* (2000) 463-465. Le sens est que les greniers étant remplis de pierres et de métaux précieux, l'orge doit être stockée dehors, sous une forme qui n'est pas très claire. Qu'elle ait été directement couverte d'argile est toutefois invraisemblable (Huber, *op. cit.* 489). **L. 39.** « Son peuple vit des lieux plaisants » (variante principale) // « connut les cérémonies funéraires » (x 3) ; sur ki sa₆-ga (// ki si₃-ga), v. en dernier lieu M.-C. Ludwig, *Santag* 2 (1990) 209 sq. ; A. Zgoll, *AOAT* 246 (1997) 374 ; M. Jaques, *AOAT* 332 (2006) 382 et n. 822. **L. 45.** « Les propres possessions de Sumer (= les gens de Sumer eux-mêmes ?) halaient des bateaux (vers Agadé) » (avec ni₂-ba-ke₄) // « Avec les propres possessions de Sumer, des bateaux étaient halés (vers A.) » (avec ni₂-ba-ta) // « A Sumer, on halait de son propre chef des bateaux (vers A.) » (avec ni₂-bi-ta). Je ne crois plus guère aujourd'hui que im-da-gid₂-de₃ puisse être une forme causative (ainsi dans RA 78, 100 et 110), sans toutefois pouvoir l'exclure absolument. **L. 51.** « Tous les gouverneurs ». **L. 53.** « préparaient selon les règles (pour Agadé) les offrandes mensuelles et le nouvel an » ; « les offrandes du début du mois » (comp. P. Attinger/M. Krebernik, *AOAT* 325 [2005] 64) laisserait -bi (au lieu de -ba(-ke₄)) inexpliqué. Au cas où ni₂daba iti-da za₃-mu-bi signifierait, malgré les problèmes grammaticaux soulevés, « les offrandes mensuelles et du nouvel an » (ainsi tout le monde), traduire alors si am₃-sa₂-e-ne par « apportaient directement là (à Agadé) ». **L. 54.** « comme la lassitude s'est appesantie ! » **L. 56.** « De même que les gens du pays, elle ne s'était pas (rassasiée =) dégoûtée du temple bien fondé qu'elle ordonnait (de construire) » // « qu'on lui construisait » (B et J₂). **Ll. 58 sq.** « Ayant été chargée de faire trembler Agadé³, elle jeta l'effroi à Ulmaš » ; mon ancienne traduction « Il (Enlil) l'avait chargée (...) » (p. 101) supposerait en sumérien mu-ni(-in)-la₂-la₂. **Ll. 61-65.** Cf. C. Wilcke, *Politik im Spiegel der Literatur, Literatur als Mittel der Politik im älteren Babylonien*, dans : K. Raaflaub (ed.), *Anfänge politischen Denkens in der Antike* (1993) 29-75 (désormais Wilcke, *Politik*) pp. 33 sq. et n. 24. **L. 61.** « Telle une jeune femme abandonnant ((cela) de son "gynécée" =) ce qu'elle a dans son "gynécée" » ; ame₂-na est un génitif sans régent (Wilcke, *Politik* 33 n. 24). **L. 63.** « Tel un guerrier s'avançant contre des (forces) armées ». **L. 64.** Plus précis « elle sortit de la ville batailles et combats ». **L. 65.** Dans l'acception « aller à la rencontre de », gaba ri est le plus souvent une construction intrans. (C. Wilcke, *ZA* 78 [1988] 8 n. 36 et *Politik* 33 n. 24 ; M. Jaques, *AOAT* 332 [2006] 68 et n. 146)⁴, trans. seulement dans *SgLeg.* B 43 et SP 2.110 (4 x a₂-sag₃-e // 4 x a₂-sag₃) ; cela corrobore mon interprétation « et les remit à des hommes ennemis » (littéral. « fit aller à la rencontre ») dans RA 78, 101 et comm. p. 112 (de même Wilcke, *Politik* 33 n. 24 et J. Black et al., *LAS* 120). **L. 67.** D'après M. Such-Gutiérrez (*MVS* 9/I [2003] 146 et n. 597), sa nam-en-na désignerait le « Netz des Herrschertums »⁵. En français, le « nerf (l'un des sens de sa) de la royauté » serait possible, mais je doute que cette métaphore ait existé en sumérien. **L. 73.** Pour une interprétation très différente (qui n'emporte pas ma conviction), cf. P. Steinkeller, *Mém. Moran* (2005) 19 sq. n. 19 : « its [...] aura, that reached the upper sky, An brought down to the nether sky ». **L. 74.** Cf. M. Civil, *Mél. Sjöberg* (1989) 57 (aucun texte n'a MA₂.GAG, dar(-gag) pourrait signifier « "anchoring peg" in a loom and "mooring pole" in a quay ») et G. Rubio, *ASJ* 22 (2000 ; paru en 2005) 206 n. 11 ((gēš)MA₂-, pas DAR, dans Y₂ et

un texte non publié). Sens : plus probabl. « poteaux d'amarrage » qu'« étais ». **Ll. 77-82.** Je me rallie maintenant à l'interprétation de C. Wilcke (citée dans RA 78, 112 sq.) : « La vie du sanctuaire Agadé arriva à terme, telle celle d'une petite carpe *suḫur* dans les eaux profondes – les villes toutes ensemble avaient les yeux fixés sur (ce spectacle). Tel un immense aurochs cornu⁶, (Agadé) fut terrassée – elles (les villes) levèrent ensemble les cornes tel un immense taureau. Elle laissa tomber la tête (sur le sol) tel un dragon mourant – comme dans une bataille⁷, elles s'emparent ensemble de ce qui faisait son importance ». **L. 87.** « il le comprit dans son for intérieur » (littéral « en ce qui concerne son cœur, il l'apprit »). **L. 88.** Ajouter G mu e₂-kur-ra [...]; T₂ ba-an-mu₂. **L. 90.** « cabine » (sumur₃); cf. en dernier lieu B. Alster, *Mél. Klein* (2005) 15 : « a cover of reeds or similar protecting the center of a boat against the sun »; traduit par « cabin » (13 iv 15). **Ll. 95 // 97.** Lire nu-mu-un-ne-ḡal₂, avec -ne- = {n(n)a + e/j} (faux dans RA 78, 114); traduire (littéral.) « le temple à construire (// construit) n'était pas pour lui dans le "chevreau" » = « l'oracle ne lui répondit pas de construire le temple ». Avec -ni-in-(P, Y₂ et probabl. S₁), le sujet doit être Enlil (« il ne mit pas le temple à construire dans le "chevreau" »). **L. 98.** « what had been entrusted to him » (T. Jacobsen, *The Harps* 365) ou « what had been inflicted(?) upon him » (J. Black *et al.*, *LAS* 120) demanderait dans le premier cas mu-na-ab-si₃-ga, dans le second mu-ni-ib₍₂₎-si₃-ga. **L. 99.** Vu ba-e₃-de₃ en Y₂, je reviens aujourd'hui à la lecture ba-en-de₃-kur₂/gur de Cooper (CA pp. 54 et 133), même si le sens reste difficile⁸. Avec kur₂, traduire peut-être « Ce qu'avait dit Enlil lui devint étranger »⁹ (/ba/ moyen-passif ?). Avec gur, et pour autant que ce ne soit pas une graphie non-standard de kur₂, on pourrait penser à l'acception fréquente « s'écarter de » (normalement -/da/- dans le SV), littéral « Ce qu'avait dit Enlil se tourna ailleurs ({e/j}) loin de lui ({n + da}) ». Dans les deux cas, l'idée serait que le contact entre Narām-Sîn et Enlil a été coupé. **L. 101.** zi gu₂ ḡar est la leçon principale. Comme il est probabl. attesté aussi dans *Iddin-Dagan A* 136 (uḡ₃ saḡ-gegge-ga zi gu₂ mu-na-a[n-...] [A] // 'zi¹ gu₂ ... [...] [F] // zi x¹⁰ 'mu¹¹-un-na-an-ḡar [B]), il n'est pas une simple variante de zi-ga ḡar « faire une levée », contaminé par ou faisant jeu de mots sur gu₂ ḡar « rassembler » de la ligne précédente (ainsi Cooper, CA 244 sq. et Attinger, RA 78, 114). En ce qui concerne zi gu₃ ḡar (R₃ [Ur III]), on le trouve encore dans *Lugalb. I* 24 B // 'zi-ga¹ [...] (II) (si gu₃ ra à la l. 25) et *Giḫ A* 48 sq. KiB¹¹; une contamination de zi gu₂ ḡar par si gu₃ ra « jouer du cor » semble très vraisemblable (v. déjà RA 78, 114 avec litt. ant.); comp. par ex. *Giḫ B* 51 sq. (zi-ga ḡar suivi de si gu₃ ra) et *GiM M* 239 sq. (zi-ga du₈ suivi de si gu₃ ra //). Le sens exact de zi gu₂/gu₃ ḡar reste mal assuré; « mobiliser » v.s. semble à première vue évident, mais va mal dans *Iddin-Dagan A* 136. **L. 103.** Pour le difficile šu-keše₂ AK, v. en dernier lieu Attinger, ZA 95 (2005) 253. **L. 105.** « Il traita le geguna comme une quantité négligeable ». Sur uš₃ giḡ₄ AK, v. en dernier lieu Attinger, ZA 95, 258; dans cette expression, giḡ₄ désignait probabl. à l'origine une mesure de poids (cf. *GiEN* 137). Le rapport entre les ll. 104 et 105 m'échappe. **L. 106.** Lire ninta dans tous les duplicats. **L. 114.** Littéral « Il aiguisa les haches *agasilig* de leurs deux côtés » = « Il aiguisa les deux tranchants des haches a. »; pour l'ablatif, comp. l'allemand « etwas von beiden Seiten schleifen ». **L. 116.** Les deux sens les mieux attestés de ki la₂ sont « excaver » (M. Civil, *AOAT* 25 [1976] 94 sq.) et « s'incliner » (par ex. *Išme-Dagan B* 16 et *LN* 310 [comm. de S. Tinney p. 182]). Ici, on peut hésiter entre « elle (la base du temple) s'enfonça jusque sur les fondements du pays » (semble le plus naturel, mais ki la₂ « s'enfoncer » n'est sinon pas attesté), « elle fut excavée jusqu'aux fondements du pays » et « les fondements du pays furent excavés » (ainsi Civil, *loc. cit.*). **L. 122.** Littéral « il arracha (tout) à ses/aux huisseries », c'est-à-dire qu'il dépouilla les huisseries de leurs ornements. La traduction usuelle par « il arracha ses huisseries » (v.s.) laisse inexplicé le loc.-term. dans au moins 4 duplicats. **L. 124.** ba-da-an-ku₅ est soit une 3^e sg. *ḫ.* trans. (« il détourna »), soit une forme verbale passive (/n/ allomorphe de {ni}). **L. 130.** Lire dub-šen « coffres du trésor » (J. Bauer, *AoN* 29 [1985] et M. Civil, *AulOr.* 5 [1987] 20). **Ll. 131-133.** Sur ce passage difficile, cf. W. Heimpel, *AOAT* 253 (1998) 133 sq.; traduire « Quoi qu'ils ne fussent pas des sacrilèges, ses *laḫama*, grands redans flanquant le temple, Narām-Sîn les fit éclater¹² au milieu du feu ». Pour saḡ₃, littéral « frapper », on peut hésiter entre « faire éclater » v.s. (je ne connais toutefois pas de passage parallèle) et « abattre (un arbre) » (cf. par ex. *Giḫ A* 46, 57 et 64 et *Giḫ B* 62); l'idée serait que NS fait un grand feu et y « abat » les *laḫama*. **L. 135.** -eš dans 7 duplicats¹³ sur 8 plaide pour un adverbialif (« à la manière de/comme ses [de l'Ekur] bois du *geguna* »), mais on voit mal quel pourrait être le sens. **Ll. 143 sq.** kar-ra dans 6 duplicats fait difficulté, us₂ n'étant normalement pas construit avec le loc., mais avec le loc.-term. (avec kar, cf. par ex. *Gudea, Cyl. A* 4:4 et *St. D* 3:6 sq.; ŠX 4; *IE II* vi 47¹⁴; *Ḥamm.* 12 : 19 sq.); malgré kar-re en B, traduire probabl. « les grands bateaux des quais » = les grands bateaux stationnés le long des quais (suggestion de J. Neeser, Berne). **L. 155.** « peuple qui ne connaît pas d'attaches ». Remarquer que W. Horowitz traduit, dans un contexte comparable, *ul 'i-di ki¹-iṣ-ru-tum* (MC 8 [1998] 72:52 B [*The Sargon Geography*]) par « who do not know construction » (p. 73; v. comm. p. 91); ce sens de *kiṣrūtum* (autant que je sache un *hapax*) est possible, mais de loin pas assuré. **L. 158.** « tels des moineaux » (*buru₅mušen-gen₇*). – La traduction de ŠID.ŠID par « innombrable » est usuelle, mais difficilement justifiable philologiquement. En contexte, *še/id* signifie normalement « compter », pas « être nombreux ». *še/id* « nombre » (= *minūtu*) est bien attesté, mais il n'est pas redoublé (on attendrait par ailleurs ici un locatif). A. Cavigneaux/F. al-Rawi (ZA 92 [2002] 49) traduisent « comme des moineaux dans leurs ébats il[s] frôlent la terre »; pour un essai de justification philologique, cf. Cavigneaux, *WO* 25 (1994) 143 : il renvoie à ŠID = *rakābu* dans Proto-Izi I 256, qu'il rapproche de /še/id/ (KAD₃/KID₂) = *rakābu* et de /šeššed/ (KID₂/KID₂) = *riikub iṣšūri* « se chevaucher (en parlant) d'oiseaux » (sens sexuel). Indépendamment du fait que ŠID = *rakābu*

reste une équation isolée, dont ni la lecture ni l'acception exacte ne sont connues, l'ordre des mots ferait difficulté (attendu buru₅mušēn šed-šed-gen₇ « comme des moineaux s'ébattant ». L. 159. Traduire plutôt comme Cooper, CA p. 59 (/n/ locatif). Ll. 162 sq. « Le messager n'allait plus sur le chemin, le bateau du courrier ne passait plus sur le fleuve » (ainsi pratiquement tout le monde ; -dib₍₂₎-be₂ = {DIB₍₂₎ + ed}); mon interprétation dans RA 78, 104 (« Ils ne laissaient pas aller le messager sur le chemin », etc.) rend inexplicable l'absence du loc. {ni}¹⁵. L. 166. La traduction proposée dans RA 78 (104 et comm. p. 117) est grammaticalement et lexicalement possible, mais ne donne guère de sens dans ce contexte ; préférable soit « La garde était montée sur les arbres (des =) le long des rives »¹⁶ (ainsi S.N. Kramer, ANET 3 [1969] 649), soit, malgré mes réticences dans RA 78, 117 n. 81, « Sur les rives boisées, la garde était montée » (comp. J.-M. Durand, AEPHE 1974/75, 170 et n. 1). L. 174. « ni sirop ni boissons alcoolisées ». L. 175. Lire im u₄ gid₂-da et traduire « Durant de longs jours, il ne tomba plus de pluie (...) » (ainsi T. Jacobsen, The Harps 369) ; pour u₄ gid₂-da, cf. par ex. Innana D 63 ; Lugalb. I 405 et 407 ; Lugalb. II 225b // 246. L. 180. ir₉-[gen₇] dans le nouveau duplicat d'Ur III (B. Alster, ASJ 15 [1993] 1-3, NBC 11119) confirme l'interprétation de Cooper (CA 58 sq. et 180). L. 183. Cf. D.O. Edzard, OBO 160/4 (2004) 566 n. 305 ; littéral « Les gens : sous l'effet de la faim, ils se martelaient (šu im-BALAG.BALAG-ne) (eux-mêmes =) le corps »¹⁷. Ll. 188 sq. Même topos dans LSU 80α ; cf. en dernier lieu J. Black, Mél. Wilcke (2003) 38 et G. Farber, Mél. Klein (2005) 114. Traduire peut-être « Les dents furent amoncelées, les corps¹⁸ jetés (pêle-mêle) (ku-ku // ku₄-ku₄), les ... furent amoncelées, les corps¹⁸ semés comme de la graine ». La séquence dub-dub (188) / dub (189) pourrait plaider pour une opposition objet pluriel vs objet singulier (« dents » vs « nez », v.s.)¹⁹. L. 200. « ils (collectif) posèrent sur le sol sept balaḡ comme le fondement du ciel repose (sur la terre) ». L. 201. /n/ est certainement l'allomorphe du loc. {ni} ; traduire « Pour lui (Enlil), les tambours ub, mezi et lilis retentirent au milieu d'eux tel Iškur ». L. 209. itima étant sûr dans ELA 390 (contra Attinger, RA 78, 119 n. 90), mes réticences n'étaient probabl. pas de mise. L'ergatif ^den-lil₂-le dans K et l'absence du loc. dans au moins 2 duplicats (Z et E₁) restent malgré tout un peu troublants. Ll. 212 sq. « traiter » n'est pas une acception usuelle de du₃ ; mieux « être bloqué » = « être réduit à l'impuissance » ? Comp. du₃ au lieu de šu la₂ dans 213 S et peut-être K. L. 214. Littéral « Dans cette ville, puissent les corps/gens y être remplis dans ses (de la ville) puits ! » L. 215. (à propos de mon comm. p. 119) : lire probabl. lu₂-lu₂-u₃ (ergatif) zu-ne ; comp. Lugalb. I 33 : lu₂-lu₇ zu-ne na-an-ni-pa₃-de₃. Ll. 229 sq. « Puissent les redans, les laḡama, après avoir été dressés (là), être précipités (...) ». L. 239. (et comm. p. 120). ku₃-ge-eš₍₂₎ est un adverbialif (pas un terminatif) ; traduire « Puisse ton pauvre priver d'eau ses enfants²⁰ (apparus à la manière de l'argent =(?)) qui devaient lui rapporter de l'argent » ? L. 241. Cf. A. Cavigneaux, JAOS 113 (1993) 255 et A. Cavigneaux/F.N.H. Al-Rawi, ZA 85 (1995) 194 et n. 14 ; il faut distinguer ge₄-ge₄ « retourner » de (aḡ₂) ge/ge₄-r « cogner, tuer ». Ici, on peut hésiter entre « faire avorter » et « cogner, tuer ». L. 246. « puisse-t-il être incapable de soulever sa sacoche jusqu'à sa selle ». L. 247. « puisse-t-il (l'âne de choix) se coucher le soir » (signe de maladie ; comme les chevaux, les ânes dorment debout) ; les textes littéraires pB distinguent u₄ šu₂-uš(-e) « chaque jour »²¹ de u₄ šu₂ « jour couchant » (par ex. dans le fréquent u₄ e₃-ta u₄ šu₂-še₃/uš²²). L. 249. « puisse-t-il (l'"autochtone") rester couché sur l'herbe ». Ll. 250-253. « Puisse celui qui s'est levé devant/pour les prémices manger les enduits de ses poutres, les courroies des grands vantaux de la maison paternelle, puisse-t-il les triturer avec les dents ! »²³ La lecture zu₂ de KA est maintenant assurée par la glose zu dans le nouveau duplicat AUWE 23, 111. Ll. 256 sq. « Sur ton kiusga fait pour les purifications, puisse le renard hantant les collines de ruines y traîner la queue ». L. 259. « l'"oiseau sommeil" ». Ll. 267 ± // 276. Cf. C. Mittermayer, AOAT 319 (2005) 61 ; le premier signe n'est pas ŠEG₉, mais U.PIRIG = KUŠU (Q et S et A₁) // KUŠU + PA (L₁) // [(x).]KIŠ (C₄, Isin) ; j'ignore s'il faut lire KUŠU bar mul « K. au corps moiré »²⁴ ou KUŠU-bar mul. – Lire muš GIR₂(-g.). Ll. 270 sq. ± // 279 sq. L'interprétation traditionnelle est probabl. préférable (forme intrans. avec -du₁₀-ge = {DU₁₀ + ed}), quoique -ni-fasse difficulté²⁵. Mon interprétation dans RA 78 (107 sq. et comm. p. 121) laisse l'absence de /b/ (270 sq.)²⁶ et de /n/ ou /b/ (279 sq.) inexplicable. Ll. 273-278. Traduire par des passés simples.

1. Noter toutefois l'exception um-ma-da-an-SIG₇-ga dans u₈-ezinam 39 B (Ur).
2. a-ša₃ ki-izim dans SNATBM 171:2 est un abrègement purement graphique.
3. Littéral peut-être « Il lui avait été suspendu de faire trembler Agadé ».
4. Ajouter DI H 6.
5. Il cherche à étayer son hypothèse en renvoyant au filet de Ninurta dans Lugale, mais son argument m'échappe ; Ninurta n'est pas un en.
6. Ou « immense éléphant ».
7. // « dans/à sa bataille » = « dans une bataille contre elle » (J₃ et B₄).
8. « He tried to alter » (Cooper, CA p. 55 ; accepté par J. Black et al., LAS 120) me semble grammaticalement un peu problématique ; j'attendrais plutôt mu(-un)-da(-an)-kur₂.
9. Comp. surtout dam mu-un-de₃-kur₂ dumu mu-un-de₃-bala (...) « (Son(?)) épouse lui devint étrangère, (son(?)) enfant se détourna de lui » (Išme-Dagan AB 84) et uru₂ ḡarza-zu im(-me)-de₃-kur₂-ra(-am₃) « Ville, tes rites te sont devenus étrangers » (LU 69).
10. -ga serait en soi possible, mais B écrit GA sinon différemment (131 et passim), ce qui rend cette lecture problématique ; gu₄ est également à peine crédible.
11. Les autres duplicats ont si gu₃ ra // à la l. 48 et gu₃ teš₂ ra // à la l. 49.
12. Ou (littéral.) « abattit » ?

13. Aussi M₂.
14. Lire [ka]r-re, pas [kar]-ra.
15. Les « raisons métriques » invoquées à la p. 116 me laissent aujourd’hui sceptique.
16. L’idée serait que l’on n’avait plus accès aux vergers.
17. Pas exclu non plus (littéral.) « ils se martelaient les uns les autres ».
18. Ainsi Farber, *loc. cit.* (comp. l. 190) ; « têtes » toutefois pas exclu.
19. Comp. T. Jacobsen, *The Harps* 370 : « numerous teeth » / « teeth » ; la séquence inverse serait toutefois de mise.
20. dumu (...) -na étant au loc., probabl. collectif.
21. Originellement adverbiale {U₄ ŠU₂ + eš(e) ?}
22. u₄ šu₂-uš-še₃/e est très rare et probabl. le produit d’une contamination.
23. Littéral « Des grands vantaux-courroies de sa maison paternelle, puisse-t-il triturer les courroies avec ses dents ! »
24. Pour bar mul, cf. M. Civil, *JNES* 43 (1984) 285 et n. 7.
25. Qu’il recouvre -ne₂- = {n(n)a + e/j} est à peine crédible, car on attendrait -ne- (comp. avec l’ergatif 2e sg. in-ne(-e)-du₁₁, u₃-ne(-e)-du₁₁, etc. [ELS 218]).
26. Comp. par ex. Gudea, *Cyl. B* 3:1 et 17:2 sq.

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47) **Omen Report No. 38** – By the account of U. Koch-Westenholz (*FS Walker* (2002) 132-133), this text is the 38th known Old Babylonian liver omen report. The text was found among a small group of tablets acquired as a gift early in the Institute’s history (ex-R. F. Harper collection).

Text: Oriental Institute A167

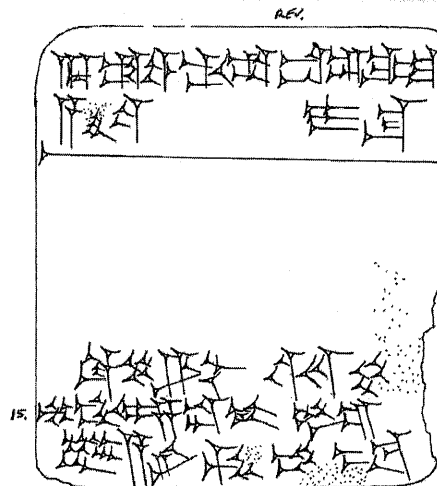
Date: Aš 12/12/21

Size: 48x53x19mm (round-type)

obv. 1 munus.áš¹.gàr *li-pi-it qá-¹ti¹*
a-¹na¹ [^dUTU²?]
 ki.gub *ka-pí-iš* [gír¹ [i-šú]
 kal *i-šú* si[lim *i-šú*]
 5 zé zag []
i-na mur *a-šar m[u-ki-il re-ši*]
 mur zag []
 zag mur ù g[ùb mur]
ki-di-it dal *e-l[ⁱ*]
 10 dal šà *ri-ša* [i¹-[šú]
12 ti-ra-n[u]
 rev. *a-na ta-wi-ti-ša¹ ša-al-ma-at*
a-¹hi-tam *i-šú*
 iti še.kin.kud u₄ 21.kam
 15 mu *am-mi-ša-du-qá* lugal.e
 alan.a.ni máš.gaba.tab.ba

Translation

obv. One female kid, for the performance of extispicy,
 for [Šamaš].
 The Presence was pinched. It had a Path.
 It had a Strength, [it had] a Well-Being.
 5 The Gall Bladder on the right side [...]
 Upon the Lung, at the place of the [Support-of-the-
Head, there was ...]
 The Lung on the right side [was ...]
 The right side of the Lung and left side of the [Lung
 were ...]
 The outer surface of the Lintel, ab[ove ... was ...]
 10 The Lintel of the Heart [had] a “Help”.
 12 Convolutions (of the intestines).
 Rev. As for her query, it was favorable –
 but it had some adverse features.
 Month 12, Day 21,
 15-16 Year: Aš 12



The obverse of the tablet is substantially damaged.

- 2 The available damaged space at the end of the line is so brief as to invite speculation that it was in fact left altogether blank. Barring this unlikely error, though, the briefest possible writing the space permits would be a divine (rather than personal) name, and the briefest god's name would be ^dUTU.
- 6 Restored as a presumed parallel to Goetze *JCS* 11 96, No. 3, ll. 9-10, which gives a similar phrase without the locative *ina*.
- 10 cf. *YOS* XI 23 10 and *CAD* R s.v. *rīṣu* s. b, *contra* Koch-Westenholz (*ibid.*, 133-4, No. 1, ll. 9 and 24) who understands *re-sà*, "its top."
- 14-16 In contrast to the blocky, right-angled script of ll. 1-13, the three lines of the date formula are written at the decidedly raked angle of the Late OB cursive ductus.

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48) Once more, *ana še'im* – After the publication of the hapax *namšēm / pīhāt(NAM) še'im* in 2005 (SC 272), a second attestation of this string of signs now appears in another text (A688) from Old Babylonian Larsa. A comparison of the contexts now leads to the conclusion that NAM must be read in both texts as *ana* (*ana še'im* in A688, *a-naNAM še'im* in SC 272). Consequently, a proposal to read the signs as one word (*namšēm*) should be abandoned.

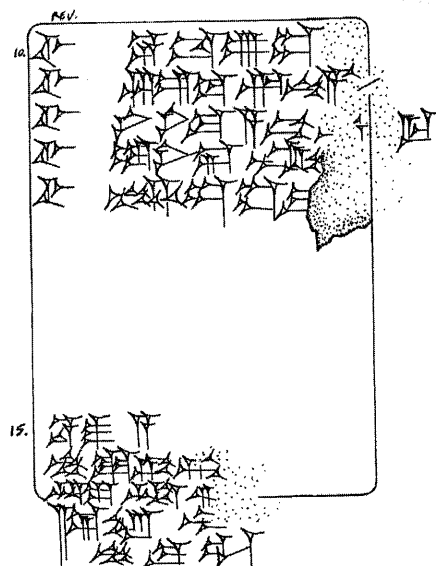
Text: Oriental Institute A688 **Date:** Rīm-Sîn 22/11/-- **Size:** 41x57x22mm

obv. 7 gín igi.6.gál 6 še kù.babbar
 nam *še-e-im* *ša-mi-im*
 ki *qí-iš-ti-ir.ra*
 ma-*hi-ša-gi-iš* nu.[bànda]
 5 ù *a-bi-sa-tum*¹
 šu.ba.an.ti.[meš]
 iti sig₄.[a]
 kar.ra *i-la-ku-ú*¹
 še ì.ág.e.<meš>
 rev. igi *e-du-ú-um*
 11 igi ^dEN.LÍL-*ma-lik* šeš.a.ṛni¹
 igi ì-*lí-ma-a-bi* [x] UR
 igi *ši-lí-é-maḥ* d[am[?].gàr[?]]
 igi *mu-ḥa-du-um* š[u.i[?]]
 15 iti zíz.a
 mu du₁₁.ga z[i.da]
 u.e. ^dEN.LÍL ^den.k[i.ga.ta]
 íd.sikil.l[a]
 mu.ba.al



Translation

obv. 7.¹/₆ shekels 6 grains silver
 for barley-purchasing,
 from Qīšti-Etra,
 Aḫi-šāgiš, lieutenant,
 5 and Aḫi-satum
 received.
 (In) Month 03,
 at the going rate of (per-unit) value,
 they will pour out the grain.
 rev. Before Edūm,
 11 before Enlil-mālik, his brother,
 before Ilīma-abī, [X X]
 before Šilli-Emaḥ, [the merchant]
 before Muḥadum, [the "barber"].
 15 Month 11,
 16-19 Year: RS 22



- 2 The proposed hapax *namšēm* would now be a *dis legomenon*; its only other attestation is known from SC 272, ll.18-20 (S. Richardson, "Axes Against Ešnunna", *Orientalia* N.S. 74:1 (2005) 42-50). At that

time, however, I identified the element /NAM/ as *pīḫatum*, translating :

kanīk PN *ša a-na* NAM *še-e-em* *ša* FN

“sealed tablet of PN, who is responsible for the barley (cultivation) of FN”

A. Goetze’s copy of SC 272 is still yet to appear in the planned *YOS XV*, but the comment in that volume’s galleys (provided courtesy B. Foster) currently identifies these same signs as the single word *namšēm*, a hapax which is not translated there.

The reading /NAM/ as *pīḫatum* in this and similar contexts must now be rejected in favor of *lanal*. This was first suggested to me by Erica Reiner in Summer 2005 for A688, but the (apparent) repetition of *ana* NAM in SC 272 seemed to militate against reading *lanal* there. On reflection, however, it now seems likely that SC 272’s *lanal* is simply a gloss on the following /NAM/. This unusual orthographic measure in SC 272 was probably the result of a (north) Babylonian scribe’s unfamiliarity with the south-Babylonian custom of using the sign /NAM/ for the preposition *lanal*. We should look to the historical context of a scribe from the realm of Ḫammurabi drafting SC 272 in recently-conquered Larsa, where the logogram NAM (= *ana*) had been in local use.

Thus l. 2 of this text is read: *ana*(NAM) *še'im šāmim*; ll. 18-20 of SC 272 should be read: “sealed tablet of PN, for the barley (cultivation) of FN.”

- 5 Although the break at the end of the sign read *-tum* very much resembles a vertical wedge, this must be an accident of the break itself: *-sa-at-[x]* does not suggest any PN known to me.
- 8 The verb *alāku* can have the meaning “to fluctuate (said of prices),” CAD A/1 s.v. *alāku* v. 3f, as in the phrase *ki.lam/maḫīrat ... alāku*. Since /kar/ is known as a term in use at Larsa meaning a rate of per-unit value (see Kozyreva, *JCS* 36:1 (1984) 85-6; see also CAD K *kāru* A s. 4, “price of a unit of merchandise”), it seems logical to understand it here as standing in the place of *ki.lam*. The phrase *kar.ra ... alāku* is, however, otherwise unattested to my knowledge.
- 9 With two borrowers, the verb of repayment should show the plural *.meš*.
- 14 An equally reasonable restoration for the title is *š[u.ku₆?*].

Seth RICHARDSON (27-06-2007)

49) The Old Assyrian List of Year Eponyms. Corrections, Additions and Chronology – In the six years since the manuscript of my *The Old Assyrian List of Year Eponyms* (2003, henceforth *OALYE*) with its Addendum (Oct. 2001) was completed some new data have become available and some corrections, taking into account also of the reviews by D.O. Edzard in *ZA* 94 (2004) 304-06, and by C. Michel, in *Afo* 51 (2007), are necessary. A few implicit corrections are given in G. Kryszat, *Zur Chronologie der Kaufmannsarchive aus der Schicht 2 des Kārum Kaneš* (OAS 2, Leiden 2004), and I also received some comments from Mr. M. Liebig in Leipzig. I intended to include the following observations in my forthcoming edition of the archive of Kuliya (kt 92 k 188-263, to which *KEL A* belongs), but in view of the ongoing discussion about the (Old) Assyrian chronology¹ I present them here.

Corrections

- p. 5, first line, read: when Puzur-Aššur II had died (Michel).
- p. 8, *95 and fig. 4, line 35 end: in *KEL B* final *-da* of Aššur-nādā is partially preserved, though missing in my copy (collated).
- p. 9, *100 and fig. 4 line 36, end: in *KEL B* part of the final *-Ištar* is preserved, though missing in my copy (collated).
- p. 10, 2.3, 8a (not 7a), B: DINGIR-*e-lá-at*.
- p. 29, note 46: *ša warki* now also attested *AKT 4* (2006) 1:13f., *ša warki 77*, month II.
- p. 35, no. 107, line 5, read: *TPK* for *KTP*.
- p. 45, note 76, read: kt c/k 512 :5f.
- p. 49, *1: there is one *ša qātē* reference, in kt v/k 159:15f. (Kryszat).
- p. 49, *4: no week-eponyms attested. *KTBI 14* (four references!) is closely related to *TC 3, 250*, for in the latter text, during eponymy *3, a payment was promised that according to *KTBI 14* was effectuated in the next year.
- p. 49, *5: *ICK 1, 119* now has a duplicate in *AKT 4* (2006) 13.
- p. 53, under b. and d., and p. 54 under b., read: *TPK* for *KTP*.
- p. 53, under i., read: *AKT 1, 35:12*; under j. read: kt 92/k 201:16’; note that kt 98/k 121 was made known by V. Donbaz in *StBoT 45* (2001) 112.
- p. 59, note 103, read: *RA 73* (1979).
- p. 64, 4. Donbaz, *Studies Larsen* (2004) 180, line 23, reads the name of the father *Ta-ki-ša*, but evidence from Mari still supports a reading *Tag/kigi*.
- p. 64, 17, read f/k 178:11f.

p. 71, right column, read: Ennum-Aššur s. Dun(n)ia 11.

p. 72 (Index), first col., read: Iddin-Suen – brother of Šuli; cancel: (Šalim-ahum).

Additions

The publication of new texts allows many additions, especially regarding the number of attestations of eponyms, but I will only mention a few more important ones. The data on eponymies attested at Mari have been updated by D. Charpin – N. Ziegler, *Florilegium marianum* 5 (Paris 2003; quoted in what follows as *FM* 5) 70ff., 82ff. and 262, and need occasional mention. Important is the discovery of a new eponym list, kt 01/k 287, by Cahit Günbatti, which, as he kindly told me, starts with eponymy 110 of *KEL* A and seems to comprise in all 143 names, which brings us down to the last quarter of the 18th century BC (middle chronology), probably near the end of the level Ib period of *kārum* Kanesh.

On eponyms 40, 40A, 40B, and 41 (p. 13, a)

The manuscripts, to which I can now add the data of kt 94/k 836 (E, already mentioned in the Addendum to *OALYE*), present the following picture:

40	40A	40B	41
A: <i>Bu-zi</i> s. dīŠKUR-GAL	A: -----	A: -----	A: <i>I-dī-Sū-in</i> ŠEŠ Šu-li
B: <i>Bu-zu</i>	B: Šu-li	B: <i>I-ri-šu-um</i>	B: <i>I-dī-Sū-in</i>
D: <i>Bu-zi</i>	D: Šu-li s. Šál-mah	D: -----	D: <i>I-dī-Sū-in a-hu-šu</i>
E: <i>Bu-zi</i>	E: Šu-li	E: -----	E: <i>I-dī-Sū-in a-hu-šu</i>

(Only ms A mentions, between 40 and 41, the succession of Irišum I by Ikūnum)

We now also have the evidence of kt 94/k 1263:28f. (courtesy Larsen), which dates a debt to the eponymy of *I-dī-Sū-in* DUMU Šu-li and turns Šuli from a brother into the father of eponym no. 41. In the light of the data in mss. A, D and E we have to assume that DUMU is a scribal mistake for the unusual ŠEŠ, a type of mistake also found in the Assyrian Kinglist. The occurrence of Irišum in ms. B, 40B cannot be explained,² but the combined evidence of mss. A, D and E shows that his name must be ignored, so that the main remaining problem is the omission of no. 40A in ms. A. There is little to add to the observations in *OALYE* 14, but the fact that ms. E confirms the existence of 40A and the actual occurrence of “Iddin-Suen, son of Šuli” in kt 94/k 1263 confirm its correctness, which is also supported by no. 41, where “brother of Šuli” would be odd if Šuli had not been mentioned before. If we assume a mistake by A, he must have omitted 40A after the remark on the accession of Ikūnum, because the 40 years reign of Erišum is confirmed by the Kinglist. Lack of independent evidence on the length of Ikūnum’s reign does not allow to settle the matter, but it seems now clear that 40A was mistakenly omitted in *KEL* A and this has to be taken into account in constructing an overall chronology. *The consequence of this correction is that the sequential numbers of the eponyms nos. 41 (Iddin-Suen) – 129 (Aššur-emūqī) have to be raised by one.*

On other eponyms

p. 14, top. At Mari the eponymy of Adad-bāni is already attested for day 7 of month I and we also have Ili-tillatī ŠU Iddin-Ištar (see *FM* 5, 166, also on the meaning and use of ŠU). When a new eponym had not yet been named/appointed one could designate him as “the eponym who has not yet been named, the successor of PN” (*līmu ša adini lā nabû ša warki* PN; *FM* 5, 83 note 60).

p. 30. Add no. 41 and its mention in kt 94/k 1263:28f. (see above). No. 47 (=48): see for the tablet in which this very old eponym occurs, G. Kryszat in *Studies Veenhof* (2001) 263ff.

p. 31. No. 68 (=69): also attested in kt 94/k 797 :2f. No. 73 (=74), note the spelling of the name of the father as Šé-ep-A-lim^{ki} in kt 94/k 1252 :22.

p. 33. No. 99 (=100): kt 93/k 581 has simple Aššur-nādā, while the parallel kt 93/k 533 also gives his patronym, son of Ili-binanni (courtesy C. Michel). The week eponymy of Innāya son of Elālī is attested in kt k/k 44:5 and kt 914/k 182 during month VII of the eponymy of Aššur-nādā, which *AKT* 4, 7:12f. now shows to be the son of Ili-binanni.

p. 33. No. 100 (=101): the full designation Ikūppi-Ištar, son of Ikua, in kt 94/k 162:23, but as DUMU *I-ku-a* only in kt 94/k 145:17’ (both courtesy Michel).

p. 35. No. 114 (=115): one reference with a single *hamuštum*.

p. 36. No. 123 (=124): also in kt f/k 90:5.

p. 37. No. 128 (=129): there are four references, also kt 92/k 220:9f., a *ša qātē* eponymy during month VI; the successor, no. 129 (=130), appears in the first two texts listed there, only in month VIII. No. 130 (=131): it is now clear that he is Tāb-Aššur, son of Uzua, see below on p. 56.

p. 41, note 63: note also kt 83/k 135 :6f. (mentioned p. 31, no. 66), dated to eponymy no. 67 (=68) a very old text.

p. 43, on Šarru-kīn: kt 87/k 552:40f. (courtesy K. Hecker), mentions Šalim-Aššur DUMU LUGAL-GLNA.

p. 49, note 84: Aššur-taklāku turned up in M.7841 (*FM* 5, 70), in the sequence Aššur-taklāku,

Haya-mālik.

p. 53, c: attested in kt 94/k 1177:71f. as Adad-bāni, son of Puzur₄-E-a (month IV).

p. 64f. Some of the kt 98/k tablets were published by V. Donbaz in *StBoT* 45. See for a survey of some eponyms attested at Mari and at Kültepe, *FM* 5, 79 note 21.

p. 64. 4: his successor (*ša qātē*, III) now in kt 2001/k 325b :35 (Albayrak, *Studies Larsen* 15).

p. 65, add: *Ni-mar-Sú-in* s. *A-šūr-ni-[im-ri]*, kt 98/k 129:3' f., II (V. Donbaz *StBoT* 45, 113).

A new eponym, *Abī-Sîn* (*A-bi-30*), turned up in F. Joannès, *Haradum* II (2006) no. 29:21f., during month XII of the Babylonian calendar, somewhere during the (end of the) reign of Abi-ešuh or that of Ammi-ditana.

Conclusion (p. 55f., 7.4)

The new late eponym list, kt 01/k 287, mentioned above, allows to fix the length of the gap between the end of *KEL* A and sequence B of the *Mari Chronicle*. As Günbattı kindly told me, it contains only three eponyms, *Ṭāb-Aššur*, *Šu-Rama* and *Sîn-išmeanni*, in that order. During the last years of *kārum* Kanesh level II therefore the following eponyms were in office in the following order:

131 <i>Ṭāb-Aššur</i>	s. Uzua	134 <i>Aššur-mālik</i>	s. Šu-Haniš	137 <i>Aššur-balāt(i)</i>	s. ?
132 <i>Šu-Rama</i>	s. Uzua	135 <i>Dan-Ea</i>	s. Abu-waqar	138 <i>Ennam-Aššu</i>	s. ?
133 <i>Sîn-išmeanni</i>	s. ?	136 <i>Ennam-Suen</i>	s. Iddin-abum		

The name of no. 138, attested only as anonymous successor of no. 137 (during month II, in *ICK* 1, 119:8f. = *AKT* 4, 13 : 8f.) is presented by *MEC* B *5. Were nos. 131 and 132 brothers?

Iddin-Suen, son of Iddin-abum, presented on p. 56 as "B" and tentatively numbered 131, does not occur in the new list. As mentioned on p. 53, e, he appears in line 13 of a memorandum that lists debt claims of Kuliya (no. 44 in my forthcoming edition of the Kuliya archive) together with eponym 132, *Šu-Rama*, all of which suggests that he is a very late eponym. Since we cannot insert him into the list and collation (see above on no. 44:13) has shown that kt a/k 324:19 (contra Balkan, *Observations* 88 no. 31) actually has *I-na-Sú-in*, we must assume that *I-df-Sú-in* was a scribal mistake for *I-na-Sú-in* (no. 136; his name is written in this way in text no. 39:10 of the Kuliya archive), in fact the same mistake as made by Balkan.

These new data require no adaptation of the chronological calculations of p. 51f. and ch. 8, because the omission of Ennam-Suen, son of Iddin-abum, is compensated by adding eponymy 41 = 40A, *Šuli*, son of *Šalmah*, omitted in *KEL* A, as shown above.

As for the table presented on p. 57, M. Liebig observed that if I assume (p. 56) that *Šamši-Adad* conquered Ekallātum in the eponymy year *MEC* B *31 = 1811 BC, the last full year of *Irišum* II (the end of whose reign must cover the years *MEC* B *28-31) should be 1812 BC. This means that, with a gap of only 3 years between the end *KEL* A and the beginning of *MEC* B, the table should read:

<i>Irišum</i> I	<i>KEL</i> 1-40	40 y	1974-1933
<i>Ikūnum</i>	<i>KEL</i> 41-55	15 y	1934-1920
<i>Šarru-kīn</i>	<i>KEL</i> 56-95	40 y	1919-1880
<i>Puzur-Aššur</i> II	<i>KEL</i> 96-103	8 y	1879-1872
<i>Narām-Suen</i>	<i>KEL</i> 104-130 +131-133	30 y	1871-1842
<i>Narām-Suen</i> + <i>Irišum</i> II	<i>MEC</i> B*1-27	27y	1841-1815
<i>Irišum</i> II alone	<i>MEC</i> gap	6y	1814-1808
<i>Šamši-Adad</i> I	at Ekallātum	3y	1811-1809
<i>Šamši-Adad</i> I	at Assur	33y	1808-1776

Absolute chronology (p. 58. 8.2)

It is impossible to deal here with the larger chronological issues and I limit myself to what is directly related to the Old Assyrian evidence and the eponymic chronology. A real challenge was combining the data of the Eponym List with the mention of a solar eclipse during eponymy year 127 (see *OALYE* 9, note 6; the year after the birth of *Šamši-Adad* I)³ and with new dendrochronological data provided by wood used in the construction of the 'Sarıkaya palace' of Ačemhöyük. It was first met by C. Michel and R. Rocher in *JEOL* 35-36 (2000) 111ff., but they soon had to revise their solution on the basis of new and better data from Ačemhöyük, which now fix the construction of the palace there in 1774 (+/-7) BC, which is 22 earlier than the previously (see S.W. Manning e.a., *Science* 294 [21 Dec. 2001] 2532-5). Since this palace contained bullae sealed by *Šamši-Adad* I, in which he calls himself "*iššiakkum* of Assur", it must at least have covered the years of his rule over Aššur, 1808-1776 BC according to the middle chronology. This could only fit this chronology if we start from the highest possible date for the palace (1778 BC) and assume that the bullae are from one of his last two years, which together is very unlikely, the more so because the palace also contained bullae sealed by Aplaḥanda of Karkemish, whose rule (which may have started during the last years of *Šamši-Adad* I) lasted until 1764 BC (middle chronology).⁴

On the basis of these data Michel & Rocher now (*NABU* 2002/20) propose to identify the solar eclipse

with the total one of 1833 BC, visible in Assur, which means that Šamši-Adad I would have died 74 years later, in 1760 BC. This requires a lowering of the middle chronology by ca. 16 years and would mean that Erišum I started his reign in ca. 1958 BC. This proposal, a “reduced middle chronology”, found support in *NABU* 2006/13, where Regine Prusinszky argued that it would fit the “Distanzangabe” of Tukulti-Ninurta I, who claims to have built the temple of Ištar in Assur 720 years after Ilušuma (= the beginning of the reign of Erišum I), if we assume that the rebuilding started in Tukulti-Ninurta’s fifth year. This implies that he became king of Assur (according to the unabridged Middle Assyrian chronology) in 1244 BC.

It is, unfortunately, impossible to support this “lowered middle chronology” by means of dendrochronological data obtained for Kültepe itself. According to Manning e.a. (*loc.cit.*) the so-called Warshama palace (contemporary with level Ib of *kārum* Kanesh; named after the king of Kanesh who received a letter from Anumhirbi of Mamma, found in it) would have been built in 1832 +4/-7 BC and have been repaired 61 years later, in 1771 +4/-7 BC. Eponym List and Mari Chronicle show that level II of *kārum* Kanesh and also level 7 of the city on the tepe must have come to a destructive end during or immediately after the last attested year eponymy (138), in my reconstruction 1837 BC (middle chronology), which becomes 1821 BC in the “lowered middle chronology”. This means that the palace of Warshama would have been built between 4 and 15 years *before* the destruction of level II of the *kārum* and level 7 of the city. This is surprising, though not impossible, because this disaster could not have been predicted, but it does mean that its construction cannot be associated with the revival of city and *kārum* at the beginning of the level Ib period, usually dated to 1800 BC or a little later, or ca. 1785 BC or a little later in the “lowered middle chronology”. And this latter date also cannot be correlated with that of the substantial repairs of the palace, in the lowered chronology between 1775 and 1764 BC. Repairs at some time during the level Ib period could be linked with the city’s history, which was marked by the conquest by Pithana, the suppression of a revolt by his son Anitta (both mentioned in the ‘res gestae of Anitta’), and a possible conquest by Zuzu of Alahzina, the “great king”, who ruled over Kanesh according to legal documents found in level Ib of *kārum* Kanesh, ‘notarized’ in his name.⁵ But we have to realize that we know little of the building history of the *kārum* of level Ib, neither when its rebuilding started, nor how long exactly the chronological gap (in eponymy years) was between the end of level II and level Ib.⁶ Moreover, the building history of the palace on the tepe, especially vulnerable to political upheaval during the level Ib period, need not have been the same as that of the living quarters in the *kārum*. It would be important if wood from level Ib of the *kārum* could be dated dendrochronologically, but the rebuilding may have been a gradual process and the length of this period (ca. 80 years) also leaves room for repairs.

The “lowered middle chronology” is not accepted by the supporters of Gasche’s low chronology and also has to face old and new advocates of a long chronology, such as P. J. Huber (*Akkadica* 119-120 [2000] 159ff.) and Chr. Eder (see footnote 1), in both of which astronomical data play an important role.⁷ Eder, probably also stimulated by the fact that my reconstruction had confirmed the reliability of the ‘Distanzangaben’ for the period between the accession of Erišum I and the death of Šamši-Adad I (which turned out to be a period of 199 years), now opts for 1846 BC as the year of Šamši-Adad’s death. But the chronological problems and uncertainties for the centuries after his death (with which also Janssen is wrestling, see footnote 1) are the real problem, due to the variants in the tradition and the damage of the Assyrian Kinglist. I also seriously doubt whether Išme-Dagan really reigned 40 years as king of Assur after the death of his father (which means after his reign as viceroy at Ekallatum), as the Kinglist claims.⁸ The publication of the above-mentioned new, Old Assyrian eponym list (discovered in 2001), which goes down to ca. 1720 BC, may bring more clarity and I can only express the hope that this extremely important document will be published without further delay.⁹

1. Note the recent publications of Chr. Eder, *Assyrische Distanzangaben und die absolute Chronologie*, *AoF* 31 (2004) 191-236; R. Prusinszky, Šamši-Adads I. ‘neue’ Regierungsdaten und Assyrische Distanzangaben, in: *Studies in Honour of Manfred Bietak* (OLA 149/3, 2006) 63-79; *Idem*, Die “verkürzte mittlere Chronologie” unter Berücksichtigung der Distanzangabe Tukulti-Ninurta I., *NABU* 2006/13; Th. Janssen, Zu den Berechnungsweisen und Resultaten assyrischer Distanzangaben, *Akkadica* 127 (2006) 73-92.

2. It might go back to a damaged(?) line in its Vorlage, mentioning that Ikūnum had succeeded Irišum (I), since this happened during eponymy-year 40.

3. This is the year of the death of Aminum, also mentioned in the “Mari Eponym Chronicle”. I assume that these two important events created special interest in this eclipse.

4. See for the bullae from Acemhöyük and the attestation of Aplahanda, K.R. Veenhof, in M.J. Mellink e.a. (eds.), *Nimet Özgüç’e Armağan* (Ankara 1993) 645, note 3, and for the reign of Aplahanda, D. Charpin – N. Ziegler, *Florilegium marianum* V (2003) 205f.

5. See for the rulers and history of the *kārum* level Ib period and for the ‘notarization’ of legal records, the observations in K.R. Veenhof, *The Old Assyrian Period*, in: M. Wäffler (ed.), *Annäherungen* 5 (OBO 160/5), Part I, Fribourg-Göttingen (in press), ch. 3.2.3/4 and ch. 4.2.5.

6. Šamši-Adad’s last rivals at Mari, Yahdun-Lim and his son Sumu-Yamam, disappeared in ca. 1794-1792 BC (see *FM* 5, 47, 1.2.11/12) and though this probably made the trade on Anatolia easier, we cannot make their defeat and disappearance a condition for the revival of *kārum* Kanesh. It may have started somewhat earlier, because it must have been primarily conditioned by the situation in Anatolia and we know that overland traders, under certain conditions could also travel between

countries at war (see D. Charpin – J.M. Durand, *MARI* 8 [1997] 378f.)

7. Notably the lunar eclipse omens, as preserved in *Enūma Anu Enlil*, where they are connected with historical events (such as the downfall of early kings), which would yield exact dates. The criticism of their use, already expressed by Hunger in *Akkadica* 119-20 (2000) 155-158 and in *Festschrift Manfred Dietrich* (AOAT 281, Münster 2002) 171-6, where he speaks of their “Bedeutungslosigkeit”, is now supported by F.N.H. Al-Rawi and A.R. George in their edition and analysis of a new source of *Enūma Anu Enlil* tablet 20 (*Iraq* 68 [2006] 23f.). The postulated absence of intercalation in Assyria before the reign of Tiglath-Pileser I, another argument for Gasche’s short chronology, has been questioned by the present writer in *Akkadica* 119-20 (2000) 137-150.

8. On p. 208f. Eder ignores the effects of Hammurabi’s subjugation, in his 32nd year (1761 BC), of “various cities of the lands of Šubartum and Ekallātum” and even assumes “eine rege Bautätigkeit” of Išme-Dagan, who would have returned after a short interregnum. But we lack evidence for it and his building activity mentioned by Enlil-nāšir I (*RIMA* 1, 95:7) – assuming that Išme-Dagan I is meant – cannot be dated.

9. See also my remarks on the chronology in *The Old Assyrian Period* (see above note 5) ch. 1.2.

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50) On some Old Assyrian linear measures – OA *idum*, attested in *CCT* 4, 44b:18f., which asks to buy and send a “15 or 20 *idum* long linen fabric (*kītum*)”, is translated by *CAD* I/J 15, 4, as the only occurrence of this meaning of *idum* “arm”, by “span”, traditionally the distance between the end of the thumb and that of the little finger of a spread hand, usually ca. 20 cm or a little more (in English 9 inches, or ca. 23,6 cm).¹ *AHW* (p. 1447, s.v.) proposes to read *ūtum*, which it considers a variant of *ūtum*, “half a cubit”, an identification taken over by Powell *RIA* 7, 473a § IA.3,² who, however, emends a clear *i-DĪ-im* twice into *ū!-DĪ-im*. Both equations would yield an acceptable figure for a linen fabric, presumably a strip (its width is not stated) of between 3 and 5 meters long. However, a meaning “span” of the common word for “arm” is etymologically unacceptable and *AHW*’s proposed equation with *ūtum* is very unlikely, since *ūtum* itself is attested in OA, as is the emendation suggested by Powell. Both solutions are also difficult to accept, because it is strange to find a substantial length of cloth defined not in “cubits”, but in “half cubits” or even “spans”. *ūtum* is normally used alongside a number of whole cubits (as in the examples given below) or alone, for items of small size, e.g. for a child of 3 *rūtu* (NA); the length of bigger objects is rendered in “cubits” (or even bigger measures) and never expressed in large numbers of “half-cubits” (or “spans”) only.

A new source, kt f/k 55:1-6 (courtesy L. Umur), contains the following statement: (1) 1 *gušūrum* (2) 4 *ina idim* (3) <u> *ūt* (4) 1 *gušūrum* (5) 3 *ina idim* (6) u *ūt*, “1 beam measuring 4 *idum* (and) 1/2 cubit (and) 1 beam measuring 3 *idum* and 1/2 cubit”. It proves that *idum* and *ūtum* are different and that, since one *ūtum* occurs as complement to *idum*, the latter was longer than half a cubit. A meaning “cubit”, the most common linear measure larger than *ūtum*, would fit well, but for it, as generally in Akkadian, OA uses *ammatum*, “forearm”. That the OA “cubit” may have been slightly longer (possibly 54 cm) than the Babylonian one, as Powell, *RIA* 7, 473f., tentatively suggests on the basis of the sizes of bricks used by Šamši-Adad I, is irrelevant here. In *TC* 1, 81:26 *ammatum* appears in combination with *ūtum*, a table of *ammāt ūt*, “1 1/2 cubit”, and alone³ it occurs to describe the size of textiles in *TC* 3, 17:34ff. (*tīše inammitim lū urukšu šamāne ina ammitim lū rupuššu*) and kt 94/k 1350:21f. (*raqqatam arbe ina amm[itim] rupuš[ša] u ešar urukša*; courtesy Larsen), which means pieces of 4,5 by 4 and of 2 by 5 meters respectively. This makes *idum*, “cubit”, unlikely, unless it was used as a more colloquial term and variant of *ammatum*. The alternative, a measure based on the length of the whole arm of an adult person, ca. 70 to 75 cms, about the size of the “big cubit” in Nuzi and NB (in Kassite texts equal to 30 “big fingers”, also called *ammāt arê*, “cubit of the pace”), is theoretically possible but rather unlikely for OA.

The situation as to linear measures in OA is anyhow complicated, since it also (once) uses *kabistu*, in the absolute state, in *OIP* 27, 62:41, where a log of wood is described as 3 *ina ammitim urukšu kà-áb-sà-at* 1 *rupuššu*, “its length 3 cubits and its width 1 *kabistu*”. *CAD* K 20 s.v., following Landsberger, *WdO* 1 (1950) 368 with footnote 37, identifies it as “foot”; Powell *RIA* 7, 473a, “step/footprint”. Since the “foot” in general is a measure of ca. 30 cm, the difference with the *ūtum* of ca. 25-27 cm is very small indeed and this makes the use in OA of two different terms for a length more or less half-way between *ubānum*, “inch” (attested in *ICK* 1, 17b:4, *tuppam 2 ubān*) and *ammatum*, “cubit”, surprising. The fact that *ūtum* is used of a beam (*gušūrum*) and *kabistu* of a log of wood is not sufficient to solve the problem. Rather than operating with possible further subdivisions of the cubit (1/3, 2/3, 3/4),⁴ I would follow the suggestion of Powell (*RIA* 7, 473, who also observed that *ūtum*, “half cubit”, seems to fill the slot occupied by “foot” in most European systems), that *kabistu* may have been part of a distinct metrological system. It might point to the existence of a dominant system starting from the length of finger, hand and arm, and a vestigial one starting from the foot or leg (though *GĪR* = *šēpum* is never used as a linear measure in Akkadian), to which not only *kabistu*, “step”, but also *kimšu*, “shin bone” (Nuzi, and according to Powell with a length of between 1/2 and 1 cubit), and *purīdu* = *GĪR*, “leg” = 3 cubits, may belong.⁵ Some measures in both systems approach each other’s values, a “fathom”

(see note 1) of ca. 6 feet would not be too different from the Babylonian *qanû* = GI = 6 cubits. And this may also have been the case with *ūṭum*, “half-cubit”, and the slightly bigger *kabistum*, “foot”. The latter may have been preferred in *OIP* 27, 62:41 for state the width or thickness of a log over one of the type 1 *ūṭum* 2 *ubān*.

1. The heading of the lemma offers two translations, “span or fathom”, which is surprising since *CCT* 4, 44b:18f. is the only occurrence of the measure *idum* in Akkadian, where “fathom”, usually equated with 6 feet, the distance between the fingertips of a man’s outstretched arms (in archaic English “any of several units of length varying around 5 and 5 1/2 feet”, Webster) is extremely unlikely, since this would yield strip of linen of ca. 30 meters long.

2. In *RIA* 7, 461b, Powell, commenting on its Sumerian equivalent ŠU.BAD, states “whether this means “open hand” with the fingers extended (which would equal “span” – K.R.V.) or two hands in some special position is unclear”. On p. 473a, commenting on the use of (*r*)*ūṭu* in northern Mesopotamia, he writes: “1/2 cubit = 15 fingers, perhaps ‘handspan’”.

3. See for other occurrences my *Aspects of Old Assyrian Trade and its Terminology* (Leiden 1972) 109, comment on line 35.

4. In ancient Egypt the “royal cubit” of ca. 52,3 cm was considered to comprise 7 “palms” of ca. 7,5 cm., each consisting of 4 “digits”.

5. An overlap or approximation of two systems is also attested in Greece and early Rome, where 4 digits (“finger’s breaths”) make 1 palm (of ca. 7,5 cm), 3 palms 1 span, but also 4 palms are equated with 1 foot. Regional differences in the actual length of such measures seem to go back to differences in the length of the “foot”, which may vary between ca. 27 and 35 cm. An illustration of the two systems is available on a famous East Greek metrological relief (ca. middle 5th century BC) in the Ashmolean Museum (ANC 154), which depicts a man’s torso with outstretched arms and hands, a “fathom”, with above his right arm a foot print.

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51) Once more on ‘the general who is above the four generals’ and his congeners – Anyone who has trawled through the astronomical diaries (Sachs and Hunger 1989, 1996) in an attempt to glean data pertaining to Babylonian history in the Seleucid and Parthian periods will be familiar with the title *lúGAL ERÍNmeš šá ana UGU 4 lúGAL ERÍNmeš*, ‘the general who is above the four generals’. Considering how unusual this title is, it is surprising that so little has been written about it. Attested in the diaries from 229 to 119 B.C., the title has one important congener, namely, *lúGAL ERÍN KUR URÍki šá ana UGU 4 lúGAL ú-qu-tú*, ‘the general of Babylonia who is above the four generals’. Discussing this title in 1999, K. Kessler suggested that, in spite of the fact that no such title exists in Greek, this would have been understood by contemporary Seleucid authorities as the *strategos* of Babylonia who ranked above the four (other) *strategiai* (Kessler 1999:178). In 2002 Y. Mitsuma suggested that the general above the four generals ‘is probably to be equated with “the Satrap of the East”... of the Seleucid kingdom’, adding, ‘One variant of the title for the latter “the General of Akkad who is above the Four Governors”’, indicates that this official controlled a number of provinces because the word “Governor (LB *lu-ma-ma-?i-ir/lúGAL UKKIN*)” here no doubt corresponds to the Gk. *satrapes*, the “governor” of a province (e.g. Babylonia), whereby the “Four Generals/Governors” will be the “Generals/Governors” of the provinces in the Seleucid East’ (Mitsuma 2002: English abstract). More recently, however, Mitsuma has suggested that the jurisdiction of the general above the four generals corresponded to the ‘Upper Satrapies, i.e. the whole of the Seleucid realm east of (the) Euphrates’ (Mitsuma 2007: 9).

The geographical extent of the term Upper Satrapies varied through time and we have no way of knowing just what jurisdiction might have been meant, if indeed this is how we are to understand the title, by the references in the astronomical diaries to the general above the four generals. Are we talking about an area as extensive as has been assumed for the lifetime of Alexander, when no fewer than 14 satrapies (Persis, Paraitakene, Karmania, Media, Tapuria, Parthia [with Hyrcania], Bactria, Areia [with Drangiana], Gedrosia, Arachosia, Paropanisus, and India) are thought to have been subsumed under this designation (von Gutschmid 1888: 6-7)? Was it the much smaller if no less far flung area assigned to the Upper Satrapies by Diodorus in his discussion of Eumenes’ attempt, after reaching Persis in 318 B.C., to gain the aid of ‘the satraps and generals in the Upper Satrapies’, which included 7 satrapies, viz. Persis, Karmania, Arachosia, Paropanisus, Drangiana, Bactria and India (Diodorus 19.14.1-8). However one is to understand Mitsuma’s interpretation, it is clear that there were far more than four eastern or upper satrapies in the Seleucid period, and that the designation ‘general above the four generals’ is not matched by a neat, quadripartite satrapal division east of the Euphrates.

Indeed, this same conclusion, reached by Mitsuma, suggested to him an alternative explanation for the presence of the numeral four. He wrote, ‘We would rather conjecture a symbolical use of the number “four” in the light of the title *šar kibrāt arba’i/erbetti* (King of the Four Quarters, i.e. the Entire World), which is used of Mesopotamian and Persian kings who ruled over vast territories.... the numeral in “the General who is above the Four Generals” may be used to imply the vastness of the territory under his jurisdiction’ (Mitsuma 2007: 10).

This hypothesis, which invokes an ancient title first attested under Naram-Sin (Hallo 1957: 49), is interesting, but it ignores an entirely different line of evidence from Iranian sources which suggests that a real, quadripartite division of authority attested in the Sasanian period may be relevant to our understanding of the

Seleucid and Parthian title under discussion here. Following a line of inquiry first opened by F. Gurnet, T. Daryaeae has interpreted four mint signatures – DYW-XW (dēwān of Xwarāsān, i.e. the northeast), DYW-AT (dēwān of Ādūrbādāgān, i.e. the northwest), DYW-KR (dēwān of Kermān, i.e. the southeast) – as evidence of quadripartition late (years 32-40, or 520-528) in the reign of Kawād I (Daryaeae 2002: 10). According to Tabari, on the other hand, it was Khusraw I (539-579) who replaced the office of general over the army of the entire empire (*Erān-spāhbed*) with four generals designated according to the cardinal points, viz. *xwarāsān spāhbed* (General of the East) *nēmroz spāhbed* (General of the South), *xwarbarān spāhbed* (General of the West), and *abaxtar spāhbed* (General of the North) (Nöldeke 1879: 155). Later writers (al-Dinawari, Yaqubi, Ferdowsi) give more precise information about the composition of these four regions. Thus, al-Dinawari says that the East comprised Khorasan, Seistan and Kerman; the West comprised Isfahan, Qom and Jibal; the South comprised Fars and Ahwaz (Khuzestan); and the North comprised Iraq up to the Byzantine border (Nöldeke 1879: 155, n. 2). Interestingly, Mas'udi says that the institution of four generals was an innovation of Ardashir's in the early 3rd century (Gignoux 1984: 5-6 and n. 25). The quadripartite division of Iran into a western (*K'usti Xorbaran*), southern (*K'usti Nemroğ*), eastern (*K'usti Xorasan*) and northern (*K'usti Kapkoh*, lit. Caucasus) is explicit in a geographical description of the Sasanian empire by the Armenian writer Pseudo-Moses of Khorēn, dating to 8th or 9th century (Marquart 1901: 6, 16-17), while the same titles attested in Tabari are preserved in the 9th century *Bundahiān* (Gyselen 2001: 5).

P. Gignoux suggested that the literary evidence of this quadripartite division of the empire was fictitious, 'un thème littéraire plutôt qu'une réalité historique', and like Mitsuma, he invoked Naram-Sin's famous title as a parallel expression simply meant to gloss domination over the entire world (Gignoux 1984: 4). Over the past two decades, however, epigraphic evidence has come to light confirming that the quadripartite division of the Sasanian empire was not a fiction. In 1988 Gignoux himself presented a fragmentary seal impression on which the legend *nēmroz spāhbed* (General of the South) could be read (Gignoux 1991) and in 2001 R. Gyselen published further seal impressions, dating to the reigns of Khusraw I (531-579) and Hormizd IV (579-590), which document the office of the other three generals of the Sasanian empire (Gyselen 2001). The seal impressions present us with titles that are effectively an amalgamation of the terminology preserved by Tabari and Pseudo-Moses of Khorēn, viz. *ērān kust ī xwarāsān spāhbed* ('General [*ērān-spāhbed*] of the side of the East'), *ērān kust ī nēmroz spāhbed* ('General [*ērān-spāhbed*] of the side of the South'), *ērān kust ī xwarbarān/xwarārān spāhbed* ('General [*ērān-spāhbed*] of the side of the West') and *ērān kust ī ādūrbādāgān spāhbed* ('General [*ērān-spāhbed*] of the side of the North').

It is not my contention that the Sasanians were cognizant of the much earlier Seleucid and Parthian title 'general who is above the four generals', but I think the Sasanian evidence for a quadripartite division of military authority must raise a question mark over the interpretation of the numeral four in the earlier title as merely a rhetorical device harkening back or parallel to the Old Akkadian 'king of the four world quarters'. The military exigencies which caused the Seleucids in Babylonia to re-organise their military command structure in the 3rd century B.C. may have been very different from those which motivated the reform of Sasanian Iran's high command. Yet, if the Seleucid generals were indeed responsible for the Upper Satrapies, as Mitsuma has suggested, and if the satrapies of 229 B.C. were even half as extensive as Diodorus' account of the situation in 318 B.C., then one can see that the geographical imperatives confronting the Seleucids and Sasanians were not entirely dissimilar. In any case, though the parallel adduced here is not precise, I do believe that the title used in the astronomical diaries very probably reflected a real, quadripartite division of military responsibility, comparable to that seen in the Sasanian period, rather than a semi-mythic allusion to the four world quarters and the vastness of the empire thereby implied.

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52) Zēria, šatammu von Esangila und die Entstehungszeit des „Strophengedichts“ – Im « Strophengedicht»¹ werden zwei Beamte genannt, die Nabonids Neuinterpretation des Marduktempels als Tempel des Šin unterwürfig zustimmten. Zum einen ist dies der *zazakku* Rēmūt, zum anderen Zēria, der *šatammu* von Esangila. Beide Beamte sind in einigen Briefen, die in das Jahr 17 Nbn datieren, und in administrativen Urkunden aus demselben Jahr belegt.² M. Jursa hat kürzlich darauf hingewiesen, dass der *zazakku* Rēmūt auch nach dem Regimewechsel noch bezeugt ist und zwar bis in das 5. Jahr Kambyses'.³

Im Folgenden wird vorgeschlagen, dass der Absender des Briefes YOS 3, 196, der in das Jahr 9 Kyr datieren muss, Zēria, der *šatammu* von Esangila, war. Folgt man der Identifikation, hätte auch dieser Beamte den Regimewechsel unbeschadet überstanden.

Die Grußformel des Briefes nennt Bēl und Nabû, was auf einen Absender aus Babylon weist, der ein Gleich- oder Höhergestellter gegenüber den Adressaten ist, da die Anrede „meine Brüder“ lautet. Der Name des ersten Adressaten, Kīnāja, ist der Kurzname des *šatammu* von Eanna, Nabû-mukīn-apli/Nādin/Dābibī, der zwischen 6 Kyr und 6 Kam im Amt war. Der zweite Adressat, Nabû-ahu-iddin, ist der *ša rēš šarri bēl piqitti* von Eanna von 17 Nbn bis 4 Kam. Zēria teilt den beiden Eanna-Beamten mit, dass ein zweiter Ulūlu eingeschaltet wurde. Somit kommt nur das Jahr 9 Kyr in Betracht (im Jahr 2 Kyr gab es ebenfalls einen Schaltulūl, aber zu dieser Zeit war noch Nidinti-Bēl *šatammu* von Eanna).

Wir befinden uns in der Zeit vor der regelmäßigen Anwendung des Saros-Zyklus. Die für die Monatsschaltungen maßgeblichen Höhen der Sterne sind für Nord- und Südbabylonien unterschiedlich, eine einheitliche Kalenderregelung musste daher von der Hauptstadt ausgehen. Der Ort der Beobachtung war der Esangila-Tempel. Die Mitteilung an andere Tempel machte der König selbst (YOS 3, 115, datiert in 15 Nbn) oder die Beamten von Esangila (YOS 3, 15, Brief der *qīpānu ša esangila*). In YOS 3, 152 berichten sechs Personen, größtenteils mit Marduk- oder Nabû-Namen, dem *šatammu*, den Tempelbetreibern (*ērib bīti*) und dem Kollegium (*kiništu*) von Eanna die Einschaltung eines Addāru. Es wird sich bei diesen Absendern um Schreiber bzw. Astronomen des Esangila-Tempels handeln. Aus diesen Gründen ist es naheliegend, Zēria, den Absender von YOS 3, 196, mit dem *šatammu* von Esangila zu identifizieren. Obwohl ein völlig sicherer Beweis nicht erbracht werden kann, ist die Wahrscheinlichkeit dieser Identifikation im Kontext der Briefe aus dem Eanna-Archiv sehr hoch. Sie ist auch nicht verwunderlich, angesichts der Tatsache, dass der *zazakku* Rēmūt noch unter Kambyses im Amt war. Dies leitet zur Frage nach der möglichen Entstehungszeit des „Strophengedichts“ über. Das schlechte Licht, in das die beiden Beamten dort gerückt werden, schließt eine Entstehung noch während ihrer Amtszeit aus. Damit wäre der *terminus post quem* das Jahr 5 Kam. Kyros und Kambyses hatten keine Veränderungen in den lokalen Machtverhältnissen vorgenommen: Wenn Beamte kollaborierten — und das taten sie in der Regel — hatten sie gute Chancen, unbehelligt zu bleiben. Die Phase der Stabilität, die für Regimewechsel keinesfalls untypisch ist,⁴ endete mit der Krise in 522 und 521. Die beiden Usurpatoren Nebukadnezar III. und IV. knüpften direkt an das babylonische Königshaus an, indem sie behaupteten, Söhne Nabonids zu sein. Wie auch immer das Andenken Nabonids in der Oberschicht gewesen sein mag, zumindest konnte ein Herrschaftsanspruch, der sich aus der Abstammung von ihm ableitete, Unterstützung finden. Angesichts des offensichtlichen Endes der direkten Nachfolge Kyros' wurde der Legitimität, die Kyros noch gemäß traditioneller babylonischer Ideologie aus der „Erwählung durch Marduk“ aufgrund des (behaupteten) Missverhaltens Nabonids ableiten konnte,⁵ eine andere Legitimität entgegengesetzt. Nach der Niederschlagung der Aufstände hat Darius im Eanna-Tempel sämtliche Beamten ausgetauscht.⁶ Seit dem Ende der neubabylonischen Dynastie in 539 waren gerade 18 Jahre vergangen, ein Zeitraum, in welchem konkrete Gegebenheiten, wie z.B. Beamtennamen, eine Hungersnot,⁷ den Palastbau in Teima⁸ oder die unorthodoxe Favorisierung des Mondgottes⁹ neben dem langjährigen Ausfallen des Neujahrsfeierlichkeiten von Babylon noch in persönlicher Erinnerung waren. Die Negativdarstellung Nabonids war nicht vollständig aus der Luft gegriffen, sondern nahm reale Gegebenheiten auf, um das „kollektive Bewusstsein“, zumindest innerhalb einer Oberschicht,¹⁰ aus der sich die Führer der Aufstände zweifellos rekrutierten, in eine gewünschte Richtung zu lenken. Ein anderer möglicher Entstehungszeitpunkt des „Strophengedichts“ wäre die Zeit nach 2 Xer. Allerdings wurden zu diesem Zeitpunkt die traditionellen Eliten nahezu vollständig entmacht.¹¹ Seit dem Ende des neubabylonischen Reiches waren bereits 55 Jahre

vergangen. Das Strophengedicht ist mit der Nennung der Beamtennamen sehr personalisiert, die Zeit nach 2 Dar ist daher m.M. nach zu favorisieren. Die Adressaten dieser Propaganda wären die verbleibenden Sympathisanten Nebukadnezars IV. innerhalb der zur Zeit Darius I. noch immer recht einflussreichen traditionellen Oberschicht Babyloniens.

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1. H. Schaudig, Die Inschriften Nabonids von Babylon und Kyros' des Großen (AOAT 256), Münster 2001, S. 563ff. mit weiterführender Literatur.

2. P.-A. Beaulieu, *JNES* 52 (1993), 258f. hat die Belegstellen zusammengestellt. Zēria wird außerdem in Holt, *AJSL* 27, Nr. 15 ([...] Nbn) genannt, cf. M. Jursa, The Transition of Babylonia from the Neo-Babylonian Empire to Achaemenid Rule. In: *Regime Change in the Ancient Near East*. Proceedings of the British Academy 136 (2007), 81¹⁹.

3. Jursa, loc. cit., 81 mit Fn. 21.

4. Jursa, loc. cit. S. 78.

5. Vgl. den „Kyros-Zylinder“, Schaudig op. cit. S. 550 ff.

6. E. Frahm und M. Jursa, *Neo-Babylonian Letters and Contracts from the Eanna Archive* (YOS 21) (im Druck); K. Kleber, *Tempel und Palast. Die Beziehungen zwischen Tempelhaushalt und königlicher Verwaltung im spätbabylonischen Uruk*. Dissertation Universität Münster (in Vorbereitung). Für die anderen Städte ist die Dokumentation nicht dicht genug, um einen Beamtenaustausch oder eine Beibehaltung zu konstatieren.

7. In 11 Nbn gab es eine Hungersnot, s. YOS 6, 154. Im „Strophengedicht“ Col. I, 3' wird eine Hungersnot erwähnt, was freilich auch bloß ein literarischer Topos sein kann.

8. Im „Strophengedicht“ Col. II, 28' ff. wird dieser Palast mit dem von Babylon verglichen, was sicher als Negativfaktum interpretiert wurde. Die Frohnarbeiter „jammern laut“ (Z. 32₄). Alle anderen Könige, inklusive Kyros, der darin explizit mit Nebukadnezar verglichen wird (Col. VI, Z. 10'), haben statt dessen Babylon ausgebaut.

9. P.A. Beaulieu, The Reign of Nabonidus, King of Babylon 556-539 B.C. (YNER 10), 1989, besonders S. 62ff. E.A. Braun-Holzinger, E. Frahm, Liebling des Marduk — König der Blasphemie. Große babylonische Herrscher in der Sicht der Babylonier und in der Sicht anderer Völker. In: J. Renger (Hrsg.), *Babylon: Focus Mesopotamischer Geschichte, Wiege früher Gelehrsamkeit, Mythos in der Moderne* (CDOG 2), Saarbrücken 1999:150.

10. Schaudig, op. cit., 564 bezeichnet das Strophengedicht als „nicht für das „Volk“, sondern als Traktat für die Gelehrten bestimmt“.

11. C. Waerzeggers, *AfO* 50 (2003/04), 150-173; K. Kessler, *AoF* 31 (2004), 237-262.

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53) Scribal Mistakes in Lexical Excerpts Found at Ebla – Not unexpectedly, in the Ebla excerpts of bilingual lists scribal mistakes may be detected. The following are some possible examples from *MEE* 4 89-90 and *MEE* 4 80.

(a) TM.75.G.3162+3163 = *MEE* 4 89-90

In this excerpt (*EV* q, Pettinato 1982: 105f., photograph not available) two mistakes have been already noted by the editor in obv. VI 7-10 (see below). The text, in which rare terms occur, is in fact full of mistakes.

(1) In obv. I 5-6 read šu:gar / *si-bi-tum*, cf. *VE* 150, šu-gar = *si-bi-tum*, *šipirtum*, Akk. *šipru(m)*, “(piece of) work, artefact, handiwork” (Pettinato 1981: 273; Krebernik 1983: 6; Civil 1984: 78, 1987a: 152 n. 27; von Soden 1988: 330; Conti 1990: 88; Fronzaroli 1996: 60). Therefore, *EV* 0272 does not exist. Interestingly, the same writing šu:gar is also found in the excerpt of monolingual list *MEE* 15 29 obv. II 43.

Notice in *MEE* 4 89-90 obv. II 5 and 7 the writings AD₆.A-na and AD₆.A-geštin, also found in the monolingual source *MEE* 15 1 rev. X 30-31¹. Two of the main bilingual lexical sources have instead the writings A.AD₆-na and A.AD₆-geštin (*VE* 634, *VE* 636). Moreover, cases of faulty writing of some signs in *MEE* 4 89-90 do occur in rev. I 8 (read šu-dag¹(É)) and 9 (read šu-ta-kal¹(GUR)).

(2) In obv. IV 2-3 read <ŠE->SIG₄-NE-NE / *gi-za-um*, cf. *VE* 665, ŠE-SIG₄-NE-NE = *ga-za-um*. The equivalence is uninterpreted (Conti 1990:178), but certainly it refers to an anatomical part or feature of the human head.

(3) In obv. IV 6-9 read :

IV 6 ŠE+TIN-dul
IV 9 gú-MAR-núm
IV 8 DUG×ŠE
IV 7 gú-NI-lu ša(-)NI-ba-nu

In one of the main bilingual sources ŠE+TIN-dul is translated as *gú-mu-ra-nu-um* (*VE* 648a). This equivalence has been interpreted as a “pile of fruits” (Civil 1987b: 236; Conti 1990: 175 n. 513; Fronzaroli 1994: 126), therefore probably *kumrānum* < **kmr*. In *MEE* 4 89-90 the scribe wrote the same translation (if *gú-mura*_x(MAR)-n_{um}) or a lexical variant (if *gú-mar*<*ra*>-n_{um}, *kumarrānum*). The first solution

seems more probable to me, and cf. (after the photograph in *MEE* 4, Tav. XXI) the problems found by the scribe of the main bilingual source *MEE* 4 47-48 in obv. IV 23 – V 3 about *VE* 645, *VE* 647 and *VE* 648: ŠE+TIN / sa-[g]a-lum / <ŠE+TIN-kur> // ša-gúm / ŠE+TIN-dul / gú-<mura_x(MAR)->núm.

Accordingly, in the next equivalence of *MEE* 4 89-90 DUG×ŠE (presumably “(a container for barley)”) is translated as *gú-NI-lu ša(-)NI-ba-nu*, which is still uninterpreted.

For these reasons *VE* 648 should be entirely rearranged.

(4-5) In obv. V 2-3 read ugu_x(SAG×NI) {SUD} / mu-ḫu-um, while in obv. VI 2-3 read mu-ní-za / <azad_x(SAG×NI.SUD)> / gi-si-da-núm.

The first equivalence certainly refers to the “skull, upper part of the head, brain”, *muḫḫum*, Akk. *muḫḫu(m)*. At Ebla in *EV* 0211 = *VE* 1144 “personal name” is mu-ní-za = *su-mu-um*, *šūmum*, Akk. *šūmu(m)* (Krebernik 1983: 40; Pettinato 1984: 29). I suggest that the scribe forgot <azad_x(SAG×NI.SUD)>. This correction of the ancient text allows us to avoid an ununderstandable translation “*gi-si-da-lum*” for mu-ní-za. I make reference to *VE* 266 and *EV* 0347, where azad_x is translated as *qištu qaqqadim*, “(a designation of hair on the head)”, of which *gi-si-da-núm*, *qištānum*, may be a lexical variant (as for ugu_x and azad_x and for the confusion made by some Ebla scribes about these terms see a paper by A. Catagnoti and myself in the next issue of *Vicino Oriente*, where the other anatomical terms dealt with in the present note are also discussed).

(6) The whole section dealing with the “hand” in obv. VI 6 – rev. I 2 has been faultily arranged by the scribe. Read

VI 6 šu-šà
 VI 8 šu-dagal-gal
 VI 7 ba-’à-núm
 <šū-sal>
 VI 9-10 gi-bí-la-du / ra-’à-tum
 VI 11 šu-tur
 I 1-2 iš-ba-um / tur

Here there are four entries. The first one deals with the “open hand, palm of hand”, here untranslated, *rāḫatum* in *VE* 516, šu-šà = *la-’à-tum* (“hand(s)” as *qātu(m)*, Akk. *qātu(m)*, and *qathātu(m)* or *qathātu(m)*, occur(s) in *VE* 690, še-šu = *ga-tum*, = *ga-da-’à-tum*, Conti 1990: 180 and nn. 534-536, with literature). The second one (*VE* 499) deals with the “thumb” (*bahānum*). The third one (*VE* 517) deals with the “palm of the hand” (*qiblatu rāḫatim*). The fourth one (*VE* 500) deals with the “finger” (*išbā’um šagirim*).

(b) TM.75.G.1965+ = *MEE* 4 80

This excerpt (*EV* h, Pettinato 1982: 99f., photograph not available), in which rare terms occur, should be joined to the small fragment TM.75.G.3220, published as *ARET* III 189 (this fragment is discussed in Bonechi 1988).

(1) *MEE* 4 80 obv. IV 5 - V 1 looks faultily arranged by the scribe (see for more on this Fronzaroli 2007). Read

IV 5’ sum₄(LAK-324)¹-dù
 IV 6’ su-mu-du-ù
 <...>
 IV 8’ SUM₄(LAK-324)¹
 IV 7’ wa-zi-gu-um
 V 1 [...]
 V 2 [...]
 V 3 na-sa-gúm

The first entry, with a forgotten translation, means “to be(come) bearded” since the Sumerian term (here with its syllabic spelling) is translated *ša-ga-núm*, *daqānum*, and *ša-gu-nu-um*, *daqqūnum*, in *VE* 199 and in *EV* 0175 (Krebernik 1983: 8, 1984a: 200, 1984d: 646, 2006: 85; Parpola 1988: 296; Fronzaroli 1989: 8 and n. 18; Steinkeller 1992: 19; Conti 1993: 104, 1996: 197; Picchioni 1997: 156 n. 23). After Fronzaroli 2007 the second entry means “tissé; tissu”.

As for V 1-3 (the text has been kindly checked by P. Fronzaroli), where there is at least one other entry, two conjectures come to mind.

The first conjecture refers to *EV* 0168, zú-ku₅ = *na-ša-gú-um wa bur-ra-zu-um a-ba-lu*, = *na-ša-gúm wa bur-ra-zu-um a-ba-a*, and to *VE* 195, zú-ku₅ = *bù-ra-zu-um*. This equivalence is only partially understood (Bonechi 2003, with literature), but *na-ša-gú-um*, *na-ša-gúm* may be interpreted as “bite”, Akk. *nišku(m)*, Ug. Akk. *našku*, < *nīk (less likely as “to bite”, Krebernik 1983: 8; Pettinato 1984: 45; Fales 1984: 176). It could be that the scribe of *MEE* 4 80 forgot the Sumerian term for “bite”. In such a case the equivalence was zú-ku₅ (lost in the lacuna or perhaps forgotten by the scribe) = *na-sa-gúm* (*naškum* after the common rules of the syllabary used at Ebla, or because *nīkum was already *naškum* at Ebla, or because of irregularities of the

syllabary similar – but in this case different – to those observed in Conti 1990: 10ff. as for one of the main sources of the Ebla bilingual list).

The second (and for me more likely) conjecture makes reference to VE 849, ne-šub₄(LAK-672) = ne-sa-gu(-um). Probably this equivalence means “kiss”, nišqum (Krebernik 1983: 33; Civil 1984: 96; Krecher 1984: 141; Conti 1990: 203, 1993: 104). It may be that in MEE 4 80 V:1-3 the equivalence was [ne-šub₄] = [(syll. spelling)] = na-sa-gúm, a lexical variant našqum of nišqum, or the infinite 0/1 našāqum, “to kiss”.

(2) It may be that in ARET III 189+ there is a fragmentary and faultily written entry to be read [inim-dug₄] / da-ba-rí {IN} ṛsa¹-ga-lum. I make reference to VE 210, inim-dug₄ = sa-ga-lu, šakrū, “word(s)” (Pettinato 1981: 260; Krebernik 1983: 6 n. 25, 1984a: 195 n. 19 and 199 n. 32, “sprechen”; Fronzaroli 1982: 104 n. 22, KA.KA, 1984a: 148, 1984b: 104 n. 22, inim-inim). In ARET III 189+ the translation could be interpreted as dabāri šakrī, probably “to utter words”. As for the use of forms of *dbr at Ebla cf. EV 0143, inim-dī = ne-me-ti = ti-da-bir₅-lum, tiddab(b)irum < *tiddab(b)irum (Krebernik 1984a: 203; Conti 1990: 95f.).

(3) MEE 4 80+ may include another peculiar feature rarely found in other Ebla lexical lists, that is a kind of “fast” spelling. In obv. V 5-6 the translation of gú-bal, gú-ga-li-bù, may be understood as “gú-li-bù and ga-li-bù”, the alternative being a quite unlikely quadriradical Semitic root with velar – velar – liquid – bilabial. The same feature is found at least in:

(a) VE 53, ninda-ì-gúg and VE 127, ninda-GÚG.GÚG, both = li-la-šu, that is “li-šu and la-šu”, litum, lawtūm, “dough” (Civil 1987b: 238 and n. 25; Conti 1990: 68 and 82f.);

(b) VE 584, šà-ki-ág = da-du-du, that is “da-du and du-du”, probably “love-making; object of love, darling, favorite”, dādum, dūdum, Akk. dādu(m) (Conti 1990: 164 and n. 467, with discussion and further literature).

However, I admit that I have no suggestions for

(c) VE 1031 = EV 0109, gú-bal = gú-ga-li-bù, that is “gú-li-bù and ga-li-bù”,

also because the other translation of gú-bal in VE 1031 = EV 0109, i.e. ù-šu-ru₁₂-um, ù-šu-lum, remains unexplained (formally this latter translation derives from a root *^ʿdr, *^ʿtr, *^ʿdr or *^ʿtr). As for the Sumerian term, probably cf. gú-bal (Nippur Ugumu 162), = arūtu (Bil. Ugumu D9), = erūtu (Hh XV 52), “nape of the neck”.

(4) In MEE 4 80+ rev. II 3-5 the text could be read ŠE-SIG₄-ŠE-SIG₄ / (blank) / [ma-ba-da]-a, cf. VE 664, ŠE-SIG₄-ŠE-SIG₄ = šu-ga-um, = ma-ba-da. This equivalence is still uninterpreted (Conti 1990: 178), but it should refer to a part of the head. ŠE-SIG₄-ŠE-SIG₄ and ma-ba-da(-a) show dual forms.

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| Bonechi 1988 | M. Bonechi, Un nouveau fragment d'un texte lexical bilingue éblaïte, <i>NABU</i> 1988/58 |
| Bonechi 2003 | M. Bonechi, The Second Prescription in the Pharmaceutical Text TM.75.G.1623 (III Millennium Ebla), <i>NABU</i> 2003/24 |
| Civil 1984 | M. Civil, Bilingualism in Logographically Written Languages: Sumerian in Ebla, in L. Cagni ed., <i>Il-bilinguismo a Ebla</i> , Napoli, pp. 75-97 |
| Civil 1987a | M. Civil, The Early History of HAR-ra: The Ebla Link, in L. Cagni ed., <i>Ebla 1975-1985</i> , Napoli, pp. 131-158 |
| Civil 1987b | M. Civil, Studies on Early Dynastic Lexicography III, <i>Or NS</i> 56, pp. 233-244 |
| Conti 1990 | G. Conti, Il sillabario della quarta fonte della lista lessicale bilingue eblaïta, in P. Fronzaroli ed., <i>Miscellanea eblaïtica</i> , 3, <i>QuSem</i> 17, Firenze |
| Conti 1993 | G. Conti, Il sistema grafico eblaïta e la legge di Geers, <i>Quaderni del Dipartimento di Linguistica</i> 4, Firenze, pp. 97-114 |
| Conti 1996 | G. Conti, Thèmes « assyriens » et thèmes « babyloniens » à Ebla, in P. Zemanek ed., <i>Studies in Near Eastern Languages and Literatures. Memorial Volume of Karel Petráček</i> , Prague, pp. 193-202 |
| Fales 1984 | F. M. Fales, A Survey of Two-Words Eblaic Entries in the Bilingual Vocabularies, in P. Fronzaroli ed., <i>Studies on the Language of Ebla</i> , <i>QuSem</i> 13, Firenze, pp. 173-187 |
| Fronzaroli 1982 | P. Fronzaroli, Per una valutazione della morfologia eblaïta, <i>SEb</i> 5, pp. 93-120 |
| Fronzaroli 1984a | P. Fronzaroli, The Eblaic Lexicon: Problems and Appraisal, in P. Fronzaroli ed., <i>Studies on the Language of Ebla</i> , <i>QuSem</i> 13, Firenze, pp. 117-157 |
| Fronzaroli 1984b | P. Fronzaroli, Materials per il lessico eblaïta I, <i>SEb</i> 7, pp. 145-190 |
| Fronzaroli 1989 | P. Fronzaroli, Il culto degli Angubbu a Ebla, in P. Fronzaroli ed., <i>Miscellanea Eblaïtica</i> 2, <i>QuSem</i> 16, Firenze, pp. 1-26 |
| Fronzaroli 1994 | P. Fronzaroli, Osservazioni sul lessico delle bevande dei testi di Ebla, in L. Milano ed., <i>Drinking in Ancient Society. History and Culture of Drinks in the Ancient Near East</i> , HANES VI, Padova, pp. 121-127 |
| Fronzaroli 1996 | P. Fronzaroli, À propos de quelques mots éblaïtes d'orfèvrerie, in Ö. Tunca - D. Deheselle ed., <i>Tablettes et images aux pays de Sumer et Akkad, Mélanges offerts à Monsieur H. Limet</i> , Liège, pp. 51-68 |
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54) Texte aus dem 'Palastarchiv' Nebukadnezars, außerhalb von Berlin¹ – Olof Pedersén's Katalog der Texte der Koldewey-Grabungen in Babylon, *Archive und Bibliotheken in Babylon* (2005), gibt erstmals eine wirkliche Vorstellung vom Umfang dieses Materials, einschließlich des zum Teil beträchtlichen Anteils an im Augenblick unauffindbaren Tafeln, die entweder in Bagdad sein sollten oder aber aus dem Grabungshaus gestohlen wurden (vgl. allgemein p. 6f.). Das aufgrund der frühen Veröffentlichung einiger Texte durch Weidner bekannt gewordene Archiv von Rationenlisten und ähnlichen Texten aus dem Nebukadnezarpalast, N1 in Pedersén's Terminologie, umfaßt im Katalog 303 Nummern, aber Pedersén zufolge sind nur 54 Tafeln physisch lokalisiert worden (47 in Berlin, sechs in Istanbul, eine im Field Museum in Chicago). Die Tafelfotos dokumentieren weiteres Material, aber von mehr als zwei Drittel der Nummern ist im Prinzip gar nichts bekannt. Bei anderen Archiven ist das Bild nicht weniger betrüblich.

Es ist allerdings zu erwarten, daß über die von Pedersén bereits in anderen Sammlungen aufgefundenen Koldeweytexte hinaus weiteres Material identifiziert werden kann; die Suche kann dank Pedersén's Arbeit ja nun begonnen werden. Da Museumskontext und Sammlungsgeschichte dabei wichtig sein werden und die Identifikation einer Tafel in einer Sammlung die Präsenz weiterer wahrscheinlich macht, empfiehlt es sich vielleicht, möglichst rasch einen Anfang zu machen. Wir geben hier eine Liste von publizierten und unpublizierten Palasttexten aus verschiedenen Sammlungen, deren Zugehörigkeit zu N1 bisher unerkannt geblieben ist.

Museumsnummer	Publikation	N1-Nummer nach Pedersén
BM 120052		238 (Bab 28325; Pedersén p. 116 Fig. 59)
ML 1.8	<i>ARRIM</i> 7, p. 47 no. 44	142 (Bab 28229; Pedersén p. 114 Fig. 55)
ROM 910x209.353	<i>ROMCT</i> 2, 40	vielleicht 104 (Bab 28191; auf der Basis des Datums)?
ROM 910x209.365	<i>ROMCT</i> 2, 43	vielleicht 112 (Bab 28199) oder 262 (Bab 28352) oder 282 (Bab 28373)
YBC 9652		?
YBC 9657		139 (Bab 28226, Pedersén p. 115 Fig. 57)
YBC 9168	<i>YOS</i> 17, 151	wahrscheinlich 235 (Bab 28322)
YBC 11563		wahrscheinlich 259 (Bab 28349)
YBC 11576		?

1. Diese Miszelle wurde im Rahmen unseres vom Fonds für die Förderung der Wissenschaftlichen Forschung (Wien) finanzierten START-Projekts *The Economic History of Babylonia in the First Millennium B.C.* verfaßt. Drei der unpublizierten Texte wurden auf der Basis von Umschriften von Projektmitarbeiterinnen identifiziert: YBC 9657 wurde von B. Janković ransliteriert, YBC 9652 und 11576 von Kristin Kleber.

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55) Remarques à propos de F. Karahashi, *The Locative-Terminative Verbal Infix in Sumerian* – Dans ASJ 22 (2000 [paru en 2005])¹ 113-133, Mme F. Karahashi a remis en question l'analyse des préfixes du

locatif-terminatif (/directif)² en élément pronominal + /i/ (ELS 234-238 ; v. en dernier lieu G. Zólyomi, dans : M.P. Streck [ed.], *Sprachen des Alten Orients* [2006] 30) et, reprenant une idée de M. Civil³, a proposé de dériver la série en -/i/ de la série en -/a/ suivi d'un loc.-term. *-I- : bi₂- < *ba + I, -ri- < *ra + I, etc. (cf. surtout pp. 116 sq.). Elle synthétise les résultats obtenus dans le tableau suivant (p. 117) :

Conjugation Préfixes	Dative (1SG., 2SG, 3SG.)	Comitative	Terminative	Locative
			Abla-Inst	LT
ba- imma- V- mu-	-a- -ra- -na-	-da-	-ši- -ta-	-ni- -ni-

Elle critique par ailleurs ma distinction entre le loc.-term. et le "préfixe local"⁴ et donne l'impression que celle-ci reposerait dans une large mesure sur la lecture différente d'un seul et même morphème : "Needless to say, it is impossible to distinguish be₇(BI₂)- from bi₂- and -re- from -ri-, be₇- and -re- containing the 'local prefix' while bi₂- and -ri- contain[ing] [sic] the locative-terminative infix" (p. 116)⁵.

Contrairement à ce que j'ai cru à la lecture de l'article de Karahashi, cette hypothèse semble être partagée par bien des collègues⁶, raison pour laquelle un bref commentaire ne devrait pas être inutile. Elle soulève essentiellement deux difficultés :

a) Sur le modèle de *ra + I > -ri- et *na + I > -ni-, on aurait attendu *ma + I > *mi-⁷. Comme ce n'est manifestement pas le cas, Karahashi propose, à titre d'hypothèse, que le datif de la 1^{re} sg. suivi de I est mu-e-, lequel remonterait à *mu + a + I (pp. 117 et 127 sq.). En fait, dans 4 des 7 exemples cités, -e- est l'ergatif de la 2^e sg., dans Houe araire 26 (ex. 35) le "préf. local" (traduire "La bière coule dans les bols"⁸). Restent Dial. 2:193 (ex. 34) et Houe araire 27 (ex. 36), tous deux de lecture et d'interprétations délicates⁹. Comparées à la relative fréquence de (-)mu(-u₃/u₈)(-) (v. n. 7), ces deux formes isolées et peu claires n'ont guère d'importance.

b) Contrairement à ce qu'affirment Karahashi et Rubio (v. n. 5), l'opposition entre loc.-term. et "préf. local" ne repose pas essentiellement sur différentes lectures du même signe. A l'ép. d'Ur III, ils sont systématiquement distingués (bi₂- vs ba-a-, -ri- vs -ra-a-, ni- vs -na-a-, etc.), à l'ép. pB distingués dans le cas de bi₂- vs ba-e- (ELS 244 sq.)¹⁰. C'est exclusivement dans le cas de -ri- que l'on peut hésiter entre plusieurs analyses différentes : loc.-term. de la 2^e sg., datif de la 2^e sg. + "préf. local" et ablatif + "préf. local" (ELS 244-246 ; pour l'ablatif, comp. Karahashi 128 sq.).

Karahashi elle-même est contrainte de supposer que ba-a- et ba-e- d'une part et bi₂- de l'autre sont différentes graphies d'une seule et même séquence morphématique, *ba + I (pp. 118 sq.)¹¹. Elle ne donne pas d'explication sur la grande rareté des alternances entre ba-e- et bi₂- dans les textes pB (pour LSU, cf. ELS 242) et sur le fait que bi₂- et ba-a- se rencontrent dans des contextes différents à Ur III (bi₂(-in)-du₁₁, ba-a-ġar/ġa₂-ar, etc. ; s'il y a "alternances", comme entre ki-ba ba-a-ġa₂-ar et ki-ba bi₂(-in)-ġa₂-ar, elles s'expliquent par l'opposition passif vs actif [B. Jagersma, NABU 2006/93]). En fait, même les quelques exemples cités par Karahashi aux pp. 119 sq. suffisent à faire ressortir une claire distinction : à une exception près (7b = Home of the fish 138 ; comp. toutefois la traduction de Black *et al.* dans LAS 243, C 1 sq.), tous les ba-a-/ba-e- sont attestés dans des formes intransitives ou passives¹² et tous les bi₂- dans des formes transitives actives. La conclusion s'impose d'elle-même : ba-a-/ba-e- et bi₂- sont morphologiquement distincts — d'après moi {ba + e/j} vs {bi}, mais là n'est pas l'essentiel.

J'aimerais terminer cette note par de brèves remarques concernant d'une part im-ma- (et im-mi-), de l'autre la structure des formes mu-ni- vs mi-ni-, mu-ra- vs ma-ra- et mu-ri- vs mi-ri-.

a) En Europe, im-ma- est aujourd'hui généralement analysé en {(i +) m + ba} (ELS 280 sq. avec litt. ant. ; B. Jagersma, BiOr. 50 [1993] 424 sq. ; D.O. Edzard, *Sumerian Grammar* [= HdO 71, 2003] 92 sq. ; G. Zólyomi, dans : M.P. Streck [ed.], *Sprachen des Alten Orients* [2006] 32)¹³, et ceci pour bien des raisons : 1) ba- et im-ma- ont une contrainte distributionnelle commune qui est frappante (ils peuvent être suivis de -an-da/ši/ta-, mais pas de -ab-da-/ši/ta-) et un comportement syntaxique identique ; 2) ils se rencontrent dans des contextes analogues (cf. par ex. B. Jagersma, *loc. cit.*) ; 3) (-)Vm-ma- alterne sporadiquement avec (-)Vm-ba- ; 4) les textes grammaticaux dérivent (-)Vm-ma- de {...} m + ba}.

Quoique Karahashi mentionne en passant (p. 115 n. 7) cette analyse, elle retourne sans discussion ni une quelconque justification à l'hypothèse de G.B. Gragg, à savoir que im-ma- est un préfixe inanalysable (p. 122 ; de même P. Michalowski [*loc. cit.* n. 6] et Rubio, *Sumerian Morphology* 1348 ["im-ma has to be regarded as a conjugation prefix in its own right"]). Je vois mal comment une telle manière de procéder peut contribuer à faire progresser la sumérologie.

b) Aux pp. 123-125, elle traite les alternances (neutralisées après préf. I) entre mu-ni- et mi-ni-, mu-ra- et ma-ra- et mu-ri- et mi-ri- (pas dans cet ordre), considérées le plus souvent comme purement graphiques. La chose me semble un peu difficile pour une double raison :

1) D'une part, il y a peu d'alternances dans les différents duplicats à l'intérieur d'une seule et même ligne. A en croire des statistiques que j'avais faites il y a quelques cinq ans, on a (dans les textes littéraires et

les inscriptions royales) :

mi-ni- // mu-ni- : 20 attestations sur 692.

ma-ra- // mu-ra- : 7 attestations sur 120.

mi-ri // mu-ri : 0 attestation sur 33.

2) Il y a par ailleurs des passages où les deux séries sont en opposition (ELS 273 ; M. Civil, JAOS 120 [2000] 676 ; Karahashi p. 124¹⁴).

Je n'ai pas d'explication convaincante à proposer pour ce phénomène, mais au lieu de le balayer sous le tapis, il faudrait l'étudier systématiquement. Théoriquement envisageable serait une opposition entre {m + ni/ra/ri} = /mni/, /mra/ et /mri/ ou /mini/, /mara/ et /miri/ vs {mu + ni/ra/ri} = /muni/, /mura/ et /muri/. La grande rareté de mi-ni- où -ni- est le loc.-term. de la 3^e sg. pers. (ELS 274 et n. 753) et l'absence de *ma-na-pourraient avoir des raisons purement phonétiques : contrairement au locatif -ni- = /ni/ (de là la non-occurrence de *mi-in-ni-), le loc.-term. (-Vn)-ni- recouvre probabl. /mni/ et le datif (-Vn)-na- /nna/ (B. Jagersma).

1. Dans le cas d'ASJ, il faudrait introduire une troisième date : celle de la livraison. Je n'ai pu obtenir ASJ 22 que par l'entremise d'amis, les voies usuelles s'étant révélées inefficaces. Je connais des collègues qui n'ont toujours pas reçu l'ouvrage — ce qui vaut pour moi concernant un autre volume de la série : S.M. Maul, *Die Inschriften von Tall Taban* [...], ASJ SS 22 (2005).

2. Les problèmes terminologiques soulevés par la dénomination de ces préfixes sont insolubles. "Locatif-terminatif" n'est pas très heureux, puisqu'inconnu en linguistique générale et en linguistique typologique, mais il a l'avantage, pour cette raison même, de ne pas susciter de fausses associations. "Directif", introduit par J. Krecher et utilisé aujourd'hui par la majorité des sumérologues, est imprécis, étant donné que dans son acception locale, /e, i/ dénote la position contiguë à un lieu, qu'il y ait ou non un mouvement précédent (un vrai directif est notre "terminatif"). Dans mes cours, j'utilisais parfois "adessif", lequel implique (contrairement à "directif", toutefois pas étymologiquement) une contiguïté sans mouvement précédent. Bref, on ne s'en sort pas !

3. Dans J.N. Postgate, JCS 26 (1974) 20 n. 11 ; cf. aussi Civil, AOAT 25 (1976) 90 n. 28.

4. Dans le cadre de cette note, la question de savoir si on a affaire à des allomorphes au sens strict (G. Zólyomi, B. Jagersma) ou dans une acception plus lâche (Attinger) est sans importance.

5. Plus explicite encore est G. Rubio, Sumerian Morphology, dans : A.S. Kaye (ed.), *Morphologies of Asia and Africa I-II* (2006) 1327-1379 (désormais Rubio, Sumerian Morphology) p. 1356 : "Attinger proposed a set of alternations that depend on different readings of the same sign (be₇- = bi₂- and -re- = -ri-), which is evidently grounded in Falkenstein's approach to similar problems of writing interface".

6. Cf. par ex. P. Michalowski, dans : R.D. Woodard (ed.), *The Cambridge Encyclopedia of the World's Ancient Languages* (2004) 44 et Rubio, Sumerian Morphology 1346 sq. et 1356 sq.

7. Selon ma reconstruction, le loc.-term. de la 1^{re} sg. est écrit (-)mu(-u₃/u₈)(-) ; v. en dernier lieu NABU 2004/75.

8. D'après Karahashi, le roi de la ligne précédente serait toujours le sujet ; si tel était le cas, il n'y aurait pas de raison de répéter lugal(-e) à la ligne suivante.

9. Dans Dial. 2:193 (cf. C. Wilcke, ZA 59 [1969] 91 n. 91), le texte publié de Nippur (TMH NF 3, 42 iii 36) a ugula ge₄-me-a-aš ġu₁₀-ne-ka šu-ġ[a₂ ...] (ce qui exclut l'interprétation de Karahashi), celui d'Ur (UET 6, 64 + 633 rev. ii' 6') [...] a-aš-ġu₁₀-ne šu-ġa₂ mu-e-šum₂. Pour Houe araire 27, cf. W. Sallaberger, *Der kultische Kalender* (1993) I 188 ; les duplicats oscillent entre lugal-e (F et H) et lugal (E et A"). Dans les deux passages, on a probabl. affaire au "préf. local" dans une forme verbale transitive, un phénomène, reconnaissons-le, très rare (ELS 242 et n. 607 ; v. aussi NABU 2000/44).

10. be₇- est une graphie non-standard pas sûrement attestée (ELS 245) ; elle n'a jamais joué un quelconque rôle dans mon argumentation.

11. Voir dans ba-a-/ba-e- une "variante" de "ba-i₃-" (comp. pp. 117 sq.) ne ferait que déplacer le problème : ba-a-/ba-e- et ba-NI- sont également strictement distingués (pour LSU, cf. ELS 242).

12. Pour na-ba-a-du₃ (6a), cf. ELS 246 et n. 627.

13. Je ne puis discuter ici la critique de M.J. Geller dans son compte rendu d'ELS (Or. 67 [1998] 131 sq.) ; elle me semble toutefois reposer sur une lecture un peu hâtive (il attribue ainsi l'analyse {i + m + b + a} à Falkenstein, laquelle aurait été adoptée par Thomsen ; pour nam-ba-/nam-bi₂-, cf. ELS 277 sq.).

14. Sa conclusion que "[t]his could show that the orthography did not matter to the scribes" est pour le moins surprenante.

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56) Le nom du désert en amorrite – ARM XIV 78 raconte une de ces histoires peu édifiantes d'où il ressort que le roi de Mari attachait plus d'importance à être solidaire (ou complice) de ses contribuables que le roi de justice dont on pare un peu trop souvent l'image royale des souverains mésopotamiens.

En reprenant le texte dans L^AP^O 18 929, j'avais été gêné par le e-ēš de la l. 11 qui clot l'énumération des noms tribaux et où il était question d'expédier les deux malheureux Zalmaquéens pour qu'on n'en entende plus parler. Il m'apparaît toujours qu'en faire comme Maurice Birot un « lieu habité », en renvoyant au mot de

glose *eššu* du CAD n'est vraiment pas une bonne idée, à aucun point de vue. À revoir la tablette (et la copie d'ailleurs de M. B.) il m'apparaît que le plus simple est en fait de lire *ia¹-ab^{ki}* et d'y voir simplement l'équivalent du *yab* ou *yabāb* des Arabes qui signifie « pays désert ». *yab* devait donc être le terme qui désignait le grand désert au sud de l'Euphrate et telle devait être la désignation des régions où passaient les Soutéens.

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57) Les rois de Tarmanni(we) — Dans ARM XXVIII (*Lettres royales du temps de Zimri-Lîm*), J.-R. Kupper a publié deux lettres (n°121 et 122) du roi Yakûn-Dêr dont la capitale était inconnue jusqu'à présent. D'après ARM XXVIII 121, il possédait une ville nommée Ṭâbâtum proche de Susâ, une capitale de l'ouest de l'Ida-Maraş. Cette Ṭâbâtum ne peut être confondue avec celle bien connue du Habur puisqu'à l'époque de Zimri-Lîm cette dernière appartenait au royaume de Mari (alors que Yakûn-Dêr présente la première Ṭâbâtum comme « sa ville ») et n'est pas localisée dans le pays d'Ida-Maraş (cf. mon article, « Lecture des *Archives Royales de Mari*, tome XXVIII : Lettres royales du temps de Zimri-Lim », *Syria* 80, 2003, p. 199-216).

Yakûn-Dêr est un roi tardif, contemporain de Šadum-Labâ (fin ZL 11-13) comme l'a mentionné J.-R. Kupper (ARM XXVIII, 179). C'est un proche de Lîmî-Addu d'Anamaš attesté en tant que roi de ZL 6 (LAP0 17 604) à ZL 12 (LAP0 17 566 [VI 31]).

En préparant un article sur Šuduhum (« Šuduhum, un royaume d'Ida-Maraş, et ses rois Yatâr-malik, Hammî-kûn et Amud-pâ-El », *Actes de la table ronde de Berlin de juillet 2006*, en préparation) j'ai trouvé parmi les archives de Mari, le fragment (inférieur droit) d'une lettre, M.9230, écrite peut-être par Ibâl-Addu ('joint' à ARM XXVIII 69 ?) ou éventuellement par Itûr-Asdû, qui doit avoir de temps en temps « prêté » son scribe à son voisin d'Ašlakkâ. Ce document donne le nom du royaume de Yakûn-Dêr. Les l. 8'-9' se laissent reconstituer ainsi : *ia-ku-un-de-er*, [*a-na lugal-tim ša ta-ar-m*]*a-ni₅-YI^{ki} be-lî ú-wa-ad-di-šu* = « Mon Seigneur a reconnu officiellement Yakûn-Dêr [à la royauté de Tarm]anniYU ». On retrouve déjà dans une lettre d'Itûr-Asdû (inédit A.482:23), où il est question du lieu et non pas des habitants, cette graphie particulière (*ta-ar-ma-ni₅-*YI^{ki}* ; cf. J.-M. Durand, *MARI* 5, 1987, p. 230). Elle est proche de celle de ARM XXVII 20:10 : « lugal *ta-ar-ma-an-ni-YI^{ki}* ». Ce suffixe -YU, qui semble devoir être distingué de la marque akkadienne de nîzbé, est peut-être d'origine hourrite, équivalent du -WE de certains toponymes de la région de Šemšâra comme Šimerrewé ou Nakabbiniwe (je ne vois pas de raison de la distinguer de Nagabbiniwe (*na-ga-ab-ba-ni-YI^{ki}*), lieu producteur de vin attesté à Mari mais sans rapport apparent avec l'Ida-Maraş !, *pace* M. Wäfler, *OBO* 2001, p. 132 et D. Charpin, *RA* 97, 2003, p. 26, qui signale la possible présence d'une Nagabbum dans le Sindjar), cf. J. Eidem, *The Shemshâra Archives 2: The Administrative Texts*, 1992).

À l'époque de Zimri-Lîm, Tarmanni(we) a été gouvernée par au moins trois rois successifs, Tamarzi, Šidqân (qui règne très peu de temps) et Yakûn-Dêr. Le détail de l'histoire politique du pays est néanmoins plus compliqué car Šidqân revint au pouvoir en ZL 13. Les candidats au trône de Tarmanni(we) furent nombreux ; on en recense au moins deux autres : Yahhab-El (inédits M.9230, A.3655) et Bunûma-Addu (cf. B. Lion, « Les familles royales et les artisans déportés à Mari en ZL 12' », *Amurru* 3, 2004, p. 217-224 et particulièrement p. 219-220). Bien que l'intrigue traitée dans le fragment M.9230 soit difficile à saisir, elle se situe lors de la crise de succession d'Ašnakkum, crise qui affecta toute la région entre le Hirmaš et le Habur (cette question sera traitée dans un prochain article « La crise de succession d'Ašnakkum et les désordres politiques dans le Sûmum », en préparation). S'y illustra notamment Ifî-Sûmu, mentionné dans M.9230.

Sur le plan géographique, on tiendra comme acquis désormais le fait que le petit royaume de Tarmanni(we) (partie occidentale du Haut-Habur) avait frontière avec Susâ à Ṭâbâtum (II) et que ces deux pays se contestaient la possession de plusieurs bourgades.

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58) « šum-ma pa-gu ú šum-ma sâ-ú-um » – J.-M. Durand vient de proposer dans un article récent « Dictons et proverbes à l'époque Amorrite », paru dans le *Journal Asiatique* 294, 2006, une relecture de plusieurs passages des textes de Mari, où il propose de voir des proverbes, sans que ces derniers soient annoncés par le mot *têltum* : « cela vaut tout particulièrement pour les soi-disant "citations littéraires", ce qui représente traditionnellement dans toutes cultures un sous-groupe de proverbes ou locutions proverbiales dans la mesure où ces citations puisant dans un stock à disposition de tout le monde servent éminemment à illustrer un propos et à l'expliquer ». (*JA*, p. 21 : les exemples sans *têltum*). Il s'agit souvent, en fait, de manières de s'exprimer, allusions à des proverbes ou employant un style proverbial. Je voudrais dans cette note proposer une nouvelle piste pour trouver d'autres expressions de ce genre.

La langue arabe, en effet, comporte plusieurs proverbes qui ont comme principe de comparer deux

animaux différents pour rendre compte de la même situation ; par exemple : pour quelqu'un qui a quitté sa femme pour une autre moins belle, on dit qu'il a remplacé ses gazelles par des singes. « بدل غزلانه بقرود » « badala gizlânahu biqurrûd ». On voit ici la comparaison implicite de la beauté de la gazelle avec celle de la guenon.

Si l'on applique à la langue akkadienne ce même principe de comparer les animaux d'après leurs particularités, on voit dans le texte A.3297+A.3775 (publié par N. Ziegler dans son article « Samsî-Addu et la combine Sutéenne » dans *Amurru* 3, p. 95-110) Samsî-Addu demander aux Sutéens de lui apporter le cadeau royal qui doit se trouver avec l'ambassade de Yamhad et Karkemiš vers Babylone, que ce soit un singe, ou un « *sâ'ûm* » :

[ù ta-ma]-^rar^l-ta-šu-[n]u ša iš-tu ma-a-tim e-li-tim na-šu-ú
[šum-m]a pa-gu-ú šum-ma sà-ú-um
[ša] ki-ma q^r-iš-ti-ia^l [š]u-bi-la-nim.

« ...[mais] leur [cadeau] royal (*tâmartum*), qu'ils apportent depuis le Haut Pays, que ce soit un singe ou bien un *sâ'ûm*, envoyez-le moi comme étant un cadeau pour moi. » (traduction d'après N. Ziegler, *ibidem*, les lignes 40-44).

L'application du principe d'opposition que nous avons énoncé ci-dessus montre que le premier devrait être un animal rare et l'autre de peu de prix. Pour ce qui est du singe, le fait est sûr car plusieurs références sont données par le CAD P, p. 17 que des tributs comprennent des singes, originaires de l'Ouest du Proche-Orient ; par exemple : de la côte syrienne : « [nams]juha pa-gu-ta rabîta... : I caught a crocodile, a large female monkey... », aussi : « namsuha pa-gu-ta rabîta ša ah tâmti lu amhur... : I received a crocodile and a large female monkey from the sea-shore », « pa-gu-tu rabîtu pa-gu-ta šihirta šêbultu ša GN ahi ša Puratte šaknu lu amhur : I received a large female monkey and a small female monkey, gifts from Tur-Adini, (which) lies on the bank of the Euphrates » ; aussi de Lubarna de Hatti « pa-gu-tu rabîtu... maddattušu amhuršu : I received a large female monkey as his tribute ».

L'article de B. Lion (« La circulation des animaux exotiques au Proche-Orient antique », dans *RAI* 38, p. 357-365, spécialement p. 359) montre que les singes sont exotiques et sont importés de l'Afrique jusqu'à la côte méditerranéenne.

Le *sâ'ûm* est plus difficile à définir. Le terme *sâyum* est connu pour désigner un vase en forme d'animal, exclusivement fabriqué en or, attesté par les travaux de M. Guichard, (*La Vaisselle de luxe des rois de Mari*, ARM XXXI, p. 295-300) ; cela nous donne la preuve que cet animal est bien existant de façon permanente en Syrie, et que les fabricants locaux qui font les vases en ont une idée précise.

On notera dans son très riche commentaire l'inédit A.2968^t, qui montre qu'un *sâyum* fait partie d'une série de présents apportés par un ambassadeur mariote à Hammu-rabi de Babylone en l'an 4 ZL, sans indiquer la matière (or ou argent) : plutôt qu'un vase, un animal vivant.

La difficulté à définir ce *sâ'ûm* vient de ce que l'on a toujours supposé qu'il devait avoir une grande valeur marchande, et qu'il soit exotique en parallèle avec les autres animaux qui font l'objet d'un cadeau aux rois étrangers.

J.-M. Durand me renvoie à un document de Sabi Abyad T.98-115, publié par F.A.M Wiggermann (« Agriculture in the Northern Balikh Valley: the Case of Middle Assyrian Tell Sabi Abyad », *PIHANS* 88, 1999), où on trouve un bordereau de dépenses de grain pour des établissements agricoles dont l'un s'appelle « uruhu-zi-ra-nu » et qui, comme son nom l'indique, devait traiter de l'élevage des porcs, et un autre « urusa-a-ú-nu ». Il est vraisemblable d'après ces textes qui proviennent de la vallée du Balih, que cet établissement s'occupait de l'élevage d'un autre animal grégaire, pour lequel l'oie (ou le canard) est sans doute le meilleur candidat *a priori*.

On trouve en effet dans une liste OB d'oiseaux publiée par J.A. Black et Al-Rawi (« A Contribution to the Study of Akkadian Bird Names », dans *ZA* 77, 1987, p. 117-126, IM. 90646, ii, 13, p. 124) que : ^rsà^l-a-ú-um^{mušen} est un nom d'oiseau. Pour un commentaire à propos de ce dernier, cf. L. Kogan (dans *SED* II, p. 32), qui pose le problème de son rapport avec l'akkadien *ûsu*.

Il est donc vraisemblable, qu'avec le singe et l'oie, « *šum-ma pa-gu ú šum-ma sà-ú-um* », nous avons affaire à un proverbe, ou au moins à une façon proverbiale de dire, utilisée par Samsî-Addu (on notera que N. Ziegler propose que la lettre a été écrite par la propre main de Samsî-Addu, *ibidem*, p. 100) pour indiquer aux Sutéens de lui apporter le cadeau qu'ils trouveront, qu'il soit de grande valeur, comme le rarissime et exotique singe, qui devrait venir d'Egypte, ou de peu de valeur, comme les oies qu'on pouvait trouver dans toutes les fermes d'occident.

On retrouve, en tout cas, dans cette façon de dire du grand roi du Nord, un niveau d'expression particulier, bien souligné par J.-M. Durand : « Beaucoup de ce que l'on considère comme caractéristique de l'expression normale d'une langue nous manque dans l'important corpus akkadien d'époque amorrite ; la connaissance de ce niveau de langage permettrait de réinterpréter plusieurs des évidences actuelles du vocabulaire » (*JA*, p. 8 : apparition des proverbes dans ces sous-corpus), à savoir le recours à un vocabulaire

qui n'est pas celui de l'akkadien standard, comme le terme *sâ'ûm* pour désigner une oie, au lieu de l'*ûsum* de la langue écrite.

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