

Werner Nahm / Michael Roaf (eds)

**Eclipses, Volcanoes,
and Eponyms**

**New perspectives on the chronology of the
early second millennium BC in Mesopotamia**

PEETERS

ECLIPSES, VOLCANOES, AND EPONYMS

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Foreword

Since this book concerns chronology, I will begin by recalling the date 8th to 9th September 2015, when a conference entitled ‘New Perspectives on the Chronology of the Early Second Millennium BC in the Near East and Egypt’ was held at the University of Lille, organised by Denis Lacambre and Werner Nahm. Nine years later, we have before us a book arising from the work presented at this conference. The contributions incorporate research that has continued since 2015 and take into account the latest discoveries. Some authors have contributed work that is quite different from the topics they covered in Lille. When the organisers asked me to provide ‘Closing remarks’ at the end of the conference, I emphasised methodological issues: it may be useful to briefly recall some of these at the outset of this volume.

Regarding relative chronology, it is very important to remain aware of the fragility of many data that are too often taken for granted. A crucial document is the text referred to by M. Birot as the ‘Assyrian Chronicles of Mari’: Nele Ziegler and the late Klaas Veenhof revisit the edition of these tablets here, and several chapters of the book address various aspects of their content. Baptiste Fiette demonstrates that the royal lists, compared with the contemporary documents of the kings cited, may contain errors. We must consider the purpose of these lists and the context of their compilation, which we need to reconstruct. Guido Kryszat highlights that the system of dating by eponyms is not as straightforward as it appears at first glance. Furthermore, the invaluable eponym lists from Kültepe (KEL) have gaps: Gojko Barjamovic discusses here the KEL G list, providing collations that necessitate revisions. We have observed with Denis Lacambre and Nele Ziegler how complicated the positioning of a specific eponym – in this case Ahiyaya – can be, with each scholar having entirely convincing objective reasons to support their conclusions, yet the conclusions are incompatible with one another. When lacking chronological lists, the epigrapher is not entirely without tools: Amanda Podany shows in the case of the ‘Land of Hana’ how a diplomatic approach to the documents (internal characteristics: formulas, etc., or external characteristics: format of tablets, etc.) can lead to progress.

The second step is establishing synchronisms: this began with major dynasties (Isin, Larsa, Babylon), and gradually expanded in scope, facing difficulties for capitals for which we do not even possess a list of kings, such as Ešnunna. It is evident that the sometimes extreme political fragmentation during the Old Babylonian period does not facilitate the work, as shown in Rients de Boer’s chapter on Kish. However, some synchronisms are crucial, such as the one established between Rim-Sin of Larsa, Hammu-rabi of Babylon, and Samsi-Addu. In this regard, I cannot help but recall a discussion that took place during the Strasbourg conference in 1984: the discussion revolved around whether Yasmah-Addu remained on the throne of Mari for five years after the death of Samsi-Addu. I remember hearing an archaeologist shrug and say, ‘What does it matter, give or take five years!’ This is obviously a perspective that Assyriologists cannot share, given the nature and richness of the written documentation from that period.

We thus arrive at viewing the first half of the second millennium as more or less a single block; the subsequent question is how to fix its absolute chronology. A first reaction might be similar to that of Å. Sjöberg when he discussed a difficulty with his team regarding the Philadelphia Sumerian dictionary. When the difficulty seemed insurmountable, he concluded the discussion with: ‘Who cares?’... Regarding absolute chronology, the question can be legitimately raised. For some types of study, it does not matter at all. But for others, the question is crucial: this is particularly true for the relationships between Mesopotamia, the Levant, and Egypt. To transition from relative to absolute chronology, two different types of approaches are possible, some relying partly on texts, and others on archaeology.

Firstly, we will mention retrospective astronomy. This primarily concerns the famous tablet from the *Enūma Anu Enlil* series, which contains omens related to Venus, with one section seemingly dating back to the reign of Ammi-ṣaduqa. Peter Huber and Teije de Jong revisit this study, combining it with other types of research: lengths of months, revised radiocarbon dating of various Anatolian sites, etc. We also have specific events linked to an astronomical

event: the most famous is the solar eclipse associated in the Mari Eponym Chronicle with the birth of Samsi-Addu, which was revealed by Jean-Marie Durand and has since generated a large bibliography. Michael Roaf cautiously re-examines this complex case here. Finally, Werner Nahm considers all the data, also taking into account the eruption of the Aniakchak volcano in Alaska and concludes in favour of the ‘Lower Middle Chronology’, which places Hammu-rabi’s rise to power in 1784 BC (rather than 1792 as per the classical ‘Middle Chronology’).

The other approach permitted by the natural sciences relies above all on dendrochronology and radiocarbon dating. These are techniques that use plant elements to date. These techniques have evolved, with successive calibration systems aimed at reducing uncertainties. Problems arise when attempting to align the results of these studies with archaeology: there are favourable cases with well-dated epigraphic discoveries due to identifiable destructions. In this regard, the palace of Mari provides a fine counter-example, with its archives sealed by the destruction of Hammu-rabi, whose soldiers kindly left behind dated labels from year 32. However, as Michael Roaf noted during the conference, in this case, unfortunately, no radiocarbon analysis data has yet been published.

The book concludes with a presentation by W. Nahm on research into chronology during the early decades of Assyriology: this allows the reader to see how much the range of disciplines used today has broadened. The result of this multidisciplinary research is now available to all. The authors sometimes disagree: what could be more normal in ongoing research? Nevertheless, upon closing this book, one feels that the moment is approaching when a definitive solution will eventually be found. This volume surely marks a milestone in the quest for truth, and we must be grateful to the authors and editors of this volume for their tireless efforts.

Dominique Charpin
Paris, 1st November 2024

Preface

For historical research, and also for reasons of wider importance, the chronology of events in the Ancient Near East has long been a prominent topic of Assyriology. In the third quarter of the 19th century the comparison with biblical chronology raised strong emotions, until the discovery of a record of a solar eclipse in 763 BC in the Assyrian Eponym Chronicle settled the issue. Today, the co-operation with science is even more important, since the timing of the three dark ages of Egyptian and Mesopotamian history in relation to climate swings informs projections of future events in the 21st century. The second of these dark ages is marked by the irreversible decline of Assyrian trade and the fall of Babylon. The elucidation of the chronology of the preceding Old Assyrian and Old Babylonian age was the focus of a conference in 2015, which eventually gave rise to the present book.

In the programme SCIE M 2000 (The synchronization of civilizations in the Eastern Mediterranean in the 2nd millennium BC) of the Austrian Academy of Science, two meetings, namely the conference ‘Mesopotamian Dark Age Revisited’ in 2002 and the ‘Workshop on Mesopotamian Chronology’ in 2010 were dedicated to this topic, but at that time the problem still seemed far from a solution. At the 58th Rencontre Assyriologique Internationale (RAI) in Leiden and the 59th RAI in Ghent chronology workshops were organized by Klaas Veenhof, with Teije de Jong in 2012 and with Gojko Barjamovic in 2013. It became clear that a limited number of texts, namely *Enūma Anu Enlil* 63, the Kültepe Eponym Lists (KEL A to G), and the Mari Eponym Chronicle (MEC) were reliable keys for decisive progress. On the scientific side, dendrochronology, the study of arctic ice layers, and volcanology joined astronomy as crucial tools. With this background, Denis Lacambre (University of Lille) and Werner Nahm (Dublin Institute for Advanced Studies) organized an international colloquium on the 8th and 9th September 2015, with Dominique Charpin (Collège de France) and Gernot Wilhelm (Academy of Science and Literature, Mainz) as advisers. Funding of local expenses as well as the travel and accommodation of the participants was provided by the University of Lille and DIAS. The title of the conference ‘New Perspectives on the Chronology of the Early Second Millennium BC in the Near East and Egypt’ shows that we had hoped to treat Egyptian chronology, too, but this turned out to be premature. The meeting began with welcome talks by Catherine Denys, Vice-President of Research, and Stéphane Benoist, Director of HALMA (Histoire, Archéologie et Littérature des Mondes Anciens) at the University of Lille, and concluded with closing remarks by Dominique Charpin.¹

Due to unpredictable circumstances, the initial aim for an early publication of the papers was not fulfilled. Rafał Koliński and Tomasz Goslar and Felix Höflmayer have published their contributions elsewhere.² Thanks to the editors of *Orbis Biblicus et Orientalis*, Series Archaeologica, it has finally been possible to publish the contributions of the other participants.

We are very grateful to Christoph Uehlinger for his help in the preparation of this volume for publication, to Bert Verrept for expert proofreading and to Peeters Publishers for producing the book.

For convenience, each chapter has been provided with its own bibliography. Within the desired format of the series, the individual preferences of the authors have been respected: thus, for example, Hammurabi and Hammurapi and Samsi-Addu as well as Šamši-Adad and Šamšī-Adad appear. It is hoped that the reader will not find this misleading and that the Index will resolve such problems. As well as indexes of Peoples and Places, Personal names, and a General index, an index of the principal issues in four of the key texts concerning the chronology, namely the Assyrian King List, the Mari Eponym Chronicle, the Kültepe Eponym Lists, and the text FM V 2, has been provided.

In the almost ten years since the conference, several of the participants have been engaged in intense fruitful discussions. Compared to 2015 the papers have been extensively revised and, in several cases, extensively altered and

¹ Videos of these talks and 12 of those presented at the conference can be viewed at <https://webtv.univ-lille.fr/grp/427/nouvelles-perspectives-sur-la-chronologie-de-la-premiere-moitie-du-iiie-millenaire-av-c-au-proche-orient-et-en-egypte/>.

² Felix Höflmayer (2017), A radiocarbon chronology for the Middle Bronze Age Southern Levant, *Journal of Ancient Egyptian Interconnections* 13: 20–33; Rafał Koliński and Tomasz Goslar (2019), Radiocarbon dating of the transition from the Early to the Middle Bronze Age in Northeastern Syria, *Radiocarbon* 61/4: 953–970.

substantially extended. The result is that this volume is not the proceedings of the conference but is an up-to-date account of the present state of knowledge.

We share three basic attitudes in our methodology. They are not new, but, from time to time, it may be helpful to repeat them. First, many aspects of history can only be understood in their natural frame. Chronology is part of this frame and needs both science and scholarship for its elucidation. Thus, scientists should study the new results on eponym counts and linked historical events, while Assyriologists should take note that the well-known astronomical markers of time have been supplemented by volcanic eruptions and solar flares attested in tree rings and ice layers. Second, as individuals we often develop or maintain risky views, but as a group we prefer to be on the safe side. The reader of this book will find risky ideas that range from the intercalation of months in the Old Assyrian calendar to the origin of the eponym system, from arguments in favour of the High Chronology to a full generation count of half a millennium of the history of Hana. Some ideas will move from the fringe to the mainstream. Others will fall by the wayside when scrutinized. In both cases, the decision will take time, and the result cannot be known in advance. Third, and with particular emphasis, such a decision needs full access to the crucial data.

King lists and synchronisms have established the relative chronology of the interlinked dynasties of Ur III, Isin, Larsa and Babylon, though some problems remain even within these four dynasties. The king lists may have included only those rulers who were accepted as legitimate by later rulers or may be unreliable as is the case for the lengths of the reigns of the last two kings of the Old Babylonian dynasty. Furthermore, there are uncertainties in the relative chronologies of other dynasties, such as those of Kish and Hana. The correlation of the southern Mesopotamian sequence with that of Assyria is fixed through the synchronism of the death of Šamši-Adad I in the 18th year of the reign of Hammurabi. Unfortunately, much of the history of northern Mesopotamia before and after the reign of Šamši-Adad is unknown. The discovery of the Kültepe Eponym Lists as well as the Mari Eponym Chronicle has allowed the restoration of the sequence of the Assyrian eponym officials whose names identified the years in the earlier period though there are still some gaps in the sequence. The mention of a possible solar eclipse in a Mari Eponym Chronicle offers the possibility for dating this part of the sequence absolutely.

The pattern of attested intercalary months shows that the Venus observations in *Enūma Anu Enlil* 63 begin with the first year of the reign of Ammi-šaduqa and that they allow only a limited number of solutions 56 or 64 years apart. This rules out the so-called Ultralow or New Low Chronology. For over twenty years ¹⁴C determinations combined with tree-ring analyses of timbers from Anatolian palaces with connections to Assyria have restricted the search for an absolute chronology to one of the six variants of the High, Middle, and Low Chronologies. Within these limits, analyses of the distribution of lunar months recorded with 30 days rather than 29 days provide probabilities of the correctness of one of these chronologies.

The interpretation of the deviant observations of Venus in the twelfth year of the reign of Ammi-šaduqa as the result of volcanic dust from a volcanic eruption has led for the search of a suitable eruption observed in tree-rings and ice-layers. The most promising candidate is the Aniakchak eruption of 1628/27 BC supporting the Lower Middle Chronology, with the reign of Hammurabi starting in 1784 BC. No other volcanic eruption is known that would fit with the other chronologies.

These topics are discussed in this volume. While there is a marked tendency in favour of the Lower Middle Chronology, the 30-day month analysis contradicts this conclusion. Several different reconstructions of the Kültepe Eponym Lists and of the Mari Eponym Chronicle are proposed in this volume. They provide information for identifying the solar eclipse in the MEC, but different conclusions are reached with the possibility that the eclipse was the partial eclipse in 1838 BC or the total eclipse of 1833 BC or perhaps was wrongly dated in the MEC: these eclipses are consistent with the Lower Middle Chronology, but there are other possible eclipses consistent with the other chronologies.

It is to be hoped that new discoveries may cast light on the remaining unresolved questions, but until then this volume marks a significant advance in our knowledge of the chronology of the early second millennium BC in Mesopotamia.

Werner Nahm
Michael Roaf
14 July 2025

1. Astronomical chronology: from Šamšī-Adad to Ammī-šaduqa

TEIJE DE JONG

This paper reviews recent progress in the absolute dating of the chronology of the Ancient Near East during the first half of the second millennium BC with special emphasis on constraints provided by astronomical data: the Venus observations of Ammī-šaduqa, Babylonian month-length statistics, a solar eclipse near the birth of Šamšī-Adad and the intercalation pattern of the Old Assyrian lunar calendar. It concludes that the present evidence favours the Lower Middle Chronology (Hammurabi year 1 = 1784 BC).

1. Introduction¹

Mesopotamian chronology is conventionally based on texts: king lists, eponym lists, dated documents, synchronisms, royal inscriptions, etc.² To put this relative chronological framework on an absolute footing ‘hard’ scientific methods must be used. Until recently astronomical chronology was the only such method available to provide absolute calibrations of historical records. During the last decades dendrochronology and radiocarbon dating have become increasingly important as additional chronological tools for the Ancient Near East.

The advantage of astronomical dating is that in principle it allows a fine-tuning of the dating process to the ultimate accuracy of one year. For the absolute dating of the Mesopotamian chronology of the first half of the second millennium BC the following astronomical data have been used:

- Observations of the planet Venus during the reign of the Old Babylonian king Ammī-šaduqa.
- Lunar months of 30-day length referred to in texts dating to the reigns of the Old Babylonian kings Ammī-ditāna and Ammī-šaduqa.
- A solar eclipse in the year after the birth of the Old Assyrian king Šamšī-Adad mentioned in the Mari Eponym Chronicle.
- The intercalation pattern of the Old Assyrian lunar calendar.³

In this paper I will review the present status of the absolute dating of the Mesopotamian chronology of the first half of the second millennium BC and the way in which each of these astronomical datasets may contribute to the dating process.

2. The Venus tablet of Ammī-šaduqa

One hundred years ago the German Jesuit Franz Xaver Kugler⁴ published his discovery that the year formula of year 8 of the Old Babylonian king Ammī-šaduqa, ‘the Year of the Golden Throne’, appeared in a list of first and last visibility observations of the planet Venus in the omen series *Enuma Anu Enlil*. Ever since that time the Venus observations of Ammī-šaduqa have occupied a central position in studies of Old Babylonian chronology.

Over the past century there have been many attempts to astronomically date the reign of Ammī-šaduqa by selecting a ‘best data set’ from the text and fitting it to computed Venus dates. An important study is that by Huber *et al.*⁵ who found

¹ This paper is based on the review presented at the International Colloquium ‘New Perspectives on the Chronology of the Early Second Millennium BC in the Near East and Egypt’ that was held at the University of Lille from 8–9 September 2015. The original manuscript was submitted for publication in the Proceedings of the Colloquium in June 2016. The present paper is a revised version re-submitted in March 2022 and updated in December 2023.

² HUNGER 2009.

³ This chronological tool was not included in the paper that I presented at the Lille Colloquium but is a result of the colloquium where I met Edward Stratford who drew my attention to his work on the intercalation of the Old Assyrian lunar calendar.

⁴ KUGLER 1912: 257–299.

⁵ HUBER *et al.* 1982.

that there are four possible solutions for the beginning of the reign of king Ammī-šaduqa between 1710 and 1580 BC: Ammī-šaduqa year 1 = 1702 BC (High Chronology), 1646 and 1638 BC (Middle Chronologies), or 1582 BC (Low Chronology). They expressed preference for the High Chronology on the basis of a best fit to Old Babylonian 30-day month-length data (see below). Since then two more chronologies have been proposed. Gasche *et al.*,⁶ reconsidering archaeological and historical data, proposed Ammī-šaduqa year 1 = 1550 BC, and Mebert,⁷ re-analyzing the Venus data, proposed Ammī-šaduqa year 1 = 1574 BC.

In an attempt to clarify the situation I re-analyzed the Venus observations⁸ using visibility criteria derived from a study of Neo- and Late Babylonian observations of Venus recorded in the Babylonian Astronomical Diaries.⁹ In this analysis I limited myself to the first twenty Venus observations covering the first eight years of the reign of Ammī-šaduqa because those contain the reference to the ‘Year of the Golden Throne’ and are generally considered to be the most reliable ones.¹⁰ From this re-analysis I concluded that:

- there are three pairs of chronologies that provide acceptable fits to the Venus observations: Ammī-šaduqa year 1 = 1702/1694 BC, 1646/1638 BC and 1582/1574 BC,
- additional independent criteria are required to choose between these candidate chronologies, and
- the ultra-short chronology Ammī-šaduqa year 1 = 1550 BC proposed by GASCHE *et al.* must be discarded because it is not supported by the Venus observations.

Thus the Venus observations allow six possible different chronology calibrations within a time span of about 130 years. Using the well-established relative chronology of the Old Babylonian dynasty¹¹ this implies six different dating possibilities for the beginning of the reign of the Old Babylonian king Hammurabi ranging from 1848 to 1720 BC.

3. Constraining the Mesopotamian chronology of the early second millennium BC by radiocarbon dating

Ten years ago Barjamovic, Hertel and Larsen¹² published an important study in which they succeeded in reconstructing the Old Assyrian eponym list over a timespan of 255 consecutive years. According to their Revised Eponym List (REL) the Assyrian king Šamši-Adad died in REL 197. Using the fact that the Old Assyrian and Old Babylonian chronologies are connected through the well-established synchronism that Šamši-Adad died in year 18 of the Old Babylonian king Hammurabi¹³ we can then combine the REL with the Old Babylonian King List¹⁴ to construct a relative chronological sequence of about 380 years, running from the beginning of the reign of the Old Assyrian king Erišum I to the destruction of Babylon near the end of the reign of the Old Babylonian king Samsu-ditāna.¹⁵

By connecting the REL to the radiocarbon dating¹⁶ of wooden beams used for the building of the Waršama Palace in ancient Kaneš (Kültepe, Turkey) and the Sarıkaya palace (Acemhöyük, Turkey) Barjamovic, Hertel and Larsen were able to put the relative chronology of the REL on an absolute basis with a margin of uncertainty of only about ± 10 years. Based on this much smaller margin of uncertainty it is straightforward to show that the number of possible candidate Venus chronologies can be reduced to the two Middle Chronologies: Ammī-šaduqa year 1 = 1646 BC or 1638 BC.¹⁷

After the publication of the REL in 2012 several problematic sections, in particular the range REL 178–198, which covers the last twenty years of the reign of Šamši-Adad, and where the REL is sensitive to the break in the Kültepe Eponym List G (KEL G) and the placement of the fragment MEC D in the reconstruction of the Mari Eponym Chronicle (MEC) have been addressed in follow-up studies. These issues were extensively discussed at the Colloquium in Lille in 2015. For the purpose of this review it is important to note that the outcome of this debate is a confirmation that the REL indeed provides a reliable relative chronological framework with a probable uncertainty margin of a few years.¹⁸

⁶ GASCHE *et al.* 1998.

⁷ MEBERT 2010.

⁸ DE JONG 2012–2013.

⁹ DE JONG 2012.

¹⁰ See REINER/PINGREE 1975; HUBER *et al.* 1982.

¹¹ See PRUZSYNSZKY 2009: 101.

¹² BARJAMOVIC/HERTEL/LARSEN 2012.

¹³ CHARPIN/ZIEGLER 2003: 174, 262.

¹⁴ PRUZSYNSZKY 2009: 101.

¹⁵ DE JONG 2012–2013.

¹⁶ MANNING *et al.* 2001.

¹⁷ DE JONG 2012–2013.

¹⁸ See BARJAMOVIC Ch. 6 in this volume; VEENHOF Ch. 4 in this volume.

Chronological marker	Eponym / Event	Upper Middle Chronology Julian date	Lower Middle Chronology Julian date
REL 1	Šu-Ištar s. Abila	1 Jan 1971 BC	1 Jan 1963 BC
REL 80	Sukkaliya s. Minanum	1 Jan 1892 BC	1 Jan 1884 BC
	Texts found underneath the Waršama Palace		
REL 110	Iddin-Ahum s. Kudanum	1 Jan 1862 BC	1 Jan 1854 BC
REL 126	Dādiya s. Šu-Ilabrat	1 Jan 1846 BC	1 Jan 1839 BC
	Birth of Šamšī-Adad	1846 BC	1839 BC
REL 127	Puzur-Ištar s. Nūr-Ilīšu	1 Jan 1845 BC	1 Jan 1838 BC
	Solar eclipse candidate	5 Aug 1845 BC	24 Mar 1838 BC
	Earliest use of timber for the Waršama Palace	1837 – 1826 BC	1837 – 1826 BC
REL 138	Ennam-Aššur	1 Jan 1834 BC	1 Jan 1826 BC
	Destruction of Kārum II in Kaneš		
REL 141	Šarrum-Adad	1 Jan 1831 BC	1 Jan 1823 BC
<i>Hammurabi year 1</i>		1 Apr 1792 BC	1 Apr 1784 BC
	Earliest use of timber for the Sarıkaya Palace	1778 – 1767 BC	1778 – 1767 BC
	Seals of Šamšī-Adad found in the Sarıkaya Palace		
REL 197	Ṭāb-šilli-Aššur	1 Jan 1775 BC	1 Jan 1767 BC
<i>Hammurabi year 18</i>		1 Apr 1775 BC	1 Apr 1767 BC
	Death of Šamšī-Adad	Dec 1775 BC	Dec 1767 BC
<i>Ammī-šaduqa year 1</i>		1 Apr 1646 BC	1 Apr 1638 BC
<i>Ammī-šaduqa year 12</i>		1 Apr 1635 BC	1 Apr 1627 BC
	Northern Hemisphere volcanic eruption	1628/1627 BC	1628/1627 BC
<i>Samu-ditāna year 31</i>		1 Apr 1595 BC	1 Apr 1587 BC
	Destruction of Babylon	1595 BC	1587 BC
(i)	(ii)	(iii)	(iv)

Fig. 1.1. Historical events, eponyms and dates for the two Middle Chronologies.

Regular script = Revised Eponym List (relative) chronology; *Italic script* = Old Babylonian (relative) chronology; **Bold face script** = absolute chronology constraints. Beginning of Assyrian year put at 1 January (near the Winter Solstice; see DERCKSEN 2011); beginning of the Babylonian year put at 1 April (near the Spring Equinox). All dates are in the Julian calendar.

A confusing factor in the study of the Late Bronze Age Chronology of the Ancient Near East over the last several years has been the shifting back and forth of the radiocarbon dating of the wooden beams used for the building of the Waršama palace and the Sarıkaya palace. In 2016 Manning *et al.*¹⁹ presented evidence that their original dating²⁰ was problematic and should be shifted backwards in time by about 15–20 years²¹ but a few years later, after a careful reconsideration of seasonal and other possible offsets in the radiocarbon calibration curves, they came up with a new robust dating²² of the timber used for the construction of each palace which differs by only one or two years from the original results in their 2001 paper.

According to their latest dating the timespan for the earliest use of the Waršama palace is 1837–1826 BC and for the Sarıkaya palace 1778–1767 BC, both with 95% statistical probability. The chronological situation is illustrated in Fig. 1.1, where I show relevant events and dates for two chronologies, the Upper Middle Chronology (UMC) characterized by

¹⁹ MANNING *et al.* 2016.

²⁰ MANNING *et al.* 2001.

²¹ This shift was first announced by Manning in a discussion paper presented during the 2nd Kültepe International Meeting in August 2015, one month before the Lille colloquium took place.

²² MANNING *et al.* 2020.

Hammurabi 1 = 1792 BC²³ and the Lower Middle Chronology (LMC) with Hammurabi 1 = 1784 BC. The other four Venus chronologies (see above) can be discarded because:

- the Waršama palace must have been built after REL 110, the latest date assigned to texts found among the burnt mudbrick debris underlying its foundations;²⁴ this excludes the pair of Low Chronologies (all chronological markers in Fig. 1.1 shifted by 64 years to later dates), and
- the construction of the Sarikaya palace must have been finished before the death of Šamšī-Adad (REL 197) because seals of his servants were found among the ruins of the palace;²⁵ this excludes the pair of High Chronologies (all chronological markers in Fig. 1.1 shifted by 56 years to earlier dates).

Note that the most recent radiocarbon dating is consistent with the archaeological evidence for near-simultaneity of the destruction of the Old Palace and of Kārum II in Kaneš.²⁶ The data in Fig. 1.1 show for the UMC that if the Old Palace and the Kārum were destroyed simultaneously between 1834 and 1831 BC, there was timber available for building the Waršama palace felled sometime between 1837 and 1826 BC, while for the LMC we have destruction between 1826 BC and 1823 BC with timber available that was felled between 1837 BC and 1826 BC.

As far as the seals of Šamšī-Adad found among the remains of the Sarikaya palace are concerned, the data in Fig. 1.1 show that there are three years between the date of the earliest possible building of the palace and the death of Šamšī-Adad for the UMC and 11 years for the LMC. While it is possible that some of Šamšī-Adad's former officials were still using their seals for a limited amount of time after his death it seems improbable that this period of continued use lasted longer than about 10 years.

Since the archaeological evidence is not sufficiently constraining to choose between the two Middle Chronologies we next consider the other astronomical dating tools mentioned above: the 30-day month-length statistics, the solar eclipse in the year following the birth of Šamšī-Adad, and the intercalation pattern of the Old Assyrian calendar.

4. Babylonian month-length statistics

The Babylonian calendar consists of lunar months of 29 or 30 days length. The length of the preceding month is experimentally determined by observation of the reappearance of the lunar crescent on first day of the next month. By comparing 30-day month-lengths, attested in Neo-Babylonian administrative and economic texts, with predicted 30-day month-lengths HUBER *et al.* 1982 found that month-lengths were correctly predicted in 67% of all cases (103 out of 153).²⁷ This comparison can be made because the intercalation pattern of the Babylonian lunar calendar during the last six centuries BC is known.²⁸ To be able to use this method to choose between different Old Babylonian candidate chronologies, we need to have a collection of attested 30-day months from Old Babylonian texts *and* must know the intercalation pattern of the lunar calendar.

From the existing literature Huber *et al.* collected twenty-one months of 30-day duration during the first sixteen years of the reign of king Ammī-šaduqa for which the intercalation pattern was attested and independently corroborated by the Venus observations²⁹. Based on an analysis of these data they expressed strong preference for the High Chronology (Ammī-šaduqa year 1 = 1702 BC) with fifteen out of twenty-one 30-day months correctly predicted (and 6 wrongly predicted). This is illustrated in Fig. 1.2. The left-hand (solid) curve gives the probability distribution for a correct chronology ($p = 0.33$) and the right-hand (dashed) curve the probability distribution for a sample of randomly distributed 30-day months ($q = 0.47$).³⁰ The vertical lines represent the observed miss rates of the four different Venus chronologies for the sample of twenty-one 30-day months during the first sixteen years of the reign of king Ammī-šaduqa (HUBER *et al.* 1982). These data indeed suggest that the High Chronology is the preferred one.

²³ From about 1970 onwards most scholars have adopted Hammurabi year 1 = 1792 BC as a working hypothesis, referring to it as the Middle Chronology (MC). This was not more than a convenient compromise within the discussion of Mesopotamian chronology (see PRUZINSZKY 2009: 19).

²⁴ BARJAMOVIC/HERTEL/LARSEN 2012: 31.

²⁵ BARJAMOVIC/HERTEL/LARSEN 2012: 43.

²⁶ BARJAMOVIC/HERTEL/LARSEN 2012: 41.

²⁷ HUBER *et al.* 1982.

²⁸ See PARKER/DUBBERSTEIN 1956.

²⁹ The fact that the textually attested intercalations are in exact agreement with intercalations required to reproduce the astronomically determined visibility and invisibility intervals of the planet Venus provides a very strong argument in support of the historicity of the Venus observations.

³⁰ For details of the statistical mathematics the reader is referred to HUBER *et al.* 1982.

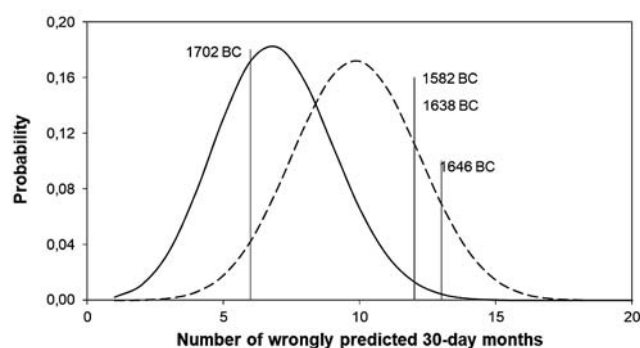


Fig. 1.2. Binomial probability distributions of the number of wrongly predicted 30-day months (miss rates) for a sample of twenty-one 30-day months during the first sixteen years of Ammī-šaduqa (HUBER *et al.* 1982: 68–69).

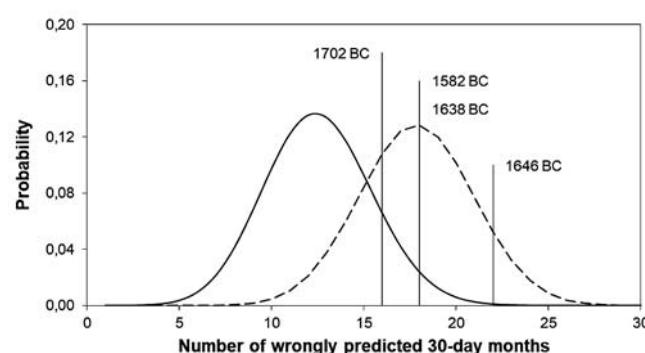


Fig. 1.3. Binomial probability distributions of the number of wrongly predicted 30-day months (miss rates) for the updated sample of 38 30-day months during the first 19 years of Ammī-šaduqa (HUBER 2017: Appendix 10.3).

The sample of 30-day months in Fig. 1.2, however, is small so that doubt has been expressed about the statistical significance of the result of this analysis.³¹ More recently HUBER (2017: Appendix 10.3) has extended the original sample to thirty-eight attested 30-day months during the first nineteen years of the reign of king Ammī-šaduqa. In Fig. 1.3 the curves represent probability distributions of the expected number of wrongly predicted 30-day months for the updated sample. Also shown in Fig. 1.3 are the miss rates in the updated sample for the same four Venus chronologies as shown in Fig. 1.2. These data show that all four chronologies have a larger probability to be wrong than to be correct.³² Even the miss rates for the most probable chronologies lie within one standard deviation (~ 3) from the mean in the right-hand random distribution of 30-day months. I conclude that at present the method of 30-day month-length statistics does not (yet) provide a reliable chronological tool on the basis of which one can choose between different chronologies.³³

5. The solar eclipse associated with the birth of Šamšī-Adad

In the Mari Eponym Chronicle³⁴ a ‘darkening of the Sun’ is mentioned in the year after the birth of Šamšī-Adad. This reference to a solar eclipse has been used in several attempts to date the reign of Šamšī-Adad.³⁵ According to the Revised Eponym List this ‘darkening of the Sun’ occurred in REL 127.³⁶ The uncertainties related to the reconstruction of the breaks in KEL G create an uncertainty margin in the absolute calibration of the REL before REL 197 (= Hammurabi year 18) of about ± 2 years.³⁷ So, if this ‘darkening of the Sun’ is to be explained by a solar eclipse, we are looking for candidate solar eclipses in $1845 \text{ BC} \pm 2$ years (UMC) and in 1837 ± 2 years (LMC).

In Fig. 1.4 I list the maximum magnitudes of all solar eclipses visible in Aššur³⁸ between 1847 and 1835 for clock-time errors δ (ΔT) less than two hours (both ways)³⁹ and including refraction at the horizon.⁴⁰ All eclipses in Fig. 1.4 are solar

³¹ DE JONG 2012–2013: 153–155.

³² ROAF (Ch. 13 in this volume) has questioned the reliability of 30-day month-length data for the last month of the Old Babylonian calendar year. Removing these months from the sample slightly changes the numerical results of my analysis but does not alter the conclusion.

³³ HUBER (2017: fig. 4 and Ch. 2 in this volume: Fig. 2.1) presents two figures similar to my Fig. 1.3 but for a sub-sample of only thirty-two of the thirty-eight 30-day months in the sample of his Appendix 10.3. He argues that based on that sub-sample the High Chronology (Ammī-šaduqa year 1 = 1702 BC) is indeed to be preferred but as shown here this preference evaporates when the full sample of thirty-eight 30-day months is considered. This illustrates the sensitivity of the month-length statistics to the size of the sample. HUBER (2017: 35) himself notes ‘that one would need 60 or more month-lengths for such a test to have a fair chance of being successful.’

³⁴ BIROT 1985. See now ZIEGLER and VEENHOF Ch. 3 and 4 in this volume.

³⁵ See ROAF 2012.

³⁶ BARJAMOVIC/HERTEL/LARSEN 2012: 32–33; VEENHOF Ch. 4 in this volume.

³⁷ BARJAMOVIC Ch. 6 in this volume.

³⁸ If the eclipse had been observed in Ešnunna where Šamšī-Adad’s family may have resided at this time, instead of Aššur, the eclipse magnitudes would change by at most 0.02.

³⁹ Clock-time error is a technical term that reflects the uncertainty in the gradual slowing down of the rotation of the Earth which affects the location on Earth of a solar eclipse and its magnitude (see DE JONG 2012–2013: 156–157). This uncertainty increases going backwards in time with an estimated standard error of about 1 hour around 1800 BC.

⁴⁰ Atmospheric refraction causes noticeable displacements of the Sun and Moon near the horizon which may change the value of the eclipse magnitude by several hundredths up to one-tenth. This effect is not included in most predictions of solar eclipses in the literature.

eclipses in which only part of the solar disk is obscured.⁴¹ Partial solar eclipses will pass unnoticed for a naked-eye observer unless the Sun is more than about 95% eclipsed⁴² or if the eclipse happens very close to the horizon (within a few degrees) so that the Sun can be looked at without blinding the observer. In Fig. 1.4 solar eclipses which reach their maximum close to the horizon (within one hour after sunrise or before sunset) are labeled R or S, respectively.

δ (ΔT) [hrs]	−2	−1	0	1	2
Julian Date	Eclipse magnitude				
02/04/1847 BC	0.11	0.27	0.50	0.77	0.91
17/08/1846 BC	—	S 0.12	—	—	—
05/08/1845 BC	—	—	S 0.07	S 0.79	S 0.86–0.20
05/06/1842 BC	0.15	R 0.16–0.30	R 0.30	—	—
24/03/1838 BC	0.87	0.94	R 0.55–0.95	R 0.57	—
12/03/1837 BC	0.25	0.08	—	—	—
26/08/1836 BC	0.22	R 0.02–0.31	R 0.45	R 0.08	—
(i)	(ii)	(iii)	(iv)	(v)	(vi)

Fig. 1.4. All solar eclipses taking place in Aššur between 1847 and 1835 BC.

When the Sun rises or sets partially eclipsed eclipses are labeled R or S. When a pair of numbers is listed the eclipse reaches its maximum after sunrise (the second number of the pair) or before sunset (the first number of the pair). The more spectacular eclipses near the horizon for which more than half of the solar disk is obscured, are indicated in bold face.

To explain an event described as ‘a darkening of the Sun’ we are looking for solar eclipses with magnitudes larger than 95% or for solar eclipses close to the horizon for which more than half of the solar disk is obscured so that they are sufficiently impressive to qualify as such. The data in Fig. 1.3 show that there are two partial solar eclipses (printed in bold face) that satisfy these criteria. By varying the clock time error for those two eclipses I find that maximal coverage of the solar disk at sunrise or sunset occurs:

- in the case of the partial eclipse of 24 March 1838 BC (LMC) for a clock time error of +0.5 hours when the Sun rose 94% eclipsed,⁴³ and
- in the case of the partial eclipse of 5 August 1845 BC (UMC) for a clock time error of +1.25 hours when the Sun set 75% eclipsed.

Of these two the first one is the more spectacular eclipse because the Sun is almost completely ‘darkened’ at the horizon and the more probable one because the clock-time error is the smallest.

It is worth noting that the LMC candidate eclipse of 1838 BC requires that an additional eponym year has to be inserted between REL 197 and REL 127 while the UMC candidate is consistent with the present REL (see Fig. 1.1). Also note that the most spectacular nearby eclipse candidate, the total solar eclipse of 24 June 1833 BC⁴⁴ has a date too far outside the uncertainty window of the REL to qualify as a candidate for the recorded ‘darkening of the Sun’.⁴⁵ Thus the partial solar eclipse at sunrise on 24 March 1838 BC is at present the most probable candidate for the one mentioned in the Mari Eponym Chronicle in the year after the birth of Šamsī-Adad. However, I think that this is insufficient to unambiguously establish the Lower Middle Chronology as the correct one so that additional independent evidence is required.

6. A new look at the Venus observations: volcanic ashes over Babylon

According to the Venus tablet, in the twelfth year of the Babylonian king Ammī-šaduqa the last visibility of Venus in the East occurred six weeks earlier than expected and the first visibility in the West was delayed by seven weeks, and in his thirteenth year the first visibility in the East was delayed by one week. Because of their deviating behavior these

⁴¹ The nearest total eclipse in time is the one of 24 June 1833 BC for which the totality phase lasted about 4 minutes.

⁴² MULLER/STEPHENSON 1975: 467.

⁴³ See also HUBER 2014 who computes for a clock-time error of +15 minutes that the eclipse was indeed quite impressive with magnitude 0.93 at sunrise on 24 March 1838 BC (his fig. 2c).

⁴⁴ Discussed by MICHEL/ROCHER 1997–2000, ROAF 2012 and NAHM 2013.

⁴⁵ ROAF (Ch. 5 in this volume) argues that the 1833 BC eclipse is the one referred to in REL 127. He proposes that the mention of the ‘darkening of the Sun’ was incorrectly assigned to the eponymy of Puzur-Ištar.

observations have been discarded as corrupted in most studies. However, it has also been suggested⁴⁶ that these observations may be genuine and that they can be explained by enhanced atmospheric extinction in Babylon due to aerosols expelled into the upper Earth atmosphere by a violent volcano eruption, possibly the one on the Greek island Thera (present-day Santorini). The Thera eruption had been dated with 95% probability by radiocarbon dating of tree-rings in the remains of several local olive trees to 1613 BC ± 14 –13 years.⁴⁷ Within this dating window a disturbed tree-ring pattern has been identified in central European and Irish oak trees and in Californian and Swedish pine trees. A detailed study of these tree-rings pointed to a major environmental disruption over the Northern Hemisphere, possibly of volcanic origin, dated to 1628/1627 BC.⁴⁸

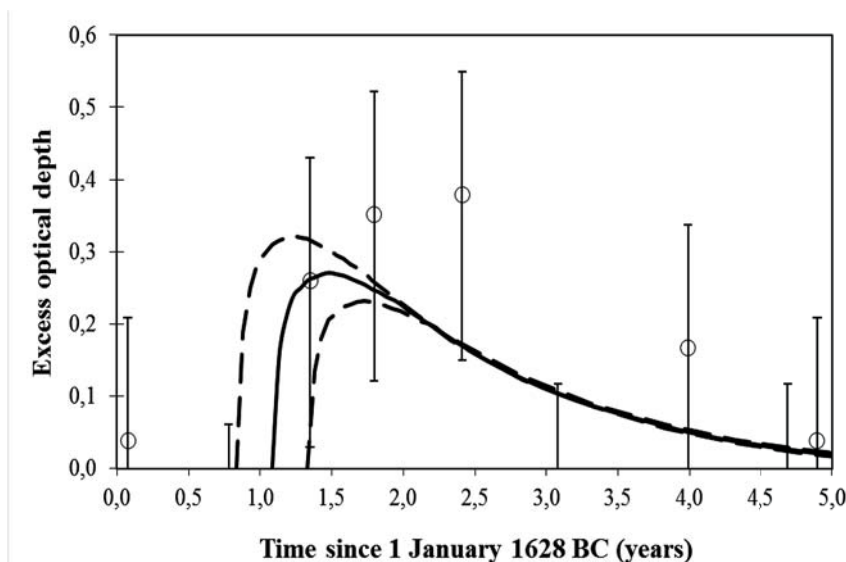


Fig. 1.5. Excess visual optical depth in the atmosphere of Babylon as a function of time. The data points are derived from the Venus observations in the 12th through 16th year of the reign of the Old Babylonian king Ammī-šaduqa adopting the Lower Middle Chronology. The curves are scaled versions of the one derived for the 1815 eruption on the island of Tambora in the Indonesian archipelago (STOTHERS 1984). The dashed curves represent limiting cases with eruption dates of 1 November 1628 BC (left) and 1 May 1627 BC (right). The solid curve represents the intermediate case with an eruption date of 1 February 1627 BC. [Corrected version of fig. 1 in DE JONG/FOERTMEYER 2010].

By subtracting the average atmospheric extinction in Babylon from the anomalously large extinction values in years 12 through 16 of Ammī-šaduqa we were able to determine the excess extinction due to the eruption.⁴⁹ This is shown in Fig. 1.5 for the LMC for which Ammī-šaduqa year 12 = 1627/1626 BC (see Fig. 1.1), together with normalized profiles of the decay of the atmospheric aerosol content due to the gradual cleansing of the stratosphere (in a few years).

The magnitude of the excess extinction indicates that this eruption was comparable in strength to that of Krakatau (Indonesia) in 1883 and that it belongs to one of the most violent volcanic eruptions of the past 4000 years. The dashed curves in Fig. 1.5 correspond to the earliest (late October 1628 BC) and latest possible (early May 1627 BC) date for the eruption.

While the presence of a volcanic signal in the Venus observations seems rather secure, its association with the Thera eruption has been questioned. A recent study of the charred remains of beans and insects found in storage jars in ancient Akrotiri on Santorini⁵⁰ suggested that the eruption occurred in June/July in disagreement with the timeframe derived from the Venus observations. But more importantly, due to the presence of a Mediterranean offset in the radiocarbon calibration curve,⁵¹ the dating window of the Thera eruption has been shifted forward in time and widened significantly to 1619–1543 BC (at 95% confidence level) so that the Thera eruption is no longer a viable candidate as a cause for the presence of volcanic ashes in the atmosphere over Babylon.

On the other hand recent work has confirmed that a strong growth disturbance in tree-rings of American, Swedish and Finnish pine wood and in German oaks, indicating cold summer conditions, which correlates with dated high-resolution deposits of volcanic origin in Greenland ice-cores, can be attributed to a major volcanic eruption event over the Northern Hemisphere, dated to 1628/1627 BC, most probably of the Aniakchak volcano in Alaska.⁵²

⁴⁶ WEIR 1972; DE JONG/FOERTMEYER 2010.

⁴⁷ FRIEDRICH *et al.* 2006.

⁴⁸ GRUDD *et al.* 2000.

⁴⁹ DE JONG/FOERTMEYER 2010.

⁵⁰ PANAGIOTAKOPULU *et al.* 2013.

⁵¹ MANNING *et al.* 2020.

⁵² PEARSON *et al.* 2018; HELAMA *et al.* 2018; MCANENEY/BAILLIE 2019; PEARSON *et al.* 2020.

If indeed the enhanced extinction in the atmosphere of Babylon is caused by a major volcano eruption over the Northern Hemisphere sometime during the 1628/1627 BC, the Lower Middle Chronology is at present the only chronology able to provide a natural explanation for this phenomenon.

7. The intercalation pattern of the Old Assyrian calendar

Based on a study of debt notes found in Kanesh level II, dating from the best attested years of Old Assyrian commercial activity (REL 80–110), Stratford⁵³ succeeded in reconstructing the Old Assyrian lunar calendar for those years. Among other things he found that this calendar was apparently kept lined up with the solar year by inserting an additional lunar month XII roughly once every three years. Using the textually attested intercalation pattern of the 30-year calendar section (REL 80–110) and assuming that already during the Old Assyrian period the decision to intercalate was driven by observations of the heliacal rising of the star KAK.SI.SÀ (Sirius, the brightest star in the sky),⁵⁴ I have proposed that this information can be used to discriminate between different chronologies.⁵⁵

The line of reasoning in this proposal is as follows. Since the relative chronology in the period covered by REL 80–130 is well established and uncontroversial, the reconstructed calendar in REL 80–110 can be directly connected to the solar eclipse in the Mari Eponym Chronicle where during the eponymy of Puzur-Ištar (REL 127, see Fig. 1.1) a ‘darkening of the Sun’ is recorded. As discussed in Section 5 above there are two candidate (partial) solar eclipses able to explain this observation, one for each of the two possible Middle Chronologies (see Fig. 1.4). Using computed dates of the first appearance of the lunar crescent (the first day of the lunar month) for the period 1900–1850 BC, fixing the REL in absolute time by putting REL 127 = 1845 BC (UMC) and REL 127 = 1837 BC (LMC) and imposing the attested intercalation pattern,⁵⁶ calendar solutions are created for the period REL 81–110 resulting in an intercalated Old Assyrian calendar for 1891–1862 BC (UMC), and for 1884–1855 BC (LMC) (DE JONG 2016–2017: Tables 1 and 2). Comparison with computed dates of the heliacal rising of Sirius shows that in the LMC calendar solution for all years in which an additional month XII has been intercalated the heliacal rising of Sirius occurs in month VIII⁵⁷ while in the UMC calendar solution such a consistent pattern is not present.⁵⁸ So the attested intercalation pattern of the Old Assyrian lunar calendar may provide an additional independent argument in favour of the Lower Middle Chronology.⁵⁹

8. Conclusions

From the evidence reviewed in this paper I conclude that today there remain two possible solutions for the Mesopotamian chronology of the first half of the second millennium BC: Hammurabi year 1 = 1792 BC (High Middle Chronology) and Hammurabi year 1 = 1784 BC (Lower Middle Chronology). The present evidence favours the Lower Middle Chronology.

⁵³ STRATFORD 2015.

⁵⁴ Evidence in support of the early use of the first visibility of stars as an intercalation criterion may be found in the so-called Astrolabe texts (HOROWITZ 2011: 154–166) in which for each of the twelve months three stars are listed whose first visibility is expected in those months. According to the Astrolabe texts the Babylonian month IV is associated with KAK.SI.SÀ. Although the earliest known Astrolabe is found on a Middle Assyrian tablet, Horowitz supposes that the first Astrolabe was composed no later than the Middle Babylonian period and perhaps as early as the Old Babylonian period. That the observation of the first visibility of Sirius to decide about intercalation is a persistent tradition in ancient Mesopotamia is confirmed by the fact that it is mentioned in the late 2nd millennium astronomical compendium MUL.APIN (HUNGER/STEELE 2019: 210) and that it apparently forms the basis of the intercalation pattern of the rigidly structured late-Babylonian calendar which was introduced shortly after 500 BC (BRITTON 2007). SACHS 1952 has shown that during the last three centuries BC all dates of Sirius’ first visibility, computed according to the so-called Uruk scheme, fell in the month Du’uzu (month IV).

⁵⁵ DE JONG 2016–2017.

⁵⁶ STRATFORD 2015.

⁵⁷ The Old Assyrian year is three months out of phase with the Old Babylonian year, which began around the Spring Equinox, so that month VIII corresponds to month V in the Old Babylonian calendar, in agreement with the Sirius intercalation algorithm of MUL.APIN.

⁵⁸ In my study I also included for completeness sake the calendar solution based on the 1833 BC total solar eclipse that has been suggested as the most obvious eclipse candidate for the ‘darkening of the Sun’ in REL 127 (MICHEL/ROCHER 1997–2000; ROAF 2012; NAHM 2013; 3 ROAF Ch. 5 in this volume). It turned out that the calendar solution based on this eclipse scores significantly lower on the intercalation criterion than the one based on the 1838 BC eclipse. However, what is more important, if the ‘darkening of the Sun’ was correctly dated to REL 127, the 1833 BC eclipse does not qualify as a viable eclipse candidate for the Lower Middle Chronology because it requires the removal of several eponyms in the REL around the break in KEL G which is not supported by the textual evidence (BARJAMOVIC Ch. 6 in this volume; VEENHOF Ch. 4 in this volume).

⁵⁹ In a more extended ongoing study of debt notes and related documents from Kanesh by the late Klaas Veenhof (private communication) he found about one hundred more references to successor eponyms. While his study overall confirms the intercalation pattern derived by Stratford it also shifts one intercalation to the next year and adds one more intercalation to Stratford’s list of attested intercalations. It remains to be seen to what extent the new data will affect my proposal.

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2. Astronomical evidence from Venus, month-lengths and eclipses

PETER J. HUBER

Currently, the preferred second millennium BC chronology is based on radiocarbon dating and dendrochronology. This paper presents a brief discussion drawing attention to conflicting evidence, mostly astronomical, which suggests a half century higher chronology.

1. Introduction

Recent advances in radiocarbon dating, dendrochronology and eponym lists have led to a practical canonization of the two Middle Chronologies.¹ Teije de Jong, in his contribution to this conference, therefore has concentrated on the question whether and how the astronomical evidence can provide support for these two chronologies and whether it can discriminate between them.

However, the courts deciding on canonization traditionally have included not only an *advocatus dei*, arguing in favor of sanctification, but also an *advocatus diaboli*, presenting contrary arguments. I feel morally obliged to take on the latter's role. We should keep in mind the notoriously forgotten admonition of Jakob BERNOULLI (1713), stated in the very first book on probability theory, namely that we ought to consider not only the arguments which prove a thing, but also all those which can lead to a contrary conclusion.

My presentation at the Lille Meeting had been based on an unpublished manuscript written in early 2014, now published as HUBER (2017). Thus it should suffice to summarize the main arguments and conclusions.

2. The Venus evidence

In the historically feasible window, only four chronologies are compatible with the Venus Text:² Ammišaduqa year 1 = 1702 BC, 1646 BC, 1638 BC, 1582 BC, in this volume denoted HC, UMC, LMC and SC, for High, Upper Middle, Lower Middle and Short Chronology, respectively. In recent years, this list has been enlarged from four to seven, but in my opinion the additions are questionable. The chronology 1550 BC, propounded by GASCHE *et al.* (1998) is not at all supported by the Venus Text, contrary to the claims made by these authors. The alternate lowered High and Short chronologies (1694 BC and 1574 BC) that have been proposed by MEBERT (2010) in my opinion are unlikely. In order to render them plausible, one seems to be forced to accept questionable textual interpretations and emendations by Mebert, and/or conjectural assumptions about differences between NB and OB observational and recording practices.³

The Middle chronologies show deviations of ± 2 days against calculation, statistically significant on the 1% level. This was noted by VAN DER WAERDEN (1965) and later elaborated by HUBER *et al.* (1982). One can perhaps discount those discrepancies, but then one must be willing to make ad hoc conjectural assumptions about differences between NB and OB observational and recording practices.

3. The month-length evidence

We now know the complete pattern of intercalary months of Ammišaduqa's reign, from the later part of Year 1 to the end of Year 17. The pattern inferred from the Venus Text agrees with that from contemporary administrative texts.⁴ This has two important consequences, First the Venus Text can now unambiguously be dated to the reign of Ammišaduqa. Second, the internal distances between the months are fixed. The pattern of month-lengths, that is, of day-30 dates occurring in

¹ BARJAMOVIC/HERTEL/LARSEN 2012, DE JONG 2012–2013, NAHM 2013.

² REINER/PINGREE 1975.

³ See HUBER 2011.

⁴ HUBER 2017: 58.

administrative texts, implying that the month had 30 days, is more difficult to interpret. There may be differences between NB and OB dating, and the frequency of improper day-30 dates may differ. For example, as Michael Roaf has pointed out, OB end-of-year dates seem to use a schematic day 30. The sets of available dates are continuously, but slowly increasing, so one is hunting a moving target, and the statistical conclusions may be biased by the choice of the set. My calculations are based on the data available in 2013.

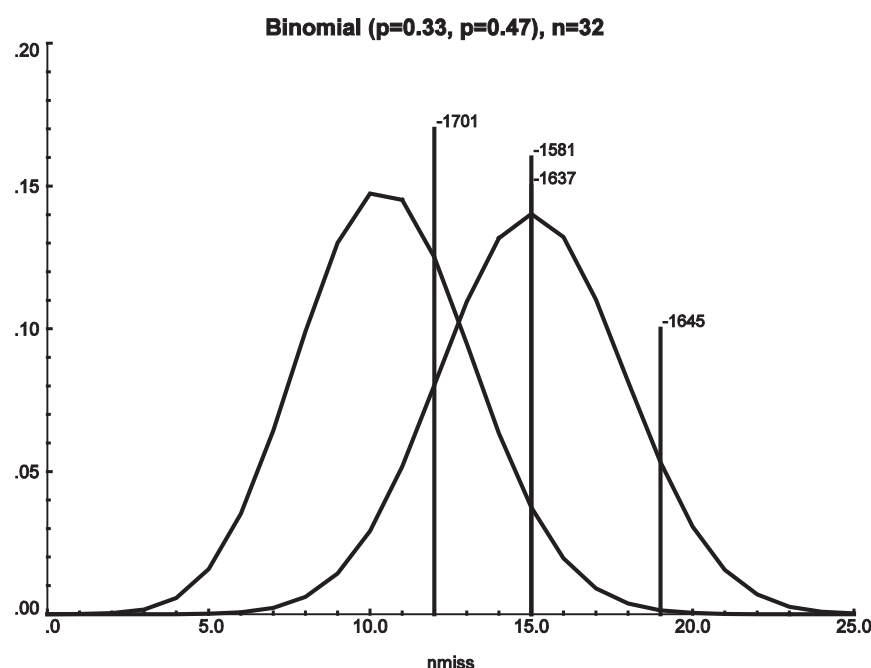


Fig. 2.1. Ammišaduqa data (set available in 2013). Binomial distributions for sample size $n = 32$, $p = 0.33$ and $p = 0.47$. The vertical lines indicate the number of misses (discrepancies between observed and calculated month-lengths) obtained for the four main chronologies with the Ammišaduqa data: -1701 : 12, -1645 : 19, -1637 : 15, -1581 : 15. (Here I am using the astronomical year counts, which are negative and 1 unit lower than the historical ones).

For wrong chronologies the frequency $p = 0.47$ of wrong day-30 dates is fixed by astronomical theory (the length 29.53 days of the synodic month), whereas the less reliable value $p = 0.33$ for a correct chronology is estimated from improper day-30 dates occurring in NB administrative texts. We note that the miss-counts in Fig. 2.1 are compatible with the assumption that all four chronologies are wrong. But if one of them is correct, and $p = 0.33$ is even approximately applicable, the figure clearly favors the HC (-1701) and strongly disfavors the UMC (-1645), while the LMC (-1637) and the SC (-1581) remain possible.

In addition to the 32 month-lengths from Ammišaduqa, we have 17 from his predecessor Ammiditana. With a total of 49 month-lengths, for a wrong chronology (i.e. a random wrong alignment) the expected number of misses is $49 \times 0.47 = 23.0 \pm 3.5$, and for a correct chronology it is $49 \times 0.33 = 16.2 \pm 3.3$; after the expected values also the standard deviations are indicated.

Unfortunately, it is not clear whether the two sets can be joined smoothly, or whether there is an undocumented additional intercalary month to be inserted between the two sets. If there is no such intercalation, the miss-counts for the four main chronologies turn out to be 23, 25, 24, 25, respectively. Thus, all four alignments appear to be wrong: none of the counts is below the value 23.0 expected for a wrong chronology, and all exceed the value 16.2 expected for a correct chronology by more than twice their standard deviation of 3.3.

On the other hand, if we insert an intercalation, the miss-counts become 15, 29, 28, 22, respectively, matching the assumption that we have one correct and three wrong chronologies. The count for the HC turns out even better than what we would expect for the correct chronology, and the counts for the Middle chronologies are devastatingly poor.

How should we interpret this? It seems that after looking at such results, people convinced of the correctness of the Middle chronologies simply tend to declare the Ammiditana month-length evidence as being worthless and to forget about it. But I think that this is too cheap an escape – in their place I should rather file it away as an obnoxious thorn in the flesh.

Since the Ammiditana argumentation involved a conjectural intercalation, I also have done Bayesian probability calculations, assuming different prior probabilities α for the existence of such an additional intercalation. If we were able to prove that there was no additional intercalation, we have $\alpha = 0$. If we could make sure that there was one, we have $\alpha = 1$. Some people might want to formalize ignorance by $\alpha = 0.5$, but most might gravitate towards a small value, say $\alpha = 0.05$ or $\alpha = 0.1$. To help with the interpretation of such prior probabilities, I may quote MOSTELLER and YOUTZ (1990: 4) on quantifying probabilistic expressions.⁵ According to them, for an average person the naked word *Certain* implies about 97% of full certainty, and *Almost Certain* about 86%. Thus, $\alpha = 0.1$ can be interpreted that you are almost certain that there is no additional intercalation. The results of the calculation for the 2013 set are listed in Fig. 2.2.

<i>Prior and posterior probabilities of an additional intercalation:</i>					
<i>prior α</i>		0.000	0.050	0.100	0.500 1.000
<i>posterior β</i>		0.000	0.731	0.852	0.981 1.000

<i>Posterior probabilities of chronologies:</i>					
<i>prior α</i>		0.000	0.050	0.100	0.500 1.000
<i>HC -1701</i>		0.460	0.843	0.906	0.973 0.983
<i>UMC -1645</i>		0.142	0.038	0.021	0.003 0.000
<i>LMC -1637</i>		0.256	0.069	0.038	0.005 0.000
<i>SC -1581</i>		0.142	0.050	0.035	0.018 0.016

Fig. 2.2. Ammišaduqa and Ammiditana data, miss counts of 2013. Posterior probabilities of the four main chronologies.

We note that $\alpha = 0.1$ suffices to boost the posterior probability of an additional intercalation to $\beta = 0.85$ and the posterior probability of the High Chronology to 0.91, thus making it almost certain, and second, that confirmation of an additional intercalation ($\alpha = 1$) would render the Middle Chronologies utterly implausible.

4. The Šamši-Adad solar eclipse

This eclipse is described as a ‘darkening of the sun’ (*nahdur Šamaš*) in the Mari Eponym Chronicle.⁶ It occurred in the year after the birth of Šamši-Adad. The most tempting identification is the eclipse of –1832 June 24, which was total a few kilometers south of Assur, but its date does not match any of the Venus chronologies within the currently accepted uncertainty bounds of the eponym lists. But there are impressive partial solar eclipses near sunrise or sunset compatible with three of the Venus chronologies: HC (–1901 March 12), UMC (–1844 August 5) and LMC (–1837 March 24). The target years for these three eclipses are –1901, –1845 and –1837, ± 2 years, respectively, 200 years before Year 1 of Ammišaduqa. The eclipse evidence thus is inconclusive.

5. A volcanic eruption?

DE JONG/FOERTMEYER 2010, unaware of the earlier suggestion of WEIR 1972, have argued that the abnormally long period of invisibility occurring in year 12 of the Venus Tablet is not a mere gross scribal error, but may be connected to a volcanic eruption. If the LMC is correct, this would approximately match the Thera (Santorini) eruption currently dated 1613 BC $\pm 14/-13$ years, and perhaps even better another volcanic event dated around the year 1628 BC (the Aniakchak eruption in Alaska). The latter is documented in ice-cores and tree-ring sequences.⁷

However, I am worried by the favorable apodosis the Venus text offers for the omens 13, 14 and 15 surrounding the period of excessive invisibility. The years 11 and 12 have ‘the land will be happy’ and year 13 ‘the harvest of the land will prosper’.⁸ Admittedly, the eruption did not necessarily cause a catastrophic ‘year without a summer’, like the Tambora eruption of 1815 AD.⁹ But in view of its worldwide reducing effect on tree growth, I certainly would have expected the

⁵ MOSTELLER/YOUTZ 1990: 4.

⁶ BIROT 1985.

⁷ See ROAF Ch. 5 in this volume for a detailed discussion.

⁸ REINER/PINGREE 1975: 13.

⁹ See STOTHERS 1984.

eruption to have had a substantial agricultural influence, to be reflected in the apodoses. For a detailed contemporary account of a climatic catastrophe (although not necessarily connected with a volcanic eruption) whose description closely resembles that of the ‘year without a summer’, one might compare the passage in the Proclamation of Telepinu, where he describes how the gods had punished the Hittite king Ammuna, who had murdered his own father.¹⁰

6. Conclusions

If the evidence from radiocarbon dating, dendrochronology and eponym lists is considered reliable to such an extent that the HC and SC can be excluded with moral certainty (that is, to quote the ancient authority of Jakob Bernoulli, with 99% or 99.9% of full certainty), then the astronomical evidence reduces attention to the HMC or the LMC, but clearly favors the latter.

But what if you accord less than Bernoulli’s moral certainty to the reliability of the above evidence? My conclusion is: even if you rate the LMC as almost certain (that is, according to Mosteller and Youtz, with 86% certainty), you should not forget that there are at least two independent pieces of contrary astronomical evidence, the first of which, namely the Venus evidence, operates against the Middle chronologies, and the other, the Ammišaduqa–Ammitana month-length evidence, favors the HC.

As a third piece of evidence, I should draw attention to astronomical materials pertaining to the late third millennium (Akkad and Ur III lunar eclipse omens, a literary solar eclipse from the time of Sargon, and Ur III month-length data). All three kinds of data are precarious, and they are only indirectly relevant to the chronology of the 2nd millennium. But their evidence is seductively consistent. They favor a unique set of coherent high chronologies for Akkad and Ur III, supported by post-Akkadian ¹⁴C dates from Tell Leilan, and they yield tightly constrained consistent values for the clock-time correction ΔT . With the currently accepted year counts for back-reckoning also they support the HC and reject lower chronologies. See HUBER (2012), and in particular HUBER (2023).

Personally, I think that we are not yet ready to reach a firm verdict, and that at present the wisest course of action is to stick with the conventional chronology, namely the UMC (Ammišaduqa year 1 = 1646 BC), even if you do not believe in its correctness.

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¹⁰ §20 ‘And Ammuna became king. The gods sought (justice for) the blood of Zidanta, his father, and in his hand [they did] not [make] him, or the grain, or [...], or the wine, or the cattle or sheep [prosper].’ Quoted from: <https://www.milestonedocuments.com/documents/view/the-proclamation-of-telipinu/text>.

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3. The Eponym Chronicles from Mari – new edition, photographs and comments

NELE ZIEGLER

Maurice Birot published the Mari Eponym Chronicles (MEC) in 1985. Since then, significant progress in the ordering of Old Assyrian eponyms has been made, notably by specialists in Old Assyrian archives. A revised publication of the Mari exemplars with better photographs is proposed here.

The editorial history of these lists is reexamined. Although all known exemplars of the MEC are from Mari, there is no reason to believe that Mari was the only place in Samsi-Addu's Upper Mesopotamian Empire where such texts were produced.

K. Veenhof proposed naming the three main exemplars of the MEC 'Editions I to III'. It is argued that Edition II, which mentions Samsi-Addu's year of birth, is older than Edition I and was perhaps created just before Samsi-Addu's sixtieth year of life.

Maurice Birot's 1985 article 'Les chroniques "assyriennes" de Mari' is one of the most amazing works for all those who are interested in 19th and early 18th century BC chronology. Today, we currently call the documents edited by M. Birot *Mari Eponym Chronicles* (MEC). Nothing indicates that these texts were original creations by a scribe in Mari, but it is clear that some if not all of the known exemplars of MEC were copied there.

Since 1985, much additional information – notably several eponym lists from Kültepe – supplements M. Birot's research. Nevertheless M. Birot's edition has not been outdated. He did a very careful, long-lasting work. His hand copies are as precise as possible. The quality of the photographs in MARI 4, published in 1985, was perhaps not the best possible one,¹ but still they were extremely useful. In 2022, work on the French ANR funded project PCEHM² started, and we collectively undertook to put in order the photographs of Mari texts. During this reorganisation work, I was able to put my hands on more recent pictures of the Mari eponym chronicles and it seems useful to give access to these documents in picture and transliteration once again. I hope my present contribution will allow scholars to use the Mari documents as conveniently as possible.

It is not my aim to fill the gaps and to reconstruct the complete order of eponyms here, and I am unable to solve the last remaining chronological questions. This is attempted by Klaas Veenhof, Michael Roaf and Denis Lacambre in this book. Earlier reconstructions are in the book of Gojko Barjamovic, Thomas Hertel, and Mogens Larsen, which I will use as reference for the numbering of eponyms (REL³), as well as in Guido Kryszat's and Rafał Koliński's studies.⁴

¹ These photos were among the first taken with the ammonium chloride technique as part of the systematic program to photograph the Mari tablets then preserved in Paris. The initiative of photographic coverage was launched by Jean-Marie Durand when he succeeded Maurice Birot in 1982 at the head of the ER 193 of the CNRS. The photos published here were taken years later, when the photographic technique used for the *Archives Royales de Mari* had been improved.

² This contribution was prepared within the framework of project *Pouvoir et culture écrite en Haute-Mésopotamie au 18^e siècle av. J.-C.* (aapg-2022-PCEHM), which is financed for 2022–2026 by the French *Agence Nationale de la Recherche*. Francesca Nebiolo, postdoctoral researcher at PCEHM and the Collège de France, prepared the photographs for publication. My thanks go to Dominique Charpin who read a first draft and to Michael Roaf who carefully read and corrected my chapter.

³ Abbreviation for 'Revised Eponym List' in BARJAMOVIC/HERTEL/LARSEN 2012. I use these references in order to avoid identification problems between homonyms. If different, the names are rendered here by their Babylonian form. No lengths for vowels in Akkadian are given in personal names (PN) and geographical names (GN).

⁴ KRYSZAT 2004 and 2008 and KOLIŃSKI 2015. See also BLOCH 2014 for the identification of the 'conquest eponyms' known by the Assyrian Kinglist but not attested by Kültepe eponym lists.

1. The MEC manuscripts

M. Birot edited the texts of the MEC fragments in a synoptic manner organised by eponym. This presentation was astute, because it made the essential information obvious, enabling the reader to immediately appreciate the succession of the eponyms and the events with which they were associated.⁵ Wars, victories and defeats are the main commemorated events, biographical elements such as accessions to the throne, the death and even the birth of a king, and some exceptional natural phenomena such as a flood and an eclipse are mentioned but astonishingly no focus is given to religious or cultic events.⁶

M. Birot chose to present these eponyms by sections that could be reconstructed thanks to the different manuscripts and called these sections A1–24, B..., C..., D..., and E... Moreover, the breaks common to the different manuscripts were in this way clearly visible. This presentation was also justified by the fact that the various manuscripts reproduce generally the same text. It has, however, the disadvantage of obscuring the overall view of the individual manuscripts.

M. Birot correctly assumed that the published fragments came from five different manuscripts,⁷ two tablets originally containing complete texts and three fragments containing excerpts.

K. Veenhof proposed calling the two main exemplars of the Mari Eponym Chronicles ‘Edition I’ and ‘Edition II’. I will follow this proposal here so that readers of the different contributions in this volume find the same terminology for the eponym texts from Mari.

BIROT 1985: 219–220 ordered the manuscripts in 5 sections:

1. A.1288 = Veenhof Edition II.
2. M.7481+M.11250 as well as the fragments S.24-1, S.24-2, S.24-3, and A.1614 which are probably from the same exemplar = Veenhof Edition I.
3. S.115-26 = Veenhof Edition III.
4. M.5911
5. M.8566

It is clear that Edition II is some years older than Edition I. I will discuss the redaction date of the two editions below. The smaller excerpt tablets all concentrate on the earlier periods and do not include the later years when Yasmah-Addu was king of Mari.

We may assume that the eponym chronicles were learnt by heart and used as historical material by Samsi-Addu’s family as well as by elite members in all centers of Samsi-Addu’s empire. This seems obvious for Mari in the time of king Yasmah-Addu. Most astonishing is the fact that even during the reign of Zimri-Lim the MEC could be used for historical reference.⁸ The biggest exemplar of the MEC, Edition I, dates to Zimri-Lim’s reign.

2. Edition I

Edition I was, when intact, very large with a width of about 10 cm and a length of approximately 35 cm.⁹ When complete, it must have been impressive because of its beautiful script and careful layout. It recounted the events of perhaps 97 eponym years in the laconic style of a chronicle. The narration of events is more complex in the later years than in the earlier ones, according to a principle known for chronicles from other periods. Its incipit is inspired by the formula that we find at the beginning of the eponym lists of Kültepe-Kaneš KEL A, D and F.¹⁰ Thus Edition I of Mari begins with the words:

‘Since the beginning of the reign of Naram-Sin, vicar of our [lord (= Aššur)]. Since the eponym was established.’

and the document ends with the words:

‘Written by (lit. “the hand of”) Habdu-malik, reader Limi-Dagan.’

⁵ For a general view see BIROT 1985: 222–227. According to him, the author of the chronicles wanted to draw a picture of the competition between Samsi-Addu’s and Ešnunna’s dynasties (BIROT 1985: 223).

⁶ This is very different from Old Babylonian year names which give at least as much importance to the commemoration of religious events as to military successes. See for Old Babylonian year names HORSNELL 1999a, 1999b and 2004.

⁷ BIROT 1985: 219–220.

⁸ For the probable use of MEC as historical source, see ZIEGLER 2023. BIROT 1985: 223 hesitated, but rejected the idea of a redaction by scribes of Zimri-Lim. See for this now below, the historical setting of Edition I. Edition II clearly dates to the time of Samsi-Addu.

⁹ The upper part of the tablet, M.7481+M.11250, is described precisely by BIROT 1985: 220: width 10.0–10.4 cm, preserved height 7.8 cm and 2 cm thick at the edges, but close to 2.8 cm in the center. The average height of the lines is 0.35 cm.

¹⁰ GÜNBATTI 2008a: 104. For this line see §3.1 below comment to Edition I M.7481+ A1 line 1.

Edition I is the most complete eponym chronicle albeit it is badly broken. It starts with the reign of Naram-Sin of Aššur, and ends during the first years of Zimri-Lim from Mari – this means, in a period when Išme-Dagan was ruler of Aššur. For the overlap of the ‘Samsi-Addu calendar’¹¹ (beginning in autumn) with the first years of Zimri-Lim, normally beginning close to the spring equinox, but shifted with respect to the solar year at the beginning of his reign, see already our commentary and the table in FM V (CHARPIN/ZIEGLER 2003: 174). The possible correlation of the eponym calendar and the Mari calendar is presented in Fig. 3.1.

Eponym	Month in ‘Samsi-Addu calendar’ (<i>conjectural</i>)	Historical event concerning Zimri-Lim (ZL)	Month in Mari calendar	Year names of king Zimri-Lim attested by archival texts
	vi*	Bannum conquers Mari. Latest eponymical text from Mari. ZL enters Terqa. ZL enters Mari.	ZL 0 ix	Throne
	vii*	Messengers from Aleppo and Ešnunna in Mari.	x	Throne
	viii*	Messengers from Aleppo and Ešnunna in Mari. Ištar Deritum festival.	xi	Throne
	ix*	Marriage between the king of Yaminite Amnaneans and princess Atrakatum.	xii	Throne
	x*		ZL 1 ix	Throne
	xi*		ii	Throne/Annunitum
	xii*	Conquest of Kahat.	iii	Annunitum
Aššur-emuqi	i*		iv	Annunitum
	ii*		v	Throne/Annunitum
	iii*		vi	Annunitum/Kahat
	iv*		vii	Annunitum
	v*		viii	Annunitum/Kahat
	vi*	Death of Bannum.	ix	Annunitum/Kahat
	vii*	Female deportees from Kahat live inside ZL’s palace at Mari, among them Samsi-Addu’s widow Akatiya.	x	Annunitum/Kahat
	viii*	Simah-ilane coming from Babylon arrives in Mari. Ištar Deritum festival.	xi	Annunitum/Kahat
	ix*	Simah-ilane in Mari.	xii	Annunitum/Kahat
	x*		xii-bis	Kahat
	xi*		ZL 2 i	Euphrates/Addu/Kahat-bis
	xii*	End of the first Yaminite revolt. Walls of Mišlan and Samanum destroyed.	ii	Euphrates/Addu
Abu-šalim	i* – ...		iii	Addu
			iv	Euphrates/Addu
			v	Euphrates/Addu
		Marriage of ZL with Šibtu.	vi	Euphrates/Addu

Fig. 3.1. The possible correlation of the Assyrian eponym calendar and the Mari calendar.

Year names attested by published texts (see CHARPIN/ZIEGLER 2003: 257–260): *Throne* = ‘Year when Zimri-Lim entered the throne of the house of his father’; *Annunitum* = ‘Year when Zimri-Lim made a statue/an image of Annunitum of Šehrum’; *Kahat* = ‘Year when Zimri-Lim conquered Kahat’; *Euphrates* = ‘Year when Zimri-Lim put in order the Banks of the Euphrates’; *Addu* = ‘Year when Zimri-Lim vowed his statue to Addu of Aleppo’.

¹¹ Month names from the ‘Samsi-Addu calendar’ are marked with *. See ZIEGLER 2022 and see already CHARPIN/ZIEGLER 2003: 155–156.

The most recent eponym mentioned in this manuscript is that of Aššur-emuqi. Following a proposal by K. Veenhof, D. Charpin and I had assumed that Aššur-emuqi could correspond to the last months of Yasmah-Addu's life, a year that is always designated in the Mari archives as the eponym 'that followed Ṭab-šilli-Aššur' (*warki Ṭab-šilli-Aššur*).¹² This identification can no longer be maintained because Kültepe Eponym List G gives the following succession of eponyms from G 83 to G 86:¹³

Ṭab-šilli-Aššur (REL 197) / Ennam-Aššur (REL 198) / Aššur-emuqi (REL 199) / Abu-šalim (REL 200).

The year '*warki Ṭab-šilli-Aššur*' thus corresponds to eponym Ennam-Aššur. It is the moment of the collapse of Samsi-Addu's empire in the months following his death. It is the year of Yasmah-Addu's end. It is also during this eponym year that king Zimri-Lim acceded to the throne of Mari.

The question remains whether Edition I was written during Aššur-emuqi, or just after the completion of this eponym year. The account of the events of year Aššur-emuqi, which is narrated in six lines, is not preserved. The colophon contains the following information:

'In all: 70 [+24⁷ years] until the victory at the gate of Saggaratum.'

From these last lines we understand that the huge and beautiful MEC Edition I, now broken into pieces and far from complete, was written several months, perhaps even more than one year after Zimri-Lim's accession to the throne. Was it written for Zimri-Lim and was Zimri-Lim the winner at the gate of Saggaratum? This 'victory at the gate of Saggaratum' could perhaps be linked to a victory of Zimri-Lim over the first revolt of the Yaminites. This event was commemorated by an unusual year name labeled thus:¹⁴

'Year when Zimri-Lim defeated the Yaminites in Saggaratum and when he seized their kings.'

This victory of Zimri-Lim perhaps dated to the very end of the eponymate of Aššur-emuqi or to the very beginning of the eponymate of Abu-šalim. But this remains to be confirmed. If this idea is true, the entry referring to the last eponym in Edition I:

M.7481+: rev. 5' '4 [...]'

perhaps mentioned the four Yaminite kings vanquished on this occasion. In this case, Edition I clearly was produced for Zimri-Lim, the last king of Mari, and was linked to a celebration of one of his victories.

Nevertheless, it is not yet clearly established that Zimri-Lim had demanded the production of this text. J.-M. Durand has made another interesting proposal: Edition I possibly was taken as booty to Mari. It could have been taken from Šubat-Enlil to Mari when Samiya left the former residence city of Samsi-Addu.¹⁵ Another hypothesis would be to link the document's redaction to the conquest of Kahat – one of Zimri-Lim's major achievements at the beginning of his reign and his final victory over the heirs of Samsi-Addu's empire. Nevertheless, no victory at Saggaratum's gate is known for this military campaign. For the time being, these questions must remain open. An implicit reason for the supposed foreign origin of Edition I is, that the reader who spelled the text to the scribe is not yet identified with any official in Mari. According to the inscribed colophon at the end of the tablet, the document was written by Habdu-malik¹⁶ and dictated by a man named Limi-Dagan, who cannot be identified with any high official or scribe from Mari.¹⁷

¹² CHARPIN/ZIEGLER 2003: 168.

¹³ GÜNBATTI 2008a: 109.

¹⁴ DURAND 1983: 144–145 no. 128 (www.archibab.fr/T14567) (day 14 month ix): (10) MU *zi-im-r[i]-li-im* (11) *da-am-da-am ša DUMU.MEŠ ia-mi-na* (12) *i-na sa-ga-ra-tim i-du-ku* (13) *ù LUGAL.MEŠ-šu-nu ik-šu-d[u]*, see comment in CHARPIN/ZIEGLER 2003: 190–191. The year name is a variant of ZL 3. Year names most often commemorated events from the former year. The victory over Yaminite kings probably dated to the beginning of year ZL 2 but this is still a question of debate.

¹⁵ DURAND 1997: 43 n. 145. This proposal was quoted in CHARPIN/ZIEGLER 2003: 168 but is not genuinely ours. Samiya left Šubat-Enlil with Ešnun-nean help in Zimri-Lim's fourth year. This seems a little bit too late in Zimri-Lim's reign.

¹⁶ Several homonyms exist, the most famous Habdu-malik was Zimri-Lim's envoy and high official, see CHARPIN 1988: 207–213; a scribe also existed, attested *ibidem*.

¹⁷ The name Limi-Dagan is barely attested in Mari.

The participle *muštassūm* is attested by the text edited by MAUL 1994: 48–50 no. 17 (www.archibab.fr/T6837): l. 20. MAUL 1994: 50 commented on this noun: 'Da *šitassūm* in den Mari-Texten auch absolut im Sinne von "lesen, (laut) vorlesen" verwendet werden kann (vgl. ARM 2 132: 5, 7), dürfte das zugehörige Partizip *muštassūm* den "Leser" bezeichnen. *muštassūm* ist auch in der Tafelunterschrift M.7481: Rs. 8' belegt (vgl. M. Birot, MARI 4, 1985, S. 232 unten; die Zeile ist wohl folgendermaßen zu verstehen: "Hand des Habdu-Malik; der der es gegengelesen hat (*muštassū*) ist

2.1. M.7481+M.11250 obverse¹⁸ (see Figs. 3.3 and 3.4)

A1			<i>iš-tu re-eš</i> ^{gis} G[U.ZA na-ra-am- ^d EN.ZU UGULA be-lí-n]e <i>iš-tu li-mu-[um iš-ša-ak-nu]</i>
A2 ^{REL104}	2	I	<i>šur</i> ¹ -[^d EN.ZU]
A3 ^{REL105}		I	[^d a-šur-ma-lik]
A4 ^{REL106}	4	I	[^d a-šur-i-mi-ti]
A5 ^{REL107}		I	[en-nam- ^d EN.ZU]
A6 ^{REL108}	6		<i>i-na</i> [i-mi-im ša a-ku-tim PN GN] ^{ki} [(o o) i-š-ba-a]t
A7 ^{REL109}		I	[ma-ši-am-DINGIR]
A8 ^{REL110}	8	I	[i-din-a-hu-um]
A9 ^{REL111}			<i>i-na sa-[ma-nim a-mi-nu-um ša-du-pé-em i-š-ba-at]</i>
A10 ^{REL112}	10		<i>i-na</i> i-l[i-en-nam ^d EN.ZU-a-bu-um ma-a-at ši-ID-KI i-š-ba-at]
A11 ^{REL113}			<i>i-na en-n[a-ma-nu-um o o o o o o o o o o o o (...)]</i>
A12 ^{REL114}	12	I ¹	[en-nam- ^d a-šur]
A12 ^{bis} REL115			<i>i-na en-na[m-^dEN.ZU i-pi-iq-^dIŠKUR a-na</i> ^{gis} GU.ZA É a-bi-šu i-ru-ub]
A13 ^{REL116}	14		<i>i-na ha-na-</i> ^r ^d ¹ [na-ri-im a-mi-nu-um da-am ⁷ -da-a-am ša i-pi-iq- ^d IŠKUR i-du-uk]
A14 ^{REL117}			<i>i-na da-di-i[a]</i> ^r ^x ¹ [o o o o o o o o o o o o o o o o (...)]
A15 ^{REL118}	16		<i>i-na ka-pa-ti-ia</i> ^r ⁱ ¹ -[pi-iq- ^d IŠKUR da-am ⁷ -da-a-am ša a-mi-nim i-du-uk]
A16 ^{REL119}			<i>i-na iš-me-^da-šur i-pi**-iq**-</i> ^d [IŠKUR zi-qú-ra-tam i-š-ba-at]
A17 ^{REL120}	18		<i>i-na</i> ^d a-šur-mu-tab-bi-il** ^r ⁱ ^{**1} -[o o o o o o o o o o o o (...)]
A18 ^{REL121}			<i>i-na šu-</i> ^d MUŠ** ^r ^x ^{**1} [o o o o o o o o o o]
A19 ^{REL122}	20		[i-na i-din]-[a-bu ¹ -um ^d EN.Z[U- o o o o o o o o o o o o (...)]
A20 ^{REL123}			[¹ (o o o o o) i-l]i-d[a-an (...)]
A21 ^{REL124}	[22]		[Aššur-imitti]
A22 ^{REL125}			[Bušaya]
A23 ^{REL126}	[24]		[i-na da-di-ia ^d UTU-ši- ^d IŠKUR wa-li-id (...)]
A24 ^{REL127}			[Puzur-Ištar]
A25* ^{REL128}	[26]		[Isaya]
A26* ^{REL129}			[Abu-šalim]
A27* ^{REL130}	[28]		[Aššur-re'i]
REL131			[Ṭab-Aššur]
REL132	[30]		[Šu-Rama]
REL133			[Sin-išme'anni]

2.2. S.24-1+ S.24-2+A.1614b (see Figs. 3.5, 3.6, and 3.7)

B1 ^{REL134}	[Aššur-malik]
B2 ^{REL135}	[Daniya]
B3 ^{REL136}	[Ennam-Sin]
B4 ^{REL137}	[Aššur-balaṭi]
B5 ^{REL138}	[Ennam-Aššur/Sin]

Limi-Dagan.”).’ More probably *muštassûm*, ‘the one who speaks out loudly’ is not a ‘control-reader’ but the person who dictated the MEC to the scribe.

¹⁸ I transliterate the initial DIŠ sign as ¹. Eponyms marked by ¹ are without events, while years with events are introduced by *ina* followed by the eponym’s name without ¹. For this reason, K. VEENHOF and M. ROAF (Chapters 4 and 5 in this volume) prefer to transliterate the initial DIŠ as ¹. The space in the broken passages is estimated by [o] or [x]: x means that there are very scarce indications of an actual sign, o means that this space could be filled with a sign or could be empty. (o o) means that the space of two signs was most probably uninscribed. (...) means that there remains space on the right edge.

Restitution of missing signs is marked by <...>, destroyed signs by >...<, and intentionally erased signs by {x}. ° is ‘sic’.

B6 ^{REL139}		[Itur-Aššur]
B7 ^{REL140}		[Šu-beli]
B8 ^{REL141}		[Šarrum-Adad]
B9 ^{REL142}		[Šu-Laban]
B10 ^{REL143}		[Aššur-imitti]
B11 ^{REL144}		[Dadaya]
B12 ^{REL145}		[Dadaya no. 2]
B13 ^{REL146}		[Ahi-šalim]
B14 ^{REL147}		[Ušur-ša-Ištar]
B15 ^{REL148}	0'	^r i ¹ -[<i>na ka-at-ta-a ...</i>]
B16 ^{REL149}		<i>i-n</i> [<i>a šu-^dEN.ZU ...</i>]
B17 ^{REL150}	2'	<i>i-na</i> ^r a ¹ -[<i>bu-ša-lim ša-ba-at x ... ša né-re-eb-tim^{ki}</i>]
B18 ^{REL151}		<i>i-na šu-d</i> [<i>a-a ...</i>]
B19 ^{REL152}	4'	<i>i-na šu-da-d</i> [<i>i-im ša-ba-at ne-re-eb-tim^{ki}</i>]
B20 ^{REL153}		<i>i-na</i> ^d a-šur-tu-kúl-t[<i>i da-am₇-da-a-am ša ú-ni-ne-im ^dUTU-ši-^dIŠKUR i-du-uk</i>]
	6'	^r ú ¹ [o o o o o o o o o o <i>i-du-uk</i>]
B21 ^{REL154}		<i>i-na</i> PUZUR ₄ -iš ₈ -tár ^d UTU-ši- ^d IŠKUR [o o o o o o o o]
B22 ^{REL155}	8'	<i>i-na a-ta-na-[ah] i-pí-iq-^dIŠKUR da-a</i> [<i>m₇-da-a-am ša o o o o o o o o i-du-uk</i>] <i>ù ma-[a-at o o o o o o o o o (...)]</i>
B23 ^{REL156}	10'	<i>i-na e-ri-ši-im ^dUTU-ši-^dIŠKUR da-am₇-</i> [<i>da-a-am ša o o o o o o o o (...)</i>] <i>i-na BÀD [o o o o o o i-du-uk]</i>
B24 ^{REL157}	12'	¹ ^d a-šur-en-nam ša ši-[<i>pí²-ra²-tim²</i>]
B25 ^{REL158}		<i>i-na in-b</i> [<i>i-iš₈-tá</i>] <i>r i-pí-iq¹-^dIŠKUR [x o o o o o o o o o (...)]</i>
B26 ^{REL159}	14'	¹ ^d a-šur-be-el-ma-al-ki-[<i>im</i>]
B27 ^{REL160}		<i>i-na be-[la-nim x] ki ir ba na a [o o o o o o o o]</i>
B28 ^{REL161}	16'	<i>i-na s</i> [<i>ú-ka-lim</i>] ^d UTU-ši- ^d IŠKUR [o o o o o o o o]
B29 ^{REL162}		<i>i-n</i> [<i>a a-mur-^da-šu</i>] <i>r ^dUTU-ši-^dIŠKUR [o o o o o o o o o (...)]</i>
B30 ^{REL163}	18'	[<i>i-na</i> ^d a-šur-n] <i>i-šu ^dUTU-ši-^dIŠ[KUR ...]</i> [...]
B31 ^{REL164}	20'	[(...)] <i>mu-na-wi-ru-um (...)]</i>
B32 ^{REL165}		[Idnaya]

2.3. S.24-3 (see Figs. 3.8 and 3.9)

D0 ^{REL166}		[^{1?}] ^r da ¹ -[<i>da²-a-ia</i>]
D1 ^{REL167}	2'	[¹] P[UZUR ₄ . ^d MUŠ...]
D2 ^{REL168}		[<i>i-na a-bi</i>]- ^r ia-ia ¹ ^d UTU-ši- ^d IŠKUR ...]
D3 ^{REL169}	4'	[<i>i-na</i>] ^r a ¹ -ta-nim ^d UTU-ši- ^d IŠKUR o o o o o o o o o o o o o o o o o o ...]
		12 LUGAL.MEŠ <i>i</i> -[o o o o o o o o o o o o o o o o o o ...]
	6'	¹ ia-ah-du-un-[<i>i-im o o o o o o o o o o o o o o o o o o ...</i>]
		LUGAL.MEŠ <i>šu-nu ut-te</i> -[o o o o o o o o o o o o o o o o o o ...]
D4 ^{REL170}	8'	[<i>i-n</i>] <i>a ^da-šur-ták-la-ku ^dUTU-ši-^dIŠKUR ...]</i>
D5 ^{REL171}		[<i>i-na i-ši-im-^dEN.Z</i>] <i>U ^rd¹U[TU²-ši-^dIŠKUR o o o o o o o o]</i>
		Break at the bottom of the tablet

No MEC fragment gives the names of eponyms preceding the conquest of Mari by Samsi-Addu. It seems now clear, that the eponyms numbered REL 179–180 by BARJAMOVIC/HERTEL/LARSEN 2012 did not exist. Their proposal was based on a misinterpretation of fragment S.24-3. See commentary *ad loc.*

In the broken space at the bottom of the obverse of Edition I, the edge and the beginning of the reverse probably 9 eponyms (REL 172–182) were quoted, as well as the remarkable events during this time-span. Unfortunately, KEL G is broken until the eponym Awiliya (REL 194).

REL172	[Addu-bani]
REL173	[Abi-šagiš]
REL174	[Ṭab-šilli-Aššur]
REL175	[Iddin-Aššur]
REL176	[Namiya]
REL177	[Ahu-šarri]
REL178	[Dadiya]
REL181	[Aššur-taklaku]
REL182	[Haya-malik]

2.4. S.24-1+ S.24-2+A.1614b reverse (see Figs. 3.10 and 3.11)

E1 ^{REL183}		[i-na š]a-[lim- ^d]a-šur ¹ {x x} ^d UTU-š ⁱ - ^d IŠKUR ma-[a-at o o o o o o o o o (...)]
E2 ^{REL184}	2'	[i-na ^d E]N.ZU-mu-ba-al-li-iṭ ^d UTU-š ⁱ - ^d IŠKUR ma- ^r a ¹ -[at o o o o o o o o o (...)]
E3 ^{REL185}		[i-na r]i-iš- ^d UTU iš-me- ^d da-gan da-am ₇ -da-a-a[m ša o o o o o o o o o i-du-uk]
E4 ^{REL186}	4'	[i-na i]b-ni- ^d IŠKUR ^d UTU-š ⁱ - ^d IŠKUR ma-a-at [o o o o o o o o o o (...)]
E5 ^{REL187}		[i-na ^d a]-šur-i-mi-ti ^d UTU-š ⁱ - ^d IŠKUR da-am ₇ -[da-a-am ša o o o o o o o o o (...)]
	6'	ša-a-tu ú-te-er ma-a-at [o o o o o o o o o o (...)]
		me-tu-ra-an ^{ki} ma-a-at [x o o o o o o o o o o (...)]
	8'	¹ da- ^r du-ša a ¹ [x x x o o o o o o o o o o o o o (...)]
E6 ^{REL188}		i-na ì- ^r li-ILLAT ¹ -t[i o o o o o o o o o o o o o o o o o o o (...)]
E7 ^{REL189}	10'	i-na ri-ig-m[a-ni]m mu-t[a ² -nu-um o o o o o o o o o o o o o o o o (...)]
E8 ^{REL190}		i-na i-ku-u[n-pi-i]a mu-na-[o o o o o o o o o o o o o o o o (...)]
	12'	da-am ₇ -da-a-a[m o o o o o o o o o o o o o o o o (...)]
		ù ^d UTU-š ⁱ - ^d IŠKUR ¹ [o o o o o o o o o o o o o o o o (...)]
	14'	me-tu-ra-an ^{ki} k[a ² o o o o o o o o o o o o o o o o (...)]
		a-na da-du-ša [o o o o o o o o]
E9 ^{REL191}	16'	i-na [ás-qú-di-i]m ^d UTU-š ⁱ - ^d IŠKUR [x o o o o o o o o o iṣ-ba-at]
E10 ^{REL192}		i-na [^d a-šur]-ma-lik iš-me- ^d da-gan da-am ₇ -da-a-a[am ša o o o o o o o o o i-du-uk]
	18'	ù ^d UTU-š ⁱ - ^d IŠKUR nu-u[r-ru- o o o o o o o o o o o o o (...)]
		¹ ki-ip-ra-am LUGAL [o o o o o o o o o o o o o o o o (...)]
	20'	¹ ia-šu-ub- ^d IŠKUR LUGAL [a-ha-zi-im ^{ki} ¹ o o o o o o o o o o o o o (...)]
		¹ ia-šu-ub-li-im LUGA[L o o o o o o o o o o o o o o o o (...)]
	22'	^r G ² LUGAL.MEŠ an-nu- ^r tim ¹ ik-s[u-ma o o o o o o o o o o o o o o o o (...)]
		[a-n]a da-d[u-ša o o o o o o o o o o o o o o o o o o o (...)]
E11 ^{REL193/194?}	24'	[i-na ...-i]a tu-ru-u[k-...]
		¹ [...]
	26'	x [...]

2.5. A.1614a (see Fig. 03.16)

A.1614a most probably is a surface fragment of M.7481+. For a proposal of restorations see below, commentary.

		[...]- ^r a ² -ki ² -[...]
	2'	[...x] LA AN ^r da ¹ -am ₇ -da-am tu-ru-u[k-...]
		[...x] KI i-du-ku ¹ ia-ás-ma-ah-[^d IŠKUR...]
	4'	[...]-da-am ša DUMU.MEŠ ia-mi-n[a ...]
		[...A.EN]GUR UD.KIB.NUN.NA ú-ga-m[e...]
	6'	[...] a KI [...]
		[...] ^r x x ¹ [...]

2.6. M.7481+1125 reverse (see Figs. 3.12 and 3.13)

G1=E15 ^{REL199}	[i-na ^d a-šur]-e-mu-q[i ...]
2'	ṛx ¹ [...]
	ṛu ¹ [...]
4'	a [...]
	4 [...]
6'	da- [...]
	ŠU.NIGIN 70[+24? ša-na-t]um a-di da-am-ṛ-de-e-em ša [K]Á sa-ag-ga-ra-[tim ^{ki}]
	(Blank)
8'	ŠU ha-ab-du-ma-lik mu-uš-ta-as-su-ú li-mi- ^d da-gan

3. Commentary Edition I**3.1. M.7481+M.11250 obverse commentary**

- A1 1** Restoration according to a proposal from J.-M. Durand *apud* VEENHOF 2003: 20 : *iš-tu li-mu-u[m iš-ša-ak-nu]*. This formulation is parallel to the Kültepe eponym list KEL A (as well as KEL D) where it probably describes the inauguration of eponym officials at the beginning of Erišum I's reign. In MEC this formulation might seem less appropriate since the dating system by eponyms existed before Naram-Sin's reign.
- A2–6 2–6** These eponyms are known from Kültepe, notably thanks to KEL A and also to a lesser extent to KEL B, see BARJAMOVIC/HERTEL/LARSEN 2012: 94.
- A6 6** VEENHOF 2003: 35 comm. 107 noted concerning the eponym Akutum 'He (scil. Akutum) must be recorded in MEC A no. 5, where the damaged first sign could be [A]K.' Photographs clearly exclude the reading A[K], but L[I] seems possible. For this restoration see also VEENHOF (in this volume Ch. 4).
- A9 9** Restoration of the eponym as Samanum from the parallel in Edition II A.1288: i 3'. KEL G: i 1 starts with this eponymate, written ¹sa-ma (REL 111). On KEL A: 62 only [...]-ṛa¹ DUMU šu-be-lúm is preserved.
- A10 10** Restoration from the parallel in Edition II A.1288: i 4'–5' where the name is complete (¹li-li-en-nam). According to the eponym lists from Kültepe, the eponym's name should be Ili-alum: KEL A: 62 (*i-li-a-lúm* DUMU *sú-kà-li-a*) and KEL G: i 2 (*i-li-a-lúm*).
- A11 11** In Edition I, there was a comment for this eponymate while Edition II A.1288: i 6' and M.5911: 2' just name the eponym. The same is true for A11–12, 14, 17–19.
- A12 12** Line 12, almost completely restored, was transliterated erroneously by M. BIROT 1985: 227. He shared his confusion with the ancient scribes of the different exemplars of MEC editions probably because of a succession of eponyms all with names in Ennam-X. For this reason I use the indication A12 Ennam-Aššur (REL 114) and A12^{bis} Ennam-Sin (REL 115) for the two successive eponyms known from Kültepe eponym lists. No event is commemorated in l.12 = [A12 Ennam-Aššur], but according to M.5911: 3'–4', the enthronement of Ipiq-Addu II of Ešnunna took place in this eponymate. Concerning this event see the comment to the next line.
- A12^{bis} 13** Restoration of the commemorated event – Ipiq-Addu's enthronement – from A.1288: i 8', but in this document, the name of eponym Ennam-Sin (REL 115) is missing. In M.5911: 3'–4' Ipiq-Addu's accession is recorded as an event that happened during the eponymate of Ennam-Aššur (REL 114); the name of Ennam-Sin (REL 115) is missing completely.
- A13 14** Restoration from A.1288: i 9'–10' and M.5911: 5'–6'.
- A14 15** M.7481+ is the only exemplar to commemorate an event which happened during this eponymate. Parallels in A.1288: i 11' and M.5911: 7' only give the official's name.
- A15 16** Restoration from A.1288: i 12'–14' and M.5911: 8'–9' which is less well preserved.
- 17–19** The signs marked ** were copied by BIROT 1985: 238 but are not visible on the photographs. Apparently, a small fragment of the tablet's surface has been lost in the meantime. M. Birot's copy is doubtless correct.
- A17 18** Perhaps the king of Ešnunna Ipiq-Addu is again mentioned in the gap.
- A18 19** See M. BIROT's handcopy – no proposal can be made for x.
- Break after 21** The names of the following eponyms are known from eponym lists from Kültepe, KEL A gives REL 125–130 and KEL G continues until REL 141 (B8).

3.2. S.24-1+S.24-2+A.1614b obverse commentary

- Break before 0'** The names of eponyms REL 134–137, which BIROT 1985: 228–229 numbered B1–14, are attested by A.1288 ii, M.8566, and S.115-26.
- B15 0'** The line clearly started with ¹i¹-[na ...]. The parallels A.1288: ii 16' and M.8566: 6' only mentioned the name of eponym (REL 148). The scribe of M.8566: 6' spelled the name *ka-at-t[a...]* – his interpretation was apparently Kattaya.
- B17 2'** The conquest of some topographical feature related to Nerebtum seems to be commemorated for this year, but the parallels are not well preserved, see A.1288: ii 18'–19' and M.8566: 8'–9'.
- B18 3'** The line clearly started with *ina* and commemorated events from this year. The parallels A.1288: ii 20' and M.8566: 10' are limited to the eponym's name.
- B19 4'** Restoration from parallels, notably M.8566: 11': *i-na šu-da-¹di¹-im ša-ba-at né-r[e-eb-tim^{ki}]*.
- B20 5'** When collating the tablet in the past, I noted, 'According to the curvature, we are here (approximately) in the middle of the tablet.'
- Restoration from the parallels A.1288: ii 22'–23', see *ad loc.* for the toponym Unine, and M.8566: 12'–13'.
- 6'** It is not sure if there is enough space to propose the same restoration as in the parallel A.1288: ii 24'–26': *da-am-da-a-[am ša ...]*, ¹mu-ut-ia-[x ... i-du-uk]. In M.8566 this part of the description of events of B20 is not preserved.
- B24 12'** The name of eponym Aššur-ennam (REL 157) is followed by the signs ŠA ŠI [...]. Eponym lists from Kültepe show now that the eponym's name simply was Aššur-ennam. BIROT 1985: 230 n. 7 hesitated to read the name Aššur-ennam-šalim but resigned himself to do so and commented 'Le nom *Aššur-ennam-šalim* est encore inconnu à ma connaissance, et peu intelligible (...)'.
K. VEENHOF, in this volume Ch. 4, opts for emendation of the clearly written signs. This may be the best solution, but perhaps *ša ši*[...] was meant as a qualification of Aššur-ennam, indicating the place of origin or his title? If the latter was true, I would propose the title *ša šipirātīm* 'ambassador' well attested in Mari, see for instance ARM 2 46 (collation cf. www.archibab.fr/T8496): 5; ARM 14 110: 11; FM II 59: 7; FM XVI 41: ii 11. Nevertheless, this proposal is not sure.
- B25 13'** The reading *i-ni-i[b-IŠ]DAR* proposed by BIROT 1985: 230 has to be corrected, IN is sure, B[I] probable. See already VEENHOF 2003: 48 n. 82, his proposal was confirmed by the publication of KEL G by GÜNBATTI 2008a.
- B27 15'** There are possible traces of two oblique wedges before KI. It seems not very probable that these are traces of the end of the official's name NIJM. Among the possibilities are NJA, BJI, TJA, ŠJA, but no solution is available for the time being.

3.3. S.24-3 obverse, note on the position of the fragment and commentary

- Note** M. BIROT (1985: 220 2) indicated that S.24-3 was a fragment from the same tablet as S.24-1+S.24-2+A.1614. The photograph of this fragment has been mislabeled and I could find it only in March 2022. It provides – in my eyes – a solution to a long-lasting chronological debate.
J.-M. Durand *apud* CHARPIN/ZIEGLER 2003: 72 n. 255 proposed the reading in l. 9' UR = LIK and the restoration [Haya-ma]lik. The suggestion was made before C. Günbatti's publication of KEL G. The latter suggested that MEC D 1–4 could overlap with KEL G 57–60.
BARJAMOVIC/HERTEL/LARSEN 2012: 7 also discussed this issue but preferred Durand's solution: they suggested that MEC D 2 should be identified with KEL G 69 and that MEC D 5 should be restored [...]–lik, = Haya-malik, the year of the conquest of Mari by Samsi-Addu.
On the contrary, LACAMBRE 2019: 521, following a former proposal of G. KRYSZAT 2008: 214 preferred the solution that MEC D 1–5 should be identified with KEL G 57–61. This is clearly the correct solution.
- D0 1'** The reading ¹DA¹ is the best possible interpretation for the fragmentary sign. The restoration of the eponym Dadaya is based on KEL G: 56 *da-da-a* = REL 166.
- D1 2'** KA is sure. Restoring KA.ŠA = PUZUR₄ seems obvious. KEL G: 57 is PUZUR₂.^dMUŠ = REL 167, Puzur-Nirah.
- D3 4'–7'** Atanum has to be identified with the eponym whose name is rendered in Old Assyrian documents as Edinum. The events that happened during this year were described in a particularly long entry of 4 lines. Yahdun-Lim played a major role as well as several other kings. All were probably subdued by Samsi-Addu.
- D5 9'** The restoration of Išim-Sin's name was proposed by LACAMBRE in 2019: 521. KEL G: 61 reads ¹i-si-im-sú-in. The former proposal to read this sign UR = LIK and to restore Haya-Malik's name here (see note to S.24-3 above) has to be abandoned.

3.4. S.24-1+ S.24-2+A.1614b reverse commentary

- E1 1'** BIROT 1985: 231 restored the eponym name « *[En-na]m⁷-[^d]A-šur* » without indicating his reasons. No published text from Mari was dated by this eponym, which corresponds to *warki* Ṭab-šilli-Aššur. KEL G is broken after REL 178 (Dadiya). BARJAMOVIC/HERTEL/LARSEN 2012: 9 suggested placing Šalim-Aššur here, combining attestations in Mari documents of two homonymous eponyms with different fathers' names: Šalim-Aššur son of Ušranum and Šalim-Aššur son of Šalimanum. Their proposal took into account an oral suggestion by D. Lacambre. I follow this proposal. The two signs preceding Samsi-Addu's name are not yet clear.
- E7–8 10'–11'** The two successive entries for the eponym Rigmanum (l. 10') and Ikun-piya (l. 11') start with words beginning with the sign MU. In l. 10', M. Birot read *Mu-x[* and in l. 11 he transliterated the signs correctly as *Mu-na-[*. CHARPIN/DURAND 1985: 311 proposed the restoration in these lines of the name Munawwirum and suggested this could be the name of a general of Samsi-Addu's Empire. Munawwirum is indeed statistically the most probable solution – searching for words starting with '*mu-na-*' in www.archibab.fr returns 152 texts, most of them with an attestations of men called Munawwirum. There are few attestations for the participles *munaggirum* and *munnabtum*. Alas, no general Munawwirum has been known until now, nor any king of this name. In our study, CHARPIN/ZIEGLER 2003: 84 and fn. 64, we objected in a more basic way to the hypothetical general 'Munawwirum' because no other high official is mentioned in the MEC.
- 10'** We proposed *ibid.* for l. 10' *mu-t[a-nu-um ...]* 'disease', perhaps an allusion to an epizootic disease. According to collations of J.-M. Durand, this proposal was epigraphically possible. Unfortunately, the new pictures neither confirm nor refute the reading *mu-t[a-nu-um]* in l. 10' which, in my eyes is still possible.
- 11'** Anyway, in l. 11', another restoration has to be proposed. Birot's reading *mu-na-[...]* is beyond doubt. Since no high ranking man called Munawwirum is attested in any text from Mari, another solution is necessary. As a very uncertain hypothesis the word *munnabtum* 'refugee' could be chosen. We know that during this year the population of the stronghold Rapiqum left their hometown and were brought to different places of Samsi-Addu's empire. Usually, these people are described in Mari documents as 'deportees' *nāsihum*, but, if the hypothetical proposal *munnabtum* is right, the MEC would have chosen to use a different word.
- E9 16'** The restoration could be *[ar-ra-ap-ha-(am)^{ki} iṣ-ba-at]*. See for this restoration CHARPIN/ZIEGLER 2003: 90 n. 111. In this same eponymate Nineveh, a cult center in the kingdom of Nurrugum, was conquered by Išme-Dagan.
- E10 17'–23'** For these events during the eponym year Aššur-malik see CHARPIN/ZIEGLER 2003: 95–111.
- 17'** In CHARPIN/ZIEGLER 2003: 97 n. 183 we suggested the restoration *i-na [^da-šur]-ma-lik iṣ-me-^dda-gan da-am⁷-da-a-[am ša DUMU.^dIŠKUR LÚ yaš-i-la-nim i-du-uk]* 'In Aššur-malik Išme-Dagan vanquished [Mar-Addu the chief of Ya'ılanum]'.
- 18'–19'** For the conquest of Nurrugum, close to Nineveh, see CHARPIN/ZIEGLER 2003: 99. Kibram perhaps was king of Nurrugum, see EIDEM/LAESSØE 2001: 22 n. 30.
- 22'–23'** In CHARPIN/ZIEGLER 2003: 97 n. 183 we suggested the restoration (l. 22') *6⁷ LUGAL.MEŠ an-nu-^rtim¹ ik-s[u-ma te-né-še-et ma-at qa-ab-ra-a^{ki}]* (l. 23') *[a-n]a da-du-ša iṣ-ri-ik* 'he bound these 6⁷ kings [and offered the population of the land of Qabra t]o Daduša.' This proposal still remains possible. Daduša mentioned deportees and precious goods from Qabra in his stela (ISMAIL/CAVIGNEAUX 2003) : (col. viii 5) *ša-al-la-as-sú ra-pa-áš-tam* (6) *ma-ak-kur a-li-im šu-a-ti kab-tam* (7) *KÙ.ZI KÙ.BABBAR NA₄ wa-qar-tam* (8) *a-ši-la-le-e-em dam-qá-am* (9) *ù mi-im-ma šum-šu* (10) *ša ma-tum ši-i ir-šu-ú* (11) *a-na ÈŠ.NUN.NA^{ki}* (12) *a-al šar-ru-ti-ia* (13) *e-te-el-li-iṣ ub-lam-ma* 'I heroically brought its huge human booty, the heavy treasure of this city, gold, silver, precious stones, fine luxury items and everything else that this land possessed to Ešnunna my royal capital city.' The booty from Qabra, especially the prisoners were the reward for the huge auxiliary troops Daduša had offered to Samsi-Addu.
- E11 24'** In our reconstruction (CHARPIN/ZIEGLER 2003) we supposed that the eponym following Aššur-malik was Awiliya (REL 194), followed by Nimer-Sin (REL 195). BARJAMOVIC/HERTEL/LARSEN 2012 proposed inserting Ahiyaya between Aššur-malik and Awiliya. This view was not shared by all colleagues, including myself, but D. LACAMBRE (Ch. 7 in this volume) continues to insist that it is the only possible way to interpret the Chagar Bazar archives. The arguments are for the time being still not conclusive, but I am confident, that the answer will come soon.
- E15=G1 1'** The chronological setting of Aššur-emuqi thanks to KEL G 85 was one of the biggest surprises for specialists. This eponymate is not the year of Yasmah-Addu's death and the breakdown of the Upper Mesopotamian kingdom after the death of its founder, Samsi-Addu, but it is the year after the capture of Mari. For the correlation with Zimri-Lim's chronology, see above.

3.5. A.1614a possible restoration (Fig. 3.16)

The problem is to know whether A.1614a is a surface fragment of S.24-1+. M. Birot copied it inside S.24-1+, and the belonging of the piece to this tablet seems quite possible. In any case, the fact that this fragment A.1614a also mentions

the Turukkeans makes one think that its text summarizes the events of eponym year Awiliya (REL 194) for which see CHARPIN/ZIEGLER 2003: 112–118. I suggest moving this piece a little higher in the layout proposed by BIROT 1985: 239 so that A.1614a 1' is the continuation of S.24-1+. See in this setting already CHARPIN/ZIEGLER 2003: 112 n. 299–300.¹⁹ The composite text S.24-1+(+)A.1614a could perhaps allow the following reading and restoration:

E11	REL 194 or 193?	24'	[i-na ...-i]a tu-ru-u[k-ku-ú ib-ba]- ^l al-ki ^l -[tu o o o o o o o o o o ...]
			^l [o o o o o o]-la AN da-am ₇ -da-am tu-ru-u[k-o o o o ...]
		26'	[o o o o o o] KI i-du-ku ¹ ia-ás-ma-a[h- ^d IŠKUR (o o ...)]
			[o o o o o o da-am ₇]-da-am ša DUMU.MEŠ ia-mi-n[a i-du-uk]
		28'	[o o o o o A.EN]GUR UD.KIB.NUN.NA ú-ga-m[e-er o o ...]
			[o o o o o o o o]-a ^{ki} [x o o o o ...]
		30'	[o o o o o o o o] ^l x x ^l [o o o o o o ...]
			(Break)

3.6. M.7481 reverse commentary

2' Cf. VEENHOF 2003: 17.

7' Compare with the unusual year name of king Zimri-Lim from Mari discussed above, and attested in ARM 21 128 (MU zi-im-^lri^l-li-im / da-am₇-da-am ša DUMU.MEŠ ia-mi-na / i-na sa-ga-ra-tim i-du-ku / ù LUGAL.MEŠ-šu-nu ik-šu-du). Mesopotamian year names generally commemorate an event of the previous year. The year commemorating this victory of Zimri-Lim against the Yaminites is year ZL 3: the eponym year before ZL 3 corresponds to the last two months of the eponymate of Aššur-emuqi and the first ten months of the eponymate of Abu-šalim (REL 200). See above Fig. 3.1.

4. Edition II

While we have the beginning and end of MEC Edition I, the exemplar A.1288, which, following the proposal of K. Veenhof, may be called Edition II, has retained neither. At first glance, this document is less carefully prepared, its writing seems less elegant, but the layout is just as neat. In the broken upper edge, col. i must have started with the first year of Naram-Sin of Aššur. It tells in col. i the events in which the predecessors of Samsi-Addu, Aminum and Ila-Kabkabu as well as Samsi-Addu himself were confronted by the kings of Ešnunna, notably Ipiq-Addu II, the beginning of whose reign is mentioned. The commemoration of the birth of Samsi-Addu, an eclipse of the sun and the death of Aminum are preserved on this tablet. Col. ii commemorates the succession of Samsi-Addu after Ila-Kabkabu and the first years of his reign. On the reverse side, all of col. iii is missing. The surface of the tablet has not preserved any mention of the conquests of either Ekallatum or Aššur and of the twenty years of Samsi-Addu's reign in these cities (col. iii). Only a small part of col. iv is readable. It is this last preserved entry that will hold my attention here because in my eyes we can deduce from these lines the date of and the reason for the redaction of tablet A.1288.

M. Birot proposed no translation of these badly broken lines, but suggested that the names of three eponyms were mentioned here which he numbered C1–C3. In 1990 J.-M. DURAND republished in MARI 6 col. iv with many interesting suggestions:²⁰

col. iv	2'	[i-na o-o-o]-im ma-a-at [...]
		[mar-da-m]a-nim še-er-wu-nim ^{r^{ki}}
	4'	ù ha-bu-ra-tim LUGAL iš-ba-a[t-ma]
		BÀD ^d [IŠKUR ^{ki}]
	6'	ù BÀD ^d UTU-ši- ^d IŠ[KUR ^{ki}]
		i-na da-ad-mi ib-[ta-mi]
	8'	da-am ₇ -da-a-am š[a su-mu-e-pu-uh]
		i-na ta-ri-š[u i-du-uk]
	10'	[r]a-pi-qa-[am ^{ki} ...]
		ù [...]

¹⁹ But see D. LACAMBRE (Ch. 7 in this volume) who pleads for the insertion of Ahiyaya (REL 193), which was not the option chosen in our book (CHARPIN/ZIEGLER 2003) and which still presents a problem for Mari chronology since only two dated texts from Mari attest this year. He suggests the order Aššur-malik (REL 192) – Ahiyaya (REL 193) – Awiliya (REL 194). This question must remain open for the time being.

²⁰ DURAND 1990: 274. I give his reconstruction and the quote below exactly as in J.-M. Durand's proposal, but render sumerograms as elsewhere in this study lugal = LUGAL, ^dIM = ^dIŠKUR etc.

J.-M. Durand translated this thus:²¹

‘Sous l’éponymat de ..., le roi s’empara du pays de ..., de Mardamân, de Šerwûnum et de Haburâtum. Puis, ayant construit Dûr-Addu et Dûr-Samsî-Addu dans le royaume d’Alep, il infligea une défaite à Sûmû-Epuh sur le chemin du retour; Râpîqum ...’

and commented on the shape and layout of A.1288 in a longer discussion (DURAND 1990: 274-275) before coming to the following conclusion:²²

‘Dès lors, la conclusion qui découle des hypothèses précédentes est que cet éponyme en [...] -im doit se comprendre comme celui d’Ibni-Addu. Je restaurerai donc le haut de C₃ comme [*i-na ib-ni-^d*]IŠKUR, ce qui convient d’ailleurs tout à fait pour la place.’

K. VEENHOF (2003: 49) rejected this proposal but not for good reasons.²³ In this volume (Ch. 4 §4.2) he proposes instead a completely different interpretation. His major objection against J.-M. Durand’s proposal is the syntax. In his opinion starting the description of events with

ina (eponym) X *mât* A, B u C *šarrum iṣbat*

is strange and completely unusual. This is true. By the way, it is the only preserved passage where Samsi-Addu is mentioned only by his title ‘king’,²⁴ and according to K. Veenhof, this is only acceptable because the king’s name was mentioned just before. K. Veenhof concludes that line 2’ has to be restored

A.1288: iv 2’ : [^dUTU-*ṣi-^d*]IŠKUR *ma-a-at*

Thus, according to K. Veenhof, the eponym is not preserved in col. iv. K. Veenhof suggests the quoted events could be dated to Haya-malik (REL 182), the year of Samsi-Addu’s conquest of Mari.

K. Veenhof’s objections are strong. Nevertheless, I prefer to stick for the time being to J.-M. Durand’s interpretation,²⁵ and I continue to accept the idea that we have in iv 2’ the name of an eponym.²⁶ The only good restoration then is [*ib-ni-^d*]IŠKUR, Ibni-Addu (REL 186), but of course we must keep in mind that it is not absolutely certain.

According to J.-M. Durand’s interpretation, the last passage preserved on this large tablet contained the events of the eponymate of [Ibni-]Addu (E4 = REL 186), that is to say about four years after the capture of Mari and probably ten years before the death of Samsi-Addu. Written in a slightly larger and more widely spaced script, the writer recounts with a profusion of details several victories of Samsi-Addu during this year.

The first passage (A.1288: iv 1’–3’) enumerates victories in Upper Mesopotamia’s north-eastern²⁷ Tigris region:

‘[In Ibni-Ad]du, the king took the countries (lit. country) of [Mardam]anum, Šerwunum and Haburatum.’

The account continues (A.1288: iv 4’–8’) with successful deeds in the west²⁸ of Upper Mesopotamia:

‘He began to build Dur-[Addu] and Dur-Samsi-Ad[du] in Dadmum. He won a victory over [Sumu-epuh] on his return.’

Before the break, Râpîqum, on the south-western edge of Upper Mesopotamia (A.1288 iv 10’) is mentioned and we may guess that the account continued with events in the south-east.

If these events took place in the eponymate of Ibni-Addu (REL 186), we can compare the extremely long entry in Edition II with that in Edition I, which is much shorter:

²¹ DURAND 1990: 274.

²² DURAND 1990: 275.

²³ VEENHOF 2003: 49 n. 85. He erroneously identified this eponymy with an eponymy « the immediate sequel of MEC B: *30 », but J.-M. Durand clearly had in mind a much more recent Ibni-Addu (now REL 186), for which dated Mari texts were already available, see CHARPIN 1985: 257.

²⁴ There could be another example in broken passages, see comment to B20 in A.1288: ii 23’. LUGAL alone also occurs in S.115.26: 15’ – it is thus not excluded during Samsi-Addu’s period.

²⁵ See already the proposal for a reconstruction of events in CHARPIN/ZIEGLER 2003: 82–83.

²⁶ In my eyes, the syntax of a reconstructed sentence [*ina* (eponym) X *Samsi-Addu mât* A, B u C *šarrum iṣbat* is impossible, while J.-M. Durand’s proposal is perhaps unusual but nevertheless possible.

²⁷ See ZIEGLER/LANGLOIS 2016: 339 s.v. Širwun and ZIEGLER 2022: 45–47 s.v. Haburatum, 89–91 s.v. Mardaman. The latter is now identified with Bassetki (PFÄLZNER/FAIST 2020).

²⁸ Dadmum is the kingdom of Aleppo. For the localisation of the fortresses Dur-Addu and Dur-Samsi-Addu see ZIEGLER/LANGLOIS 2016: 84 and 86.

S.24-1+: rev. 4' [i-na i]b-ni-^dŠKUR ^dUTU-ši-^dŠKUR ma-a-at [o o o o o o o o o o (...)]

‘[In I]bni-Addu Samsi-Addu [conquered] the land of [...].’

Unlike Edition I, Edition II (A.1288 col. iv) left behind the telegraphic style of chronicles. Col. iv looks more like a royal commemorative inscription, demonstrating by the enumeration of victories in the four cardinal directions that Samsi-Addu was indeed ‘king of the universe’, *šar kiššatim*, as he claimed to be in several inscriptions.²⁹

In my eyes, MEC Edition II ended with these events, and it seems likely to me that the text was written down after their completion or in the following eponymate.³⁰ If the date of the events enumerated in col. iv is indeed the eponym year Ibni-Addu, and, if the document was written at this moment, the question remains, for what reason?

We know that the construction of a sanctuary for goddess Belet-Akkade was undertaken during this year in Mari.³¹ One administrative document from the Smaller Palace ‘Chantier A’ in Mari documents bread distributions for Amaduga, the female servants (wives) of the king³² and the children of the king at the moment of the *urubātum* ceremony of the temple of Belet-Akkade. The document is dated to day 7 of month xii Ibni-Addu.³³

Was this a moment when Samsi-Addu personally came to Mari? Was Yasmah-Addu crowned to be king in Mari on this occasion or had he already been installed on the throne? The only thing we know is that the last weeks of the eponym year Ibni-Addu were full of events. When the following year started, the selection of a new eponym was apparently delayed. One administrative document in Mari, recording the distribution of beer for the ‘meal of the king and his army’ dated to day 21 of the first month in ‘the eponymy which was not yet named and which follows Ibni-Addu’.³⁴

The eponym year which followed Ibni-Addu, Aššur-imitti is less well attested by documents. Edition I devotes four long lines to this eponymate, which are unfortunately broken. In these lines, several victories of Samsi-Addu, the city Me-Turan and Daduša are mentioned in a completely broken context. Perhaps Samsi-Addu was occupied in the eastern margins of the Empire.

Interestingly, the eponymate of Aššur-imitti is Samsi-Addu’s sixtieth year of life.³⁵ I would suggest, that Edition II was created at the very end of Ibni-Addu to celebrate this event. This fact could explain, why the MEC mentions an event which is not known for any other king of Mesopotamia, namely Samsi-Addu’s birth:

A23 (REL 126) ‘During Dadiya the king, Samsi-Addu, was born.’

Dadiya (REL 126), Samsi-Addu’s first year of life; can be roughly dated to 1844 BC according to the Upper Middle Chronology and Aššur-imitti (REL 187), his sixtieth, to 1785 BC. In our modern view, Samsi-Addu was 59 years old, but I imagine, that the scribe counted inclusive years. According to my suggestion, the MEC Edition II could have been composed at the end of Ibni-Addu and perhaps was read out during festivities in the Belet-Akkade-temple in the last weeks of this year in presence of several high ranking ladies and children of Samsi-Addu among whom Yasmah-Addu was the most famous and perhaps already at this date king of Mari.

4.1. A.1288 (see Figs. 3.14 and 3.15)

The tablet A.1288 is flat and measures 13.5 cm × 10.8 cm, with a maximum thickness of less than 3 cm. It contains four columns. For their layout, a string was apparently lightly impressed on the surface which is visible in col. i after l. 8' = col. ii 4'–5', between col. i 17' and col. ii 12' as well as at the end of col. i 22' = the beginning of col. ii 17'–18'. Nevertheless, the scribe did not respect this division well and col. i frequently overlaps it notably in its upper part (lines 4', 9', 10').

The upper and lower edges are missing. The original size cannot be reconstructed with certainty. Column iii is completely lost, and col. iv is only preserved in its upper part.

The loss of the top of the tablet can be reconstructed from Edition I: at least 6 lines are missing.

²⁹ FRAYNE 1990: 47–63; RIMA 0.39.1: 2, 0.39.2: top 4, 0.39.8: 2, 0.39.12: 2 and WAETZOLDT 2000: i 4.

³⁰ KOLIŃSKI 2015 interpreted col. iv completely differently. He proposed that col. iii and col. iv covered the eponymates until Abi-šagiš (REL 173).

³¹ See for the importance of Akkade in Samsi-Addu’s family history CHARPIN 2004: 372–376.

³² GEME₂.MEŠ LUGAL probably means ‘wives’ of the king, since the following line enumerates children of the king. Female servants would have figured after princes and princesses.

³³ Text TH 82.236 quoted e.g. in CHARPIN/ZIEGLER 2003: 80 and n. 27. See for an edition www.archibab.fr/T22761.

³⁴ TH 84.42, quoted in CHARPIN/ZIEGLER 2003: 83 n. 60. But who was this king leading armies: was he Yasmah-Addu or Samsi-Addu?

³⁵ This event could explain why the Edition II text was so long and so detailed for Aššur-imitti.

The script is straight, deep and generously spaced, its size rather large, with the lines ca. 0.5 cm high. This kind of handwriting is more frequent for the time of Samsi-Addu than for Zimri-Lim's scribes.

col. i

A1		[iš-tu re-eš ^{giš} GU.ZA na-ra-am- ^d EN.ZU UGULA be-li-mi]
A2 ^{REL 104}		[^l šu- ^d EN.ZU]
A3 ^{REL 105}		[^{ld} a-šur-ma-lik]
A4 ^{REL 106}		[^l a-šur-i-mi-ti]
A5 ^{REL 107}		[^l en-nam- ^d EN.ZU]
A6 ^{REL 108}		[i-na li-mi-im ša a-ku-tim PN GN ^{ki} iṣbat]
A7 ^{REL 109}		[^l ma-šī-a-am ^l -DI[NGIR]
A8 ^{REL 110}	2'	[^l i-din-a-hu-um]
A9 ^{REL 111}		[i-na sa-m]a-nim a-mi-nu-um ša-du-pé-em iṣ-ba-at
A10 ^{REL 112!}	4'	[i-na] ^l i ^l -li-en-nam ^d EN.ZU-a-bu-um ma-a-at ši-ID KI iṣ-ba-at
A11 ^{REL 113}	6'	[^l en-nam-ma-nu-um]
A12 ^{REL 114}		[i-na] en-nam- ^d a-šur
A12 ^{bis? REL 115?}	8'	[^l i-p]i- ^l iq- ^d IŠKUR a-na É a-bi-šu i-ru-ub
A13 ^{REL 116}		[i-na ha-n]a-na-ri-im a-mi-nu-um da-am ₇ -da-a-am ša i-pi- ^l iq- ^d IŠKUR i-du-uk
A14 ^{REL 117}	10'	[^l d]a-di-ia
A15 ^{REL 118}	12'	[i-na k]a-pa-ti-ia [^l i-pi- ^l iq-] ^d IŠKUR da-am ₇ -da-a-{AM}-am ša a-mi-nim i-du-uk
A16 ^{REL 119}	14'	[i-na iṣ-me]- ^d a-šur i-pi- ^l iq- ^d IŠKUR zi-qú-ra-tam iṣ-ba-at
A17 ^{REL 120}	16'	[^l a-šur-mu-ta-bi-il]
A18 ^{REL 121}	18'	[^l š]u- ^d MUŠ
A19 ^{REL 122}		[^l i-din ^l -a-bu-um]
A20 ^{REL 123}	20'	[^l i-[li]-da- ^l an ^l]
A21 ^{REL 124}		[^l a- ^l šur ^l -i-mi-ti]
A22 ^{REL 125}	22'	[^l bu ^l -ša-a-ia]
A23 ^{REL 126}		[i-na da-d]i- ^l ia ^l LUGAL ^d UTU-ši- ^d IŠKUR wa-li-id
A24 ^{REL 127}	24'	[i-na PUZUR ₄ -iṣ ₈ -tá]r ^l na ^l -ah-du-ur ^d UTU
A25* ^{REL 128}	26'	[i-na i-sà-a-ia m]u- ^l ut ^l a-mi-nim
A26* ^{REL 129}		[i-na a-bu-ša-lim o x o] ^l ša ⁷ ^l - ^d IŠKUR
A27* ^{REL 130}	28'	[o o o o o o o o o] ^l u ⁷ [.....x]

(Break of probably 2 lines and the lower edge. Two or three of the eponyms enumerated below are to be restored at the end of col. i.)

REL131	[Ṭab-Aššur]
REL132	[Šu-Rama]
REL133	[Sin-išme'anni]
B1 ^{REL 134}	[Aššur-malik]
B2 ^{REL 135}	[Daniya]
B3 ^{REL 136}	[Ennam-Sin]
B4 ^{REL 137}	[Aššur-balaṭi]

B5 ^{REL138}	[Ennam-Aššur]
B6 ^{REL139}	[Itur-Aššur]

col. ii

(Break of ca. 8 lines. 6 or 7 of the eponyms enumerated above are to be restored at the beginning of col. ii.)

B7 ^{REL140}		<i>i-na šu-bé-[li² i-la-kab-ka-bu-ú]</i>
	2'	<i>šú-up-r[a-a-am iṣ-ba-at]</i>
B8 ^{REL141}		<i>i-na LUGAL.[^dIŠKUR da-am₇-da-a-am]</i>
	4'	<i>ša i-pí-i[q-^dIŠKUR LÚ.ELAM i-du-uk]</i> <i>ù LUGAL ^rd¹[UTU-ši-^dIŠKUR]</i>
	6'	<i>[a-na É a-bi-šu i-ru-ub]</i>
B9 ^{REL142}		^I <i>šu-[la-ba-an]</i>
B10 ^{REL143}	8'	<i>i-na ^da-š[ur-i-mi-ti]</i> <i>^rlu¹-ul-lu-ú da-am₇-da-[a-am ša LUGAL]</i>
	10'	<i>i-na la-za-pa-tim i-du-[ku]</i>
B11 ^{REL144}		<i>i-na da-da-a-ia mu-ut-a[b-bi-ih o o (...)]</i>
B12 ^{REL145}	12'	<i>i-na da-da-a-ia MÌN i-p[i-iq-^dIŠKUR]</i> <i>ar-ra-ap-ha-am^{ki} iṣ-ba-[at]</i>
B13 ^{REL146}	14'	<i>i-na a-hi-ša-lim ṣa-ba-at ga-[o o ...]</i>
B14 ^{REL147}		^I <i>ú-ṣur-^rša¹-iš₈-tá[r]</i>
B15 ^{REL148}	16'	^I <i>ka-ta-a [x o]</i>
B16 ^{REL149}		<i>i-na šu-[^dEN.ZU x x x x ...]</i>
B17 ^{REL150}	18'	<i>i-na ^ra-bu-ša¹-li[m ṣa-ba-at ...]</i> <i>ša né-[re-eb-tim^{ki}]</i>
B18 ^{REL151}	20'	^I <i>šu-da-[a-ia]</i>
B19 ^{REL152}		<i>i-na šu-da-di-im ṣa-ba-[at né-re-eb-tim^{ki}]</i>
B20 ^{REL153}	22'	<i>i-na ^da-šur-tu-^rkú¹[-ti da-am₇-da-a-am]</i> <i>^rša¹ ú-ni-ne-a-[yi-im LUGAL[?] i-du-uk]</i>
	24'	<i>[ú² ...]</i> <i>da-am₇-da-a-[am ṣa ...]</i>
	26'	<i>^lmu-ut-ia-[x ... i-du-uk]</i>
B21 ^{REL154}		<i>[i-na] P[UZUR₄-iš₈-tár ^dUTU-ši-^dIŠKUR]</i>

(Break of probably 2-3 lines. It may be supposed that the text continued on the lower edge.)

col. iii

(completely missing)

col. iv perhaps E4

E4 ^{REL186}		
	2'	<i>[o o o o]-IM ma-a-at</i> <i>[ma²-ar²-da²-m]a² -nim še-er-wu-{x} -nim^{rki¹}</i>
	4'	<i>ù ha-bu-ra-tim LUGAL ^riṣ-ba¹-a[t]</i> <i>BÀD ^d[o o o o o]*</i>
	6'	<i>ù BÀD ^dUTU-ši-^dIŠ[KUR]*</i> <i>i-na da-ad-mi ib-[o o o o o]</i>
	8'	<i>da-am₇-da-a-am ^rša¹ [o o o o o]</i> <i>i-na ta-ri-š[u i-du-uk (o o o)]</i>
	10'	<i>ra-pí-qa-[</i> <i>ù [x ...]</i>

5. Commentary Edition II

5.1. A.1288 commentary

- Upper edge lost** The restorations or proposals are based on the beginning of Edition I, M.7481+: 1–6, but I assume that the first entry in A.1288 was shorter.
- A9 i 3'** See comment above to Edition I M.7481+A9 l. 9.
- A10 i 4'** This line presents several problems.
The eponym's name is different in the Kültepe eponym lists. It is in KEL A: 62 ¹*li-a-lum* DUMU *sú-kà-li-a* and in KEL G: i 2 ¹*li-a-lum*. For the interpretation of Old Assyrian personal names written *-a-LIM*, see DERCKSEN 1991 contradicted by DONBAZ 1993, who pleads for the interpretation *Ilī-ālum*.
ŠI ID KI : BIROT 1985: 227 n. 1 and 236 n. 2 did discuss two interpretations. The reading of this unknown toponym is uncertain. In ŠI ID KI, the final KI could be part of the country's name or could be the determinative ki. M. Birot wondered first if this toponym could be compared to Šidqan mentioned in the still unpublished text M.11509, but later, in a final note to his article, thanks to remarks by J.-M. Durand, he observed that another unpublished text M.8934 mentions a 'land of Šit' (*ma-a-at šit*).
Sin-abum is generally identified with Sin-abušu (cf. KOPPEN 2009–2011). For chronological issues concerning his long reign, see VEENHOF in this volume Ch. 4.
- A12 i 7'** I restored at the beginning of l. 7' [*i-na*] but [¹] is also possible.
- A12^{bis} i 8'** Restoration from the parallel in M.5911: 3. VEENHOF 2003: 11 first thought that M. Birot omitted by mistake the eponym's name Ennam-Sin, but there is no space for it in A.1288. Perhaps the ancient writer was disturbed by three successive eponym names starting with Ennam-: KEL 112 Ennamanum – KEL 113 Ennam-Aššur – KEL 114 Ennam-Sin (= REL 113–115) when Ipiq-Adad came to the throne. The scribe of A.1288 attributed this event erroneously to the eponymate of Ennam-Aššur, i 7'. This is clear from the parallel fragment M.5911: 3'–4' where the same mistake was made.
- A22 i 22'** This collation had already been proposed by DURAND 1997: 43.
- A23 i 23'** Before the publication of the Kültepe eponym lists, DURAND 1997: 43 collated the tablet and proposed the reading: [*i-na*] [¹*i-na*-ia** LUGAL ^dUTU-šⁱ-^dŠKUR *wa-li-id*. This proposal cannot be maintained.
- A24 i 25'–29'** BIROT 1985: 228 indicated 'Lacune L₁', he was not sure about the existence of A22–24, and could not know the following eponyms. I use A25*–27* for these entries.
- A25* i 26'** DURAND 1997: 43 collated this passage and established the reading *nahdur šamšim* 'solar eclipse'. For the implications for Middle Bronze Age chronology, see M. ROAF's contribution in this volume Ch. 5.
- A26* i 27'** DURAND 1997: 43 proposed, as a hypothesis, that these lines continued the description of events during the eponymy in which an eclipse had taken place by restoring i 26': [*ib³-ba²-šⁱ-ma m*] *u*-ut* a-mi-nim*. It must be observed that a line divider separates i 25' from i 26'. It seems more probable to me that the two events were commemorated for two different eponym years. See also in this sense K. VEENHOF's and M. ROAF's contributions (Chapters 4 and 5 in this volume).
- A27* i 28'** The Kültepe eponym list KEL A ends with Aššur-re'um (REL 130) whose name perhaps figured here in i 20'.
- B7 ii 1'** Restoration according to S.115-26: 7'–8' (¹*šu-be-li*). The writing of the official's name, šu-BI-[LI] is unusual in Mari.
- B8 ii 3'–6'** Restoration according to S.115-26: 9'–12' and see there for comments. For a parallel to the expression *ana bīt abīšu erēbum* (and not the more usual *ana kussi bīt abīšu erēbum*) see above comment to A12^{bis} col. i 8'.
- B10 ii 8'–10'** See the more detailed comment below §6.4, S.115-26: B6 ll. 9'–12'. For these events, and a possible proof that the Mari eponymy chronicles were known and used as historical material by administrators at the time of king Zimri-Lim, see ZIEGLER 2023.
- B11 ii 11'** The name is restored from S.115-26 l. 17' but remains difficult, see the discussion below in the commentary to S.115-26. Only one verb can be restored, perhaps 'Mut-A[bbih died (*imtut*)]'?
- B13 ii 14'** Toponyms starting with GA are not legion. Gasur, the ancient name of Yorgan Tepe, later Nuzi, is not yet attested in Old Babylonian documents, but would have been close to Arrapha conquered in the previous year.
- B18 ii 20'** The eponym's name is attested by M.8566: 10' and by KEL G: 41 ¹*šu-da-a*. Nevertheless, the traces at the end of the line would make D[I] possible. Šu-Dadim is the following eponym (REL 152).
- B20 ii 23'** The restoration LUGAL is hypothetical, see below comm. to M.8566: 13'.
The defeated enemy seems to be 'the (man) from Unine'. For this toponym which has to be localised in the Suhum region close to Rapiqum (probably Tell Anbar), see ZIEGLER/LANGLOIS 2016: 380.
- ii 24'** Restoration of [*i*] according to S.24-1 obv. 1'.

6. Smaller fragments and excerpts of MEC

Several smaller texts contain excerpts of the Mari eponymy chronicles. It is not clear for which purpose these texts were written. K. VEENHOF (Ch. 4 in this volume), argues that these smaller documents were complete editions of the MEC but written in several complementary tablets.

6.1. M.5911 (see Fig. 3.17)

The fragment M.5911 cannot be attributed to one of the two bigger exemplars of the chronicles published by Birot and must be part of another edition. The layout is carefully done, but several mistakes were made by the scribe – the most striking one is the omission of the eponym Ennam-Sin (REL 115). It could have been written by a less experienced scribe.

A10 ^{REL112}	0'	[i-na i-li-en-nam ^d EN.ZU-a-bu-um] [(o o o) m]a- ^r a-at š ⁱ l-[ID KI i-š-ba-at]
A11 ^{REL113}	2'	^r l ¹ en-na-ma-[nu-um]
A12 ^{REL114}	4'	i-na en->NA-<nam ^d a-š[ur i-pi-iq- ^d IŠKUR] a-na ^{gis} GU.ZA ^r É ¹ [a-bi-šu i-ru-ub]
A13 ^{REL116}	6'	i-na ha-na-na-ri-[im a-mi-nu-um] da-{A}am ^r -da-a-am [ša i-pi-iq- ^d IŠKUR i-du-uk]
A14 ^{REL117}		[^l] da-da-[a-ia]
A15 ^{REL118}	8'	^r i ¹ -na ka-pa-ti-ia ^r i ¹ -[pi-iq- ^d IŠKUR] da-am ^r -da- ^r a ¹ -[am ša a-mi-nim i-du-uk]

6.2. M.5911 commentary

Note		K. VEENHOF (Ch. 4 in this volume) wonders whether M.5911 is a fragment of M.8566.
A10 1'		Restoration from the parallel in A.1288: i 10, see comment there. The eponym's name is <i>i-li-en-nam</i> in A.1288: i 10 from Mari, but the eponym lists from Kültepe indicate the name is Ili-alum.
A12 3'–4'		The eponym's name has to be corrected to Ennam-Aššur. Restoration by combination with A.1288: i 7'–8'. The present fragment M.5911 uses the correct formulation <i>PN ana kussi bīt abīšu īrub</i> while A.1288 gives a shorter variation <i>Ipiq-Addu ana bīt abīšu īrub</i> . This same shorter sentence is used also for Samsi-Addu's entering the house of his father (§6.3 B8 in S.115-26: 11'–12'). The accession of Ipiq-Addu is probably recorded in a broken passage in Edition I (M.7481+: 13) as taking place the following year, during the eponymate of Ennam-[Sin] (REL 115), whose name is missing in this document.
A13 5'–6'		Restoration from the parallel in A.1288: i 9'–10'.
A16 8'–9'		Restoration from the parallel in A.1288: i 12'–14'.

6.3. S.115-26, Veenhof's 'Edition III' (see Fig. 3.19)

The fragment S.115-26 is the lower part of a flat tablet with round corners. The fragment is a third or perhaps only a quarter of the original text. It seems to be an excerpt tablet, but we can't say with which eponyms the tablet started and ended.

Obverse

B1 ^{REL134}		I ¹ ^d a- ^r šur ¹ -ma-li[k]
B2 ^{REL135}	2'	i-na da-ni-ia ša-ba-at hu-up-ši-im ^r ki ¹
B3 ^{REL136}		i-na en-nam- ^d EN.ZU mi-lum ša ma-a-tim ru-uq-ti[m]
B4 ^{REL137}	4'	I ¹ ^d a-šur-ba-la-ṭi
B5 ^{REL138}		I ¹ en-nam- ^d a-šur
B6 ^{REL139}	6'	I ¹ i-túr- ^d a-šur
B7 ^{REL140}		i-na šu-be-li i-la-kab-ka-bu-ú
	8'	šu-up-ra-a-am i-š-ba-at

Lower edge

B8 ^{REL141}	10'	i-na LUGAL. ^d IŠKUR da-am ^r -da-a-{AM}-am ša i-pi-iq- ^d IŠKUR LÚ.ELAM i-du-uk
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Reverse

		ʾu ¹ LUGAL ^d UTU-ši- ^d IŠKUR a-na É a-bi-[šu]
	12'	i-ru-bu ^o
B9 ^{REL142}		¹ šu-la-ba-an
B10 ^{REL143}	14'	i-na ^d a-šur-i-mi-ti
		lu-ul-lu-um da-am ⁷ -da-a-am ša LUGAL
	16'	i-na la-za-pa-tim i-du-uk
B11 ^{REL144}		¹ i-na ¹ [da-d]a-a-ia mu-ut-ab ² -bi- ¹ ih ² ¹ [x x o o]
B12 ^{REL145}	18'	[i-na da-da-a-ia MİN] i-pi-iq- ^d [IŠKUR] [ar-ra]- ¹ ap-ha-am ¹ [^{ki} iš-ba-at]

6.4. S.115-26 commentary

- Note** K. Veenhof identified this text as his Edition III. The script of S.115-26 is very different from A.1288. It seems clear that this tablet contained an extract that did not start with the beginning of Naram-Sin's reign (REL 104) – the expected layout does not allow the restoration of 30 entries in the lost upper part of the tablet. If a historical event was chosen, it could be the birth of Samsi-Addu ... but this seems too short since only 8 eponym entries would be missing. Another possibility would be the beginning of Ipiq-Addu's reign (REL 114 or 115).
- B5 5'** KEL G: 28 records for this year the name ¹e-na-si-in for which see BARJAMOVIC/HERTEL/LARSEN 2012: 98 comment to REL 138. They doubt which of the readings is correct. According to VEENHOF 2003: 55–57, the end of kārūm Kaneš II should be situated close to this eponymy. BARJAMOVIC/HERTEL/LARSEN 2012: 71–72 suggest REL 139.
- B7 7'** The writing *-be-li* is less frequent in Mari, where scribes generally prefer *-be-lí*. Edition II (A.1288: ii 1' šu-bé-[li]) is even more exceptional.
- B8 9'–12'** The scribe recorded this series of events on the lower edge and the upper part of the reverse of the tablet. Is it by chance that the eponymy name Šarrum-Adad written LUGAL-^dIŠKUR is the year when 'king Samsi-Addu' written LUGAL ^dUTU-ši-^dIŠKUR, acceded to the throne and became king? Or did the writer of the MEC underline this eponym's name as a kind of good omen? I would opt for the second solution. The eponym's name is not complete in KEL G: 31.
- The place where Samsi-Addu became king at this time of history is unknown. From the little we know about Samsi-Addu's family, we would guess that it was Zaralulu or some other place in the neighborhood of Ešnunna. But see below, the comment to B10.
- 12'** The subjunctive *irubu* is probably a scribal error. Or should we interpret it as what we call in French a 'subjonctif d'insistance' (see critically KOUWENBERG 2010: 227 fn. 57), which could be translated as 'and the king Samsi-Addu really entered the house of his father!'
- B10 14'–16'** See the parallel in A.1288: ii 8'–10'. I commented on this historical event and its remembrance in ZIEGLER 2023. According to Sammetar, the sender of the letter A.3006 to king Zimri-Lim of Mari published in ZIEGLER 2023, the Lullu mountain people were known for their misdeeds. Sammetar starts his letter with this prediction (A.3006: 5–12):
- 'The Lullu (mountain) people are sinners! A first time they sinned in Tuttul! A second time during Aminum's (reign). A third time during Samsi-Addu's (reign). A fourth time they sinned during Yahdun-Lim's (reign). Now these Lullu (mountain people) won't fail to sin!'
- In ZIEGLER 2023, I argued that Sammetar knew about the event from Samsi-Addu's days thanks to the MEC.
- 16'** Lazapatum is known from the Mari and Tell Leilan archives and probably was close to the latter, i.e. it lay in the neighborhood of Šubat-Enlil/Šehna. See for bibliography and attestations ZIEGLER/LANGLOIS 2016: 203 s.v. Lazapat. If Samsi-Addu was defeated by Lullu mountain people at this place in the Habur triangle, do we have to understand that he was already active in Upper Mesopotamia?
- B11 17'** BIROT 1985: 229 transliterated Mu-ut-Ab¹-bi-ih and commented on this in n. 3 'La lecture *-AB¹-bi-ih* m'a été proposée par J.-M. Durand, qui a retrouvé l'anthroponyme Mut-Abbi dans ARM XXI (corr. ARM XXII) 3 II 12. On pourrait l'identifier avec Mutu-Abih, nom d'un ennemi de la famille de Yasmah-Addu (ARM V 2:12)'. ARM 5 2 dates from the last weeks of Yasmah-Addu's reign. Among enemies who were killed, Yasmah-Addu enumerates l. 12' Iššur-Dagan, brother of Mutu-Abih ([¹i-š]ur-^dda-gan ŠEŠ mu-tu-a-bi-i[h]): both are otherwise unknown.
- Although the sign transliterated AB¹ is in its shape closer to UM or TUB than to AB, no interpretation comes to mind. The interpretation could be *mūt* PN 'death of PN'.
- Nevertheless, a PN seems more probable. For PNs beginning with Mut-, see DURAND 1991 and in the MEC for instance A.1288: ii 26' (B20) for another PN ¹mu-ut-ia-[x...]. Mut-Abih is the full name of a king of Tell Leilan,

ancient Šehna, whose name generally was abbreviated Mutiya and who was a contemporary of Samsu-iluna. For Mutiya's full name see for instance his seal RIME 4.27.4.2 in FRAYNE 1990: 755–756. It would be interesting to know if MEC B10 alluded to an ancestor of Mutiya from Šehna.

If the interpretation of PN Mut-Abbi[h] is correct, the sentence probably ended with one single verb e.g. *im-tu-ut* 'Mut-Abbi[h] died'.

B12 18'–19' Very little remains from the upper edge of l. 19'. Restoration from A.1288: ii 12'–13'.

6.5. M.8566 (see Fig. 3.18)

A small fragment of an excerpt tablet. According to M. BIROT, the remaining fragment probably stems from the lower left edge of the obverse. But see K. VEENHOF's contribution in this volume (Ch. 4 §2 and §5.2). He quotes the oral proposal of W. Nahm suggesting that the fragment could be the upper left edge of a tablet of which only the reverse is readable. If this proposal is right, the events from B20, which in A.1288: ii 22'–26' are much more detailed should have continued on the left edge of the tablet. The transliteration below follows W. Nahm's oral proposal.

Obverse?

A1[?] [iṣ-tu r]e-eš₁₅ ^{rgiṣ}[GU.ZA ...]

A2[?] 2' [o o o o o o] [x ...]
[...]

Reverse?

B11^{REL144} [i-na da-da]-^ra-ia¹ m[u-ut-...]

B12^{REL145} 2' [i-na da]-^rda¹-a-ia MĪN [i-pi-iq-^dİŠKUR]
^rar-ra¹-ap-h[a-am^{ki} iṣ-ba-at]

B13^{REL146} 4' [i-n]a a-hi-ša-lim ṣa-ba-a[t ...]

B14^{REL147} ^I ú-ṣur-[ša-iṣ-tár]

B15^{REL148} 6' ^I ka-at-t[a-a]

B16^{REL149} i-na ṣu-^d[E]N-^rZU x x¹ [...]

B17^{REL150} 8' i-na a-bu-ša-lim ṣa-ba-at x[. ...]
ša né-re-eb-tim ^[ki]

B18^{REL151} 10' ^{r1} ṣu-da-a-[ia]

Lower edge?

B19^{REL152} i-na ṣu-da-^rdī¹-im ṣa-ba-at né-r[e-eb-tim^{ki}]

B20^{REL153} 12' i-na ^da-ṣur-tu-kúl-ti da-am⁷-[da-a-am]
[o] ^rša¹ ú-ni-ne-^rim¹ [LUGAL[?] i-du-uk]

6.6. M.8566 commentary

A1 obv.[?] 1' Unfortunately, there is no photograph of this part of the document. The hand copy of M. Birot clearly shows IŠ. W. Nahm orally proposed to K. VEENHOF (in this volume Ch. 4) that this could be A1, the first line of the obverse, see above.

B11 rev.[?] 1' See comment to B11 in S.115-26: 17' (§6.4).

B12 2'–3' Restorations from A.1288: ii 11'.

B17 8' The 'taking of x[...] of Nerebtum' is commemorated in this eponymate. B17 is attested by three texts but unfortunately all exemplars are broken. The broken sign here could be EN or G[I]. I wonder if *gišrum* (CAD G 107 *gišru* A) 'barricade' could be restored here, less probable is EN.NUN 'watch, guard'. Two years later (B19) the capture of Nerebtum itself is recorded.

B20 13' All three exemplars are broken, the best preserved is A.1288: ii 22'–26'. The subject is missing everywhere. Since there is very little space for it, I propose LUGAL 'the king', i.e. Samsi-Addu. Another possibility is LÚ.ELAM, but this is less probable.

For Unine see comment to A.1288: ii 23' (B20 §5.1).

7. Synoptic view

Fig. 3.2. Synoptic table correlating the names of the eponyms, KEL G, REL, the sections and the various editions of the MEC, the illustrations, and the years after Naram-Sin's accession.

i. Name	ii. KEL G	iii. REL	iv. MEC	v. Ed. I	vi. Ed. II A.1288	vii. Other manuscripts	viii. Illustration	ix. Naram-Sin Year+x
Šu-Sin		104	A2	M.7481+: 2				1
Aššur-malik		105	A3	” : [3]			3.3–4	2
Aššur-imitti		106	A4	” : [4]			3.3–4	3
Ennam-Sin		107	A5	” : [5]			3.3–4	4
Akutum		108	A6	” : [6]			3.3–4	5
Mašiam-ili		109	A7	” : [7]	col. i 1'		3.3–4, 14	6
Iddin-ahum		110	A8	” : [8]	2'		3.3–4, 14	7
Samanum	1	111	A9	” : '9'	'3'		3.3–4, 14	8
Ili-ennam	2	112	A10	” : '10'	4'–5'	M.5911: [0'–1']	3.3–4, 14, 17	9
Ennamanum	3	113	A11	” : '11'	6'	” : '2'	3.3–4, 14, 17	10
Ennam-Aššur	4	114	A12	” : [12]	7'	” : '3'–4'	3.3–4, 14, 17	11
Ennam-Sin	5	115	A12 ^{bis}	” : '13'	<8'>	” : < >	3.3–4, 14	12
Hanna-Narim	6	116	A13	” : '14'	'9'–10'	” : 5'–6'	3.3–4, 14, 17	13
Dadiya	7	117	A14	” : '15'	11'	” : '7'	3.3–4, 14, 17	14
Kapatiya	8	118	A15	” : 16	12'–14'	” : 8'–9'	3.3–4, 14, 17	15
Išme-Aššur	9	119	A16	” : 17	15'–16'		3.3–4, 14	16
Aššur-mutabbil	10	120	A17	” : 18	17'		3.3–4, 14	17
Šu-Nirah	11	121	A18	” : 19	18'		3.3–4, 14	18
Iddin-abum	12	122	A19	” : '20'	19'		3.3–4, 14	19
Ili-dan	13	123	A20	” : '21'	20'		3.3–4, 14	20
Aššur-imitti	14	124	A21		21'		3.14	21
Buṣaya	15	125	A22		22'		3.14	22
Dadiya	16	126	A23		'23'–24'		3.14	23
Puzur-Ištar	17	127	A24		'25'		3.14	24
Isaya	18	128	A25*		[26']		3.14	25
Abu-šalim	19	129	A26*		[27']		3.14	26
Aššur-re'i	20	130	A27*		[28']		3.14	27
Ṭab-Aššur	21	131						28
Šu-Rama	22	132						29
Sin-išme-anni	23	133						30
Aššur-malik	24	134	B1			S.115-26: 1'	3.19	31
Daniya	25	135	B2			” : 2'	3.19	32
Ennam-Sin	26	136	B3			” : 3'	3.19	33
Aššur-balaṭi	27	137	B4			” : 4'	3.19	34
Ennam-Aššur	28	138	B5			” : 5'	3.19	35
Itur-Aššur	29	139	B6			” : 6'	3.19	36
Šu-beli	30	140	B7		col. ii 1'	” : 7'–8'	3.19	37
Šarrum-Adad		141	B8		'2'–6'	” : 9'–12'	3.19	38
Šu-Laban		142	B9		'7'	” : 13'	3.19	39
Aššur-imitti		143	B10		'8'–10'	” : 14'–16'	3.19	40
Dadaya		144	B11		11'	M.8566: 'r.1' S.115-26: '17'	3.18–19	41
Dadaya no. 2		145	B12		12'	M.8566: 'r.2'–3' S.115-26: [18']	3.18–19	42
Ahi-šalim		146	B13		13'	M.8566: r.4'	3.18	43
Ušur-ša-Ištar	37	147	B14		14'	M.8566: 'r.5'	3.18	44
Kattaya	38	148	B15	S.24-1+: [o.0']	16'	M.8566: r.6'	3.5–7, 18	45
Šu-Sin	39	149	B16	” : [o.1']	'17'	M.8566: r.7'	3.5–7, 18	46
Abu-šalim	40	150	B17	” : 'o.2'	18'–19'	M.8566: r. 8'–9'	3.5–7, 18	47
Šudaya	41	151	B18	” : 'o.3'	'20'	M.8566: r.10'	3.5–7, 18	48
Šudadum	42	152	B19	” : 'o.4'	'21'	M.8566: r.11'	3.5–7, 18	49

i. Name	ii. KEL G	iii. REL	iv. MEC	v. Ed. I	vi. Ed. II A.1288	vii. Other manuscripts	viii. Illustration	ix. Naram-Sin Year+x
Aššur-tukulti	43	153	B20	” : r.0.5'–6'¹	22'–26'	M.8566: r.12'–13'	3.5–7, 18	50
Puzur-Ištar	44	154	B21	” : o.7'	27'		3.5–7	51
Atanah	45	155	B22	” : r.0.8'–9'¹			3.5–7	52
Erišum	46	156	B23	” : o.10'–11'			3.5–7	53
Aššur-ennam	47	157	B24	” : o.12'			3.5–7	54
Inbi-Ištar	48	158	B25	” : r.0.13'¹			3.5–7	55
Aššur-bel-malkim	49	159	B26	” : r.0.14'¹			3.5–7	56
Belanum	50	160	B27	” : r.0.15'¹			3.5–7	57
Sukkallum	51	161	B28	” : r.0.16'¹			3.5–7	58
Amur-Aššur	52	162	B29	” : r.0.17'¹			3.5–7	59
Aššur-nišu	53	163	B30	” : r.0.18'–19'¹			3.5–7	60
Munawwirum	54	164	B31	” : [o.20']			3.5–7	61
<i>Idnaya</i>	55	165						62
Dadaya	56	166	D0	S.24-3: o.1'			3.8–9	63
Puzur-Nirah	57	167	D1	” : r.0.2'¹			3.8–9	64
Abiaya	58	168	D2	” : r.0.3'¹			3.8–9	65
Atanum	59	169	D3	” : o.4'–7'			3.8–9	66
Aššur-taklaku	60	170	D4	” : o.8'			3.8–9	67
<i>Isim-Sin</i>	61	171	D5	” : o.9'			3.8–9	68
<i>Addu-bani</i>	62	172						69
<i>Abi-šagiš</i>	63	173						70
<i>Ṭab-šilli-Aššur</i>	64	174						71
<i>Iddin-Aššur</i>	65	175						72
<i>Namiya</i>	66	176						73
<i>Ahu-šarri</i>	67	177						74
<i>Dadaya</i>	68	178						75
<i>Aššur-taklaku</i>	69	181						80
<i>Haya-malik</i>		182						81
Šalim-Aššur		183	E1	” : r.1'¹			3.10–11	82
Sin-muballiṭ		184	E2	” : r.2'			3.10–11	83
Riš-Šamaš		185	E3	” : r.3'			3.10–11	84
Ibni-Addu		186	E4	” : r.4'	col. iv?		3.10–11, 15	85
Aššur-imitti		187	E5	” : r.5'–8'			3.10–11	86
Ili-ellati		188	E6	” : r.9'			3.10–11	87
Rigmanum		189	E7	” : r.10'¹			3.10–11	88
Ikun-piya		190	E8	” : r.11'–15'¹			3.10–11	89
Asqudum		191	E9	” : r.16'¹			3.10–11	90
Aššur-malik	r.79¹	192	E10	” : r.17'–23'¹			3.10–11	91
...iya (Awiliya²)	80	193/4	E11	” : r.24'–26'¹			3.10–11	92
[...]			E11	A.1614a: r.24'–30'¹			3.16	92
(see LACAMBRE, in this volume Ch. 7, who proposes the sequence Ahiyaya – Awiliya)								
<i>Nimer-Sin</i>	81	195						93
<i>Addu-bani</i>	82	196						94
<i>Ṭab-šilli-Aššur</i>	83	197						95
<i>Ennam-Aššur</i>	84	198						96
Aššur-emuqi	85	199		M.7481+: r.1'–6'¹			3.12–13	97

Fig. 3.2 (cont.). Synoptic table correlating the names of the eponyms, KEL G, REL, the sections and the various editions of the MEC, the illustrations, and the years after Naram-Sin's accession.

Abbreviations

ARM 2	JEAN 1950	KEL D	GÜNBATTI 2008b: 125ff
ARM 5	DOSSIN 1952	KEL F	GÜNBATTI 2008a: 104
ARM 14	BIROT 1974	KEL G	GÜNBATTI 2008a
ARM 21	DURAND 1983	MEC	Mari Eponym Chronicles see BIROT 1985
ARM 22	KUPPER 1983	NABU	Nouvelles Assyriologiques Brèves et Utilitaires
CAD	The Assyrian Dictionary of the Oriental Institute at the University of Chicago	PIHANS	Publications de l'Institut historique et archéologique néerlandais de Stamboul
FM II	CHARPIN/DURAND 1994	REL	Revised Eponym List see BARJAMOVIC/HERTEL/LARSEN 2012
FM V	CHARPIN/ZIEGLER 2003	TTKY	Türk Tarih Kurumu Yayınlari
FM XVI	RECULEAU 2018	ZL	Zimri-Lim
KEL A	VEENHOF 2003 and GÜNBATTI 2008b: 127ff		

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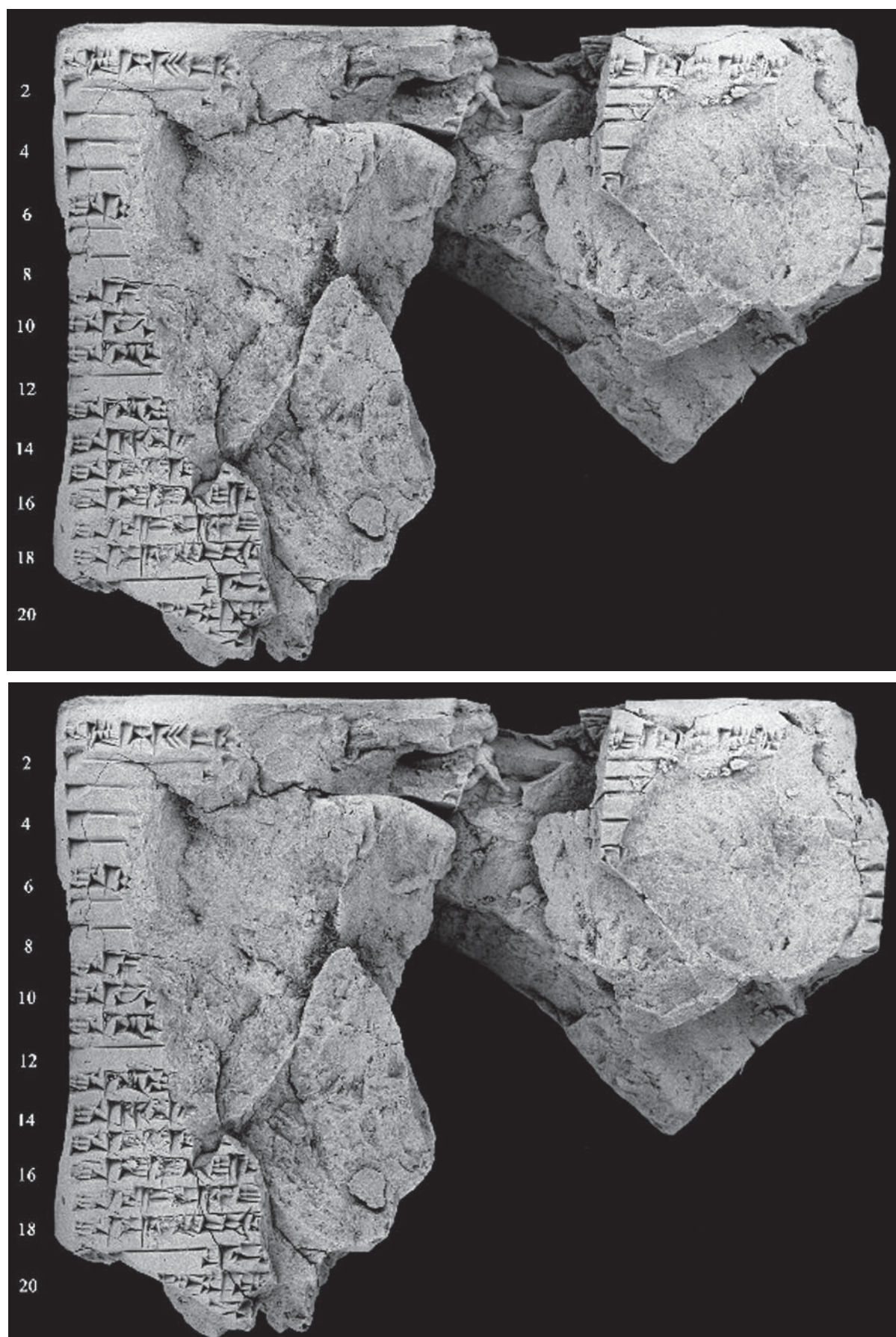
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Figs. 3.3 and 3.4. Edition I: M.7481+M.11250 obverse. For the reverse of this fragment see Figs. 3.12 and 3.13 below. Pictures prepared by F. Nebiolo (ANR project PCEHM). © Archives Royales de Mari.



Fig. 3.5

Fig. 3.6

Fig. 3.7.

Edition I: S.24.1+S.24.2+A.1614b obverse. For reverse of this fragment see Figs. 3.10 and 3.11.
Picture sppared by F. Nebiolo (ANR project PCEHM). © Archives Royales de Mari.

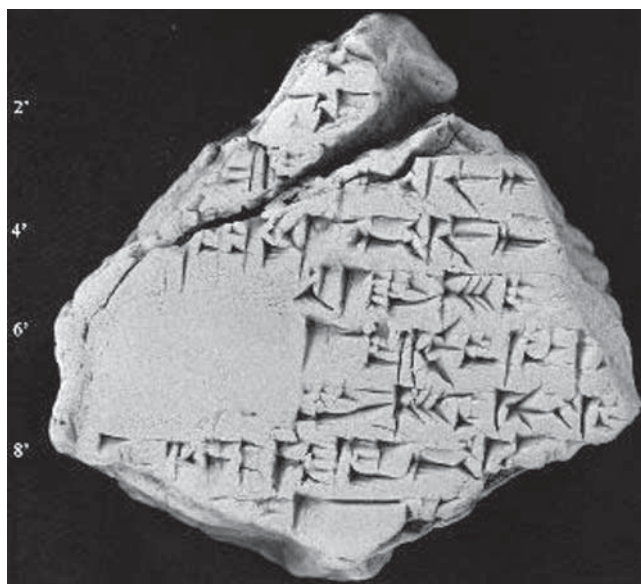


Fig. 3.8. Edition I: S.24-3.

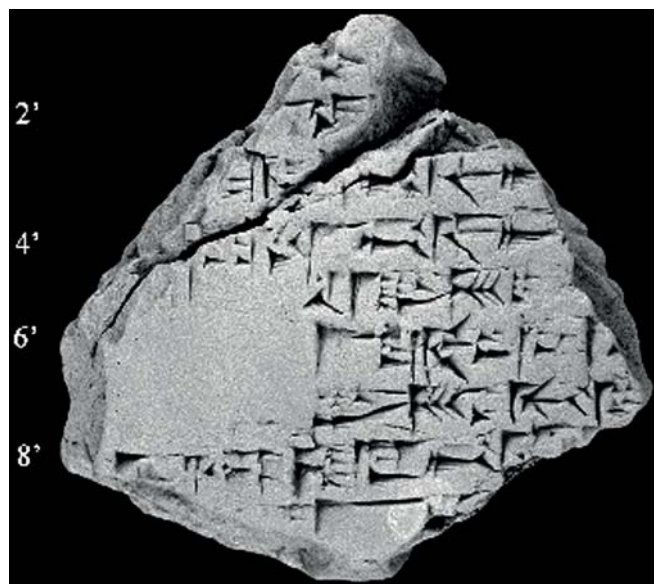


Fig. 3.9. Edition I: S.24-3

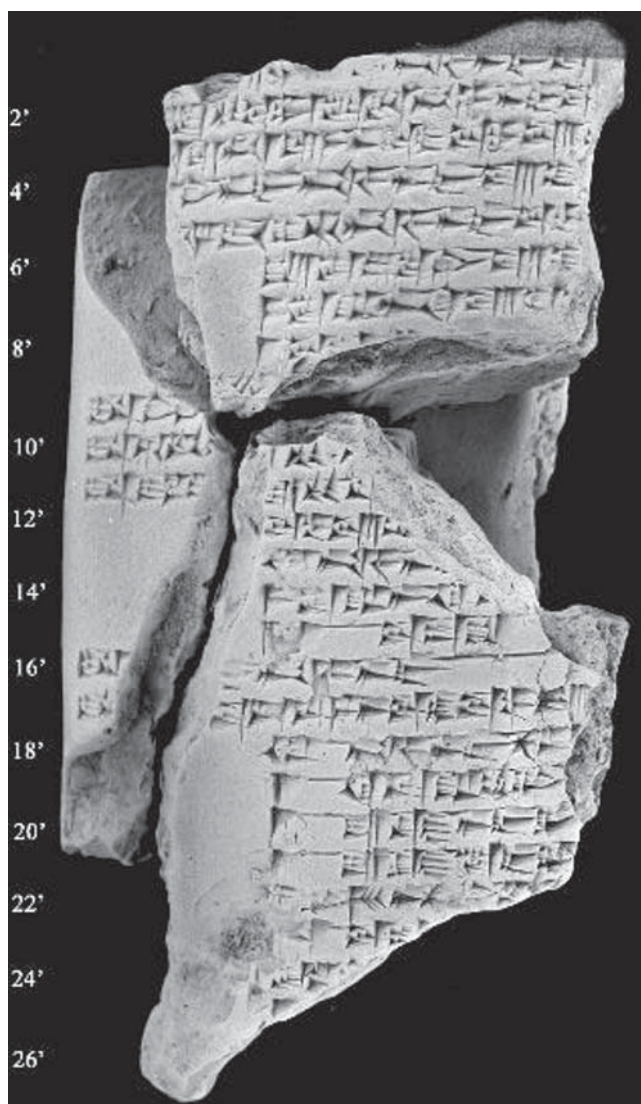


Fig. 3.10.

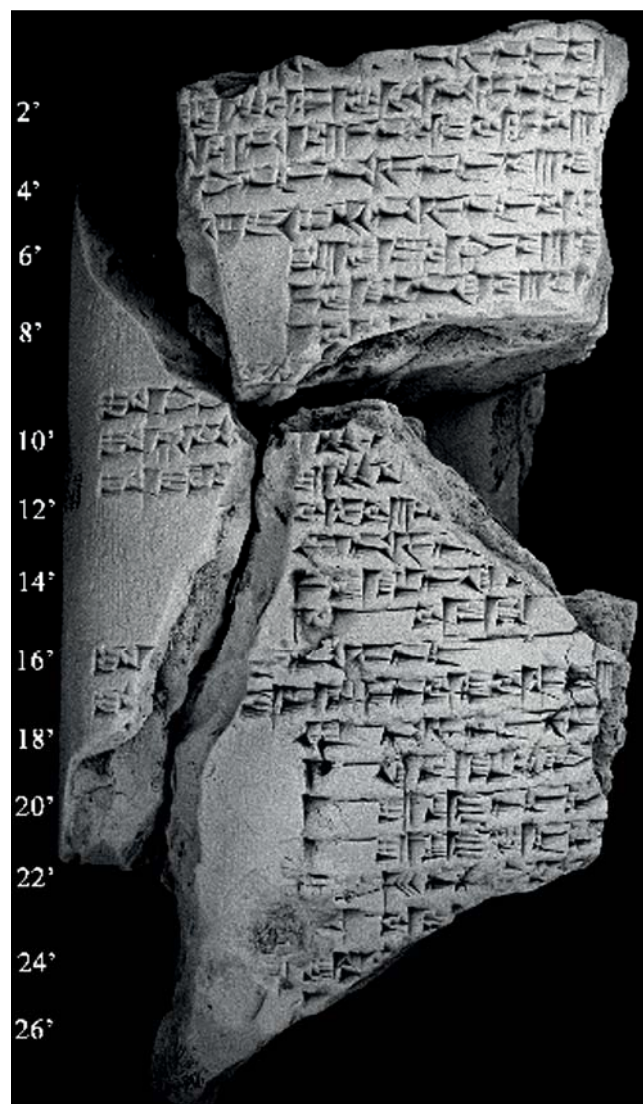
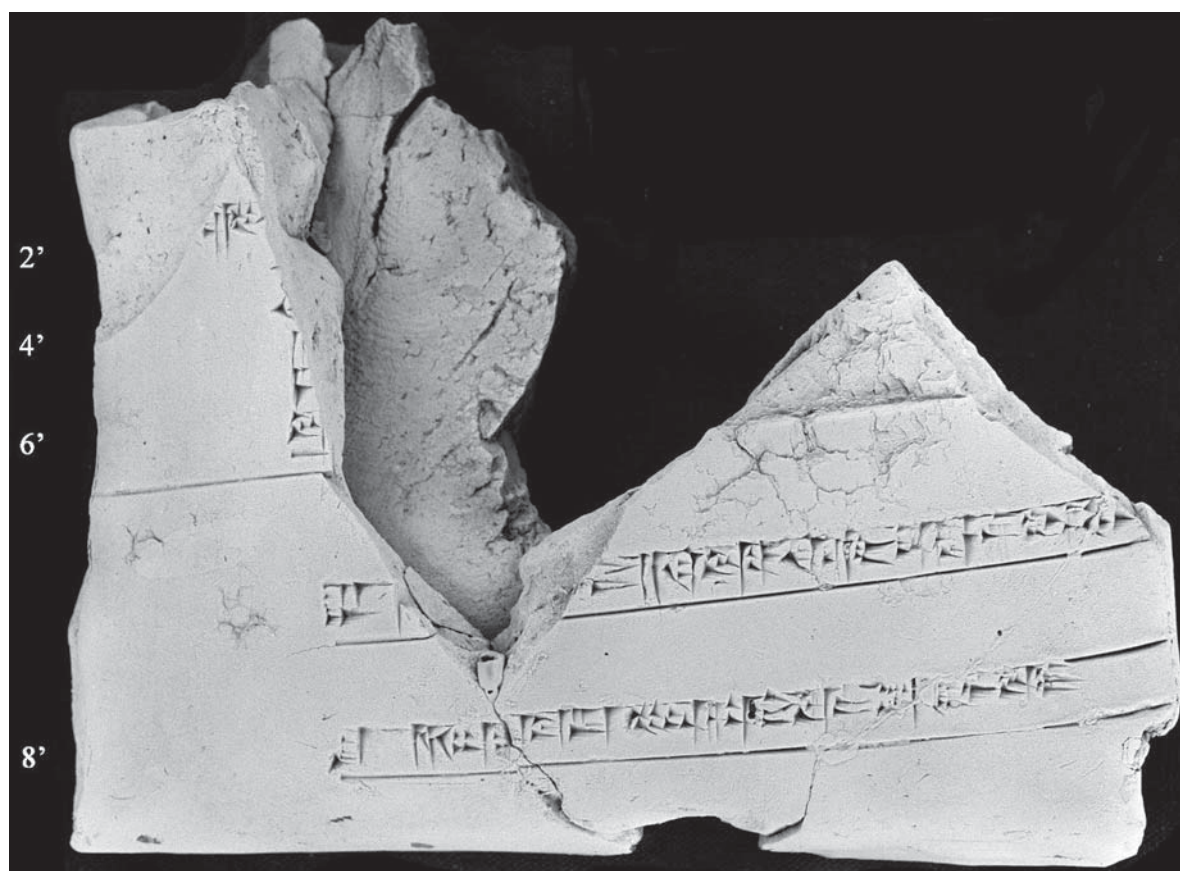
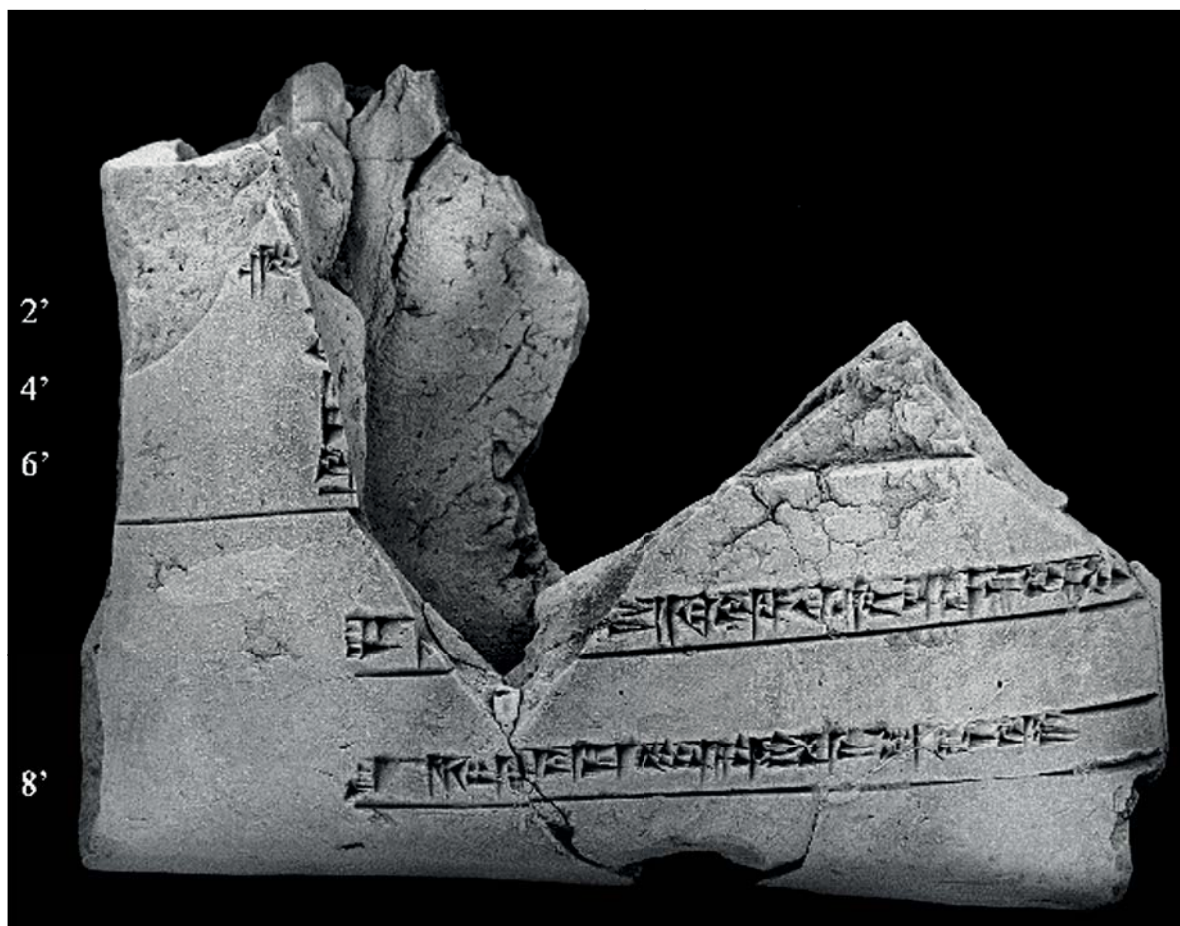


Fig. 3.11.

Edition I: S.24-1+ S.24-2+A.1614b reverse. For obverse, see Figs. 3.5, 3.6, and 3.7.
 Pictures prepared by F. Nebiolo (ANR project PCEHM). © Archives Royales de Mari.



Figs. 3.12 and 3.13. Edition I: M.7481+M.11250 reverse. For obverse of this fragment see Figs. 3.3 and 3.4.
Picture prepared by F. Nebiolo (ANR project PCEHM). © Archives Royales de Mari.

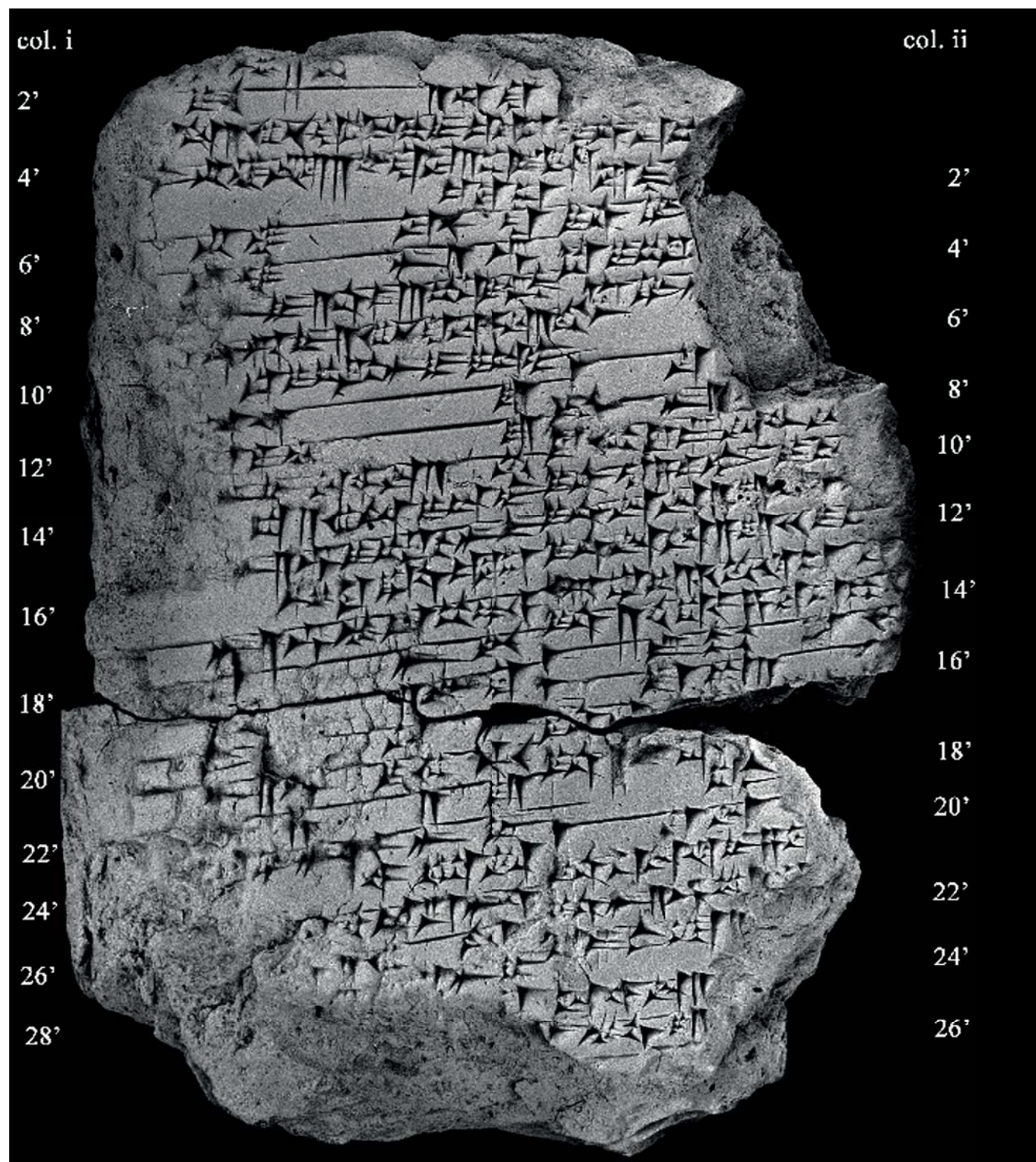


Fig. 3.14. Edition II: A.1288 obverse. Picture prepared by F. Nebiolo (ANR project PCEHM). © Archives Royales de Mari.



Fig. 3.15. Edition II: A.1288 reverse. Picture prepared by F. Nebiolo (ANR project PCEHM). © Archives Royales de Mari.

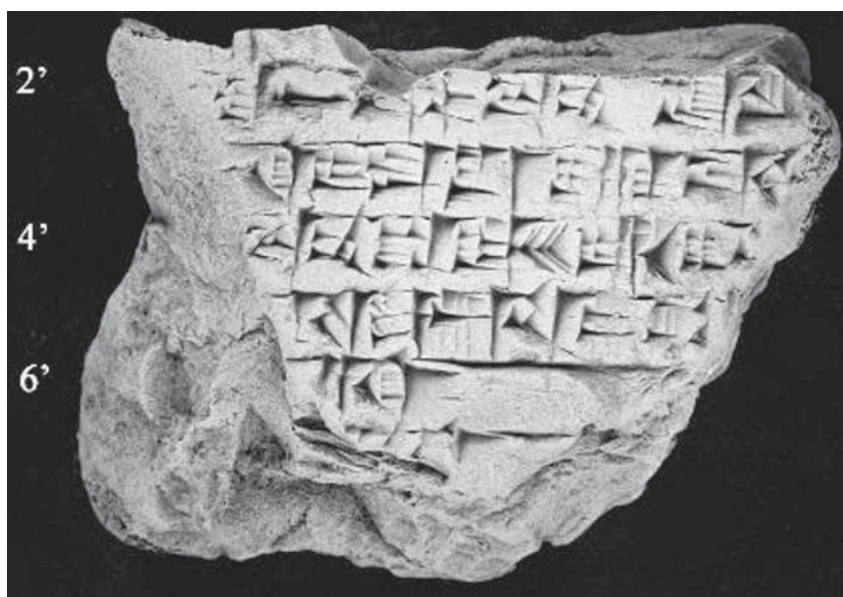


Fig. 3.16. Edition I. Fragment A.1614a.

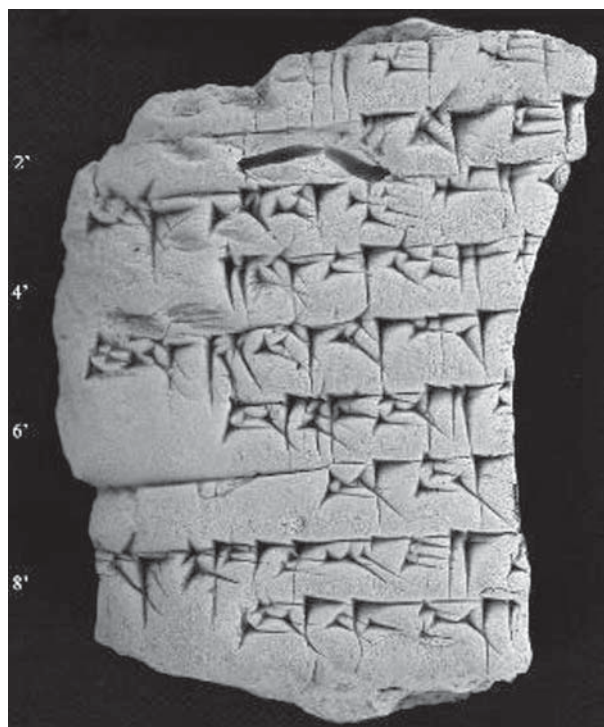


Fig. 3.17. M.5911



Fig. 3.18. M.8566.

Pictures prepared by F. Nebiolo (ANR project PCEHM). © Archives Royales de Mari.

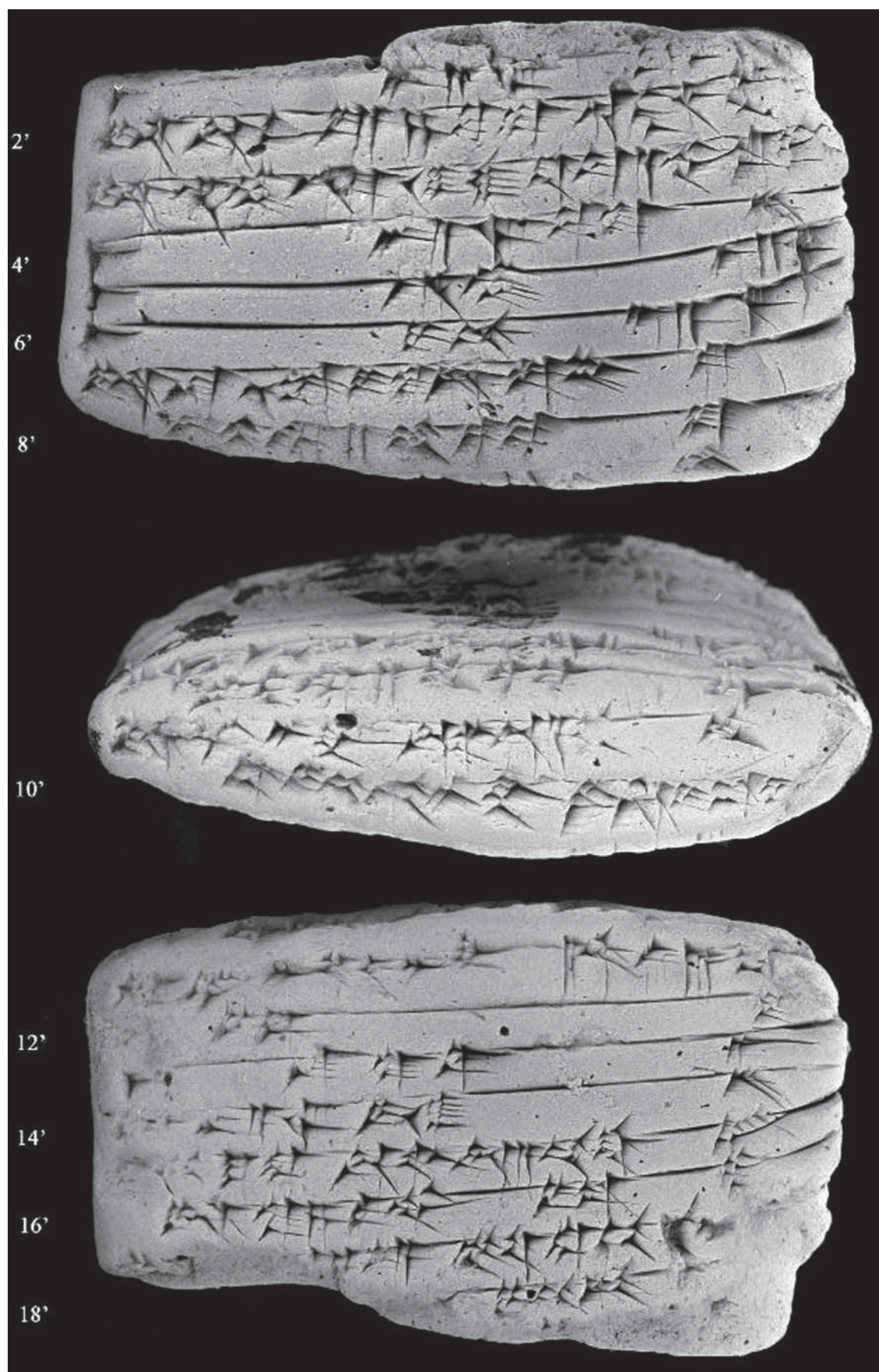


Fig. 3.19. S.115-26. Veenhof's 'Edition III'. Picture prepared by F. Nebiolo (ANR project PCEHM).
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4. The reconstruction of the Mari Eponym Chronicle

KLAAS R. VEENHOF †

Three editions of the Mari Eponym Chronicle are identified and partially reconstructed and a composite translation is provided. Historical implications of the chronicle are evaluated and open problems are investigated. It is argued that fragment D from Edition I must be placed at the years 180–183 of the eponym count starting with the accession of Erišum I and that fragment C from Edition II must be placed with Eponymy year 183.

1. Introduction¹

The ‘Mari Eponym Chronicle’, henceforth MEC,² is an important source for chronological and historical studies of the period of roughly a century, ending in ca. 1775 BC (dates mentioned in this article are according to the Upper Middle Chronology). It was identified in 1939 by G. Dossin and published in 1985 by M. Birot, with transliteration, translation, cuneiform copies, photos (though not of all fragments), and comments. The text is rather fragmentary and Birot used capital letters (A to G) to designate the (clusters of) fragments he presented in chronological order. Soon after his presentation of the text during a colloquium in 1983, ‘À propos d’un cinquantenaire: Mari, bilan et perspectives’³ (which resulted in BIROT 1985), he kindly supplied me with prints of the original photos he had published, which were helpful, although many problems remain due to the fragmentary state of the originals. They have now been returned to Syria, so that collation is difficult.

The texts have received due attention in studies on the history and chronology of the early second millennium BC and Mari specialists have made various proposals for improved readings based on collations, the use of new textual sources and new interpretations. As examples I mention J.-M. Durand’s identification of the solar eclipse in Eponymy year 24 and his observations in *MARI* 6 on the reading and position of MEC C.⁴ Many references to and proposals for restoring broken passages of the ‘chroniques éponymales’ are found in *FM* V,⁵ which on p. 9 calls it ‘le document le plus important’, and on pp. 70–73 publishes an important record with early eponym dates. This implies that Birot’s *editio princeps* can no longer be used as an authoritative source.⁶

I discussed MEC in KEL A, Ch. 7.1, because the Old Assyrian eponym list overlaps with MEC, which allows the restoration of the names of the eponyms until MEC 26 and because calculating the original size of MEC’s main manuscript (called below ‘Edition I’) would provide clues for the total number of eponyms listed in MEC, whose number is unfortunately broken in the colophon. The increase of historical data from Mari and especially the publication (in 2008) of KEL G, which overlaps (with a few gaps) the whole period covered by MEC, makes a new reconstruction desirable. A first attempt was soon undertaken by G. Kryszat, who dealt with the reading and position of MEC text D.⁷ An important step was the publication in 2012 of *U&D*, whose ‘Revised Eponym List’ (REL) was matched with the data preserved in

¹ Klaas Veenhof died on 28 July 2023. He revised his manuscript in February 2022. The editors have added some notes marked [Editors] with more recent references and information.

² For easy reference the eponyms listed are numbered MEC 1 etc., on the basis of my reconstructed list in §5.1.

³ [Editors] Published as DURAND/MARGUERON 1985.

⁴ DURAND 1990: 274–275.

⁵ See the notes on the eponyms in the reconstructed text of MEC, below §4.2.

⁶ [Editors] Nele ZIEGLER Ch. 3 in this volume contains a new edition and photographs of the various fragments of the Mari Eponym Chronicles, as well as comments on the texts. The photographs (Figs. 04.01–03) are details taken from Nele Ziegler’s chapter. Although Klaas Veenhof had access to the photographs, he did not have access to Ziegler’s text and his transliterations and interpretations differ in places from hers.

⁷ KRYSZAT 2008: 214–215.

MEC. *U&D* discussed in detail the many problems it raised and included collations of damaged lines of KEL G.⁸ Its publication coincided with a renewed interest in the chronology of the early second millennium BC and attempts to arrive at an absolute one, to which also two sessions during the *Rencontres Assyriologiques Internationales* of 2012 and 2013 were devoted, where I also discussed MEC. The present attempt of course uses *U&D* and other recent publications, some of which react to *U&D*'s proposals.⁹ I could also make use of some observations made and data communicated during a recent symposium on chronology in Lille, in Sept. 2015 (including the latest collations by G. Barjamovic of disputed eponym names in KEL G), along with comments from some participants of this symposium on the preliminary edition of MEC, which I had made available for discussion, since I could not attend the symposium. Later Michael Roaf kindly gave me a copy of his manuscript of his contribution on the eponym lists and the solar eclipse in MEC,¹⁰ on which we exchanged views, and Werner Nahm made an important and convincing suggestion on the nature and position of the fragment M 8566 and proposed some corrections to my manuscript.

Its overlap with KEL A shows that MEC started with Eponym 104, the one of the accession year of Narām-Sîn of Assur. It ended, surprisingly, with the eponym of Aššur-emūqī, the second one after Šamši-Adad's death,¹¹ which occurred at the very end of the eponymy year of Ṭāb-šilli-Aššur (missing in MEC due to a gap). According to sources from Mari his death occurred in the 18th year of Hammurabi of Babylon or 1775 BC (Upper Middle Chronology) and knowing exactly how many years separated Šamši-Adad's birth (mentioned in MEC 23) and accession (mentioned in MEC 38) from his death is important for the chronology. It will also help us to date a variety of historical events mentioned in MEC, in which kings Ipiq-Adad II and Daduša of Eshnunna, Amīnum, Ila-kabkabū, Šamši-Adad I, his sons Išme-Dagan and Yasmah-Addu and also Yahdun-Lim of Mari figure. Unfortunately, due to the gaps in MEC and KEL, the exact number of eponymy years covered by MEC is not known and proposals range from 92 to 98 (the latter figure based on the Assyrian *Distanzangabe*, discussed in KEL A, Ch. 7.2).

In the discussions on the chronology, the Old Assyrian eponym lists and the MEC figure as one set of data alongside many others, notably astronomical ones (the Venus data and the solar eclipse mentioned in MEC) and those provided by dendrochronology. These will not be discussed here, although the mention of a solar eclipse in MEC during Eponym 24, that of Puzur-Ištar (see Fig. 4.3 below) of course is of vital importance in the search for an absolute chronology,¹² as are the recent higher dendrochronological data for the building history of the palaces in Kültepe and Acemhöyük.¹³

Those who use MEC have to be certain about its text and realize the problems it poses, due to gaps and serious damage and the existence of three different 'editions' of the text. These editions differ in lay-out (on tablets with one or two columns) and in the presence and the amount of historical data added to the names of eponyms. MEC 10, 14, 17, 19 and 48 in Edition I are followed by historical data, which are lacking in Edition II, in M.5911 and in M.8566,¹⁴ and for other years the amount of historical data varies. For the reconstruction one must try to find out how many lines the various editions of the text contained, how many eponyms the preserved fragments may have listed, how damaged names have to be read or restored, where fragments can or must be fitted into the text, how big the gaps are, and how many and which eponyms might be inserted in them. This requires calculations of the possible size of the tablets and the possible position of the fragments, as well as observations on the possible dates of the exploits of rulers mentioned in the historical notes.

⁸ REL numbers for the eponyms after no. 40 are one higher than in my edition in KEL A, because REL inserted Šuli as no. 41, in which it followed my suggestion in VEENHOF 2007.

⁹ Notably KOLIŃSKI 2013; NAHM 2013; BLOCH 2014; NAHM 2015 and a preliminary version of LACAMBRE Ch. 7 in this volume.

¹⁰ [Editors] Also ROAF Ch. 5 in this volume.

¹¹ It has been suggested (see *FMV*: 168 with note 658) that at least one of the tablets (obviously the one I call 'Edition I') was taken along as booty from Šubat-Enlil, which came into Zimri-Lim's hands in his 4th year, but this is difficult to prove. See below in §3.3, the remarks on the colophon.

¹² See for recent overviews of problems and solutions DE JONG 2012–2013 and ROAF 2012.

¹³ They are ca. 15 years higher than the previous ones: ca. 1848 BC for the building of Warshama's place at Kültepe and ca. 1790 BC for the palaces at Acemhöyük. The impact of these new dates figure in VEENHOF 2017.

[Editors] More recent ¹⁴C determinations differing from those available to Klaas Veenhof are discussed by Michael ROAF in Ch. 13 of this volume.

¹⁴ There are also some graphic differences in the data added to MEC 37 and 40. A.1288 writes *Šu-bi-[li]* and *Lu-ul-lu-ú*, and S.115-26 *Šu-be-li* and *Lu-ul-lu-um*. A.1288 gives the name of MEC 9 as *Ilī-ennam* instead of *Ilī-ālum*. More serious is that two manuscripts, A.1288 and M.5911 by mistake omit the eponymy of Enna(m)-Suen (MEC 12), who is listed in M.7481, but this is explainable as *aberratio oculi*, because MEC 10–12 are a sequence of three eponyms whose names all begin with Ennam-. Note also that MEC 35 (in B:5) has *En-nam-^dA-šur*, for which KEL G 28 has *En-na-Si-in*, the latter the correct reading, attested in two Old Assyrian contracts, see KEL A, 53, e, where Balkan's reading *I-dī-Sū-in* in Kt a/k 324, on the basis of a collation by Günbatti, must be corrected to *I-na-Sū-in*; see VEENHOF 2010: 147, note on l. 13, for another case of writing *I-dī-* instead of *I-na-* in a name.

2. Three different editions, I–III

We can distinguish three different ‘editions’ of the text, which I designate in what follows by bold capitals: **I**, **II** and **III**. Edition **I**, as reconstructed by BIROT 1985, 238–239, consists of M.7811+11250, part of a large tablet with one wide (ca. 10.4 cm) column, with lines that are between 3.5 and 4 mm high, to which also the fragments S.24-1, 2 and 3 and A.1614a and b belong, although not all of them can be joined and there are several gaps in the text. S.24-3 and A.1614a are large flakes from the reverse of the tablet, with writing on one side only, whose text is not duplicated by the other editions. In my reconstruction the former ‘floats’ in the upper part of the reverse, the latter must precede the rev. of M.7481+, because of the historical data it mentions.

Edition **II** is represented by A.1288 (BIROT 1985, 237), a large tablet with two columns, each ca. 6 cm wide, whose top and bottom are missing and of whose reverse only a small part of col. IV is preserved. It will be discussed in detail in §4. Edition **III** is represented by S.115-26, the bottom of a one-column tablet, also ca. 6 cm wide and still ca. 3.5 cm. long, with many indented second lines to accommodate historical data; it overlaps much of A.1288 col. II. Its obverse starts with eponym MEC 31 and ends with MEC 38, its reverse continues until Eponym 42. If it started with the same eponym as **I**, its obverse must have contained at least 45 lines and have been ca. 16 cm long. While this is perhaps imaginable, its reverse could never have accommodated the remaining 60 eponyms, to which also more frequently historical data are added and for which at least some 90 lines would have been needed. Therefore S.115-26, unless it represents an edition that ended with a much earlier year, may have been the first of a three-tablet edition. It cannot be joined with M.8566, as W. Nahm pointed out to me, because they overlap in what are lines 43–44 in my edition. He also makes the convincing suggestion that the reverse of M.8566 is its obverse, starting with [x x r]i-iš x[.....], which equals the beginning of the Chronicle as preserved in M.7481:1 (*iš-tu re-eš* ^{giš}GU.ZA, etc.). Since tablet M.8566 ends with the upper edge (see the photo in *MARI* 4, 242), which lists the Eponyms 49 and 50 (first half; the second half perhaps on the left edge?), it must be the first one-column tablet of a multi-tablet edition. Adding perhaps ca. 16 additional lines for the heading and for additional historical data that did not fit on its short lines, we may postulate a tablet with ca. 32 lines on each side that must have measured ca. 13 by 6 cm. The remaining data on Eponyms 51–97 + colophon according to Edition **I** would have covered at least ca. 82 lines and certainly many more considering the lack of data on Eponyms 61–77 and the short lines of a tablet like M.8566. This implies that two more one-column tablets, similar to Edition **III** are required, represented by S.115-16. M.5911, which covers Eponyms 9 to 15, might have been a fragment of the obverse of M.8566.

In all editions eponyms to which no historical information is added start with a vertical wedge (rendered by | in my list), followed by a blank space before the name. Of the 61 eponyms listed on the obverse of **I** at least 19 are of that type. Eponyms followed by historical data begin with *ina*, ‘during ...’, which is preserved or can be restored for 36 eponyms. When many data are recorded, a second (and in some cases a third etc.) indented line is added. Important for reconstructing the size of the tablet of **I** is whether and how many historical notes were added to the missing eponyms, whose names we can restore on the basis of KEL A and G, in MEC 26–30, the gap between MEC A and MEC B, and in MEC 61–76, on the missing bottom, lower edge and the beginning of its reverse. Second lines for MEC 26–30 are unlikely and only a few are expected before the reverse, since there are only three such cases on the preserved obverse of **I**. Its long lines (10.4 cm wide) normally are sufficient to record the historical data (including those for which **II** and **III** would need two lines), which on the obverse of **I** that covers the earlier period are in general fairly short. But on the reverse, where we can check it, the number of second lines increases considerably and here the lack of data on MEC 94–96 is particularly annoying.

3. Edition I

To determine the length and the composition of MEC a reconstruction of the large one-column tablet of **I**, with lines ca. 3.5 mm to 4 mm high, is essential. Birot reconstructed it as follows:

Obverse: M.7481 + M.11250 obv. (A), lines 1–21. Its contents overlap with A.1288 col. I (**II**), which continues for 8 lines after the end of M.7481. After a gap, partly filled by overlapping fragments of other editions (S.115-26 = **III**; A.1288 col. II = **II**, and M.8566) we have S.24-1+S.24-2 +A.1614b obv. = (B:16ff.), of which 19 lines are preserved. Its damaged lower end has room for ca. 4 lines and there are some lines missing below it.

Lower edge: Missing, but probably 2 cm high (like the upper edge), with room for ca. 5 lines.

Reverse: S.24-3 (D), lines 1'–9', to be placed somewhere in its top part, followed after a small gap by S.24-2 + S.24-1 (rev.) + A.1614b (rev.) (E), with together (the remains of) 25 lines. After a gap, in which Birot places A.1614a (F), whose line 7' he

aligns with the first line of the reverse of M.7481 + M.11250 (G) (but there is no physical join), G offers 6 lines followed, after a dividing line, by a colophon that occupies ca. 3.4 cm at the bottom (see Fig. 4.2).

Thanks to KEL A and G we know or can restore the names of nearly all eponyms, but there is uncertainty in the reading and identification of some of them and in the place where the isolated fragments MEC D and F have to be inserted (see below §3.2.1–2 and 3.4), all on the reverse. After studying the various proposals and recent collations, I follow a slightly adapted reconstruction of the gap between KEL G cols. I and II, in which REL 179–192 become 179, 179a, and 180*–193*. The only place in I, where there is no overlap and where one (or perhaps even two) eponyms might be inserted, is after MEC 76. This is important for the chronology. More difficult is to make out how many lines MEC comprised, which also depends on the number of second etc. lines that have to be assumed. This is important for the question whether there is room for inserting a missing eponym (MEC 77) and where to insert the historical data preserved on C = A.1288 IV (II; see §4.2). It is impossible to be certain and we must be aware of the possible margins. If at the bottom of the obverse 1 cm more is missing than I have tentatively assumed, 5 to 6 more lines are available on the reverse. The risk here is that assumptions of this kind are influenced by the wish to reach certain goals, such as support for the Assyrian *Distanzangabe* of 199 years between the accession of Erišum I and the death of Šamši-Adad I.

3.1. The obverse of Edition I

Its first part (M.7481+), still ca. 7.8 cm high, restored with the help of A.1288 I, lists at least 25 eponyms, one (MEC 12) more than Birot, who reduced three partially broken names beginning with Ennam- to two (see above footnote 14). At its end KEL A and G allow us to restore MEC 22–26. It is doubtful whether A.1288 I: 28' lists a new eponym; no ruling is visible under the end of line 27' and therefore the damaged sign preserved at its end is taken as the continuation of the historical note of line 27' (see Fig. 4.3, below). There is no reason to insert an additional line here, since the two lines A.1288 I: 27'–28' fit on a single line in I. The eponyms of the remainder of the obverse (until MEC 38) are known from KEL G or can be restored with the help of duplicates. As mentioned above there is no need to assume second lines for MEC 27–30.

As pointed out in the next paragraph (§3.2), we may assume that the tablet was ca. 28 cm long and that there were ca. 28 lines at the bottom of the obverse after Eponym 48 = line 50. This means that there were ca. 12 lines after Eponym 60 = line 66 (which include the four damaged (script lost) + x (completely missing, perhaps 4 or 5) lines at the bottom of the obverse, so that there remain ca. 3–4 lines to be filled, to which we must add ca. 4 lines on the lower edge. For these ca. 7–8 lines eponyms starting with KEL G 61 are available, but we do not know how many, since we can only guess at the number of extra lines that were needed for historical information. The reverse of Edition I then would have started with ca. line 83.

3.2. The reverse of Edition I

W. Nahm has pointed out to me that if one joins S.24-1+ and S.24-2+ by a 'sandwich join' as obverse and reverse of the same fragment, the distance between obv. l. 50 (Šu-a[a-a]) and the top of the tablet, in my reconstruction 50 lines, should be the same as that between [As-qu-di-i]m, eponym 90 in my reconstruction, and the bottom of the reverse.

Subtracting 3.4 cm = 9 or 10 lines for the colophon and the empty space, there remain ca. 40 to 41 lines for the eight eponyms from Asqūdum to Aššur-emūqī, which is ca. 5 lines per eponym, more or less in agreement with the number of lines attested for Eponyms 91–97. Between Šudaya and Asqūdum we have 41 eponyms, with 16 documented extra lines (including 2 for MEC C). For Eponyms 61–76 one can estimate ca. 8 extra lines (the 16 preceding lines have 4 extra lines, the 12 succeeding ones 12 extra lines). This yields in all ca. 65 lines, about 30 on each side and 4 on the edge, which means a tablet of about 28 cm.

Uncertainty about the exact number of lines has a bearing on the question whether there is room for an additional MEC 77 (see below §3.3). Other such problems, to be analysed below, are a possible gap between MEC D and B (no physical join) and the join between F = A.1614a and A.1614b = the bottom of E. In addition, there is the question, addressed in §3.2, where the text of C = A.1288 col. IV (II) must be inserted on the reverse of I.

3.2.1. MEC D

The first preserved piece of the reverse is MEC D = S.24-3, but it ‘floats’. It is a flake with writing on one side only, which must be placed at some distance from the top of the reverse, after MEC 75/76 and before MEC E, which contains the ‘Mari sequence’, beginning with Šalim-Aššur.

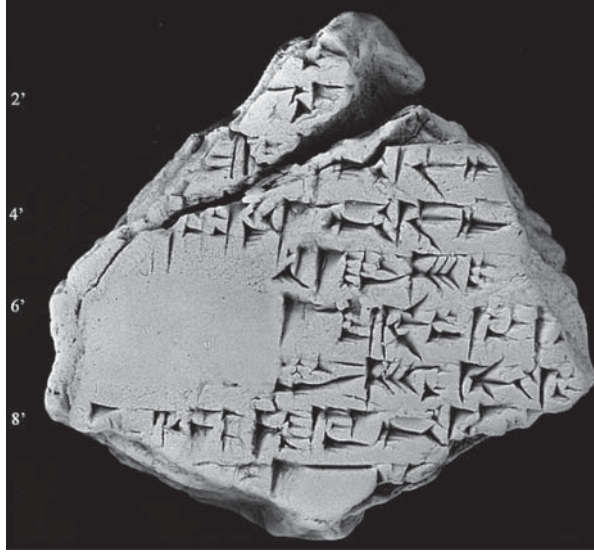


Fig. 4.1. S.24-3, MEC text D.
© Archives Royales de Mari.

MEC D can be read as follows:

1'	[		]	x	[		]
2'	[i-na ²	x	x x]	(x) ²	K[A ²		]
3'	[i-na x]- ¹ ia/e ² -ia ¹			¹ dUTU ^{si} - ^d [IM			]
4'	[i-na] A-ta-nim			^d UTU ^{si} - ^d [IM			]
5'				12 LUGAL.MEŠ i-[-			]
6'				^m Ya-ah-du-un-[- ^d Lim			]
7'				LUGAL.MEŠ-šu-nu ut-te-[-			]
8'	[i-n]a ^d A-šur-ták-la-ku			^d UTU ^{si} - ^d [IM			]
9'	[i-na Ha-ià-ma]-lik			^d [UTU ^{si} - ^d IM			]

For its interpretation the indentation of the lines is essential and it is clear that lines 3', 4', 8', and 9' start with an eponym ([ina] PN), followed by a historical datum, mostly on the same line, but those following Atānum occupying four lines (4'–7'). While previously we only had Birot's copy of this fragment, in 2022 Nele Ziegler made a photo of it available (Fig. 4.1), which allows some corrections and precisions. The first and main is that in l. 3' Birot's tentative reading ^dA-š[ur] has to be corrected to what is written in the transcription presented above, presumably x-ia/e²-ia. The photo also shows that the remains of the ruling under l. 1' start earlier than where lines 5'–7' are indented and confirms that the remains of the first sign in l. 2' show that there was no open space as in lines 5'–7', and suggest the presence either of the long name of an eponym or a historical note. This shows that lines 1' and 2' belonged to two different eponyms. The first sign preserved in 1' cannot be identified; it could be part (the end?) of the name of the eponym or beginning of the second, indented line of a historical note added to the name of an eponym named in line 0'. Birot read the first legible sign of l. 2' as KA, which might be the beginning of *Puzur*₄ (cf. *Puzur*₄ in MEC 51 = MEC B:21 = S. 24-1).¹⁵

KRYSZAT 2008 proposed the following reconstruction for the sequence beginning with MEC 60:

KEL G 53	Aššur-nišu	MEC B:30	[Aššur-n]i-šu
54	Munawwirum	-	in the gap between MEC B and D
55	Idnāya		"
56	Dadāya		"
57	Puzur ₄ - ^d MUŠ	D: 2'	Pu[zur ₄ - ^d MUŠ]
58	A-be-a-a	D: 3'	[A-b]e ¹ -a-a ²
59	E-dī-num	D: 4'	A/E ² -ta-num
60	^d A-šur-ták-lá-ku	D: 8'	A-šūr-ták-lá-ku
61	I-si-im-Sú-in	D: 9'	 (no proposal)
62–67			gap between D and E
68	Da-da-[a]		"
69	A-NI-m[a-lik]		" (here Haya-mālik)
70	[Ša-lim-A-šur]i-mi-ti		perhaps = E: 1'?

There are many problems with this equation. Restoring *Pu[zur₄-DN]* in D: 2' is a problem, because of the remains of a damaged sign before it.¹⁶ This shows that it was not a line with an eponym without historical note, since then his name should be written starting with a ‘Personenkeil’ (cf. MEC 44 and 45) followed by a blank space. Therefore, taking KA as the beginning of the name of an eponym is impossible. Another problem is the very unlikely equation of *A-ta-num* (sic, not *E¹-ta-num*) with *E-dī-num*. He is followed both in KEL G 60 and in MEC D: 8' by Aššur-taklāku, but these eponyms

¹⁵ [Editors] There are differences between the transliteration and restoration given by ZIEGLER Ch. 3 in this volume and that given by Veenhof, leading Ziegler to equate S.24-3 1'–9' with KEL G 56–61 instead of KEL G 76–81.

¹⁶ [Editors] ZIEGLER (Ch. 3 in this volume) does not recognise the existence of a damaged sign before K[A²].

are not necessarily identical, for we have another Aššur-taklāku in M.7841, who was indeed followed by suggested-mālik (whom Durand proposes for D: 9'). He was in turn followed, as we know from Mari, by Šalim-Aššur, son of Uš(u)rānum, found in S 24-2: rev. 1' = REL 183 (a reading first suggested by D. Lacambre, see *U&D*: 9), who has now turned up in a recently published Kültepe text.¹⁷ In Kryszat's proposal he should be followed by *I-si-im-Su-in* of KEL G 61, whose name in MEC should have been spelled *I-ši-im-^dEN.ZU* (MEC does not use the syllabic spelling *Su-in*). But neither reading agrees with the traces of MEC D: 9', where there is too little room for [*i-na I-ši-im-^dEN.ZU*].¹⁸ That KEL G 69 should be equated with the postulated Haya-mālik known from Mari is unlikely, since it is now clear that the name is to be read *A-ni-m[a-....]*. Haya-mālik, suggested by Durand and the data from Mari, is preferable and since MEC D and E do not join there is room for one or two additional lines, where the conquest of Mari, a year later than the demise of Yahdun-Lim, could have been mentioned.

This shows that equating KEL G 57–61 with MEC D: 2'–9' is very unlikely. An additional problem is that this equation reduces the number of eponyms by four compared with the reconstructions of REL and myself (see below). The latter are based on the convincing material reconstruction of KEL G, presented in *U&D*: Appendix 4. They imply a temporal distance between MEC 1 = Erišum I year 1 and the death of Šamši-Adad I, in the eponymy of Ṭāb-šilli-Aššur or his successor Ennam-Aššur, of 197 or 198 years, according to REL (see *U&D*: 27) and 198 or 199 years (103 + 95 or 96 years) in my reconstruction. This figure (almost) exactly matches the distance recorded in two different Assyrian *Distanzangaben*. With Kryszat's proposal the difference is much bigger. A final problem is that in Kryszat's reconstruction the eponyms KEL G 54–56 and 62–68 have to be fitted in the gaps between MEC B and D and between D and E. Assuming the presence of at least some historical notes at least ca.15 lines would be needed for them and in particular (though this is not decisive) a big gap between MEC D and E seems rather unlikely.

Another question is whether one may link MEC D: 1'–2' with KEL G (68–)69 = MEC (75–)76 by taking these lines as the beginning of historical notes added to these two eponyms: MEC 75, [*i-na Da-da-a*] x [.....], MEC 76, [*i-na A-ni-ma-x*] K[A[?]....]. The first eponym is short enough to be fitted in the missing beginning of MEC D: 1', but the second (reading based on the collation in *U&D*: 114) is too big. If one dissociates MEC (75–)76 from the beginning of MEC D: 1'–2' there could be room for one (perhaps even two) additional eponyms before the beginning of MEC D, and this is the only place on the tablet where there is room for such an insertion, since it is covered neither by KEL G nor by MEC I. For lack of textual sources, the choice also depends on the question how many lines are available on the rev. of I. In my reconstruction, I have tentatively added one additional eponym: MEC 77.

In MEC D: 9' the conquest of Mari, assumed to have taken place during Haya-mālik's eponymy, can have been mentioned in the ca. 6 cm available to the right of his name (where the trace of a sign might be the beginning of ^d[UTU^{ši}-^dIM]), but it probably required a second line, lost in the break between MEC D and E (see above). Since there is no physical join between MEC D and E more lines could be missing here and this offers the opportunity of inserting here the text preserved in A.1288 IV, discussed below in §4.2. But again, what is theoretically possible must also be possible within the total number of lines available on the reverse.

An important question is to which events the historical note added to the eponymy of Atānum (D: 4'–7') refers. With *FM V*: 73, we may think of a victory of Šamši-Adad over Yahdun-Lim, but a letter from Mari (*FM V*: 47) states that he was eliminated by his son Sumu-yamam. It could mention a defeat of vassal cities or states of Yahdun-Lim in the western part of the Sinjar, known from Mari (*FM V*, 51ff.) and perhaps that 'their kings' now had to acknowledge him as their overlord. One may compare the situation after the demise of Yasmah-Addu, when the rulers of the Sinjar, especially Benjaminites, had to be forced to acknowledge Zimrī-Lim as their new overlord, as documented in the letters edited by J.-M. DURAND (2019). This could have happened after Yahdun-Lim had already disappeared, in which case the historical note added to Eponym 78 might mention that fact.

¹⁷ In Kt 08/k 28:18–19 (with the mention of his father, DUMU *U-šur-a-nim*), a contract from level Ib of *kārum* Kanesh, published by M. ÇAYIR 2015: 40–41. One wonders what other eponymies might be found in this archive, whose publication might throw light on the gap at the end of col. II of KEL G.

¹⁸ [Editors] ZIEGLER (Ch. 3 in this volume) nevertheless restores the line as [*i-na i-ši-im-^dEN*].¹ZU¹ ^dU[TU-^{ši}-^dIŠKUR o o o o o o o o].

3.2.2. The bottom of the reverse of I

The last part of I contains the eponyms known from Mari and we can supply in MEC E their names beginning with Awīliya (S.24-1-2 + A.1614b: 24'), but we do not know how many additional lines were needed for historical data which, as shown where the text is preserved, can occupy several lines (4 for Aššur-imitti, 5 for Ikūn-piya, 7 for Aššur-mālik).

F (A.1614a; Fig. 04.14), a fragment of 7 lines, as Birot suggests (accepted by *FM V*: 112, with notes 299–300) records events from the eponymy of Awīliya. This means that it links up with the bottom of A.1614b, whereby A.1614a: 1' could be part of the line A.1614b: 15'. Following *FM V*: 112, note 300, we can read:

A.1614b: 15'	A.1614a: 1'	[i-na A-wi-li-i]a Tu-ru-u[k-ku(-ú) ib-ba-a]l-k[i-tu]
A.1614b: 16'	A.1614a: 2'	^m [.....ú..]-la-an da-aw-da-am <ša> Tu-ru-u[k-ki-(i)]
	A.1614a: 3'	[i-na] ^{ki} i-du-ku ^m Ya-as-ma-ah-[^d IM]
	A.1614a: 4'	[.....da-aw]-da-am ša DUMU.MEŠ Ya-mi-n[a]
	A.1614a: 5'	[.....a-ah] ^{id} UD.KIB.NUN.NA ú-ta-[.....]
	A.1614a: 6'	[[?]]-a ^{ki} x [.....]
	A.1614a: 7'	[[?]]-a-ša [?] [.....]

Assuming that the beginning of l. 3' mentions the town where the victory was gained, the plural subject of *idukkū* must be hidden at the end of l. 1' and the beginning of l. 2' (^m-la-an). The information that Yasmah-Addu defeated the Benjaminites (who had been instigated by Yamhad) in the year of the Turukkū revolt and so gained control of the whole 'Euphrates [Valley]' fits the data from Mari (*FM V*: 114 and 118f.), but the identity of [.....]lān remains unknown. The last two lines, where the open space in l. 6' is strange, could belong to the next eponymy, that of Nimer-Sîn, or to him (one line) and his successor Adad-bāni. The drawing of BIROT (1985, 239) suggests that A.1614a: 7' is the end of the damaged first preserved line of M.11250 ([i-na A-šur]-e-mu-[qí ..]), but this is impossible, even when (which is unlikely) Nimer-Sîn and Adad-bāni occupied each only one single line. For this leaves no room for Tāb-šilli-Aššur and his successor Ennam-Aššur (whom we know from KEL G). Should the damaged bottom of A.1614b indeed touch the equally damaged top of A.7481 (as Birot's drawing suggests), we would have only 8 lines available for 5 eponyms (Awīliya – Ennam-Aššur), while for Awīliya alone already 5 lines are used. We need several additional lines, also considering the political events of these eventful years (see *FM V*: 148–150), such as the campaign in Zalmāqum, the death of Šamšī-Adad, his succession by Išme-Dagan, the demise of Yasmah-Addu and Zimri-Lim's conquest of Mari. Assuming a minimum of two lines for each of the successors of Awīliya the distance between MEC E: 24' (the first line of the eponymy of Awīliya) and MEC G: 1' (Aššur-emūqī) becomes 13 lines.

3.3. The size of Edition I and the colophon

According to my reconstruction, the reconstructed tablet with Edition I, ca. 28 cm long, would have had in all (after deducting the 3.4 cm occupied by the colophon and the empty space) ca. 150 lines available for 97 eponym entries. To find out whether that figure is correct W. Nahm used the manuscript of KEL G as a check, because he believed there were 'tally marks', added after 60, 120, etc. eponyms, but collation has shown that this is not the case.¹⁹ The figure in the colophon of MEC I is damaged. It begins with a vertical, after which one sees part of the head of a lower 'Winkelhaken'. This excludes *m[e-at]*, '100' (or *m[e-at LÁ ...]*, '100 minus ...') and suggests 60+10[+10+10+x], where x depends on the total number of eponyms listed and the room available. My reconstruction arrives at 97, which is one less than the Assyrian *Distanzangabe* requires (see below §6.2).

¹⁹ NAHM 2013: 367. His proposal is not acceptable, because collation by G. BARJAMOVIC (Ch. 6 in this volume) shows that the supposed tally marks after Eponyms 120 and 240 were part of an erasure or not on the tablet.

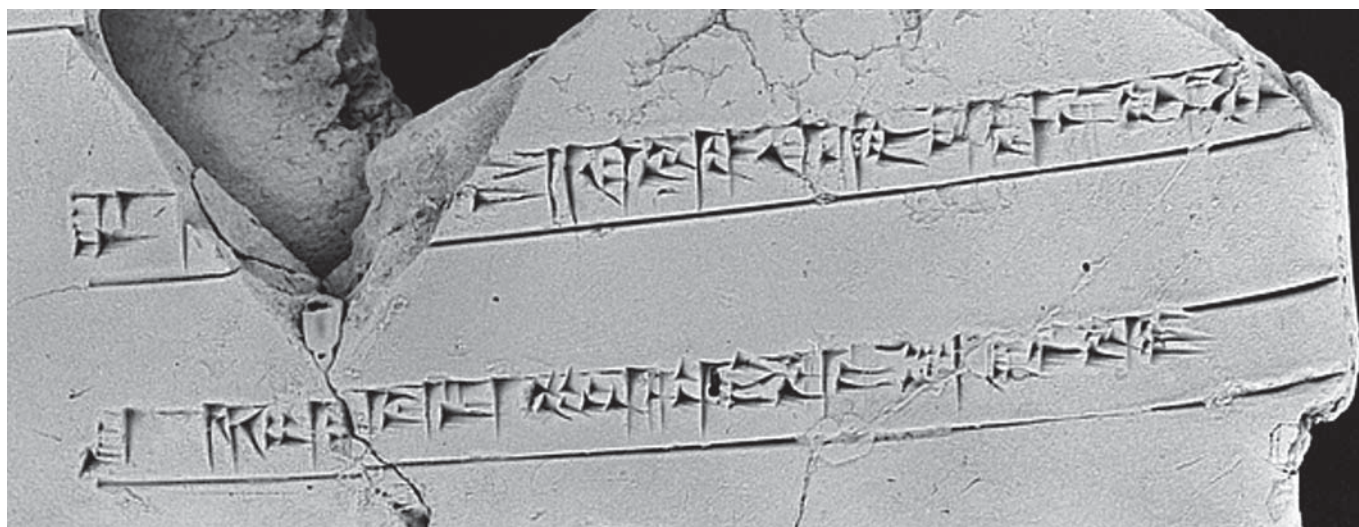


Fig. 4.2. The summary and the colophon at the bottom of M.7811 + M.11250.

The text of the colophon reads as follows:

*ŠU.NIGIN 60.1[0+ ša-na-t]um adi da-aw-de-e-em ša KÁ Sa-ag-ga-r[a-tim^{ki}]
 ŠU Ha-ab-du-ma-lik mu-uš-ta-as-sú-ú Li-mi-^aDa-gán*

The event the colophon mentions as the end of MEC is surprising in two ways: in the first place because one might have expected here the end of the reign or the accession of a king, since the chronicle starts with the accession of a ruler. It also does not mark the end of the last-mentioned eponymy year, in the OA system the winter solstice, which is not the time of military activities. Also surprising is that it ends in the second year after Šamši-Adad's death, while the strong impression is that the text focuses on the history of his 'dynasty'. The 'battle at the gate of Saggarātum' evokes, as Jack Sasson reminds me, the event mentioned in M. 6427+, quoted in *FM V*: 139 with note 521.²⁰ There Yasmah-Addu tells his brother about 'enemies before the gate of Saggarātum', a group split off from a main army that, with their king (obviously Zimrī-Lim), besieges Terqa, events that most probably took place in the spring of the eponymy year of Ennam-Aššur (MEC 96), after Šamši-Adad's death. But the battle our colophon refers to took place in the next year (MEC 97); had it dragged on or been resumed later? It was anyhow long after the latest 'texte éponymal', from month VI of MEC 96, and the demise of Yasmah-Addu, when Zimrī-Lim had conquered Mari (see *FM V*: 155). Was it a decisive moment, Zimri-Lim's victory over the last pocket of resistance of Yasmah-Addu's government?

Another question is who wrote the final part of the chronicle that covered the time immediately after Zimri-Lim's conquest of Mari. Was it a scribe in Mari, who rounded the chronicle off, perhaps even at the instigation of the new king, by recording in no less than six long lines, how the 'kingdom of Upper Mesopotamia' finally came to an end and the victorious Zimri-Lim – the last line before the colophon may well start with *dawdūm*, 'victory' – became king? I cannot answer this question, but a scribal activity in Mari seems more likely than the assumption that the tablets of the chronicle were imported from elsewhere.

3.4. Three problematic eponyms

a) *Id-na-^aA-šur*, son of Abu-šalim, on a tablet found at Tell Taya. Koliński has disputed the identification with Iddin-Aššur of KEL G 65 = REL 175 = ca. 1797 BC, because the tablet is sealed by 'Hāšidānum, servant of Šamši-Adad', attested as a governor of a district south-east of the Jebel Sinjar since REL 190 (ca. 1783 BC), fifteen years later.²¹ This criticism seems convincing, but there is no other place where we can insert him, since the eponyms of the last years of Šamši-Adad's life are all known. Identification with MEC E: 24' (REL 194), where the last sign of the name of the eponym looks somewhat like a damaged [š]ur, is excluded, because this must be [A-wi-li-i]a, successor of Aššur-mālik. While VILLARD (2001: 95) could still suggest a date during the early years of Išme-Dagan, KEL G, with the complete list of more than 30 eponyms after Awīliya, makes this impossible. The only solution still seems to be equating him with REL 175 (1798 BC),

²⁰ See also CHARPIN 2014: 208 with note 105.

²¹ KOLIŃSKI 2013: 738.

which implies that Hašidānum entered the service of Šamšī-Adad much earlier, in the very region where he is later attested as governor. This is not impossible since our knowledge of the early years of reign of Šamšī-Adad is extremely limited. The four eponyms that succeeded Iddin-Aššur are in fact only known from KEL G.

b) Ahiyaya. There are three eponyms of that name; the two others are Ahiyaya, son of Takiki, REL 203 = KEL G 89, and *A-hi-a-a^{sic}*, REL 244 = KEL G 132,²² but the issue here is the occurrence of Ahiyaya, son of Lāqēpum, in various texts alongside well-known eponyms from the period of the domination by Šamšī-Adad. While he occurs only once at Mari, he is well documented in the texts discovered at Chagar Bazar (two dozen references) and Tell Bi'a (4×). He has been discussed in *FM V*: 163–166, in *U&D*: 9–13, in LACAMBRE/MILLET-ALBÀ 2008, see esp. 160, table 8.3, by LACAMBRE in NABU 2009/18, by BLOCH 2014: 202–207, and again by D. Lacambre in his paper at the chronology session during the RAI 59 at Ghent, the provisional text of which he kindly made available to me.²³ The main question is whether he is an additional eponym, to be inserted in MEC and KEL G, or an eponym who was replaced by another (because he had died?). *U&D* follow Lacambre's suggestion to identify him with MEC 92 (E: 24') by restoring [*A-hi-ia-i*]a (= REL 193) and inserting him as an additional eponym (followed by Awīliya in the break after MEC E) and they find him also in KEL G 79 (where Günbatti restored *A-š[ūr-ma-lik]*), even though – as they admit on p. 109 – repeated collation of the second sign of the name does not support a reading H[I] (see their hand copy on p. 114).²⁴ *FM V*: 165 assumes that Ahiyaya was replaced later in the year by Ilī-(t)illatī and Bloch also assumes replacement, because adding Ahiyaya as an additional eponym for one of the last years of the Mari sequence is impossible. Year-names from Tell Harmal indicate that 5 years elapsed between the conquest of Qabrā, which took place during the eponymy of Aššur-mālik (MEC 91; see *FM V*: 97 note 183) and the death of Šamšī-Adad, during year 5 of Ibal-pī-El, and this would become 6 by inserting Ahiyaya as an additional eponym.²⁵ Since Ahiyaya is attested at Chagar Bazar and Tell Bi'a until month X, he can only have been replaced by a new eponym during the last months of the year and with some hesitation Bloch suggests that this was Aššur-imittī, although we have only one attestation of him (in Mari, from 10-IX, in M.18006).²⁶

c) Ennam-Aššur. In *U&D*: 9 its reading in MEC E: 1' (which led to Günbatti's restoration of it as KEL G 70) was replaced by Šalim-Aššur, who appears there as REL 183 (see *U&D*: 100, 15). Two eponyms called Ennam-Aššur are well attested. 1) Ennam-Aššur 'the successor of Ṭāb-šilli-Aššur', identified as such by KEL G 84 = REL 198, five times attested at Tell Leilan, in a group of texts discovered in 1985,²⁷ in which also the eponym Aššur-emūqī occurs. Dating him immediately after the death of Šamšī-Adad I, as *FM V*: 166–168 suggested, is confirmed by KEL G, where he figures as no. 84.²⁸ He might be identical with his namesake who occurs in the text M.11465, from room 108 of the palace of Mari, which deals with silver (see *MARI* 4: 257), but it is unpublished as far as I know. 2) Ennam-Aššur of KEL G 99 = REL 213, probably the son of Aššur-taklāku, attested during month IX in KBo 9, 40: 17–18 and in Kt n/k 33: 14,²⁹ a text from the archive of Eddin-Aššur, son of Ahiyaya, with texts dated to the years between KEL 208 and 219 (see *U&D*: 37, with note 125).

²² KEL 132, *A²-x-a-ri*, turned into *A-hi²-a-tal²* in REL 244, is *A-hi-a-a¹*, as suggested in NAHM 2013: 366f. and confirmed by collation by Barjamovic in 2015 (written over erased *Pu-ha-num* = the next eponym).

²³ [Editors] See now LACAMBRE Ch. 7 in this volume.

²⁴ Their choice must be influenced by the fact that Ahiyaya occurs in two texts during month X.

²⁵ BLOCH 2014: 206–207.

²⁶ DURAND 2009: text 203.

²⁷ WHITING 1990: esp. 185–186.

²⁸ That he is not attested at Mari until 11/V (see *MARI* 4: 264f.) must be due to the turbulent political situation in the year after Šamšī-Adad's death, when the identity of the new eponym only became known at Mari after a couple of months.

²⁹ Published by V. DONBAZ 2008: 54.



Fig. 4.3. A.1288, bottom of obverse, MEC A = Edition II, Col. I: 21'–28' and Col. II: 20'–27'.

4. Edition II, A.1288

4.1. Size and structure of Edition II

The text of the bottom of the obverse of A.1288, Col. I: 23'–28' and Col. II: 20'–27', reads as follows:

col. I	REL	col. II	REL
23' [i-na Da-di]-ia' LUGAL ^d UTU ^š - ^d IM	126	20' ^m Šu-da-[a-a]	151
24' <u>wa-li-id</u>		21' i-na Šu-da-di-im ša-ba-[at]	152
25' [i-na Puzur ₄ -Išta]r na-ah-du-ur ^d UTU	127	22' i-na ^d A-šur-tu-ku[l-ti da-aw-da-a-am]	153
26' [i-na I-sà-(i)a mu-u]t A-mi-nim	128	23' ša' Ú-ni-ne-a-[im]	
27' [x š]a ² ^d IM		24' x []	
28' [ina A-bu-ša-lim ² x k]u ²	129	25' da-aw-da-a-[am ša]	
		26' ^m Mu-ut-Ya idūk]	
		27' [] x []	

J.-M. DURAND (1990: 274f.) observed on A.1288 that ‘la silhouette de la tablette est quasiment totalement conservée’ and that it would have had columns of ca. 35 lines. But new evidence shows that we have to assume columns of ca. 41 lines, plus 2 or 3 lines on the edge, which suggests a tablet ca. 17,5 cm long, with in all ca. 175 lines, on average 4.3 mm high.

KEL A shows that at the top of col. I of A.1288 at least 7 lines (the introduction preserved in I, written on two lines in II, plus 5 eponyms) are missing. The written text breaks off after 28 lines (with MEC 26, which covers 2 lines). Between this line and the first preserved one of col. II (MEC 37, equal to l. 38 of I) 10 eponymies, MEC 27–36, must be inserted, of which 27–30 are only known by name (from KEL). MEC 32 and 33 have a historical note added in I and this probably also applies to some of MEC 27–30.³⁰ In all this probably requires ca. 15 lines (10 eponymies + 5 second lines), of which ca. 10 are missing at the top of col. II (II: 1' starts at the level of I: 3').

Col. II breaks off after (10+27=) 37 lines, after MEC 50 (on 5 lines!). The ca. 6–8 missing lines (of the bottom and the lower edge) probably covered MEC 51–52 or 53 (3 or 5 lines in I), so that A.1288 III may have started with MEC 53 or 54. Since at the top of col. IV we lack ca. 5 lines, we have to establish how the ca. 46–48 lines between MEC 52 or 53 and the text on col. IV were filled to find out which eponym (his name is broken off) is presented in col. IV (= MEC C). We must take into account that the lay-out of II required more additional lines for registering historical data than I,³¹ but occasionally also omitted some. II has them like I (as far as we can see, when its text is preserved) after MEC 9, 11, 13, 15, 16, 23, 37, 38, 40, 42, 47 and 50, each time on 2 lines, but omits those mentioned in I after MEC 10, 14, 17 and 19. I

³⁰ I assume that these eponymies, as most in this part of I, covered only one line each; the first eponymy using two lines in I is MEC 38, which records two facts, the second the birth of Šamši-Adad I.

³¹ An additional complication is that in col. II of A.1288 second and following lines needed for historical data are usually not indented, unless the last line of such a note did not need a whole line (lines 6', 18' and 23').

only needs second lines for Eponyms 38, 50 and 53 (not preserved in **II**); with other eponyms its long lines offered enough room for a historical datum on a single line. We may compare S.115-26 (Edition **III**, Birot text B), with a lay-out similar to **II**, with short lines, which uses 2 lines for MEC 37, 3 for MEC 40, and even 4 for eponym MEC 38. The pattern in the missing columns of A.1288 must have been similar, but it is impossible to know exactly how many second lines per eponym must be added when we cannot compare the corresponding passage in Edition **I**.

4.2. MEC C = A.1288 col. IV

After a gap at the top of ca. 5 lines, I read, using most of the restorations proposed by DURAND in *MARI* 6 (1990: 274f.):

1' [x x x x x] x x [x x x (x)]	5' BÀD- ^d [IM ^{ki}]	9' i-na ta-ri-š[u i-du-uk-ma]
2' [x x x x-]IM ma-a-at [ø]	6' ù BÀD- ^d UTU ^š - ^d [IM ^{ki}]	10' Ra-pí-qa-[am x x x x x]
3' [Mar-da-m]a-nim Še-er-wu-nim ^{ki}	7' i-na da-ad-mi ib-[ta-ni]	11' ù []
4' ù Ha-bu-ra-tim LUGAL iṣ-ba-a[t]	8' da-aw-da-a-am š[a x x x x x]	

Durand restored in l. 2' the eponymy of [Ib-ni-^d]IM (= MEC 85, ca. 1785 BC) and in 8' [Su-mu-e-pu-uh] as defeated king of Yamhad. He was followed by *FM* V: 82f., which considers the exploits mentioned here as evidence for 'des avancées du royaume de Haute-Mésopotamie dans trois directions'. In the west, on the boundary with Yamhad, two fortresses were built, in the east lands and cities between Khabur and the Upper Tigris were conquered and in the south this may have happened (the verb is missing) with Rapiqum. The restoration [Mardam]ānum in l. 3' is no problem, for this form of the name of the town, attested many times (also in OA) as Mardaman, occurs also in CHARPIN/JOANNÈS *et al.* 1988: 482 no. 512: 7', and its combination with Širwun(um) and Haburātum³² fits the geographical and historical pattern. Dating the events recorded in this text in the eponymy year of Ibni-Adad also seems to fit in the framework of the history as analysed in *FM* V, although we must realize that its picture also depends on Durand's reading of A.1288 IV. The use of LUGAL in l. 4' is remarkable,³³ but perhaps understandable if the king's name had been mentioned before. Durand's convincing readings of lines 2', 7' and 9' imply that the whole fragment deals with one single eponymy year and not with three, as Birot assumed. Surprising, however, is a historical note that starts with *māt* [GN] as foregrounded object, for such notes normally begin with the king's name as subject of *iṣbat* (MEC 8 and 83), with as variant, when the conqueror is not mentioned, *šābat* GN (MEC 32, 43, 47). I would therefore prefer to find in l. 2' the royal subject, [^dUTU^š-^d]IM, and this need not be a problem, if we assume that the eponym dating (*ina* PN) had been given in the/a preceding line.

A basic problem, however, is that if Durand is right, this eponymy would have been covered in **I** by one single line and in **II** by ca. 10 lines, which is difficult to accept. Another problem is that the appearance of MEC 85 at the top of A.1288 IV does not fit the size and lay-out of this tablet. In the previous paragraph I concluded that there are ca. 46–48 lines available between MEC 52 or 53, the calculated end of col. II, and the text of col. IV. If C deals with MEC [84+]85 these lines (A.1288 III and the missing first lines of col. IV) must have treated MEC 53/54–83. In this space 30 or 31 lines are needed as first lines with the names of the eponyms, so that ca. 16 additional lines are available for historical notes. Comparison with **I**, to see whether this is possible, is difficult, since **I** has a gap for MEC 61–75, but only two of MEC 53–60 in **I** have no historical notes (2 lines for MEC 53 and perhaps 60), which means that for them alone **II** needs probably at least 8 additional lines. Of MEC 61–75 we only know the names from KEL G, but if we apply the ratio of MEC 53–60, **II** may have needed ca. 8 to 10 additional lines. This brings the total number of lines used for MEC 53–75 to ca. 38–40, so that for MEC 76–83 there remain only ca. 8–10 lines. This is too little, for all of them had historical notes in **I** (MEC 79 even one of 4 lines!), which together probably required at least 25 lines in **II**, which are not available. Even when we raise or lower these figures by a few, the result remains the same: MEC C cannot have started with MEC [84+] 85; these eponyms may at best have been treated at the bottom of col. IV.

These calculations allow the conclusion that A.1288 III + lower edge covered MEC 54–78 and that A.1288 IV started with MEC 80, so that the preserved text of MEC C can be assigned to MEC 81, the eponymy of Haya-mālik. This is attractive, because there is a gap in **I** between the end of MEC D and the beginning of MEC B, which is the only place where we can insert an eponym with a long historical note (ca. 3 lines in **I**), a solution that also solves the problem of a startling difference between **I** and **II** in their treatment of the same eponymy. MEC 81 is usually assumed to mark the year

³² See DURAND 1988: 294, a) and for Širwunum also CHARPIN/JOANNÈS *et al.* 1988: 268 No. 405:14' ('the king of the land Širwunum'). See for the suggested location of Mardaman and Haburātum the map of N. Ziegler in *FM* VI (2002): 267.

[Editors] For the identification with the site of Bassetki and a detailed history of Mardaman see now PFÄLZNER/FAIST 2020.

³³ We find it only in MEC 23 and 38, probably because they mention his birth and his accession.

when Šamši-Adad I conquered Mari, having ousted Sumu-yamam, who had deposed his father two years earlier (see *FM* V: 47f.) and this implies that the conquest of Mari is to be added to the exploits listed in MEC C. An alternative solution would be to link C with one of the eponyms MEC 75–76, because the physical link between these eponyms and the beginning of MEC D is uncertain (see above §3.2). But here historical considerations come into play. Dating the exploits of Šamši-Adad mentioned in MEC C, especially his construction of fortifications on the Euphrates, in the time of MEC 75–76, before he had eliminated his rival Yahdun-Lim (in ca. 1794 BC and to which MEC 78 could have referred) is very unlikely. An expedition to Rapiqum also would be difficult before the conquest of Mari. Both seem possible in MEC 81, when Mari was captured and this may also have been the time when Sumu-epuh of Yamhad entered the picture as Šamši-Adad's western rival. I admit that there is no evidence for dating these exploits in that year, but this may be due to the general lack of historical data before ca. 1790 BC and there does not seem to be an alternative place for MEC C.

In December 2015, after my manuscript had been finished, R. KOLIŃSKI, who shared my dissatisfaction with Durand's interpretation of MEC C, published a new analysis: 'The Mari Eponym Chronicle. Reconstruction of the lay-out of the text and the placement of Fragment C'. It calls for a reaction.³⁴

His solution is to restore in l. 3' of C [*i-na E-d*]*i-nim* instead of Durand's [*Mar-da-m*]*a-nim*, i.e. eponym MEC 66 = KEL G 59 = REL 169.³⁵ This proposal faces the problem (mentioned above) that a historical note beginning with *māt* [...] is not really acceptable. If my idea to restore therefore in l. 2' [^dUTU^šⁱ]-*d*IM is accepted, the name of the eponym is missing and Koliński's proposal rests on the reconstruction of the lay-out of A.1288 II and depends on the historical probability of dating the exploits mentioned in MEC 66.

His solution moves MEC C 15 years up (from MEC 81 to 66), which equals ca. 23 lines in I and at least ca. twice as many (short) lines in II. The ca. 46 lines available between the beginning of A.1288 col. III and IV: 3' then would deal with only 13 eponymies (MEC 53/4–65) which, to fill these lines, have to be accompanied by a lot of historical data. Koliński assumes that MEC 61–65 (not preserved in I) took up 26 lines, more than 5 per year, arguing (p. 73) that these were eventful years, with many facts to record. But the general impression gained from the obverse of I is that the composer of the chronicle was rather selective in what he mentioned, certainly for the period before Šamši-Adad's great conquests. Moreover, if Koliński's reconstruction is accepted, these eponymies in I too must have been accompanied by more historical data, on average at least 2 lines per year. But there is not enough space in I, for I calculated that for the 16 eponymies from MEC 61 until MEC 76 (= the beginning of MEC D = S.24-3) only ca. 26 lines are available, which leaves far too little room for the additional lines required by Koliński's solution, even when the tablet of I is 1 cm longer than I have assumed.³⁶

Are the exploits of 'the king' (apparently Šamši-Adad) mentioned in MEC C historically feasible in ca. 1807 BC, shortly after his conquest of Assur? Our knowledge of this early period is extremely limited and we have no other information about the building of the fortresses and the name of the defeated king is unfortunately also not preserved.³⁷ But that Šamši-Adad [conquered?] the strategic town of Rapiqum in the south, on the Middle Euphrates very soon after the conquest of Assur, would be rather surprising. And while he may have extended his power to the north, into the Tigris valley (where Šerwunum and Haburātum are situated), the building of fortresses in the bend of the Euphrates, to keep Yamhad in check, is not acceptable as long as Yahdun-Lim of Mari, his bitter rival, was still in control of the area in the western bend of the river. I therefore have to conclude that Koliński's proposal is not acceptable.

³⁴ KOLIŃSKI 2015: 61–86.

³⁵ This is the only name of an eponym ending in *-num* that allows this reading; the other, Atānum (MEC 79), is followed by a different historical note.

³⁶ On p. 79 KOLIŃSKI mentions that between the obverse and reverse of what is preserved of I 'extra lines can be easily added' and while he estimates the length of the tablet on p. 72 as 26 to 27 cm, on p. 74 and 83 he mentions 28 to 29 cm, assuming columns of ca. 80 lines (each cm added on the obverse and the reverse gives room for ca. 6 extra lines). I realize that exact figures cannot be given, but it seems to me unlikely that at the bottom of the obverse of I, underneath MEC B, ca. 15 lines (= 5 cm) would be missing. I agree with Koliński's rejection of the idea of *U&D*: 95 to restore the three eponyms listed as REL 164–166 = MEC 61–63 in MEC C, where they appear somewhat cryptically as 'C L 2'. They must have figured on the missing bottom and lower edge of I, where there is room for ca. 12 lines (see above §3.1).

³⁷ Durand restored the name of Sumu-epuh, the king of Yamhad, who was Šamši-Adad's rival, who in ca. 1779 BC conquered the two fortresses mentioned in the text, see *FM* V, 112. He died in 1778 BC, but was he already king 26 years earlier? His son and successor anyhow writes that he died before he was old (*FM* V: 112 note 385, *šibū[ssu] ul iṣīb*).

5. A reconstruction of the Mari Eponym Chronicle (MEC)

5.1. The reconstructed text of Edition I

<i>Italics</i>	preserved text of I	<i>u, u'</i>	damaged, restored and uncertain signs
[]	restorations in I and in the text of II and/or III		line starts with a vertical wedge followed by a blank
{ }	text in II or III, not in both		space before the name indicating that there is no
()	restorations in the text of II and III		historical note.
/	separates lines in II and III	(i-na)	historical note of I absent in II and III
//	names from parallels with KEL A and G	↓ and ↑	beginning and end of a fragment or gap
?	before eponyms only known from KEL G: uncertainty about the presence of a historical note	84*	numbers in KEL G and <i>U&D</i> that differ from those in their original editions

Birot's designations of the fragments of MEC are as follows:

A = M.7481+M.11250 obv., A.1288 I, M.5911; B = S.115-26, A.1288 II, M.8566, S.24-1+2 obv.; C = A.1288 IV; D = S.24-1+2 rev +A.1614b; F = A.1614a; G = M.7481 rev. [A.1614a (F) almost joins A.1614b (E) in lines 31'-32']

For the Eponyms 10–12, where A.1288 I, M 5911 and Birot's transcription (on p. 227) have omitted a line, see below in §5.2 the note on these lines.

Line numbers before MEC 61 are based on textual evidence; after that, due to gaps, a continuous numbering is impossible and I give separate sequences (lines 1'ff., 1''ff. and 1'''ff.). The numbering of the eponyms is continuous (thanks to KEL G), with uncertainty after MEC 76, where I have tentatively inserted MEC 77.

Fig. 4.4. The reconstructed text of Edition I of the Mari Eponym Chronicle.

line	ep. no.	name	historical events	manuscript	A-G	KEL A/G	REL
.1		<i>iš-tu re-eš</i> ^{gib} G[U.ZA ša ^d Na-ra-am- ^d EN.ZU UGULA be-li-n]e <i>iš-tu li-m</i> [u-um i-ši-ik-nu-ni]		↓ M.7481+ obv.	A ↓		
.2	1 //		Š[u- ^d EN.ZU]			A 103	104
.3	2 //		[A-šur-ma-lik]			A 104	105
.4	3 //		[A-šur-i-mi-ti]			A 105	106
.5	4 //		[En-nam- ^d EN.ZU]			A 106	107
.6	5 //	<i>i-na</i>	[i-mi-im A-ku-tim]] x []-šur [?]			A 107	108
.7	6 //		[(M)a-ši-a-am-DINGIR]	↓ A.1288 I		A 108	109
.8	7		[I-di-a-hu-um]			A 109	110
.9	8	<i>i-na</i>	Sa-[(m)a-nim A-mi-nu-um Ša-du-pé-em iš-ba-at]			A 110	111
.10	9	<i>i-na</i>	I-[i-en-nam ^d EN.ZU-a-bu-um ma-a-at Ši'-ID-KI / iš-ba-at]	↓ M.5911		A 111	112
.11	10	(i-na)	En-n[^d a-ma-nim]			A 112	113
.12	11		En-na[m- ^d A-šur]			A 113	114
.13	12	<i>i-na</i>	En-na[m- ^d EN.ZU (^m I-pi-iq- ^d IM a-na { ^{gib} GU.ZA} É a-bi-šu / i-ru-ub]			A 114	115
.14	13	<i>i-na</i>	Ha-na- ^d [Na-ri-im A-mi-nu-um {/} da-aw-da-am / ša I-pi-iq- ^d IM i-du-uk]			A 115	116
.15	14	(i-na)	Da-di-i[a]			A 116	117
.16	15	<i>i-na</i>	Ka-pa-ti-ia {/} I-[pi-iq- ^d IM da-aw-da-am / ša A-mi-nim i-du-uk]	↑ M.5911		A 117	118
.17	16	<i>i-na</i>	Iš-me- ^d A-šur I-pi-iq- ^d IM / zi-qû-ra-tam iš-ba-at]			A 118	119
.18	17	(i-na)	^d A-šur-mu-tap-pi-il I-[pi-iq-			A 119	120
.19	18	(i-na)	Šu- ^d MUŠ]			A 120	121
.20	19	(i-na)	I-d[i-a-bu-um ^d EN.ZU [?]]			A 121	122
.21	20	[()]	↑ M.7481+ obv.			A 122	123
.22	21	[()]	^d A-šur-i-mi-ti]			A 123	124
.23	22 //	[i-na	B]u-ša-a-ia]			A 124	125
.24	23 //	[i-na	Da-di-i)a LUGAL ^d UTU ^{si} - ^d IM / wa-li-id]			A 125	126
.25	24 //	[i-na	Puzura-lštar na-ah-du-ur ^d UTU]			A 126	127
.26	25 //	[i-na	I-sá-a-ia mu-ut) A-mi-nim]			A 127	128
.27	26 //	[i-na [?]	A-bu-ša-lim x x x x ša [?] ^d IM / [x x x x -ku [?]]	↑ A.1288 I	A ↑	A 128	129
.28	27 //	[]	^d A-šur-SIPAD]	↓ gap		A 129	130
.29	28 //	[i-na	DU ₁₀ - ^d A-šur]			G 21	131
.30	29 //	[i-na	Šu-Ra-ma]			G 22	132
.31	30 //	[]	^d EN.ZU-iš-me-a-ni]	↑ gap		G 23	133
.32	31	[]	^d A-šur-ma-lik]	↓ S.115-26	B ↓	G 24	134
.33	32	[]	Da-ni-a]			G 25	135
.34	33	[i-na	En-nam- ^d EN.ZU mi-lum ša ma-a-tim ru-uq-ti(m)]			G 26	136
.35	34	[i-na	^d A-šur-ba-la-ti]			G 27	137
.36	35	[]	En-nam- ^d A-šur]			G 28	138
.37	36	[]	I-túr- ^d A-šur]			G 29	139
.38	37	[i-na	Šu-Be/i-li I-la-kab-ka-bu-ú / Šu-ub-ra-a-am iš-ba-at]	↓ A.1288 II		G 30	140
.39	38	[i-na	LUGAL- ^d IM da-aw-da-a- ^{eras} -am / ša I-pi-iq- ^d Adad LU [?] NIM i-du-u(k) / ù LUGAL ^d UTU ^{si} - ^d IM a-na É a-bi-šu / i-ru-ub'(BU)]			G 31	141
.41	39	[]	Šu-La-ba-an]			G 32	142
.42	40	[i-na	^d A-šur-i-mi-ti Lu-ul-lu-um/ù da-aw-da-a-am ša LUGAL / i-na La-za-pa-tim i-du-uk]			G 33	143
.43	41	[i-na	Da-da-a-ia mu-ut Ab-bi-x x [x x x]			G 34	144
.44	42	[i-na	Da-da-a-ia M]iN I-pi-iq- ^d IM / Ar-ra-ap-ha-am ^{ki} iš-ba-at)]	↓ M.8566		G 35	145
.45	43	[]	A-hi-ša-lim ša-ba-at GA-x-[x x (x)]	↑ S.115-26		G 36	146
.46	44	[]	U-šûr-ša-lš(tar)]			G 37	147
.47	45	<i>i-na</i>	Ka-{at}-ta-a]]	↓ S.24-1+		G 38	148
.48	46	<i>i-na</i>	Šu- ^d EN.Z(U)]			G 39	149
.49	47	(i-na)	A-[bu-ša-lim ša-ba-at x [] / ša Né-re-eb-ti(m) ^{ki}]			G 40	150
.50	48	<i>i-na</i>	Šu- ^d a-a]]			G 41	151
.51	49	<i>i-na</i>	Šu-Da- ^d [i-im ša-ba-at Ne-r[e-eb-tim ^{ki}]			G 42	152
.52	50	<i>i-na</i>	^d A-šur-tu-kû[l-ti da-aw-(da-a-am) / ša Ú-ni-ne-{a}-i(m ^d UTU ^{si} - ^d IM / i-du-uk)]	↑ M.8566		G 43	153
.53			ù [/ da-aw-da-a-a(m ša x x x) ù [?]] / ^m Mu-ut-Ya-x (x x i-du-uk)]				
.54	51	<i>i-na</i>	Puzura-lš[^d tar] ^d UTU ^{si} - ^d IM []	↑ A.1288 II		G 44	154
.55	52	<i>i-na</i>	A-ta-na-[a]h I-pi-iq- ^d IM da-a[w-da-a-am ša]			G 45	155

line	ep. no.	name	historical events	manuscript	A-G	KEL A/G	REL
56			<i>ù ma</i> -[a-at x x x i-du-uk				
57	53	<i>i-na</i>	<i>E-ri-ši-im</i>	<i>dUTU⁸¹.dIM da-aw</i> -[da-am ša		G 46	156
58			<i>i-na BĀD</i> -[	i-du-uk			
59	54		<i>A-šur-en-nam</i>	<<ša-lim>>	B ↑	G 47	157
60	55	<i>i-na</i>	<i>I-ni-ī</i> [b-lš] <i>tar</i>	<i>I-pi-iq^dIM</i> [		G 48	158
61	56		<i>dA-šur-be-el-ma-al-k</i> [i]			G 49	159
62	57	<i>i-na</i>	<i>Be</i> -[la-nim	] <i>ki-ir-ba-na-a</i> [		G 50	160
63	58	<i>i-na</i>	<i>S</i> [ū-ka-lim]	<i>dUTU⁸¹.dIM</i> [		G 51	161
64	59	<i>i-na</i>	[A-mur- ^d A-šu] <i>r</i>	<i>dUTU⁸¹.dIM</i> [		G 52	162
65	60	[i-na	^d A-šur-n] <i>i-šu</i>	<i>dUTU⁸¹.dIM</i> [		G 53	163
66			] x [		↑ S.24-1+		
	61 //	[?	Mu-na-wi-ru-um		↓ gap	G 54	164
	62 //	[?	Id-na-a			G 55	165
	63 //	[?	Da-da-a			G 56	166
	64 //	[?	Puzur- ^d Nirah			G 57	167
	65 //	[?	A-bia-a-a			G 58	168
	66 //	[?	E-dī-num			G 59	169
	67 //	[?	^d A-šur-ták-lá-ku			G 60	170
	68 //	[?	I-si-im-Sú-in			G 61	171
	69 //	[?	^d Adad-ba-ni		(bottom obverse of I?)	G 62	172
	70 //	[?	A-bia-ša-gi-iš		(bottom edge of I?)	G 63	173
	71 //	[?	Tāb-ši-lá-A-šur		(bottom edge of I?)	G 64	174
	72 //	[?	I-dī- ^d A-šur		(top of reverse of I?)	G 65	175
	73 //	[?	Na-mī-i-[a]			G 66	176
	74 //	[?	A-hu ¹ -šar-r[i]			G 67	177
	75 //	[?	Da-da[-a]		↑ gap	G 68	178
1'	76 //	[?	A-ni-m[a ⁷ -x]	x [	↓ S.24-3	D ↓	G 69
2'	77	[?		]K[A ²			
3'	78	[i-na	]e/ia ² -i[a	^d U]TU ⁸¹ .dIM		↓ gap	179a
4'	79	[i-na]	<i>A-ta-nim</i>	<i>dUTU⁸¹.dIM</i>			180*
5'				<i>12 LUGAL.MEŠ i-</i>			181*
6'				^m <i>Ya-ah-du-um</i> -[Li-im			
7'				<i>LUGAL.MEŠ-šu-nu ut-t</i> [e			
8'	80	[i-n]a	<i>dA-šur-tak-la-ku</i>	<i>dUTU⁸¹.dIM</i>			182*
9'	81	[i-na	<i>Ha-ia-ma]-lik</i>	<i>dUTU⁸¹.dIM</i>	↑ S.24-3	D ↑	183*
				here the long text A.1288 IV: 1'-11' ?		C ?	
1"	82	[ina	Š[a-lim- ^d]A-šur	[x x (x)] <i>dUTU⁸¹.dIM ma</i> -[a-at	↓ S.24-1+ rev.	E ↓	184*
2"	83	[ina	^d E]N.ZU-mu-ba-al-li-iš	<i>dUTU⁸¹.dIM ma</i> -a-[at			185*
3"	84	[ina	R[i-iš- ^d UTU	<i>lš-me^dDa-gan da-aw-da-a-a</i> [m ša			186*
4"	85	[ina	I]b-ni- ^d IM	<i>dUTU⁸¹.dIM ma</i> -a-at [			187*
5"	86	[ina	^d A]-šur-i-mi-ti	<i>dUTU⁸¹.dIM da-aw</i> -[da-a-am ša			188*
6"				ša-a-tu ú-te-er ma-a-a[t			
7"				<i>Me-Tu-ra-an^{ki} ma-a-at</i> x[			
8"				^m <i>Da-du-š</i> [a	↓ S.24-1+ rev.		
9"	87	<i>i-na</i>	ī-[i-ī]LLAT ⁽¹⁾	[			189*
10"	88	<i>i-na</i>	Ri-ig-m[a-ni]m	<i>mu-t</i> [a-nu ²			190*
11"	89	<i>i-na</i>	I-ku-u[n-pi]-ia	<i>mu-n</i> [a-wi-ri-im			191*
12"				<i>da-aw-da-a-a</i> [m ša			
13"				ú <i>dUTU⁸¹.dIM</i>			
14"				<i>Me-Tu-ra-an^{ki} x</i> [			
15"				<i>a-na Da-d</i> [u]-š[a			
16"	90	<i>i-na</i>	[As-qú-di-i]m	<i>dUTU⁸¹.dIM</i> [		↑ gap	192*
17"	91	<i>i-n</i> [a	[^d A-šur]-ma-lik	<i>lš-me^dDa-gan da-aw-da-a</i> -[am ša		G 84*	193*
18"				ú <i>dUTU⁸¹.dIM Nu-ur</i> -[ru-ga-am ^{ki} iš-ba-at ú]			
19"				^m <i>Ki-ip-ra-am LUGAL</i> [			
20"				^m <i>Ya-šur-ub^dIM LUGAL</i> [A-ha-zi-im ^{ki}			
21"				^m <i>Ya-šur-ub-Li-im LUGAL</i> [			
22"				6[+x] <i>LUGAL.MEŠ an-nu-tim ik-s</i> [ú			
23"				<i>a</i> -[n]a <i>Da-d</i> [u]-ša .	↑ S.24-1+ rev.	E ↑	
24"	92	[ina	A-wi-li-i]a	<i>Tu-ru-u</i> [k-ku-ú ib-ba-a]/-k[i-tu	↓ A.1614a	F ↓	G 85*
25"				^m [x-x]-la-an da-aw-da-am <ša> <i>Tu-ru-u</i> [(k-ki-i)]			
26"				[i-na x x x] ^{ki} <i>i-du-ku^m Ya-ás-ma-ah^dIM</i>			
27"				[da-aw]-da-am ša <i>DUMU.MEŠ Ya-mi-na</i> [i-du-uk]			
28"				[ú a-ah] ¹⁷ <i>BURANUN ú-ga-m</i> [e-er]			
29"	93 //	[ina	Nimer- ^d Suen	]a ^{ki} [			G 86*
30"				[.]a-na [	↑ A.1614a	F ↑	
	94 //	[?	^d Adad-bāni		↓ gap		G 87*
	95 //	[?	Tāb-šilli- ^d Aššur				G 88*
	96 //	[?	Ennam- ^d Aššur		↑ gap		G 89*
1'''	97	[ina	^d Aššur]-e-mu-[qí		↓ M.7481	G ↓	G 90*
2'''				x [			
3'''				ú ² [			
4'''				x [			
5'''				4 ² [			
6'''				d[a-aw-da-a-am ²			
7'''				ŠU.NIGIN 60+10[+20+7 ² ša-na-t]um a-di da-aw-de-e-em ša KA Sa-ag-ga-r[a-tim ^{ki}]			
8'''				ŠU Ha-ab-du-ma-lik mu-uš-ta-as-sú-ú Li-mi- ^d Da-gan			

Fig. 4.4 (cont.). The reconstructed text of Edition I of the Mari Eponym Chronicle.

5.2. Comments on selected lines and references to literature

- 6 Reading proposed by W. Nahm, since the first preserved sign is neither A nor A[K].
- 10 The name of Eponym 9 in KEL A is Ilī-ālum, A.1288 has Ilī-ennam, what I had is not known.
- 11–13 The sequence of names beginning with *En-nam*- has confused the scribes. The score is:

M.7481: 11–14

i-na En-n[a-ma-nim]
^m [...]
i-na En-na[m-^dEN.ZU]
i-na Ha-na-^d[Na-ri-im]

M.5911: 2'–5'

[^m]*En-na-ma-[num]*
i-na En-na-nam-^d[A-š[ur]
a-na ^{gi8}GU.ZA É [...]
i-na Ha-na-na-ri-[im]

A.1288 I: 6'–9'

[^m]*En-nam-ma-nu-um*
[i-na] En-nam-^dA-šur
[I-p]i-iq-^dIM a-na É a-bi-šu ...
[i-na Ha-n]a-na-ri-im

KEL A: 112–115

[*En-n*]*a-ma-nim*
^r*En¹-um-A-šur*
[En-na-S]ú-in
Ha-n[a-n]a-ru-um

M.7481 has all three eponyms, with Ennam-Aššur certainly to be restored in l. 11; this was a year without historical note. The omission of Ennam-Sîn in M.5911 and A.1288 apparently had the effect that their writers transferred the accession of Ipiq-Adad to the previous year. In my reconstruction it is therefore recorded under Ennam-Sîn = MEC 12. The mistake in M.5911: 3' (*En-na-nam-...*) may reflect the confusion of its scribe, as does the fact that in M.7481 Ennamānum is followed by a historical note (*ina* E.), while M.5911 and A.1288 only list his name. BIROT, without knowledge of KEL A, could not have known the truth and omitted Ennam-Sîn on p. 233. Note a similar confusion in MEC 35, Ennam-Aššur in S.115-26: 5', for which KEL G 28 has *E-na-Sú-in*, which I assume to be a mistake (it repeats no. 26).

- 12 The word between {...} only in M.5911.
- 13 This eponym is omitted in A.1288 and M.5911 and also in Birot's edition.
- 15 In M.5911: 9' *Da-da-[...]*.
- 18 In A.1288 spelled ^d*A-šur-mu-ta-pi-il*.
- 19 Remains of a vertical wedge, not at the very beginning of the line, suggest [*i-n*]*a* rather than the 'Personenkeil' and the open space before the name suggests a rather short historical note.
- 20 Sîn-Abum is suggested by the note after Eponymy 9, but the reading of damaged ^dEN.ZU is uncertain.
- 24–27 See Fig. 4.3.
- 24 The first sign preserved in l. 24 looks more like *ša* than *ia*.
- 26–27 The text is only preserved in A.1288 I: 26'–28', where the beginnings of the lines are missing. My division of the remains of the text over two eponymies is a guess, but based on the conviction that in l. 26 *mūt Amīnim* is the complete historical note and that the two lines ranged under Abu-šalim could have been written on one line in I.
- 28–31 Nowhere preserved, names from KEL G, historical data possible, but unknown.
- 38 *FM V*: 34 note 32 suggests to take *Šu-up-ra-a-am* not as the city Šuprum, but as a nisbe, «the man/ruler of Šuprum», but the verb *šabātum* always refers to the conquest of a town.
- 39–40 The amount of historical data is too big to be accommodated on one line in I.
- 42 A.1288 has *Lu-ul-lu-ú*, S.115-26 *Lu-ul-lu-um*.
- 43 S.115-26: *mu-ut Ab bi x x [x]*, A.1288: *mu-ut x [x]*.
- 49 M.8566 here writes *NI-re-eb-ti[m]*, but in l. 51 *NE-r[e-eb-tim]*.
- 50–53 See Fig. 4.3.
- 54 Traces of the last line of A.1288 II support Ištār.
- 59 <<*ša-lim*>> is a mistake, not the beginning of a historical note; the line consists of the name only.
- Ep. 66 See above, §4.2, for Koliński's idea about this eponymy.
- Ep. 72 For Iddin-Aššur as eponym, see above, §3.4.
- Ep. 79–83 See *FM V*: 72–73 and notes 251–255.
- Ep. 81 I place here A.1288 IV, see §4.2; see for the history of this year *FM V*: 78–79.
- Ep. 83–89 See Fig. 4.1, with comments, and *FM V*: 80 for the sequence and distribution of records with these eponyms in the two palaces at Mari.
- Ep. 85 See for the history of this year *MARI* 6: 274 and *FM V*: 82–83.
- Ep. 86–88 See for this period of 'trois années mal connues', *MARI* 6: 274f. and *FM V*: 83–85, where the authors suggest to identify the year of Ilī-tillafī with that of Ahiyaya; see above §3.4. Since Eponym 87 was an Assyrian, his name should read Ilī-ellitī.
- 19' *FM V*: 84 note 64 states that according to Durand's collation the beginning of TA is very probable.
- 20' *FM V*: 86 note 77, reads *mu-t[a-...]* and considers a reading *mu-t[a-nu-um]*, as in l. 19', which would mean that the epidemic of the previous year continued (see its note 64). But this eponym, with the added qualification *munawwirum*, occurs also in TALON 1997: text 87: 71–72, see KOPPEN 1999–2000: 341. See *FM V*: 88–89 for the events of this year, which ended with a treaty between Šamšī-Adad and Daduša.
- Ep. 89 See *FM V*: 72 with note 255 and for the events of this year *FM V*: 73 and 78–79.
- 23' The mention of Mē-Turan (Tell Haddad), on the middle course of the Diyala, suggests a military confrontation with Eshnunna under Daduša, see *FM V*: 83.
- Ep. 90 The year of the conquest of Arrapha, related in Šamšī-Adad's victory stele, see GRAYSON 1987 RIMA 1: 63–64 A.0.39.1001; *FM V*: 90, note 111 suggests restoring this line as [*Arrapham^{ki} iṣbat*], and see its note 114 for the mention of Mē-Turan.
- Ep. 91 See *U&D*: 9 and above §3.1.2, and for this eventful and well documented year, CHARPIN 2004a in OBO 160/4: 166–189, *FM V*: 97–111 and the observations by CHARPIN 2004a: 163–166, commenting on the stele of Daduša. *FM V*: 97 note 183 suggests the following restoration: ¹⁷ *i-na [^da-sur]-ma-lik iš-me-^dda-gan da-am₇-da-a-[am ša dumu-^dIM lú ya₈-i-la-nim i-*

du-uk] ^{18'} *ù* ^dutu-ši-^dIM nu-u[r-ru-ga-am^{ki} qa-ab-ra-a^{ki} *ù* NG iṣ-ba-at] ^{19'} *l*ki-ib-ra-am lugal [NG, NP lugal NG] ^{20'} *l*ia-su-ub-^dIM lugal [ah-za-yi^{ki} NP lugal NG] ^{21'} *l*ia-su-ub-li-im luga[l NG, NP lugal NG (etc.)] ^{22'} 6(+x)¹ lugal-meš an-nu-tim¹ ik-s[u te-né-še-et ma-at qa-ab-ra-a^{ki}] ^{23'} [a-n]a da-du-sa iṣ-ri-ik ...]. This is convincing, although I believe there is not enough room in l. 18' to add a third city. I have taken over the verb in l. 22', *ik-s*[*ù*], «he put in fetters», although the last verb is not fully certain.

Ep. 92 *U&D*: 100 restores [*A-hi-ya-y*]a in S.24-2: 24' and inserts him as REL 193 before [*Awīliy*]a, which is difficult to accept, see above §3.4b. See for the history of this year, with revolts of the Turukkū and the Benjaminites, above §3.2.2 with *FM V*: 112–124. From its notes 299 and 300 (on p. 112) I have taken over the reading [*ibba*]/*lk*[*itū*] in l. 32', which provides the link between S.24-1 and A.1614a. See for this last fragment, above §3.2.2.

Ep. 96 See for eponyms with the name Ennam-Aššur, above §3.4.

7"–8" See above Fig. 4.2 and §3.3, with the comments on its restoration and historical explanation.

6. Historical observations

In what follows I refer to the eponymies as MEC + number on the basis of the reconstructed text presented above, occasionally adding their REL number; those I have changed (REL 180*–193*) are provided with an asterisk. Dates given are according the Upper Middle Chronology. AKL, the Assyrian King List, is quoted according to GRAYSON 1980–1983: 101–115.

The historical information provided by MEC for the period from MEC 9 to 60 and from 78 to 85, as far as preserved, is in many respects unique and difficult to check. This applies not only to the political events but also to the mentions of the birth of Šamšī-Adad I in MEC 23, of his succession of his father in MEC 38 (when he was fifteen years old), and of a solar eclipse in MEC 24. But there is no reason to doubt its data and, as mentioned above, its colophon inspires trust. In using what is stated about various rulers we can now take – if my reconstruction is accepted – MEC 95 = REL 197 as the year of Šamšī-Adad's death, equal to Hammurabi year 17/18 = 1776/5 BC.

6.1. Dating Šamšī-Adad's conquests of Assur and Ekallātum

AKL mentions Šamšī-Adad's conquest of Ekallātum during the eponymy of Ibni-Adad, and that of Assur three years later, during the eponymy of Ātamar-Ištar. MEC, as far as preserved, does not mention these victories and KEL G, which offers no historical data but is complete for the period in question, does not register these eponyms. This creates a serious problem. To identify the years of these conquests we can count back from the year of Šamšī-Adad's death, MEC 95. In Günbattu's reconstruction of KEL G, where this is no. 83, the conquest of Assur would have taken place in KEL G 51, during the eponymy of Sukkallum, and that of Ekallātum during KEL G 48, that of Inbi-Ištar. The reconstruction in *U&D* (see p. 25), which adds a few more years in the gap after KEL G 69, dates these conquests to KEL G 55 = MEC 62 = REL 165 (Idnāya) and KEL G 52 = MEC 59 = REL 162 (Amur-Aššur). The former eponymy is not preserved in MEC and for the latter MEC registers an activity of Šamšī-Adad of which only his name is preserved, but there is room for mentioning a conquest. Various explanations for the occurrence of two unattested year eponyms in AKL have been given. M. Liebig³⁸ assumed that the wars of Šamšī-Adad during these years would have disrupted communication between Assur and Kanesh, so that the eponyms of the conquest years and also the two intervening ones would not have been known and registered. But this is unlikely in a society preoccupied with timekeeping for financial reasons and, if it had happened, it is strange that a carefully drafted Assyrian eponym list, written only 35 years later, would not have repaired this failure: 'carefully drafted', as the colophon of KEL G (cf. DERCKSEN 2008: 71) states: 'Nisaba be praised! (MÍ.ZA) Aššur-ba[....] is the lady (*bēlat*, 'owner') of this tablet, Ennam-Sîn, son of Šulmu-ša-Aššur, wrote it (*iṣturšu*). And this happened 'in the presence of' (IGI/*mahar*) seven Assyrians (among them the last eponym registered), who probably are mentioned because they 'saw, checked' the text. This, of course, does not completely rule out an occasional mistake (see above footnote 14), but where the list contains eponyms found in contemporary archival texts (from Kültepe, Tell Leilan, Boğazköy, Tell Rimah, etc.) it proves to be correct (cf. *U&D*: fig. 2, pp. 21–22). The colophon is similar to that appended to KEL A,³⁹ where after the scribe a 'reader' (*muštassûm*) is mentioned, who must have been the person who had access to records that served as

³⁸ LIEBIG 2012.

³⁹ Read *iṣturšu* (as shown by the parallel in the colophon of KEL G), not *iṣtur* followed by *Šu-H*[*a*².....], as proposed in KEL A, where I took *İR-sà* = Warassa as the name of the scribe. The possibility that the colophon started with the name of a woman, whose servant the scribe is said to be (cf. the person designated as the 'mistress / female owner of this tablet', *bēlat ṭuppim annīm*), in the colophon of KEL G, referring to the person who had commissioned the list, cannot be excluded.

sources for the sequence of the eponyms and read (= dictated) them to the writer. This activity is similar to that of ‘the reader’ Limi-Dagan, mentioned in the colophon of MEC (M.7481: rev: 8’), but the latter’s task was more difficult, because he must have been responsible for correctly connecting eponymies with important events that took place during their term of office. While there is evidence that during the reign of Yasmah-Addu, at least at Mari, alongside year eponyms occasionally also year-names were used,⁴⁰ ‘native’ Assyrian rulers did not use year-names mentioning their exploits, while eponym dating remained en vogue in Northern Mesopotamia, also after the disappearance of Šamši-Adad’s dynasty. The ‘reader’ of MEC therefore had to use other sources, hardly building inscriptions, since they do not mention dates, but perhaps copies of triumphal inscriptions, such as the so-called Mardin Stele that reports Šamši-Adad’s conquest of Arrapha and Qabrā and mentions the month (the missing beginning might have mentioned the year) in which ‘the fortresses of the land of Arbela were captured’ (GRAYSON 1987 RIMA 1: A.0.39.1001).⁴¹ Although we have no idea what sources he might have had at his disposal, collecting all the historical information was no mean accomplishment, even if he worked in a royal chancery or a library, for MEC covers a long period and mentions many different rulers.

Another explanation for the absence of the two conquest eponyms was proposed by BLOCH (2014). He assumed that the scribe of AKL made mistakes, not simply graphic ones (by misreading or miswriting cuneiform signs), but due to «dicté intérieure», whereby he read his *Vorlage* and wrote while ‘rehearsing the text in his own head’. Bloch believed that the ‘conquest eponyms’ were Inbi-Ištar and Amur-Aššur, MEC 55⁴² = KEL G 48 and MEC 59 = KEL G 52, and he tried to explain how they became Ibni-Adad and Ātamar-Ištar. Notwithstanding the interesting examples he offered for various changes or mistakes in names, both in Old Assyrian and New Assyrian lists, his solution does not convince. There are no parallels for such drastic changes of names, simultaneously altering the divine name and its predicate, and for that to happen twice within the distance of a few years.⁴³ His proposal also forced him to make the distance between the two conquests, which was three years according to AKL, four years (from KEL G 48 to 52). And because the eponymy year MEC 55, which he preferred for the conquest of Ekallātum, was one marked by a military exploit of Ipiq-Adad, he suggested (in his footnote 30) that this king of Eshnunna may have forced Šamši-Adad to flee to Babylonia (see for this possibility, below). On pp. 202ff. he treated the question whether taking Amur-Aššur as the eponym of the conquest of Assur tallies with the information in AKL that this event took place 33 years before Šamši-Adad’s death in MEC 95. This depends on how one fills the gap between the obverse and reverse of KEL G, between Dadaya (68) and Aššur-mālik (79). It is 10 years in Günbatti’s edition of KEL G (69–78) but becomes 13 years in REL (179–191) (*U&D*: 95)–, which inserts here the eponyms of MEC D (in my reconstruction even one year more, MEC 76–90). This yields for Šamši-Adad’s reign 36 (or 35 if, as Bloch proposed, one omits the eponym of the accession year). While this figure is of course not absolutely certain (due to the uncertainty about the beginning of MEC D), it nevertheless shows that taking Amur-Aššur as the eponym of the conquest of Assur is not acceptable. This is not surprising, because this eponym seems to have been chosen not primarily on the basis of chronological data, but because his name was the most likely candidate (as Bloch has tried to prove) for having been transformed into Ātamar-Ištar.

⁴⁰ See CHARPIN 1992, with older literature. He mentions his discovery that ‘the year in which Šamši-Adad captured Mār-Addu and built the temple of Dagan’ can be equated with the eponymy of Aššur-mālik (MEC 91). In *FM* V: 93f. this victory is now dated to the end of the preceding eponymy year, that of Asqūdum (MEC 90 = REL 191). This year has only one line mentioning exploits of Šamši-Adad (and there is even a blank after his name!), which is surprising, since this was also the year of the conquest of Arrapha and Ninet / Nineveh. This indicates that the author of MEC for some years was rather selective in mentioning such events, but for others listed many exploits, e.g. for the next eponymy year (MEC 91) he even filled seven lines with historical data. The reason for these differences, perhaps conditioned by the importance attached to the events recorded or by the scribe’s ‘*Vorlage*’, remains unclear.

⁴¹ To which extent such inscriptions provided dates is not clear. The copy of an inscription of Šamši-Adad, published in WAETZOLDT 2000 (see also the interpretation and restorations proposed in *FM* V: 119), although damaged, does not seem to have mentioned a date (*FM* V: 119: ‘pas un fait tardif’).

⁴² [Editors] Veenhof following BIROT 1985: 230 read the eponym name MEC 55 in S.24-1: 13’ as *I-ni-i[b-Iš]tar*: ZIEGLER Ch. 3 in this volume reads the name as *In-b[i-išs-tā]r*. Ziegler writes ‘The reading *i-ni-i[b-Iš]DAR* proposed by BIROT 1985: 230 has to be corrected, IN is sure, B[I] probable. See already VEENHOF 2003: 48 n. 82, his proposal was confirmed by the publication of KEL G by GÜNBATTI 2008a.’

⁴³ See also the examples listed in my footnote 14, where always only one part of the name is changed or miswritten and where the most serious mistake (the omission of an eponym) is explainable by *aberratio oculi* in a series of three very similar names.

6.2. The reliability of AKL and MEC

The absence of the two ‘conquest eponyms’ from MEC and KEL G raises the question how reliable these texts are. In their attempts to solve the problems of the chronology, several authors have suggested that the scribes and ‘the reader’ of MEC may have made mistakes. But these are more or less contemporary records and the data they supply, as far as we can check them, seem to be correct and can be matched with historical information gleaned from the texts from Mari. The question therefore becomes whether AKL might be corrupt. It does contain some mistakes (notably in the filiation of some kings, as shown by comparison with some actual building inscriptions), but two complete misreadings within a few lines is too much to swallow. Moreover, names of the type Ātamar-DN, well-known from Babylonia and the Middle Assyrian period, do not occur among the thousands of Old Assyrian names we know. The question is also when the data of Šamšī-Adad and his exploits were inserted into the king list. If what AKL writes on Šamšī-Adad is based on an eponym chronicle (as suggested long ago by Landsberger) and his family history was presented by ‘at least some scribes working for him and his son’ (see BLOCH 2014: 193f., with notes 10 and 21), it is difficult to see how contemporary scribes, using contemporary sources, could come up with completely wrong names of eponyms. This suggests that the data on Šamšī-Adad, as also the use of the term Karduniaš for Babylonia indicates, were inserted by a later scribe, who knew that such royal exploits in Assyria had to be dated by year eponyms and, unable to trace them, simply picked two names.

Doubts about the correctness of AKL are also raised by what it writes about Amīnum, who figures as ruler no. XXVI (the first one after ‘the kings living in tents’) and as son of Ila-kabkabū and brother of Šamšī-Adad I. While his insertion there is explained by the curious way in which Šamšī-Adad’s ancestors were incorporated into AKL,⁴⁴ his genealogy is hard to accept, for MEC records 17 years of activity of him, from MEC 8 (when he conquered Šaduppūm, not necessarily at the very beginning of his career!) until MEC 25, which presumably mentions his death ([*mu-u*]t *A-mi-nim*), two years after the birth of Šamšī-Adad I. Moreover, for eponymy year MEC 37, 12 years later, it reports the conquest of Šuprum (a town a little north of Mari on the Euphrates) by Ila-kabkabū,⁴⁵ who according to the AKL would be Amīnum’s father. Even when Amīnum’s military activity had started at an early age, his ‘brother’ Šamšī-Adad would have been born ca. 35–40 years after him and his father would still wage war at least 45 years after his birth. In a recent note, Th. Janssen has dealt with Amīnum and suggested that he and Šamšī-Adad were half-brothers, both sons of Ila-kabkabū, born some 35 years apart and that Amīnum was old enough to have been Šamšī-Adad’s father, which may be the reason why he features as such in AKL.⁴⁶ Amīnum, according to Janssen, may have ruled independently, under the aegis of his father, and therefore his death (according to MEC 25 before his father died!) is simply mentioned, not by stating, as with other kings, who succeeded him on the throne (unfortunately the remainder of the historical note which accompanies MEC 25 is damaged). There may be truth in these observations, but it is difficult to prove them, and we must anyhow admit that the compiler of AKL was either not aware of or disguised the fact that Amīnum was not Šamšī-Adad’s father.

In my historical reconstruction the reliability of the data mentioned in MEC, a contemporary source, is taken for granted, but it has quite recently been questioned in an article of M. Guichard, where he edits the text of an early Old Babylonian treaty.⁴⁷ In it Ibal-pī-El of Eshnunna swears that he will not abandon Sīn-iddinam of Larsa and Sīn-kašid of Uruk by siding with Sabūm of Babylon and Ikūn-pī-Sīn of Nērebtum. This implies the contemporaneity of these kings and therefore, as shown by Guichard, the treaty must date from ca. 1844–1843 BC (MEC 26/27=REL 129/130), the very short period during which Sīn-iddinam and Sabūm were contemporaries (cf. CHARPIN 2004a in OBO 160/4: 385–390). But MEC suggests that during these years Ipiq-Adad (II) ruled Eshnunna (he is mentioned in MEC 38, 42, 52 and 55) and not Ibal-pī-El. It mentions, as far as preserved, no other ruler of Eshnunna until the eponymy of Aššur-imittī (REL 188*), when Daduša, a son of Ipiq-Adad, appears.⁴⁸ Guichard points to the fact that there is a gap in MEC, covering Eponyms nos. 27–31 in my reconstruction, and that Ipiq-Adad is not mentioned between Eponyms 17 and 38, which leaves room for political developments in Eshnunna. He suggests that the Ibal-pī-El of the treaty was either a temporary usurper or that Ipiq-Adad (II) was succeeded by a son, Ibal-pī-El (II), who was in turn was succeeded by Ipiq-Adad (III), a solution that is not impossible considering the very long reign assumed for Ipiq-Adad (II). But Guichard also raises the question whether the

⁴⁴ KRAUS 1965: esp. §§4–7.

⁴⁵ See FM V: 33f. with notes 30–32. The use of the verb *šabātum* strongly suggests that Šuprām means the city and not ‘the man of Šuprum’. Ila-kabkabū must have conquered it from Yaggid-Lim.

⁴⁶ JANSSEN 2015.

⁴⁷ GUICHARD 2014.

⁴⁸ He was the younger brother of Narām-Sīn, but the latter does not figure in the remains of MEC.

historical data dealing with various non-Assyrian kings from the Diyala area and Northern Mesopotamia, assembled by MEC's author ('reader'), were always correctly arranged and linked with eponyms of the Assyrian dating system. Noting that the text seems to compare and contrast Šamšī-Adad I with Ipiq-Adad, he asks whether ideological considerations might not have induced the author of MEC to manipulate and twist the historical reality. Since thus far hard data for this last, rather disturbing assumption are lacking and MEC leaves room for other solutions, the proposal of an additional Ibal-pī-El (II) and Ipiq-Adad (III) is more attractive. The treaty published by Guichard makes us aware of our limited knowledge of the early Old Babylonian period, also by introducing Ikūn-pī-Sîn as ruler of Nērebtum, a man thus far only known from a single year-name (from Tutub), which records his capture of Diniktum. The dates implied by the treaty allow Guichard to make him the successor of the above mentioned Sîn-abušu/abum, who in that case would have reigned 17+x years (x = the years between his accession and his first mention in MEC 8), which is entirely possible (see above).

6.3. MEC and the history of Eshnunna

The historical data in MEC are interesting for the history of Eshnunna, mostly the enemy and rival of Šamšī-Adad and his ancestors, but occasionally (in the later years of king Daduša) its ally. MEC covers the period between the accession of Ipiq-Adad II, in MEC 12 and the accession of Ibal-pī-El II, not mentioned, but known to have occurred during Hammurabi's 15th year, 1778 BC, corresponding to MEC 92/93. In the period between the death of Ipiq-Adad and this year the reigns of kings Narām-Sîn, Dannum-tāhāz, Iqīš-Tišpak, Daduša (brother of Narām-Sîn and son of Ipiq-Adad II, who first appears in MEC 86), and perhaps Ibbi-Sîn (with one year) have to be placed. We have for them damaged or partial king lists and at least 24 year-names, but the exact lengths of their reigns are not known.⁴⁹ In my reconstruction of MEC the temporal distance between Ipiq-Adad's accession and the accession of Ibal-pī-El II is 81 years.

For van Koppen and Lacambre, who base themselves on Günbatt's edition of KEL G, the last mention of Ipiq-Adad II in MEC 55 would be contemporary with Sîn-muballiṭ year 2 and Narām-Sîn would have succeeded his father in 1810 BC = Sîn-muballiṭ year 3.⁵⁰ For him and his successors there would be 33 or 32 years available until Ibal-pī-El II. This, however, is not acceptable if the Narām-Sîn, in whose time (*ina tarṣi*), according to AKL §12, Šamšī-Adad fled to Babylonia, some years before his conquest of Ekallātum (in 1811 BC), is the ruler of Eshnunna, as I claimed in KEL A p. 61.⁵¹ However, as M. Liebig observed in a letter to me, the Narām-Sîn, introduced in AKL without identifying him, only dates Šamšī-Adad's flight and is not necessarily the one responsible for it. The flight might have happened under his namesake, king of Assur, if we assume that the latter ruled [5]4 years, from REL 104 (MEC 1 = ca. 1869 BC) until REL 157 (MEC 54 = ca. 1816 BC).⁵² This does not agree with AKL, since Šamšī-Adad's flight is mentioned after the accession of Erišum II (AKL §11), but one might solve this by considering AKL §12 a flashback, to explain the sudden appearance of the ruler who ousted Erišum II. Nevertheless, a 54-year reign for Narām-Sîn of Assur is rather unlikely and if Narām-Sîn is after all the king of Eshnunna, it would be odd if he was only mentioned to date the event and not because he caused it. Moreover, as observed by W. NAHM in *NABU* 2017/98, the last mention of Ipiq-Adad II in MEC 55 is not the year of his death and probably mentions an exploit of his. If he died in MEC 56 or 57 he could have been succeeded by Narām-Sîn at the earliest in 1814 or 1813 BC and the latter's identification with the king mentioned in AKL would only be possible if we assume that Šamšī-Adad's stay in Babylonia was very short indeed. But we have no information on it and there are thus too many unknowns to solve this puzzle.

MEC suggests that the main opponent of Šamšī-Adad's 'dynasty' was Ipiq-Adad of Eshnunna. Confrontations between Amīnum and Ipiq-Adad are already mentioned for MEC 13–15 and the latter may have proved to be the stronger one, since MEC 16–17 only mention his successes. If Ila-kabkabū already felt the pressure of Eshnunna, Ipiq-Adad's defeat by the king of Elam (MEC 38), in the very year when Šamšī-Adad succeeded his father, may have brought some relief. Ipiq-Adad's conquest of Arrapha, in MEC 42, must have posed a serious threat, if Šamšī-Adad by then reigned from Ekallātum (Daduša, in his stele, col. X: 9, still calls him 'king of Ekallātum'). MEC 52 and 53 report victories first by Ipiq-Adad and

⁴⁹ VAN KOPPEN/LACAMBRE 2008–2009: esp. 174–175; D. CHARPIN does not mention Ibbi-Sîn in OBO 160/4: 389.

⁵⁰ KOPPEN/LACAMBRE 2008–2009: 174f.

⁵¹ Accepted by CHARPIN 2004a: 131, with note 568.

⁵² These dates are given in *U&D*, based on a distance of 197 years between the accession of Erišum I and the death of Šamšī-Adad I. If, as the Assyrian *Distanzangaben* claim, this was 199 years, the dates become 2 years higher, which would create a little more room within an anyhow very tight chronological scheme.

next by Šamšī-Adad, and MEC 55 again by Ipiq-Adad. Although the damaged text does not allow the identification of their opponents and goals, it suggests a power struggle between the two a few years before Šamšī-Adad's (re-)capture of Ekallātum, in 1811 BC. It is therefore tempting to assume that the action of Ipiq-Adad recorded for MEC 55 (ca. 1815/14 BC in my reconstruction), as was suggested by Bloch,⁵³ led to Šamšī-Adad's flight to Babylonia.⁵⁴ This could, as pointed out above, have taken place at the very end of the reign of Narām-Sîn of Assur, but it is of course also possible that he, after a rule of 44 years, had been duly succeeded by Erišum II, in ca. 1825 BC (ca. MEC 44/45, years for which MEC does not register a historical event). In that case AKL §12 must after all refer to Naram-Sîn of Eshnunna, otherwise Šamšī-Adad would have fled to Babylonia even before he had succeeded his father (in MEC 38). Whatever the solution, it does not have a serious effect on our chronological reconstruction, because neither of the two Naram-Sîns is mentioned in the historical notes in MEC as far as they are preserved.

The long reign of Ipiq-Adad of Eshnunna, at least 45 years, starting in eponymy MEC 12, is surprising. That these dates are acceptable is shown by what we know of Sîn-abum/abušu, a ruler in the area of the lower Diyala,⁵⁵ the date of whose accession is unknown. MEC 9 mentions a military success of his and what may be one of his year-names mentions the death of Amīnum, a fact most probably recorded in MEC 25, 17 years after Sîn-abum's first mention in MEC. This implies a long reign, which is supported by the large number of his preserved year-names.⁵⁶ Their disappearance from Nērebtum, Šaduppûm and Tutub suggests, as pointed out by De Boer, that he was later removed by his overlord Ipiq-Adad.⁵⁷ It is doubtful whether the historical notes in MEC 47 and 49, mentioning the capture and recapture of Nērebtum, have to do with it, for this is more than 20 years later.

6.4. The later years of Šamšī-Adad I

After MEC 58–60, which mention various actions of Šamšī-Adad, there follow 16 years for which we have only the names of the eponyms (from KEL G). MEC resumes with fragment D (S.24-3), treated above in §3.2.1. If we accept the reading and identification of MEC 81 as Haya-mālik, who marks the year of Šamšī-Adad's conquest of Mari, the mention of military successes, two years earlier (MEC 79), against a coalition of kings headed by(?) Yahdun-Lim, can be seen as the culmination of a long power struggle between him and Šamšī-Adad.⁵⁸ After recording more exploits of Šamšī-Adad (MEC 82–83), his son Išme-Dagan appears as a victor in MEC 84 (ca. 1789 BC), but damage to the text deprives us of information about their opponents. There is no need to comment on the following years, which are well documented in the Mari archives and whose historical data are found in *FM* V, where also several restorations in the broken text of MEC are proposed (see above, §5.2, the comments on the reconstructed text, and §3.2.2, on the link between A.1614a and b, which affects MEC 92).

After MEC 92 the text breaks off and the scribe must have made a selection from the many historical facts to be recorded in MEC 93–96, for there is only room for a limited number of second (and third) lines to record them. The surprising listing of Aššur-emūqī as the last eponym and the mention of a military exploit in this year are discussed above, in §3.3.

⁵³ See BLOCH 2014, as mentioned above in §6.1. W. NAHM has suggested in NABU 2017/98 that MEC 55 could have mentioned the conquest of Rapiqum, which occurred late in Ipiq-Adad's reign.

⁵⁴ This would be even more understandable if Akkade was the seat of his 'dynasty', as claimed by CHARPIN 2004: §1.1.

⁵⁵ See for him VAN KOPPEN 2011: 512f., CHARPIN 2004a: 99f., and the dissertation of R. DE BOER, *Amorites in the Early Old Babylonian Period* (defended at Leiden in May 2014): 222–228.

⁵⁶ One of his year-names mentions the death of king Ibal-pī-El (I) of Ešnunna, which agrees with the fact that the latter's son Ipiq-Adad II succeeded him four years after the first mention of Sîn-abum.

⁵⁷ DE BOER 2014.

⁵⁸ See *FM* V: 38–47, where it is assumed that Yahdun-Lim's reign ended in 1794 BC.

7. Translation of the Mari Eponym Chronicle

? before names only known from KEL indicates that the presence of a historical note is uncertain. I render the names of these Assyrian eponyms according to their standard form in their own dialect (e.g. Suen-išme'anni, Ilī-illitī, Iddin-ahum, but we know that Suen in the later phase of OA was pronounced Sîn). See for the partially restored historical data also the remarks on the eponyms in §5.2.

	From the accession of Narām-Suen, the regent, our lord, after the year-the eponym had been installed:	57	<i>in</i> Bēlānum: a clod of earth [.....]
1	Šu-Suen	58	<i>in</i> Sukkallum: Šamši-Adad [.....]
2	Aššur-mālik	59	<i>in</i> Amur-Aššur: Šamši-Adad [.....]
3	Aššur-imitti	60	<i>in</i> Aššur-nišū: Šamši-Adad
4	Ennam-Suen	61	? Munawwirum
5	<i>in</i> Akkutum: . [.....]	62	? Idnaya
6	Mašiam-ilī	63	? Dadaya
7	Iddin-ahum	64	? Puzur-Nirah
8	<i>in</i> Samānum: Amīnum conquered Šaduppūm	65	? Abi'aya
9	<i>in</i> Ilī-ennam: Sîn-abum conquered the country of Šidki?	66	? Edinum
10	Ennamānum	67	? Aššur-taklāku
11	Ennam-Aššur	68	? Isim-Suen
12	<i>in</i> Ennam-Suen: Ipiq-Adad ascended the paternal throne	69	? Adad-bāni
13	<i>in</i> Hanna-nārum: Amīnum defeated Ipiq-Adad	70	? Abī-šagiš
14	<i>in</i> Dadiya: [.....]	71	? Tāb-šilli-Aššur
15	<i>in</i> Kapatīya: Ipiq-Adad defeated Amīnum	72	? Iddin-Aššur
16	<i>in</i> Išme-Aššur: Ipiq-Adad conquered the ziqqurrat	73	? Namiya
17	<i>in</i> Aššur-mūtappil: Ipiq-Adad [.....]	74	? Ahu-šarri
18	Šu-Nirah	75	? Dadaya
19	<i>in</i> Iddin-abum: Suen-[.....]	76	<i>in</i> ? Ali-mālik
20	Ilī-dān	77	[.....][.....]
21	Aššur-imitti	78	<i>in</i> [.....]-Aššur: Šamši-Adad [.....]
22	Bušāya	79	<i>in</i> Atānum: Šamši-Adad [.....] 12 kings [.....] Yahdun-Lim [.....] their kings he [.....]
23	<i>in</i> Dadiya: king Šamši-Adad was born	80	<i>in</i> Aššur-taklāku: Šamši-Adad [.....]
24	<i>in</i> Puzur-Ištar: darkening of the sun	81	<i>in</i> Haya-mālik: Šamši-Adad [.....] the king conquered the land of Mardamānum, Šerwunum and Haburātum. He then built Dūr-Addu and Dūr-Šamši-Adad in the territory of Yamhad. On his return he defeated [.....] and [conquered?] Rapiqum [.....] (<i>from</i> A.1288 IV)
25	<i>in</i> Isāya: death of Amīnum	82	<i>in</i> Šalim-Aššur: of Šamši-Adad, the land [.....]
26	<i>in</i> ? Abu-šalim: [.....] of Adad?	83	<i>in</i> Sîn-muballit: Šamši-Adad ... the land of [.....]
27	? Aššur-rē'i	84	<i>in</i> Rīš-Šamaš: Išme-Dagan defeated [.....]
28	? Tāb-Aššur	85	<i>in</i> Ibni-Adad: Šamši-Adad [.....] the land of [.....] father
29	? Šu-Rama	86	<i>in</i> Aššur-imitti: Šamši-Adad defeated [...] he brought back, the land of [.....] Mē-Turan, the land of [.....] Daduša
30	? Suen-išme'anni	87	<i>in</i> Ilī-illitī: [.....]
31	? Aššur-mālik	88	<i>in</i> Rigmānum: an epidemic [.....]
32	<i>in</i> Dan-Ea: conquest of Hupšum	89	<i>in</i> Ikūn-pīya munawwirum: [.....] defeat of [.....] and Šamši-Adad [.....] Mē-Turan [.....]to Daduša [.....]
33	<i>in</i> Ennam-Suen: flood from a far-away country	90	<i>in</i> Asqudum: Šamši-Adad [.....]
34	? Aššur-balā'i	91	<i>in</i> Aššur-mālik: Išme-Dagan defeated and Šamši-Adad [conquered] Nurrugum and Kipram, king of, Yašūb- Addu, king of [Ahāzum], Yašub-Lim, king of [.....], these 6[+x] kings he [.....], these 6[+x] kings he bound, to Daduša [he gave the people of Qabra].
35	? Ennam-Aššur	92	<i>in</i> Awīliya: the Turukkū revolted [and]lān defeated the Turukkū [in]. Yasmah-Addu defeated the Benjaminates and gained control of the whole of the Euphrates Valley.
36	? Itūr-Aššur	93	<i>in</i> Nimer-Sîn: [.....] the town of [.....] to [.....]
37	<i>in</i> Šu-Bēlī: Ila-kabkabū conquered Šubrum	94	<i>in</i> Adad-bāni: [.....]
38	<i>in</i> Šarrum-Adad: the man of Elam defeated Ipiq-Adad and Šamši-Adad succeeded his father	95	<i>in</i> Tāb-šilli-Aššur: [.....]
39	Šu-Labān	96	<i>in</i> Ennam-Aššur: [.....]
40	<i>in</i> Aššur-imitti: Lullūm defeated the king at Lazapatum	97	<i>in</i> Aššur-emūqī: [.....] and [.....] 4 [.....] he de[feated] ?]
41	<i>in</i> Dadāya: death of Abbi-[.....]		
42	<i>in</i> Dadāya II: Ipiq-Adad conquered Arrapha		
43	<i>in</i> Ahi-šalim: conquest of G/Qa-[.....]		
44	Ušur-ša-Ištar		
45	Kat(t)aya		
46	<i>in</i> Šu-Suen: [.....]		
47	<i>in</i> Abu-šalim: conquest of of Nērebtum		
48	<i>in</i> Šudaya: [.....]		
49	<i>in</i> Šu-Dādum: conquest of Nērebtum		
50	<i>in</i> Aššur-tukultī: defeat of Unine'ayum [by] and defeat of [...] Mut-Ya.... by [.....]		
51	<i>in</i> Puzur-Ištar: Šamši-Adad		
52	<i>in</i> Ātanah: Ipiq-Adad defeated [...] and the land of [...]		
53	<i>in</i> Erišum: Šamši-Adad defeated [.....] in Dūr-[.....]		
54	Aššur-ennam		
55	<i>in</i> Inib-Ištar: Ipiq-Adad [.....]		
56	Aššur-bēl-malki(m)		

In all 60+10+[20+7²] years until the defeat at the gate of
Saggarātum.

Written by Habdu-mālik, reader: Līmi-Dagan.

Abbreviations

AKL	Assyrian King List, quoted after GRAYSON 1980–1983
FM V	CHARPIN/ZIEGLER 2003
FM VI	CHARPIN/DURAND 2002
KEL A	Kanesh Eponym List A, published in VEENHOF 2003, with some additions and corrections in VEENHOF 2007
KEL G	Kanesh Eponym List G, published in GÜNBATTI 2008
OBO 160/4	CHARPIN/EDZARD/STOL 2004
RIMA 1	GRAYSON 1987
U&D	BARJAMOVIC/HERTEL/LARSEN 2012

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5. Assyrian eponym lists and the solar eclipse in the Mari Eponym Chronicle

MICHAEL ROAF

The fragmentary Mari Eponym Chronicle contains a possible reference to a solar eclipse whose identification could prove to be the key to solving the problem of the absolute chronology of the first half of the second millennium BC in Mesopotamia. The chronological relationship between this event and the death of Šamšī-Adad and thus with the chronology of Babylonia is provided by reconstructing the sequence of eponyms in the Mari Eponym Chronicle using the information of Kültepe Eponym List G. In this chapter two possible reconstructions (with variants) of the Mari Eponym Chronicle are investigated and their implications for dating the eclipse according to the High, Middle and Low Chronologies are assessed. It is unlikely that a partial eclipse at sunrise or at sunset would have been thought worthy of inclusion in the Mari Eponym Chronicle and only the Lower Middle Chronology gives a date for the eclipse that is close to the total solar eclipse of the 24th June 1833 BC, the most impressive of eclipses visible in Mesopotamia in the first quarter of the second millennium BC. The reconstructions of the Assyrian Eponym List are not exactly compatible with the 1833 eclipse and there remain doubts that the mention of an eclipse can solve the outstanding chronological problem.

1. Introduction

In recent years, considerable advances have been made in reconstructing the Assyrian eponym sequence for some 250 years starting at the beginning of the reign of Erišum I (*U&D*: 1–40 and 89–125).¹ The main sources are the Kültepe Eponym Lists (KEL A to G) found in the lower town at Kültepe, ancient Kaneš, in central Turkey, and fragments of various versions of the Mari Eponym Chronicle (MEC) found in Tell Hariri, ancient Mari, in Syria, supplemented by texts from Turkey, Syria and Iraq that record the names of eponyms. Some scholars think that the identification of the solar eclipse mentioned in the eponymy of Puzur-Ištar in the Mari Eponym Chronicle could, if the association with that eponym is accurate and if the Assyrian Eponym List can be correctly reconstructed, resolve the issue of the absolute chronology of the first half of the second millennium BC.

2. The possible eclipse in the Mari Eponym Chronicle

The possible solar eclipse is described as a ‘darkening of the Sun’ (*nahdur šamši*) (MEC A.1288 I 23’–25’).² *V MEC* (§4.1, §5) restored the passage as follows:

23'	[i-na Da-di]-ia' LUGAL ^d UTU ^{šī} - ^d IM	[In Dad]iya: king Šamšī-Adad
24'	<u>wa-li-id</u>	<u>was born</u>
25'	[i-na Puzur ₄ -Išta]r na-ah-du-ur ^d UTU	[In Puzur-Išta]r: darkening of the Sun
26'	[i-na I-sà-(i)a mu-u]t A-mi-nim	[In Isāya] death of Amīnum.

¹ I am grateful to Gojko Barjamovic, Teije de Jong, Guido Kryszat, Werner Nahm, and Klaas Veenhof for commenting on earlier versions of this chapter. Abbreviations used in this chapter are listed at the end before the Bibliography.

² It is widely accepted that the ‘darkening of the Sun’ refers to a solar eclipse, and it is difficult to think of a phenomenon other than a solar eclipse that could be intended in this entry of the MEC. The word *na’duru* / *nanduru* (‘became dark’) is less common than *antallūm* as a term for an eclipse. MICHEL/ROCHER (1997–2000: 113 n. 16) discussed these words and noted that according to ROCHBERG-HALTON (1988: 48) in *Enūma Anu Enlil* there is a tendency for *nanduru* to be used to indicate the physical occurrence of an eclipse and *antallūm* its ominous character. The N-stem of *adāru* can also be used for other forms of darkening of the Sun as for example when ‘the face of the Sun was darkened by the dust (of battle)’ (*i-na tur-bu-’ti-šū-nu na-’-du-ru* IGI ^dUTU-^{šī}) in an inscription of Nebuchadnezzar I (FRAME 1995: B.2.4.11 i 31).

If this is correct, the solar eclipse (darkening of the Sun) is dated to the eponym year of Puzur-Ištar and is not associated with any other event, neither the birth of Šamši-Adad nor the death of Amīnum.³ This might suggest that the eclipse recorded in that year was not considered an omen whether good or bad,⁴ unless it referred in some way both to the significant event in the previous year and to that in the following year.

When I looked at this question in my 2012 article (ROAF 2012: 160–168), I realised that by far the most impressive eclipse of the first quarter of the second millennium BC in northern Mesopotamia took place on the 24th of June 1833 BC (Gregorian 8 June 1833).⁵ The only other mention of an eclipse in an Assyrian eponym list occurred at the same time of the year in the third month of the Babylonian year (i.e. in the summer) of the eponymy of Bur-Sagalê.⁶ The identification of this eclipse with that of 15 June 763 BC (Gregorian 7 June 763) is not disputed. It took place in the middle of the morning and was of magnitude 0.99 in Nineveh and total some 50 km to the north. The 1833 BC eclipse occurred in summer when the Sun was high in the sky in the middle of the afternoon and was total from 10 km south of Assur to 95 km north of Babylon, and thus probably total in the region where the dynasty of Šamši-Adad came from and where the events recorded in the early part of the Mari Eponym Chronicle took place (see below).

However, while acknowledging that the 1833 BC eclipse was the most spectacular eclipse of the first quarter of the second millennium BC, many scholars, because they were constrained by their understanding of the Assyrian Eponym List and of the Venus Tablets of Ammi-šaduqa or for other reasons⁷ decided that the 1833 BC eclipse was impossible; and therefore resorted to identifying the eclipse in the MEC either with one that occurred close to sunrise or sunset or with one of lesser magnitude that probably would not have been observed, since solar eclipses with a magnitude of less than 0.95 are unlikely to have been noticed.⁸ In 2012 I did not consider in detail the possibility that the eclipse mentioned occurred close to sunrise or sunset.⁹ In my opinion it is very unlikely that the eclipse recorded in the MEC occurred close to sunrise or to sunset (see below for detailed arguments), but it is *possible* that such an eclipse *might* have been observed: indeed about 1200 years later, a professional Assyrian astrologer did observe a very partial solar eclipse at sunrise.¹⁰

It has been suggested that a tablet with the text of the MEC was brought to Mari from Šubat-Enlil after it was captured by Zimri-Lim (*FMV*: 168 and n. 658) ‘but this is difficult to prove’ (*V MEC* §1 n. 6) and it is more likely that the colophon and the final entries were composed in Mari (*V MEC* §3.3).¹¹ The events mentioned earlier in the MEC include not only those involving Šamši-Adad and his predecessors but also those involving the rulers of Ešnunna and other cities in the Diyala region where the eponym dating system was not in use.¹² Various suggestions have been made about the original capital of Šamši-Adad’s family including Terqa, Šubat-Enlil, Ekallatum, the Diyala region (perhaps Zaralulu), and Agade

³ ZIEGLER (Ch. 3 in this volume) gives an almost identical transliteration and restoration. Barjamovic, Hertel, and Larsen (*U&D*: 32 n. 104) suggested that the ‘darkening of the Sun’ took place in the same eponym year as the death of Amīnum, either in the eponym year of Dadiya or that of Puzur-Ištar. In Veenhof’s transliteration, however, there are horizontal rulings under *wa-li-id* and *-ur*^dUTU, and VEENHOF (2015: 11) explicitly noted ‘the presence of (partially preserved) rulings between the eponyms’, suggesting that these indicate a change of eponyms. Furthermore, the indent before *wa-li-id* also shows that a new eponym follows in line 25’.

⁴ This may also be indicated by it being described with the word *na’dur*. See footnote 2.

⁵ At Mari and in the Diyala region this eclipse would have been total and would have lasted more than five minutes.

⁶ This eclipse, written AN.MI (= *attallū*) appears to have been recorded because it was a memorable occurrence and not because it occurred in a particularly significant year, as the only other event included in the Assyrian Eponym Chronicle at this time was a revolt in Assur that was mentioned both in the year before the eclipse and in the next year (MILLARD 1994: 41(B1), 58).

⁷ MICHEL/ROCHER (1997–2000) considered the possibility of the 1833 BC eclipse but rejected it because of the then current estimates of the radiocarbon dating of the Sarıkaya Palace in Acemhöyük. A new (but still incorrect) revision of the radiocarbon dating allowed Cécile MICHEL (2002) to reinstate the 1833 BC eclipse as the most probable.

⁸ Or even less than 0.98; for details and discussion see ROAF 2012: 160–168 with reference to MULLER/STEPHENSON 1975.

⁹ This deficiency was criticised by COLE (2014: n. 50 on p. 5).

¹⁰ ‘A certain Akkullanu has written: “The sun made an eclipse of two fingers at the sunrise (^dUTU / *ina na-pa-ḫi-šu ma-a / i-ba-āš-ši a-ki* 02 ŠU.SI AN.MI *i-sa-kan*). There is no apotropaic ritual against it. It is not like a lunar eclipse. If you say, I’ll write down the relevant interpretation and send it to you.”’ (PARPOLA 1993: 113, SAA 10, 148: 3–14). Peter HUBER (2014: 73, 82) identified this eclipse with that of 27 May 669 BC and has calculated that this may have been observed at a magnitude of less than 0.2.

¹¹ Guido Kryszat has informed me that the spelling of the names of the eponyms in the MEC does not follow the typical orthography used in Old Assyrian texts from Assur and Kültepe and therefore it is unlikely that the text was composed in Assur.

¹² Klaas Veenhof has pointed out to me that rulers other than those of the dynasty of Šamši-Adad may only have been mentioned because of their connection with Šamši-Adad and his dynasty (see *V MEC* §6.3): the text is so fragmentary that such links might have been recorded in the missing passages.

(for details see CHARPIN 2004: 148–149). The most plausible of these is Agade.¹³ The observation of the eclipse could therefore have taken place neither at Mari nor at Assur but somewhere further to the south.

3. Possible solar eclipses

Julian date BC	Assur			Mari	Babylon			comment
	magnitude	range	probability		magnitude	range	probability	
18 Feb. 1919	0.893			0.875	0.853			at sunrise
24 June 1917	0.915	35–40	0.14	0.957	0.964	41–46	0.16	annular
7 Dec. 1916	0.838	57–80	0.13	0.795	0.790			total
13 March 1902	0.852			0.857	0.945	45–52	0.21	at sunset
3 May 1896	0.84			0.835	0.78			at sunrise
9 Jan. 1861	0.848	06–34	0.22	0.858	0.775	6–28	0.089	total
15 May 1859	0.927	5–39	0.36	0.968	1.009	15–53	0.75	total
27 Oct. 1858	0.846	22–30	0.10	0.878	0.893	27–36	0.17	annular
5 Aug. 1845	no eclipse			0.735	no eclipse			after sunset
24 March 1838	0.933			0.914	0.880			at sunrise
24 June 1833	0.997	28–57	0.76	1.028	0.972	18–69	0.96	total eclipse
14 June 1832	0.845			0.848	0.789			at sunrise
5 June 1823	0.510			0.509	0.578			at sunrise
16 Aug. 1808	0.904			0.884	0.969	37–61	0.64	annular eclipse
8 Sept. 1791	0.921	34–38	0.11	0.942	0.950	41–45	0.13	annular eclipse, at sunset
17 July 1778	0.977	41–57	0.44	no eclipse	1.002	44–51	0.22	before sunrise
7 July 1769	1.006	43–52	0.27	no eclipse	no eclipse			before sunrise
20 Dec. 1768	0.954	35–45	0.30	0.997	0.992	41–50	0.29	after sunrise in Assur, at sunrise in Mari and Babylon
25 April 1765	0.975	35–44	0.27	0.940	0.894			at sunset, after sunset at Babylon
8 Oct. 1764	0.849	30–33	0.060	0.884	0.889	36–38	0.056	annular
11 Dec. 1759	0.292			no eclipse	0.22			before sunrise
18 Sept. 1754	0.825			0.800	0.738			after sunrise in Assur, at sunrise in Mari and Babylon
5 March 1744	0.858	23–35	0.20	0.867	0.782	11–29	0.10	annual-total
22 Dec. 1741	no eclipse	32–42	0.28	0.951	no eclipse	26–37	0.20	after sunset
10 Oct. 1737	0.935			0.919	0.901			annular eclipse, at sunset

Fig. 5.1. Solar eclipses visible in Assur (Lat. 35.46°N Long. 43.26°E), Mari (Lat. 34.55°N Long. 40.89°E) and Babylon (Lat. 32.54°N Long. 44.42°E) between 1860 and 1730 BC (based on ROAF 2012, figs. 3 and 4). The columns give the Julian date, the magnitudes at Assur, Mari, and Babylon assuming that the estimate of ΔT is correct, the longitudinal range in °E for which the calculated magnitude of the eclipse is greater than 0.95 at the latitudes of Assur and Babylon, and the estimated probability that the eclipse (with magnitude > 0.95) would have been visible at Assur and Babylon using an estimated standard error of ΔT of 49 minutes, corresponding to 12.25°. No allowance has been made for possible variations in latitude. Information about eclipses in this table is taken from the NASA Eclipse Web Site (ESPENAK/MEEUS 2009) and is courtesy of Fred Espenak, NASA/Goddard Space Flight Center.

The solar eclipses that might have been visible at Assur, Mari, and Babylon between 1920 and 1730 BC are listed in Fig. 5.1. The magnitudes of the eclipses would have been different had the eclipse been viewed from a different site such as Ešnunna.¹⁴ Because of cloud cover not all of these eclipses would actually have been observed: those occurring in the winter are less likely to have been seen than those occurring in summer. The eclipses that occurred near sunrise or sunset

¹³ ZIEGLER (2021: 123) supports the identification of Šamši-Adad's home with Agade and notes the similarity of Šamši-Adad's calendar to that of Ešnunna. WESTENHOLZ (1999: 31–34) and READE (2002: 262–269) have convincingly suggested that Agade was located near Samarra.

¹⁴ The paths of solar eclipses are very irregular and there are differences in the magnitudes of the eclipses between Babylon and Ešnunna: the reader should, however, check on a solar eclipse website such as ESPENAK/MEEUS 2009 for a particular eclipse before assuming that there is little difference.

would have been beneath the horizon over about half of the range, but at the same time they could have been observed at a smaller magnitude.¹⁵ In most cases, the eclipse would have been observed for only part of the time of its theoretical duration, being below the horizon for part of the time and being visible for about ten minutes before reaching an altitude of ca. 2° above the horizon (4 solar diameters) and therefore being too bright to be observed.¹⁶ Given the uncertainty in estimating clock time (ΔT) in the early second millennium, the maximum magnitude of some of those listed as being before sunrise or after sunset might have occurred in the daytime and vice versa.

An example may clarify the situation. DE JONG (2012–2013: 156–157, also Ch. 1 in this volume) suggested that the MEC solar eclipse was the annular eclipse of 24 March 1838 BC. According to the NASA solar eclipse website accepting the estimated change in clock time, the eclipse started below the horizon both at Assur and at Ešnunna (Fig. 5.2).¹⁷ As the Sun rose above the horizon, the magnitude of the eclipse was 0.56 at Assur and 0.48 at Ešnunna; ten minutes later, when the Sun reached 2° above the horizon and the eclipse could no longer be observed, the magnitudes were 0.69 and 0.63.

Event	Assur			Ešnunna		
	Magnitude	Time (UT)	Altitude	Magnitude	Time (UT)	Altitude
Start of partial eclipse		02:55	− 08.3°		02:50	− 07.6°
Sunrise	0.56	03:27	− 0.1°	0.48	03:20	− 0.1°
Maximum magnitude	0.93	04:02	05.6°	0.90	03:59	06.6°
End of partial eclipse		05:20	20.8°		05:18	22.3°

Fig. 5.2. Details of the eclipse of 24 March 1838 BC (Gregorian 8 March 1838) at Assur (35.46°N 43.26°E) and at Ešnunna (33.49°N 44.74°E). Negative values of the altitude indicate that the centre of the Sun was below the horizon.

Such an eclipse is likely to have passed unnoticed. Since the magnitude was less than 0.95, it would not have been observed when the Sun had risen more than 2° above the horizon and the eclipse would only have been recognisable in the ten minutes after sunrise when it was possible to look at the Sun without being dazzled. An observer would only have noticed it, if he had got up before dawn, been outside looking in the direction of the rising Sun,¹⁸ and realised that there was something peculiar about the Sun – something that was similar to what happens to the Moon during a lunar eclipse.¹⁹ All of this would have had to have taken place within 10 minutes. Furthermore it would not have been enough for it to have been observed by a single spectator. For it to have been considered worthy of inclusion in the MEC, it would have had to have been a notable event and a considerable number of people would have had to have been impressed by it.²⁰ The magnitude of the 1838 eclipse was more than 0.5 for about 80 minutes and, allowing for the possible error in clock time of ca. 50 minutes, the probability that the magnitude was greater than 0.5 in the ten minutes after sunrise is about 60%. Furthermore, in early March there is a 30% chance that clouds would have obscured the eclipse.²¹ Combined there is less than a 50% chance of the eclipse having been visible.

¹⁵ In Fig. 5.1 in the column labelled comment, ‘at sunrise’ and ‘at sunset’ mean that the partial eclipse began before sunrise or lasted until after sunset, ‘before sunrise’ and ‘after sunset’ mean that its maximum occurred before sunrise or after sunset.

¹⁶ I am grateful to Teije de Jong for information about the conditions under which a partial eclipse could be observed close to the horizon. See also HUBER 2014: 80–81 for illustrations of the appearance of the 1838 eclipse, which for clarity unrealistically represent the surrounding sky solid black, even when the Sun is above the horizon.

¹⁷ Similar figures are given by DE JONG (2012–2013: Table 6 and Fig. 1.2 in this volume) and by HUBER (2014: 71–72).

¹⁸ Knowing that there might be a solar eclipse on a particular day, increases greatly the chance that a partial eclipse would be noticed at sunrise or sunset, but there is no evidence that it was possible to predict possible solar eclipses in the Old Babylonian period, other than they took place at the New Moon.

¹⁹ The Neo-Assyrian astrologer Akkullanu wrote that the partial solar eclipse that he saw at sunrise was ‘not like a lunar eclipse’, but he may have been referring to its ominous consequences (PARPOLA 1993: 113, SAA 10, 148: 3–14). See above footnote 10.

²⁰ SALLABERGER/SCHRAKAMP (2015: 9–10) offer similar arguments against a partial eclipse at sunrise or at sunset being worthy of inclusion in the Mari Eponym Chronicle.

²¹ For average cloud cover see, for example, <https://weatherspark.com/>. In the 7th century BC an astrologer wrote to the Assyrian king as follows: ‘there are clouds everywhere; we do not know whether the eclipse took place or not. Let the lord of kings write to Assur and all the cities, to Babylon, to Nippur, to Uruk and Borsippa; maybe they observed it in these cities’ (SAA 10, 114: 5–10 cited by FINCKE 2021: 109). This letter was probably written in the first month of the year, thus at about the same time of year as the 1838 BC eclipse.

On the other hand, there is only a 5% chance of cloud cover on a summer afternoon in early June when the 1833 eclipse took place and the eclipse would have been very noticeable even if there were clouds, because the sky would have become increasingly dark as the air temperature dropped, the birds stopped singing, the sky became black, and the stars came out where they were not obscured by clouds. A partial eclipse at sunrise or sunset is a purely visual event and lacks the associated phenomena which make a major solar eclipse such a memorable event. Combining the uncertainties of cloud cover and of clock time, there is less than a two-thirds chance that the 1838 BC eclipse could have been noticed. Given these uncertainties and the comparatively unimpressive nature of an eclipse at sunrise, it is most unlikely that the eclipse of 24 March 1838 BC would have been mentioned in the MEC. The same arguments apply to other partial eclipses that took place at sunset or sunrise.²²

4. The Revised Eponym List and the Mari Eponym Chronicle

Barjamovic, Hertel, and Larsen (*U&D*: 91–97) published a list of more than 250 Assyrian *limmu* officials whose names were given to individual years from the reign of Erišum I to the reign of Išme-Dagan, the son of Šamšī-Adad. In this list the eponym year of Puzur-Ištar was REL 127 (Revised Eponym List) and the death of Šamšī-Adad took place in the autumn or winter either at the end of REL 197 or at the beginning of REL 198 in Hammurabi's 18th year.²³ This year according to the UMC was 1775 BC and according to the LMC was 1767 BC (Fig. 5.3).

Months	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	June	July	Aug	Sept	Oct	Nov	Dec	Jan	Feb	Mar	Apr		
REL earlier	REL 196			REL 197												REL 198					
REL Š-A	REL 197												REL 198								
Assyria Š-A	Ṭab-šilli-Aššur												Ennam-Aššur								
Babylonian date	Hammurabi 17						Hammurabi 18												19		
HC	1832			1831												1830					
UMC	1776			1775												1774					
LMC	1768			1767												1766					
SC	1712			1711												1710					
Death of Š-A																					
Months	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	June	July	Aug	Sept	Oct	Nov	Dec	Jan	Feb	Mar	Apr		

Fig. 5.3. Correlation of the date of the death of Šamšī-Adad (Š-A) with dates in the Julian, Assyrian and Babylonian calendars and with the HC, UMC, LMC, and SC. The Babylonian year began close to the spring equinox. Before the reign of Šamšī-Adad the Assyrian year (row REL earlier) began close to the winter solstice but in the reign of Šamšī-Adad it began close to the autumn equinox (rows REL Š-A and Assyria Š-A). Šamšī-Adad was reported to have been extremely ill in the last month of the eponym year Ṭab-šilli-Aššur (REL 197) and it is assumed that he died not long afterwards. The shaded area marks the probable interval in which he died if the year began in the autumn. Because of uncertainties in the intercalary months, the beginnings of both the Assyrian and the Babylonian years might have started a couple of months earlier or later. Whenever the Assyrian eponym year began, the death of Šamšī-Adad occurred in Hammurabi's 18th year.

The solar eclipse occurred in REL 127. At that time the eponym year began close to the winter solstice. Thus according to the REL the eclipse took place 70 eponym years before REL 197 or 69 eponym years if the eclipse took place in the autumn. Having established to their satisfaction that the Upper Middle Chronology fitted the archaeological, historical, and dendrochronological evidence,²⁴ the authors of *U&D* (25) concluded that the eclipse took place in ca. 1847 BC,²⁵ with the closest eclipses being those of 5 August 1845 BC, 2 April 1847 BC (this is the eclipse selected in *U&D*: 33), 13 April

²² It is more likely that a partial eclipse would be noticed at sunset, since groups of people are more likely to have been outside at sunset than at sunrise.

²³ For the arguments in favour of this date see *FMV* (136–138). For discussions of Old Assyrian calendars see MICHEL 2021 and ZIEGLER 2021. The winter solstice and the autumn equinox occur on or near the 21st December and September according to the Gregorian calendar. They occurred about 16 days later in the 18th century BC according to the Julian calendar.

²⁴ The date was based on the then current dendrochronological dating, which has been revised several times since then (e.g. MANNING/GRIGGS *et al.* 2016, MANNING/BARJAMOVIC/LORENTZEN 2017, MANNING/KROMER *et al.* 2020): there are still significant problems in the calibration of the Anatolian tree-ring sequence, and the supposed accuracy is overoptimistic (ROAF Ch. 13 in this volume).

²⁵ Due to some confusion about the beginnings of the Assyrian and Babylonian years and the assumption that the eclipse took place in the eponymy of Dadiya not Puzur-Ištar, they calculated the year to be 1847 instead of 1845 BC.

1848, and 17 Oct. 1849 BC, but, as discussed in my previous article, none of these have a high probability of being visible with a magnitude of over 0.95 at Assur.²⁶

Although much of the Revised Eponym List is generally accepted, there are still some uncertainties (see also BARIAMOVIC Ch. 6 in this volume). The part of the REL covering the years between the eclipse mentioned in the MEC and the death of Šamšī-Adad is based on the evidence of the Kültepe Eponym List G (KEL G) and the Mari Eponym Chronicle. Some 117 names of eponyms are preserved on KEL G (GÜNBATTI 2008, *U&D*: 94–102, 109–114). Most of the sequence is not disputed, but there are three gaps, two of which can be partly filled from the information in the Mari Eponym Chronicle (MEC). Unfortunately much of the MEC is missing.

Eleven numbered fragments of the MEC were found in Mari and published by Maurice BIROT (1985). After joins had been made between M.7481+M.11250 and S.24-1+S.24-2+A.1614b, the number of separate pieces was reduced to eight. Birot considered that these belonged to several tablets but were parts of a single text. On the basis of their contents, Birot assigned the surviving parts of the text to eight different groups of eponyms, which he labelled A to G (Fig. 5.4).²⁷ There are several different manuscripts with different arrangements of the text. Veenhof (*V MEC*) recognised that the surviving fragments of the MEC belonged to at least three ‘editions’, by which he means different layouts of the text on different tablets: Edition I has a single column 10 cm wide, Edition II has two columns 6 cm wide on each side, and Edition III has a single column 6 cm wide. Two further fragments, M.5911 and M.8566, could not be assigned with certainty to one of these three editions. It is generally accepted that the four fragments (M.7481+M.11250, S.24-1+S.24-2+A.1614b, S.24-3, and A.1614a) assigned to Edition I came from a single tablet (BIROT 1985: 220). Some of the fragments contain a single text passage, others have several text passages with different groups of eponyms. There are twelve such Text Passages (TP 1-TP 12) (Fig. 5.4).²⁸

Text Passage	Fragments	Dimensions			Number of lines	Group	Section	Edition	lines in Edition I
		Ht	W	Th					
TP 1	M.7481+M.11250	7.8	10.4	>2.8	21	A	1	I	1–21
TP 2	S.24-1	7.0	4.6		20	B	2	I	47–66
TP 3	S.24-3	3.5	3.9	–	9	D	?	I	(82–83), 1'–7'
TP 4	S.24-1+S.24-2+A.1614b [rev.]	11	5.6	4	22	E	3	I	10'–31'
TP 5	A.1614a	2.9	4.1		7	F	3	I	32'–38'
TP 6	M.7481+ M.11250 [rev.]				8	G	4	I	1"–8"
TP 7	A.1288 Col. I	13.5	10.8	c.2.5	27	A	1	II	6–27
TP 8	A.1288 Col. II				26	B	2	II	37–53
TP 9	A.1288 Col. IV [rev.]				11	C	?	II	?8'–9'
TP 10	S.115-26 [obv. + rev.]	3.4	5.6	1.9	18	B	2	III	32–44
TP 11	M.5911	4.5	4.0	–	9	A	1	?	10–16
TP 12	M.8566	>10.2	4.8	–	14	B	2	?	43–53

Fig. 5.4. Extant Text Passages on different tablet fragments of the Mari Eponym Chronicle arranged in the Editions proposed in *V MEC* with the numbers of lines extant, suggestions for the lines that they cover in Edition I, Birot's assignments to Groups of eponyms, and the Sections of the reconstructed text of Edition I. Also included are the dimensions of the joined fragments in cm as given by BIROT (1985: 219–220). Dimensions in italics have been measured from the drawings of the fragments.

5. Reconstructing KEL G from Puzur-Ištar to Ṭab-šilli-Aššur

The text of KEL G between Puzur-Ištar (G17) and Ṭab-šilli-Aššur (G83) is well preserved except for two gaps, one at the end of column i on the obverse and the other at the end of column ii on the obverse, the lower edge, and the beginning of column iii on the reverse. The gap at the end of column i starts after G30 and G31 read as [Šu-B]e-lim and [Šarrum]-dIM and ends with the first entry of column ii *Ú-šur-ši-Ištar*. This gap can be filled with five eponyms from the MEC (Šu-

²⁶ Only the eclipse of 5 August 1845 BC had a magnitude greater than 0.6 and this probably occurred after sunset (see Fig. 5.1 and ROAF 2012: 162 fig. 3).

²⁷ Birot's groups are not identical with the fragments: a single fragment may contain passages from several groups and a single group may have text from several fragments.

²⁸ The corresponding text passages in the different editions are not identical. The surviving fragments suggest that the sequence of eponyms is the same in all editions (except that Ennam-Sin was omitted in M.5911 and A.1288 Col. I), but there are some differences in the presence and amount of historical information added to the names of the eponyms (*V MEC* §1).

Laban, Aššur-imittī, Dadaya, Dadaya MAN and Ah-šalim) on TP 8 and TP 12 (reconstructed as lines 39–45 in Edition I). Assuming that this is correct, the subsequent entries on KEL G can be given consecutive numbers (Fig. 5.5).

The second gap at the end of column ii is more problematic. The last two partly legible lines of column ii are G69 and G70, then follows the gap which GÜNBATTI (2008: 104) estimated as 8 lines long assigning to these missing entries the line numbers G71 to G78. This gap is followed by another partly legible two lines (G79 and G80).

GÜNBATTI (2008: 108) read line G69 as *A-ni-x₁-[...]*. Nele Ziegler and Dominique Charpin at the 54th Rencontre Assyriologique Internationale in 2008 proposed the reading *A-ni-m[a-lik]* and suggested that this was a writing of the eponym Haya-mālik since the sign NI can also be read *ià* (KRYSZAT 2008: 211). Barjamovic, Hertel, and Larsen (*U&D*: 114) did not agree and proposed a reading ^r*E-nam*^{1.r^d1} [*A-šur*], which could then be reconciled with traces in TP 3 (S.24-3 line 3').²⁹ NAHM (2013) proposed *E/HA-NI-m[a]*.³⁰ VEENHOF (2015) read it as *E²-ni²-x* [*y*]. After collation BARJAMOVIC (Ch. 6 in this volume) states that the first sign could be either A or E but is unlikely to be HA. And the following signs are probably NI and MA. The sign NI can also be read *li* and in *V MEC* the name is confidently restored as *A-li-m[a-lik]*.³¹

BARJAMOVIC (Ch. 6 in this volume), following a suggestion by Werner NAHM (2013: 368 fig. 1), agrees that the fragmentary sign on the next line (G70) is compatible with the reading [*Ša-l*]*i*[*m-A-šur*], but, since the only evidence for this is the top of the vertical in the sign ŠI, there are many other possible reconstructions of this line.

U&D (109, 114) restored lines G79 and G80 as *A-h[i-a-a]* and *A-w[i]-l[i-a]*, but noted that the sign that they wanted to read as HI in G79 actually looked more like NA, ŠUR or BE and considered reading the name as *A-š[ur-ma-lik]* but then rejected that reading in favour of *A-h[i-a-a]*.

GÜNBATTI (2008) restored the missing names in this gap following G70 from MEC TP 4 (S.24-1+S.24-2+A.1614b [rev.] Group E). The eleven names listed in this passage have been reconstructed by Veenhof as:

10'	[Š] <i>a-l</i> [<i>im</i> - ^d] <i>A-šu</i> [<i>r</i>]	14'	[^d <i>A</i>]- <i>šur-i-mi-ti</i>	25'	[<i>As-qu-di-i</i>] <i>m</i>
11'	[^d <i>E</i>]N.ZU- <i>mu-ba-al-li-i</i> t	18'	<i>l</i> -l[<i>i-ILL</i>]AT	26'	[^d <i>A-šur</i>]- <i>ma-lik</i>
12'	[<i>R</i>] <i>i-iš</i> - ^d UTU	19'	<i>Ri-ig-m</i> [<i>a-ni</i>] <i>m</i>	32'	[<i>A-wi-li-i</i>] <i>a</i>
13'	[<i>l</i>] <i>b-ni</i> - ^d [IM]	20'	<i>I-ku-u</i> [<i>n-pi</i>]- <i>ia</i>		

The line numbers are those of Edition I as reconstructed in *V MEC* §5.1. The restoration of the names is not contentious, as these eponyms are well known from dated texts. Only the last presents an uncertainty, as the traces on the tablet are also compatible with the name Ahiyaya. The most obvious solution is to insert MEC E into the gap in KEL G with the first name [*Š*]*a-l*[*im* -^d]*A-šu*[*r*] as G70 since the traces on KEL are compatible with the sign ŠI (see above), which can be read as the syllable *lim*, and the last two with G79 and 80 read by Gunbatti as *A-š[ur-ma-lik]* and *A-w[i]-l[i-a]*.

U&D (6–9), however, proposed reconstructing the missing names by inserting names taken not only from TP 4 but also from TP 3 (S.24-3 Group D) of the Mari Eponym Chronicle, which they placed overlapping with G68 and G69 immediately before Fragment E. Unfortunately, S.24-3 is not well preserved and some of the readings are uncertain. It has 9 fragmentary lines. Veenhof (*V MEC* §3.2.1) read the text as follows:

1'	[	x[	]
2'	[<i>i-na</i> x x (x) ²]	K[A ²]	]
2'	[<i>i-na</i> x]- ^r <i>ia/e</i> ² - <i>ia</i> ¹	^r <i>d</i> UTU ^{š_i} - ^d [IM	]
4'	[<i>i-na</i>] <i>A-ta-nim</i>	^d UTU ^{š_i} - ^d [IM	]
5'		12 LUGAL.MEŠ <i>i</i> -[	]
6'		^m <i>Ya-ah-du-un</i> -[^d <i>Lim</i>	]
7'		LUGAL.MEŠ- <i>šu-nu ut-te</i> -[	]
8'	[<i>i-n</i>] <i>a</i> ^d <i>A-šur-tak-la-ku</i>	^d UTU ^{š_i} - ^d [IM	]
9'	[<i>i-na</i> <i>Ha-ià-ma-l</i>] <i>ik</i>	^d [UTU ^{š_i} - ^d IM	]

Lines 1' and 2' belonged to two different eponyms (*V MEC* 3.2.1). Birot read the sign in the second part of line 2' as KA and Veenhof more cautiously as K[A²]. Guido KRYSZAT (2008: 214) suggested that it might be the beginning of the name

²⁹ KOLIŃSKI (2013: 738–739) gives reasons for not accepting this reading.

³⁰ NAHM (2013: 368–369). Curiously Nahm (2015: 40) cited this article as being published in AfO 40 not AoF 40.

³¹ LACAMBRE (Ch. 7 in this volume) proposes reading KEL G 69 as: *a-šur*-tā[k*-lā-ku]*, and this is followed by ZIEGLER (Ch. 3 in this volume) in her synoptic view, but as discussed above a recent collation (BARJAMOVIC Ch. 6 in this volume) does not support this reading.

of the eponym Puzur-^dNirah written with the sign PUZUR₄ instead of PUZUR₂ with which sign this eponym's name was written on KEL G (G57), since the first part of the sign PUZUR₄ is the same as the first part of the sign KA.³²

Birot's reading -^dA-šu[r]? in line 3' has been corrected. KRYSZAT (2008: 214) proposed reading *A-b]e-a-a* corresponding to *A-be-a-a* on KEL G (G58). Veenhof, with the aid of a photograph of S.24-3 provided by Nele Ziegler, now reads the name of the eponym as [x]-^ria/e²-ia¹. ZIEGLER (Ch. 3 in this volume) with slightly less hesitancy gives the reading of line 3' as [*i-na a-bi*]-^ria-ia¹ ^dUTU-ši-^d[IŠKUR ...].

KRYSZAT (2008: 214) also suggested that the first preserved sign in line 4' might be E with the reading *A/E²-ta-num*, and this is confirmed by NAHM (Ch. 11 in this volume).³³ This is not dissimilar to *E-di-num* who appears as G59 on KEL G.

The name of the eponym in line 8' ^dA-šur-tak-la-ku is uncontroversial. An Aššur-taklāku is found on KEL G (G60): if the two are not identical then this eponym and the whole of S.24-3 must be placed in one of the gaps on KEL G.

The left hand sign in line 9' was read as UR/lik by Charpin and Ziegler (*FMV*: 97 and n. 183) and proposed that this was the end of the name *Ha-ià-ma-lik*. The sign is, however, only partly preserved and other restorations are possible. ZIEGLER (Ch. 3 in this volume) now rejects the reading UR/lik and suggests that the sign could be ZU and that the line could be restored as [*i-na i-si-im*]-^dEN].^rZU¹ ^dU[TU-ši-^dIŠKUR ...] containing the name of the eponym Isim-Sîn (G61).

Fig. 5.5 is an attempt to reconstruct the sequence of eponyms on the basis of KEL A and KEL G and the fragments of the MEC.³⁴ Two possibilities are included based on the placement of the fragment S.24-3 (MEC D): Position 1 (in bold outlined with a thicker line) assumes that there was only one eponym called Aššur-taklāku and that his mentions in KEL G (G60) and in TP 3 8' refer to the same individual (KRYSZAT 2008, NAHM 2013, NAHM Ch. 11 in this volume, ZIEGLER Ch. 3 in this volume), while Position 2 (in bold outlined with a double line) assumes that there were two different eponyms called Aššur-taklāku and that S.24-3 1'-2' correspond to the poorly preserved eponyms G69 and G70 (*U&D*, *V MEC*).³⁵ The eponym Ahiyaya, whose existence as a separate eponym is now generally accepted but whose placement is disputed, is not included in Fig. 5.5 and in the reconstruction in *V MEC* (§5),³⁶ but like *V MEC* it includes the dubious extra eponym REL 179a.

6. Suggested reconstructions of parts of the Assyrian Eponym List and of the Mari Eponym Chronicle.

Undisputed parts of the list (G20 to G29, G38 to G46, and G62 to G67) are omitted. The REL count is that of *V MEC*, which does not include Ahiyaya (REL 193 according to *U&D*) but does include the dubious extra eponym REL 179a. Two possibilities are indicated for the placement of S.24-3: Position 1 (in bold outlined with a thicker solid line) and Position 2 (in bold outlined with a double line). The number of years after the eponym Puzur-Ištar, in which the possible solar eclipse took place, for the two alternatives are recorded in the two left-hand columns. KEL G eponym numbers are either those of GÜNBATTI (2008) or, when marked with an asterisk, the revised numbering of *V MEC*, not the revised numbering of *U&D*. The MEC eponym counts are marked with superscript 1 for Position 1 and superscript 2 for Position 2. MEC line is either that of a particular fragment of the MEC or that of the text of Edition I as reconstructed in *V MEC*.

³² Although the KA is indented and might be thought to be the beginning or continuation of a historical note, 'eponyms to which no historical information is added start with a vertical wedge ... followed by a blank space before the name' and so the KA could be the beginning of the name of a new eponym (*V MEC* §2, §3.2.1). I am grateful to Klaas Veenhof for clarification about the readings. ZIEGLER (Ch. 3 in this volume) accepts the reading P[UZUR₄.^dMUŠ...].

³³ *V MEC* gives the reading *A-ta-nim* (written with a dot under the A to indicate an uncertain reading). Similarly ZIEGLER (Ch. 3 in this volume) gives [*i-na*] ^ra¹-ta-nim ^dUTU-ši-^d[IŠKUR ...]. NIM and NUM are the same cuneiform sign.

³⁴ I am grateful to Werner Nahm for correcting an earlier draft of this figure.

³⁵ There are objections to both reconstructions that are discussed by other scholars elsewhere. Here I do not want to go into the details, which are debated by experts more competent than me, but merely to investigate the implications of the two most likely possible reconstructions.

³⁶ Ahiyaya is attested as eponym only once in Mari in the period of Assyrian rule. Since it has been thought that there are no gaps in the list of eponyms at Mari for the reign of Šamši-Adad, it was proposed that his name might have been an alternative name for one of these years or he might have been *limmu* after Šamši-Adad's death. For earlier discussions of Ahiyaya see (*U&D*: 9–13, NAHM 2013: 370, *V MEC* §3.4b). For a more recent detailed discussion of Ahiyaya see LACAMBRE (Ch. 7 in this volume), who notes that he is attested four times in Tell Bi'a and numerous times in Chagar Bazar. Lacambre also gives cogent reasons for inserting Ahiyaya after Aššur-mālik and before Awīliya.

Years after P-1			REL		KEL G		MEC			
Pos. 1	Pos. 2	count			eponym	eponym name	eponym	line	eponym name	comment
		127			G17	Púzur-Ištar	24	25	[Puzur ₄ -Ištar] _r	
01	01	128			G18	I-sá-i-a	25	26	[I-sá-a-ia]	
02	02	129			G19	A-bu-ša-lim	26	27	[A-bu-ša-lim]	↑ TP 7 (<i>A.1288 I</i>) ↑
03–12	03–12				G20–29		27–36	28–37		↓ TP 10 (<i>S.115–26</i>)
13	13	140			G30	[Šu-B]e-lim	37	38	Šu-Be-li	↓ TP 8 (<i>A.1288 II</i>)
14	14	141			G31	[LUGAL]- ^d IM	38	39–40	LUGAL- ^d IM	accession of Šamši-Adad
15	15	142			[G32]		39	41	Šu-La-ba-an	
16	16	143			[G33]		40	42	^d A-šur-i-mi-ti	
17	17	144			[G34]		41	43	Da-da-a-ia	↓ TP 12 (<i>M.8566</i>)
18	18	145			[G35]		42	44	Da-da-a-ia MĪN	↑ TP 10
19	19	146			[G36]		43	45	A-hi-ša-lim	
20	20	147			G37	Ú-šur-ši-Ištar	44	46	Ú-šur-ša-Iš[ar]	
21	21	148			G38	Kà-ta-a	45	47	Ka-at-ta-[a]	↓ TP 2 (<i>S.24. I+</i>)
22–29	22–29				G39–46		46–53	48–58		↑ TP 8 ↑ TP12
30	30	157			G47	A-šur-e-na	54	59	^d A-šur-en-nam	
31	31	158			G48	In-be-Ištar	55	60	I-ni-i[b-Iš]-tar	? Ibni-Adad of AKL
32	32	159			G49	A-šur-be-el-ma-al-ki	56	61	^d A-šur-be-el-ma-al-k[i]	
33	33	160			G50	Be-lá-num	57	62	Be-[la-nim]	
34	34	161			G51	Sú-kà-lúm	58	63	S[ú-ka-lim]	
35	35	162			G52	A-mur-A-šur	59	64	[^r A ¹ -mur- ^d A-šur]	? Atamar-Ištar of AKL
36	36	163			G53	A-šur-ni-šu	60	65	[^d A-šur-ni-š]u	↑ TP 2
37	37	164			G54	Mu-na-wi-ru-um				
38	38	165			G55	Id-na-a				
39	39	166			G56	Da-da-a				
40	40	167			G57	Púzur- ^d Nirah				
41	41	168			G58	A-be-a-a				
42	42	169			G59	E-dí-num				
43	43	170			G60	^d A-šur-ták-lá-ku				
44	44	171			G61	I-sí-im-Sú-in				
45–50	45–50				G62–67					
51	51	178			G68	Da-da-[a]				
52	52	179			G69	A-lí-m[a-lik]				
	53	179a*			[G70*]					
	54	180*			[G71*]					
	55	181*			[G72*]					
	56	182*			[G73*]					
	57	183*			[G74*]					
53	58	184*			[G70/75*]	[Ša-l]i[m- ^d A-šur]				
54	59	185*			[G71/76*]					
55	60	186*			[G72/77*]					
56	61	187*			[G73/78*]					
57	62	188*			[G74/79*]					
58	63	189*			[G75/80*]					
59	64	190*			[G76/81*]					
60	65	191*			[G77/82*]					
61	66	192*			[G78/83*]					
62	67	193*			G79/84*	A-š[ur-ma-lik]				
63	68	194			G80/85*	A-w[i]-l[i]-a				
64	69	195			G81/86*	Ni-mar-S[ù-in]				
65	70	196			G82/87*	^d IM-ba-ni				
66	71	197			G83/88*	DU ₁₀ -ši-lá-A-šur				

Position 1 (S.24-3, MEC D)			
	1'		↓ TP 3 (<i>S.24-3</i>)
64	2'	Pu[zur-^dNirah]	
65	3'	[A-be]-e/ia²-ia	
66	4'–7'	A/E-ta-num	
67	8'	^dA-šur-tak-la-ku	
68	9'		↑ TP 3

Position 2 (S.24-3, MEC D)			
76	1'		↓ TP 3 (<i>S.24-3</i>)
77 ²	2'		
78 ²	3'	[x]-ia/e²-ia	
79 ²	4'–7'	A-ta-nim	
80 ²	8'	^dA-šur-tak-la-ku	
81 ²	9'	[Ha-ia-ma-l]ik²	↑ TP 3
77 ¹ /82 ²	10'	[Š]a-l[im- ^d]A-šur[r]	↓ TP 4 (<i>S.24- I+ rev.</i>)
78 ¹ /83 ²	11'	[^d E]N.ZU-mu-ba-al-li-iṭ	
79 ¹ /84 ²	12'	[R]i-iš- ^d UTU	
80 ¹ /85 ²	13'	[I]b-ni- ^d IM	
81 ¹ /86 ²	14'–17'	[^d A]-šur-i-mi-ti	
82 ¹ /87 ²	18'	ī-l[ī-ILL]AT	
83 ¹ /88 ²	19	Ri-ig-m[a-ni]m	
84 ¹ /89 ²	20'–24'	I-ku-u[n-pi]-ia	
85 ¹ /90 ²	25'	[As-qu-di-i]m	
86 ¹ /91 ²	26'–32'	[^d A-šur]-ma-lik	↑ TP 4
87 ¹ /92 ²	33'–37'	[A-wi-li-i]a	↓↑ TP 5 (<i>A.1614a</i>)

Fig. 5.5. Suggested reconstructions of parts of the Assyrian Eponym List and of the Mari Eponym Chronicle. Undisputed parts of the list (G20 to G29, G38 to G46, and G62 to G67) are omitted. The REL count is that of *V MEC*. Two possibilities are indicated for the placement of S.24-3: Position 1 (in bold outlined with a thicker solid line) and Position 2 (in bold outlined with a double line). The number of years after the eponym Puzur-Ištar, in which the possible solar eclipse took place, for the two alternatives are recorded in the two left-hand columns. KEL G eponym numbers are either those of GUNBATTI (2008) or, when marked with an asterisk, the revised numbering of *V MEC*, not the revised numbering of *U&D*. The MEC eponym counts are marked with superscript 1 for Position 1 and superscript 2 for Position 2. MEC line is either that of a particular fragment of the MEC or that of the text of Edition I as reconstructed in *V MEC*.

7. Reconstructing the text of the Mari Eponym Chronicle

The text of the Mari Eponym Chronicle has been thoroughly investigated by Klaas VEENHOF (2015, 2018, *V MEC*) and he has proposed a reconstructed version (*V MEC* §5.1) which must be the basis for any future research. The main problem is that there are gaps of unknown length in Edition I: these can be partly but not completely resolved with information from the other editions of the MEC and from the eponym lists KEL A and KEL G. A further problem is that, unlike KEL G in which each eponym occupies a single line, in the MEC the presence of historical notes means that each eponym may take up more than one line.³⁷

There are three gaps in the reconstructed text of Edition I of the MEC. I have labelled the resulting four sections of text as Sections 1, 2, 3, and 4 (Fig. 5.4). The first gap (between Sections 1 and 2) starts after *l-l[i-dan]* (TP 1 M.7481+M.11250 line 21). The beginning of the gap (i.e. the end of Section 1) can be filled by text from Edition II (TP 7) as far as *A-bu-ša-lim* (A.1288 Col. I 28') and the end of the gap (i.e. the beginning of Section 2) can be filled from passages belonging to Editions II (TP 8 A.1288 Col. II) and III (TP 10 S.115-26) as well as from the passage TP 12 on fragment M.8566, so that the second portion of the text begins with *ḏA-šur-ma-lik* (S.115-26 1' corresponding to the first line of Section 2. The remaining gap between *A-bu-ša-lim* and *ḏA-šur-ma-lik* is covered by KEL A and G which both assign four eponyms to it.

The first eponym entry with a historical note that would have taken up more than a single line is that of Šarrum-Adad (TP 10 S.115-26 lines 9'–12', G38), which described the victory of the Elamite king over Ipiq-Adad of Ešnunna and the accession of Šamši-Adad. This note took up four lines in Edition III and would therefore have taken up two lines in Edition I. Since for all the preceding entries of the MEC either there are no historical notes or the historical notes associated with them are short and would not have taken up more space than that available in a single line of Edition I, *V MEC* (§3.1) proposes that each of the four missing eponyms restored from KEL A and KEL G as well as those entries restored from Editions II and III would also have taken up a single line in Edition I.³⁸ He therefore reconstructed the total number of lines in the reconstruction of Edition I to the end of Section 2 (TP 6 S.24-1 line 20') as 66.

The next gap starts after *ḏA-šur-n[i]-šu* (TP 2 S.24-1 20') and before *[š]a-lim-[ḏA-šur]* (TP 4 S.24-1+S.24-2+A.1614b rev. 1'). Since TP 2 and TP 4 are written on the obverse and reverse of S.24-1, the gap includes the bottom of the obverse and the top of the reverse of Edition I. The first part of this gap can be filled from KEL G (G54 to G69) but thereafter there is a gap in both texts which might be filled by TP 3 on S.24-3 and TP 9 on A.1288 Col. IV rev. 1'–11' (*V MEC* §3.2.1 and §4.2).

The end of this gap is the beginning of TP 4. The end of TP 4 overlaps with TP 5. The last two legible eponyms on TP 4 are *ḏA-šur-ma-lik* and *[A-wi-li-i]a* which, as discussed above, can be identified with *A-š[ur-ma-lik]* (G79) and *A-w[i]-l[i-a]* (G80) on KEL G. The following entries on KEL G are:

G81	<i>Ni-mar-S[u-en₆]</i>
G82	<i>ḏIŠKUR-ba-ni</i>
G83	<i>DU₁₀-š[i-lá-A-šur]</i>
G84	<i>E-nam-A-šur</i>
G85	<i>A-šur-e-mu-qí</i>

This last name is compatible with the first name *ḏA-šur-e-mu-[qí]* in TP 6, the final section of the text, which also includes a two-line colophon. The gap between TP 5 and TP 6 can therefore be filled with four eponyms from KEL G, including the eponym Ṭab-šilli-Aššur (G83), as well as possible historical notes.

There are several problems with the reconstruction with S.24-3 in Position 1. First the eponym Haya-mālik is missing. There is no doubt that Haya-mālik was an eponym but his eponymy is only attested in Mari where several tablets Mari dated to the eponymy of Haya-mālik and one tablet mentioning the eponymy of Haya-mālik were found. So it is possible that Haya-mālik was a name used in Mari for an eponym year which had a different name elsewhere.³⁹ This could be a case similar to that has been proposed for Ahiyaya (see above and *V MEC* §3.4b).

³⁷ This makes the reconstruction more difficult, but at the same time the corresponding uncertainty gives more flexibility in the reconstruction, because a certain number of lines must not contain the same number of eponyms but could have fewer eponyms with the additional lines containing historical notes.

³⁸ Since the longer entry for Šarrum-Adad is only 8 years after the end of the gap and is followed after one year by another longer entry, there is a definite possibility that the gap in the text of Edition I of MEC could have been longer than four lines.

³⁹ LACAMBRE (Ch. 7 in this volume), however, argues that the tablet mentioning the name of Haya-mālik (M.7841) was not written in Mari but was brought there from Ekallatum and that the eponyms included in the tablet were those of Ekallatum and were not recognised in Mari. See below for discussion of the contents of this tablet.

Five tablets containing inventories of the resources of the Mari palace were dated to the 11th and 12th months of the eponymy of Haya-mālik (CHARPIN 1983, ZIEGLER 2021: 120). Charpin and Ziegler (*FMV*: 79) proposed that this inventory was carried out soon after Šamšī-Adad had entered Mari.⁴⁰ This is a reasonable assumption, but Šamšī-Adad could have and probably did order inventories to be carried out several times in the course of his reign.

The second text mentioning the eponymy of Haya-mālik is very fragmentary. It contains lists of deficits which are summarized for different years in lines beginning with LÁ.Ú.HÁ followed by the name of an eponym (M.7841, *FMV*: 78–80) (Figs. 5.6 and 5.7).



Fig. 5.6. Left edge, obverse and reverse of *FMV*: 2 (M.7841, www.archibab.fr/T6499). © by Archives Royales de Mari.

	numbers of lines	eponyms in the text	restorations
obverse	ca. 11 lines		
	line 2'	lá-u-há [<i>li-mu</i> ^d]A-šur-ták-[<i>la-ku</i>]	Aššur-taklāku
	6 lines		
	line 9'	lá-u-há <i>li-mu ha-ià</i> -[<i>ma-lik</i>]	Haya-mālik
	ca. 11 lines		
reverse	ca. 10 lines		
	line 4'	[lá-u-há <i>li</i>]- <i>mu</i> ^d su'en-mu-[<i>ba-li-iṭ</i>]	Sîn-muballit
	ca. 16 lines		
edge	1 line		
	line 2"	[... <i>li-mu ri-iš</i>] ^d UTU ¹	Rīš-Šamaš

Fig. 5.7. The layout of tablet M.7841 with approximate numbers of lines and suggested identifications of the eponyms. After *FMV*: 70–71.

⁴⁰ 'Sitôt Mari conquise, Samsi-Addu fit faire l'inventaire des richesses de son palais: les coffres scellés qui contenaient la vaisselle royale, les métaux précieux, des instruments de musique, etc. furent ouverts un par un et leur contenu soigneusement inventorié. Le changement politique n'entraîna pas dans l'immédiat de bouleversement dans le palais de Mari: les mêmes administrateurs continuèrent à remplir leur charge' (*FMV*: 79).

The names of three eponyms are partly preserved as is the name of the eponym in the date. These are identified in *FM* V (78–80) with Aššur-taklāku, Haya-mālik, Sîn-muballiṭ, and Rīš-Šamaš. The first two have been restored in *V MEC* on S.24-3:

8' ^dA-šur-tak-la-ku
9' [Ha-ià-ma-l]ik²

and the last two, Sîn-muballiṭ and Rīš-Šamaš, preceded by Šalim-Aššur are found on adjacent lines of the reverse of MEC S.24-2:

1' [Š]a-l[im-^d]A-šu[r]
2' [^dE]N.ZU-mu-ba-al-li-iṭ
3' [R]i-iš-^dUTU

It is therefore possible that the deficits in the eponymies of Šalim-Aššur and Sîn-muballiṭ were listed in the last 16 lines on the obverse and the first 10 lines on the reverse and that the deficits in the eponymy of Rīš-Šamaš were recorded in the 16 lines after the line mentioning the eponymy Sîn-muballiṭ.⁴¹

Since Rīš-Šamaš follows Sîn-muballiṭ on S.24-2, it is possible that the five eponyms (if the name of Šalim-Aššur is correctly restored) were consecutive. This is not possible in the Position 1 scenario unless Aššur-taklāku and Haya-mālik were inserted immediately before Šalim-Aššur in the MEC. This would, however, necessitate Aššur-taklāku and Haya-mālik to be placed after Ali-mālik (G69) in KEL G, thereby increasing the count of years from Puzur-Ištar to Ṭab-šilli-Aššur by at least two years and would not be significantly different from the sequence of Position 2.

If the eponyms in M.7841 were not consecutive and there was only one eponym called Aššur-taklāku then Haya-mālik was identical with the eponym G69 whose name probably began with the signs A-NI-MA (as discussed above). KRYSZAT (2008: 211) suggested reading this name as *A-ià ma-lik* since the sign NI can also be read as IÀ: this is the way that the second syllable of the name of Haya-mālik was written in M.7841 line 9'.⁴² Alternatively Haya-mālik might have been the local Mari or Ekallatum name for the eponymy that elsewhere was known as Ali-mālik.

Even if one of these explanations were correct, there would still be the difficulty, as stressed in *V MEC* §3.2.1, that Aššur-taklāku appears as G60 in KEL G some nine years before the other eponyms on M.7841.

KEL G Position 1.1			KEL G Position 1.2			MEC	
count	number	eponym	count	number	eponym	number	eponym
51	G68	<i>Da-da-[a]</i>	51	G68	<i>Da-da-[a]</i>		
52	G68a	<^dA-šur-tak-la-ku>					
53	G69	<i>A-lí-m[a-lik]</i>	52	G69	<i>A-lí-m[a-lik]</i>		
			53	G70	Aššur-taklāku		
			54	G70a	Haya-mālik		
54	G70	[Ša-l]i[m- ^d A-šur]	55	G70b	[Ša-l]i[m- ^d A-šur]	82	[Š]a-l[im- ^d]A-šu[r]
55	G71	[Sîn-muballiṭ]	56	G71	[Sîn-muballiṭ]	83	[^d E]N.ZU-mu-ba-al-li-iṭ
56	G72	[Rīš-Šamaš]	57	G72	[Rīš-Šamaš]	84	[R]i-iš- ^d UTU
57–65	G73–81		58–66	G73–81		85–93	
66	G82	^d IM-ba-ni	67	G82	^d IM-ba-ni	94	
67	G83	DU ₁₀ -ši-lá-A-šur	68	G83	DU ₁₀ -ši-lá-A-šur	95	

Fig. 5.8. Two restorations of KEL G with S.24-3 in Position 1 which maintain the sequence of eponyms from Aššur-taklāku to Rīš-Šamaš deduced from M.7841. Additional eponyms are in bold.

There are two possible restorations of KEL G which would allow S.24-3 to be placed in Position 1 in the MEC and keeps the sequence of eponyms deduced from M.7841. The first possibility (Position 1.1) is that Aššur-taklāku was omitted between G68 and G69 (left hand columns in Fig. 5.8).⁴³ This would require the acceptance of the suggested restorations of Haya-mālik (instead of A-lí-m[a-lik]) and [Ša-l]i[m-^dA-šur] discussed above and would extend the time between Puzur-Ištar and Ṭab-šilli-Aššur to 67 years. The second (Position 1.2) is to insert Aššur-taklāku, Haya-mālik, and Šalim-Aššur in

⁴¹ There is room for an additional eponym at the beginning of the text, but it could have listed only the deficits in the eponymy of Aššur-taklāku.

⁴² *V MEC* reads this name as *A-lí-m[a-lik]*.

⁴³ Although one hesitates to propose that an eponym has been missed out of the list, VEENHOF (2007: 59) noted that the eponym Šuli was omitted in KEL A. LACAMBRE (Ch. 7 in this volume) instead suggests reading G69 as : *a-šur*-tá[k*-lá-ku]* followed by the eponym Haya-malik thus obviating the need to suppose that the name of Aššur-taklāku had been omitted. This would mean that the time between Puzur-Ištar and Ṭab-šilli-Aššur was 67 years (or 68 years with the insertion of Ahiyaya between Aššur-mālik and Awīliya) for both Position 1.1 and Position 1.2.

the gap after G69 with other names from S.24-1+ until Aššur-mālik (G79 = MEC 90) (middle columns in Fig. 5.8). This would bring the time between Puzur- Ištar and Ṭāb-šilli-Aššur to 68 years.

If M.7841 had not been discovered, the restoration with Position 1 as restored in Fig. 5.5 might be thought possible, but the most straightforward interpretation of M.7841 makes this restoration impossible. This means that, if Position 1 is correct, one of the two further possibilities adumbrated in Fig. 5.8 has to be accepted and, although neither of these can be excluded, neither of them are particularly attractive. Nevertheless I will pursue the possible arrangements of the text according to both Position 1 and Position 2 as these two possibilities for Position 1 are possible if not plausible.

8. Reconstructing the layout of Edition I of the Mari Eponym Chronicle

The gap between TP 1 and TP 2 can be filled with six eponyms from KEL A and G, and it has been established that TP 5 follows immediately after TP 4, and the gap between TP 5 and TP 6 has been filled with eponyms from KEL G. These include six of Birot's groups of eponyms but leave out Group D (TP 3, S.24-3) which belongs to Edition I and Group C (TP 9) which belongs to Edition II. TP 9 is very fragmentary. *V MEC* (§4.2) discusses TP 9 in detail, refuting KOLIŃSKI's (2015) suggestion for its placement and concluding that it is part of a long historical note without mentioning the name of an eponym. It also calculates that TP 9, which filled at least 10 lines in Edition II, would have needed 3 lines in Edition I. According to *V MEC* historical information in TP 9 suggests that it belonged to one of the eponyms before the beginning of Section 4. The precise position is uncertain, though, if TP 3 could be precisely placed, the possibilities would be more limited.

As shown in Fig. 5.5 two different suggestions have been made for the position of S.24-3. One (Position 1) corresponds to G57 to G60 as suggested by GÜNBATTI (2008) and KRYSZAT (2008) and the other (Position 2) at the end of the gap between Sections 2 and 3 as proposed in *V MEC* (§3.2.1) and thus filling part of the gap between TP 2 and TP 4.

Veenhof (*V MEC* §3.2.1) rejected the first possibility because there is insufficient space at the top of the reverse for S.24-3 (or rather too much space for the three eponyms following the end of S.24-1+ and before the first eponym on S.24-3), but, as we will see, it is possible that S.24-3 should be placed at the bottom of the obverse.⁴⁴ A detailed calculation of the numbers of lines in the various sections are needed to establish whether this is possible (see below).

On the obverse, Sections 1 and 2 took up 66 lines, the last two lines being the continuation of the historical note attached to Aššur-nišu.

The last section on the reverse (Section 4) consists of six lines containing the name of Aššur-emuqi and the associated historical notes. This is separated from the two line colophon by a ruled line. The distance from this line to the bottom edge of the tablet (3.5 cm) is the equivalent of 9 lines if they were each the same height as those attached to Aššur-emuqi.

Between the end of Section 3 and the beginning of Section 4, there were, as we have seen above, the entries associated with four eponyms from KEL G, Nimer-^dSîn, ^dAdad-bani, Ṭab-šilli-^dAššur, and Ennam-^dAššur. It is probable that there were long historical notes attached to all these eponyms, as the previous two eponyms and the following eponym had long historical notes. A summary of the frequency of historical notes extending over more than one line is presented in Fig. 5.9.

Text Passage	Fragments	Number of lines	Number of lines with names of eponyms and historical notes	Number of eponyms	Lines per eponym	Section	Average line height in mm
TP 1	M.7481+M.11250	21	20	20	1	1	3.63
TP 2	S.24-1	20	18	15	1.2	2	3.76
TP 3	S.24-3	9	7	4	1.75	?	3.67
TP 4	S.24-1+S.24-2+A.1614b [rev.]	24	22	9	2.44	3	3.83
TP 5	A.1614a	6	5	1	5	3	3.80
TP 6	M.7481+M.11250 [rev.]	6	6	1	6	4	3.83
	Total		78	50	1.56		

Fig. 5.9. Numbers of eponyms, the lengths of the entries associated with them, average numbers of lines per eponym, and average line heights (see footnote 45) for different Text Passages of Edition I. TP 4 and 5 overlap making a combined total of 27 lines. It is possible that TP 5 included more than one eponym since the left hand side is not preserved, but it is easy to interpret the surviving text as belonging to a single historical note.

⁴⁴ As implicitly admitted in *V MEC* §3.2.1.

The length of each entry increases throughout the text from an average of one line per eponym at the beginning to over 5 lines per entry at the end of the text. The average for TP 4+TP 5 is 2.7 lines per eponym. The gap between TP 5 and TP 6 with four eponyms is therefore likely to have taken up between 12 and 24 lines. The previous 7 eponyms took up 24 lines and the following eponym, the final one in the MEC, took up six lines. It would therefore not be unreasonable to suggest that this gap took up between ten and twenty lines. The calculated length of the gap fits this well.

Sections and gaps	Text Passages	Number of lines	Suggested possible number of lines	Number of eponyms	Lines per eponym
Section 1	TP 1	27	27	26	1
Gap		?	4	4	c.1
Section 2	TP2	35	35	30	1.17
Gap		?	x		
Bottom of obverse / Top of reverse					
Gap		?	y		
Section 3	TP 4 + TP 5	27	27	10	2.7
Gap		?	4–24	4	1–6
Section 4	TP 6	6	6	1	6

Fig. 5.10. Estimates of the numbers of lines in the different Sections of Edition I. It is possible that Section 3 included more than 10 eponyms (see caption to Fig. 5.9).

Assuming a gap of x lines at the end of the obverse and y lines at the beginning of the reverse, there were 67+x lines on the obverse and a minimum of 47+y lines on the reverse. Clearly there is plenty of room for fitting the 9 lines of S.24-3 (TP 3) at the top of the reverse by reducing the gap between Sections 3 and 4 even if there was no gap at the end of the obverse.

The lengths of the four surviving reconstructable sections of the text of Edition I can be proposed as 27 lines for Section 1, as 35 lines for Section 2, as 27 lines for Section 3, and as 6 lines plus the colophon for Section 4. The corresponding gap between Sections 1 and 2 can be estimated as 4 lines and that between Sections 3 and 4 as between 4 and 24 lines.

There then remains the question of the length of the gap between Sections 2 and 3 covering the end of the obverse and the beginning of the reverse. A further uncertainty is the question of whether the lower edge of the tablet was inscribed. This edge was about 2 cm in thickness and could have accommodated as many as 5 lines of text.

On average the height of the lines on the obverse is 0.369 cm while that on the reverse is 0.383.⁴⁵ So the 66 lines on the obverse would have taken up ca. 24.4 cm. Because the lines are higher on the reverse, the same height on the reverse would have taken 63.6 lines.

On S.24-3, six lines are 2.2 cm high giving an average line height of 0.367 cm: this suggests that it is more likely to have been part of the obverse than of the reverse of the tablet. If S.24-3 was on the obverse, it would have to have been in Position 1. There would then be two or three eponyms between the end of S.24-1 and the beginning of S.24-3. These eponyms could have taken up as few as 2 lines but if there was historical information at the rate of Sections 3 and 4 they might have taken 3 or 4 lines. It would be reasonable to assume that at least two lines are missing at the end of S.24-3. In this case the total number of lines on the obverse would have been 66+4+9+2 = 81 lines giving a total height of about 29.9 cm (Fig. 5.11 left).

If S.24-3 (in Position 1) was placed at the top of the reverse (Fig. 5.11 right), there would have to have at least one line above it and the two eponyms after the end of Section 2 and before the beginning of Section 3 (G54 Munawwirum and G55 Idnaya) would have had to occupy the remaining space on the obverse as well as the bottom edge if that was inscribed. The number of lines on the obverse would then probably not have been more than 71 giving a height of 26.2 cm. The height of the reverse without the colophon (22.7 cm) would then have been the equivalent of 59 lines. 9 of these lines would have been occupied by S.24-3, 27 by Section 3 and 6 by Section 4 making a total of 42 lines, leaving 17 lines for the missing 17 eponyms (the 13 eponyms before the beginning of Section 3 and the 4 eponyms in the gap between Sections

⁴⁵ On M.7481+M.11250 19 lines are 6.9 cm high, on S.24-1 17 lines are 6.4 cm high, on S.24-1+S.24-2+A.1614b [rev.] 23 lines are 8.8 cm high, on A.1614a 5 lines are 1.9 cm high, and on M.7481+ M.11250 [rev.] 6 lines are 2.3 cm high. Measurements taken from Birot's drawings. BIROT (1985: 220) estimated an average of 0.35 cm for the line height on M.7481+M.11250. *V MEC* (§3) states lines of Edition I are ca. 3.5 mm to 4 mm high. See Fig. 5.9.

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Fig. 5.11. Reconstructions of the obverse (left) and reverse (right) of Edition I. The left hand column gives the count of lines from the top on the obverse and from the bottom on the reverse. Shaded cells in the middle column indicate that the eponym is present on KEL A or KEL G. The right hand column gives the line count of the fragments or Sections. Left: Obverse with S.24-3 (TP 3) at the bottom of the obverse. Right: Reverse with S.24-3 (TP 3) at the top of the reverse. 21 lines have been allotted for the four eponyms and the associated historical notes filling the gap between Sections 3 (TP 4+5) and 4 (TP 6). If S.24-3 (TP 3) was on the reverse, there could have been less than 81 lines on the obverse. The height of the reverse could have been reduced (or increased) by moving the position of S.24-3 or by changing the numbers of lines between A.1614a (TP 5) and M.7481+M.11250 [rev.] (TP6).

If S.24-3 was placed on the reverse in Position 2, there would be 15 eponyms after the end of Section 2 and before S.24-3 to be distributed over the bottom of the obverse and the top of the reverse. Given the numbers of lines per eponym

in the previous and following sections one might suggest that these 15 eponyms took up about 23 lines. After the 9 lines of S.24-3 there might have been two or more lines of historical information including that contained in TP 9 (Group C), which would have been followed by the 27 lines of Section 3, the 4 eponyms in the gap, the 6 lines of Section 4, and the colophon.

Assuming the obverse had 71 lines the height of the tablet would have been ca. 26.2 cm and there would have been 14 lines on the obverse after Section 2. As calculated above, the height of the reverse without the colophon would then have been 22.7 cm, the equivalent of 59 lines. There would then have been some 9 lines before S.24-3, some of which might have been written on the lower edge. S.24-3 and the additional lines from TP 9 would have contributed some 11 lines. Adding in the 27 lines from Section 3 and the 6 lines from Section 4 would allow some 6 lines for the missing 4 eponyms in the gap between Sections 3 and 4 or 12 lines if 6 lines were written on the bottom edge. As discussed above it is probable that more than 13 lines would have been needed for the penultimate four eponyms, but this could easily have been accommodated if the obverse had contained more than 71 lines (Fig. 5.11 right).

9. Chronological implications

The chronological implications of these reconstructions for the possible dates of the event that took place in the eponymy of Puzur-Ištar are summarised in Fig. 5.12.

	Position 1: 1 Aššur-taklāku	Position 2: 2 Aššur-taklākus
Interval between Puzur-Ištar and Ṭab-šilli-Aššur	66 years (or 67 or 68)	71 years (or 70)
HC date for Puzur-Ištar	1897	1902
LHC date for Puzur-Ištar	1889	1894
UMC date for Puzur-Ištar	1841	1846
LMC date for Puzur-Ištar	1833	1838
SC date for Puzur-Ištar	1777	1782
LSC date for Puzur-Ištar	1769	1774

Fig. 5.12. Time intervals between the solar eclipse (in the eponymy of Puzur-Ištar) and death of Šamši-Adad (eponymy of Ṭab-šilli-Aššur or in the following year) according to the two reconstructions of the Assyrian Eponym List (Fig. 5.5) with the corresponding dates BC for the solar eclipse according to the High, Middle, and Short Chronologies.

For Position 1 to be compatible with the sequence of eponyms in M.7841 (Fig. 5.7) two restorations of possibilities for the restoration of KEL G (Positions 1.1 and 1.2) were suggested above (Fig. 5.8). These would change the time interval to 67 and 68 years respectively. The omission of the dubious eponym MEC 77[?] reduces the count for Position 2 to 70 years (Position 2.1).

The count of years could be reduced by one, if the second Dadaya (*Da-da-a-ia* MĪN G 35)⁴⁶ were omitted, and could be increased by one, if an eponym such as Ahiyaya had been omitted in KEL G or MEC.⁴⁷ For Position 2 it would be easy to increase the count by inserting Ahiyaya or other eponyms between G69 and S.24-3 3' or to reduce the count by omitting the putative eponym MEC 77[?]. For Position 1 the identification of [Ša]-l[im-^dA-šūr] (G 70) with the Šalim-Aššur attested on the MEC and on other documents would have to be rejected for one or more additional eponyms to be inserted after G69. For both positions it would be possible to insert Ahiyaya after G79 *A-š[ur-ma-lik]* as suggested in *U&D* (9–13, 100), reading the corresponding entry in A.1644a as [*A-hi-a-i*]a.

⁴⁶ It is unusual for successive years to have the same eponym name, the only other case in the 250 eponyms in REL is when Aššur-mālik, son of Agatum, was followed by Aššur-mālik, son of Ennāniya (REL 56 and 57), and so in this case, when the fathers' names are not given, it is possible that this dittography referred to a single individual. The reading *Da-da-a-ia* MĪN is clear, but the precise significance of the additional MĪN (= MAN) is not clear to me. When a scribe dated a document with the name of the eponym of the previous year, the name was qualified as *ša qāte* or *warki* and not as MĪN. It is possible that the historical information in the second line (G 35) referred to the same eponym year as the information in the first line (G 34), but for some reason the scribe mistakenly repeated the name in the blank space before the second line of the historical note. For a name that was written in the wrong place in KEL G then erased and written one line later, see BARJAMOVIC (Ch. 6 in this volume). The MĪN might have been added subsequently by a copyist who was puzzled by the repetition of the name. The fact that 'intercalary months are usually expressed by the sign "MĪN" in Mari' (ZIEGLER 2021: 123 n. 34) may be relevant.

⁴⁷ *V MEC* (§1 n. 4 and §5.2) notes the omissions of an eponym in KEL A (Šuli) and another in M.5911 and A.1288 I (Ennam-Sîn). BLOCH (2014: 197 n. 25) also discussed possible omissions in KEL and MEC.

The above discussion has shown it is possible to reconstruct the layout of Edition I with TP 3 (S.24-3) either in Position 1 or Position 2. There is adequate space at the bottom of the obverse for Position 1 if there were 81 or more lines on the obverse. This makes a taller tablet than Birot originally envisaged, but this is not impossible. Some scholars may find difficulties in the particular reading of the signs in TP 3 in order to make them compatible with KEL G, but this may not be an obstacle for others and some of them can be avoided if Position 1.2 is accepted (Fig. 5.8).

The historical information contained in TP 3 can also contribute to the correct placement of S.24-3. The relevant text and partial translation based on *V MEC* read as follows:

3'	[i-na x]- ^f ia/e ² -ia ¹ ^d UTU ¹ š ⁱ - ^d IM	]	[in x]-ya: Šamšī-[Adad	]
4'	[i-na] A ² -ta-nim ^d UTU ¹ š ⁱ - ^d IM	]	[in] Atanum: Šamšī-[Adad	]
5'	12 LUGAL.MEŠ i-x-[	]	12 kings [	]
6'	^m Ya-ah-du-un- ^d Li-im	]	Yahdun-[Lim	]
7'	LUGAL.MEŠ-š ⁱ u-nu ut-te-[	]	their kings he [	]
8'	[i-n]a ^d A-šur-tak-la-ku ^d UTU ¹ š ⁱ - ^d IM	]	in Aššur-taklāku: Šamšī-[Adad	]

If S.24-3 is placed in Position 1, the eponymy of Atanum, in which Šamšī-Adad and Yahdun-[Lim] were mentioned, would have taken place 24 years before the death of Šamšī-Adad and thus is compatible with the dates suggested in *FMV* for the deposition of Yahdun-Lim, 19 years before the death of Šamšī-Adad and the capture of Mari by Šamšī-Adad two years later (Fig. 5.13).⁴⁸

Historical event	REL	MEC	UMC	LMC
<i>Position 1: Yahdun-Lim mentioned in MEC</i>	169	66	1804	1796
Yahdun-Lim deposed			1794	1786
			1793	1785
Šamšī-Adad's rule in Mari started			1792	1784
<i>Position 2: Yahdun-Lim mentioned in MEC</i>	181*	79	1792	1784
Death of Šamšī-Adad	197	95	1775	1767

Fig. 5.13. Dates of mention of Yahdun-Lim with S.24-3 in Position 1 and Position 2 counting back from the death of Šamšī-Adad which is assumed to have taken place at the end of REL 197. REL and MEC numbers according to *V MEC*. Dates in bold are those suggested in *FMV*.

If on the other hand S.24-3 is placed in Position 2, Šamšī-Adad's rule in Mari started in MEC 78 (= REL 180*) and Yahdun-Lim was deposed in MEC 76 (= REL 179a) or in MEC 75 (= REL 179) if the dubious eponym MEC 77² (= REL 179a) is omitted. In this scheme Yahdun-[Lim] was mentioned two years after he had been deposed when Šamšī-Adad had seized power in Mari (Fig. 5.13).⁴⁹

There could be explanations for this. The text of S.24-3 is very fragmentary and about 6 cm are missing to the right of the extant text. Clearly there are many possibilities about how the text might be restored. For example, the reference might indeed have been posthumous, referring perhaps to a descendant of Yahdun-Lim or to a past deed of Yahdun-Lim. Alternatively it might refer to a Yahdun-Lim who had not been the king of Mari or to someone else whose name began with Yahdun. It is also possible that Yahdun-Lim might not have been killed when Sumu-yamam seized his throne and might have lived to fight another day.

Such explanations are, however, not necessary: the resolution of this potential difficulty lies in realising that the absolute dates suggested in *FMV* are provisional proposals included to aid discussion and not intended as fixed points and that the length of Šamšī-Adad's control of Mari is uncertain.⁵⁰ The list in *FMV* (157 also 262) of eponyms attested in Mari documents from Aššur-taklāku to Rīš-Šamaš shows that there are two more eponyms early in this list than are included in *V MEC* and *U&D*.

⁴⁸ According to *FMV* (47, 78, 136–137) and repeated by CHARPIN (2004: 145, 152, 188), the death of Šamšī-Adad took place in ca. 1775 UMC, his rule in Mari began in ca. 1792 UMC, and Yahdun-Lim was deposed by Sumu-yamam in ca. 1794 UMC. According to *U&D* (95) Haya-mālik was eponym and Šamšī-Adad's rule in Mari began in 1791 UMC, because they thought the death of Šamšī-Adad took place in 1776 not 1775 UMC and they inserted Ahiyaya between Aššur-mālik and Awīliya.

⁴⁹ It makes no difference whether Šamšī-Adad died in REL 197 or in REL 198 as this just adds an additional eponym to those in his reign.

⁵⁰ This is explicitly stated on page 1: 'On a parfois indiqué des dates absolues comme repères pour aider le lecteur, utilisant par commodité la chronologie dite «moyenne». La précision des dates est d'autant plus grande qu'on se rapproche de la fin de Mari en 1761 ... on ignore toujours la durée exacte de la période de domination par Samsi-Addu' (*FMV*: 1).

<i>FMV</i>			<i>V MEC</i>		REL <i>U&D</i>	
	a	Aššur-taklāku	80	^d <i>A-šur-tak-la-ku</i>	181	Aššur-taklāku
1792	b	Haya-mālik	81	[<i>Ha-ià-ma-l</i>] ^{ik} ²	182	Haya-mālik
1791	c	Šalim-Aššur son of Šalim-Anum				
1790	d	Šalim-Aššur son of Ušranum	82	[<i>Š</i>] ^{a-l} [<i>im</i> - ^d] <i>A-šu[r]</i>	183	Šalim-Aššur
1789	e	Ennam-Aššur				
1788	f	Sin-muballit	83	[^d E] <i>N.ZU-mu-ba-al-li-iṭ</i>	184	Sin-muballit
1787	g	Riš-Šamaš	84	[<i>R</i>] <i>i-iš</i> - ^d UTU	185	Rēš-Šamaš

Fig. 5.14. The sequence of eponyms from Aššur-taklāku to Riš-Šamaš according to *FMV*, *V MEC*, and *U&D* with the dates suggested in *FMV* (262).

FMV c and d (Šalim-Aššur son of Šalim-Anum and Salim-Aššur son of Ušranum), as already suggested in *FMV* (145), are now considered to be a single individual with the difference in the names of the fathers being a simple scribal mistake (*U&D*: 9) and *FMV* e (Ennam-Aššur) is now equated with *warki* Tab-šilli-Aššur the first eponym of the reign of Šamši-Adad's son Išme-Dagan (*V MEC* §3.4c). Four of these eponyms, Aššur-taklāku, Haya-mālik, Sin-muballit, and Riš-Šamaš, occur in the same order on M.7841 with a gap where an entry for the eponym year of Šalim-Anum can be restored (Fig. 5.7). Omitting Šalim-Aššur son of Šalim-Anum and Ennam-Aššur the date for Haya-mālik would be 1790 UMC (Fig. 5.15).

UMC date		LMC date		Eponym	Historical events	MEC	REL	KEL G
start	end	start	end					
9/1792	9/1791	9/1781	9/1780	Atanum	<i>Yahdun-Lim deposed</i>	79	180/181*	
9/1791	9/1790	9/1780	9/1779	Aššur-taklāku		80	181/182*	
9/1790	9/1789	9/1779	9/1778	Haya-mālik	<i>Šamši-Adad in Mari</i>	81	182/183*	
9/1789	9/1788	9/1778	9/1777	Šalim-Aššur		82	183/184*	
9/1788	9/1787	9/1777	9/1776	Sin-muballit		83	184/185*	
9/1787	9/1786	9/1776	9/1775	Riš-Šamaš		84	185/186*	
9/1786	9/1785	9/1775	9/1774	Ibni-Adad		85	186/187*	
9/1785	9/1784	9/1774	9/1773	Aššur-imitti		86	187/188*	
9/1784	9/1783	9/1773	9/1772	Ili-tillati		87	188/189*	
9/1783	9/1782	9/1772	9/1771	Rigmanum		88	189/190*	
9/1782	9/1781	9/1771	9/1770	Ikuppiya		89	190/191*	
9/1781	9/1780	9/1770	9/1779	Asqudum		90	191/192*	
9/1780	9/1779	9/1769	9/1768	Aššur-mālik		91	192/193*	G79/84*
9/1779	9/1778	9/1768	9/1767	Awiliya		92	194	G80/85*
9/1778	9/1777	9/1767	9/1766	Nimer-Sin		93	195	G81/86*
9/1777	9/1776	9/1766	9/1765	Addu-bani		94	196	G82/87*
9/1776	9/1775	9/1765	9/1764	Ṭab-šilli-Aššur	<i>Death of Šamši-Adad?</i>	95	197	G83/88*
9/1775	9/1774	9/1764	9/1763	Ennam-Aššur	<i>Death of Šamši-Adad?</i>	96	198	G84/89*

Fig. 5.15. Revised order and dates of eponyms from Atanum to Ennam-Aššur according to the UMC and the LMC assuming that the list of eponyms is complete. The dates assume that Šamši-Adad died in the autumn of the Julian year 1775 UMC in the middle of the Babylonian year. REL and KEL G dates marked with an asterisk are those of *V MEC*.

According to this scheme the historical note in the eponymy of Atanum in the MEC could have referred to Yahdun-Lim's loss of his throne in Mari. If there were an additional eponym after Haya-mālik, the mention of Yahdun-Lim would have been posthumous.

BLOCH (2014: 197) controversially suggested that the two eponyms mentioned in the Assyrian King List, Ibni-Adad and Atamar-Ištar, dating Šamši-Adad's return to Ekallatum and his conquest of Assur three years later, could be identified with Inbi-Ištar (G55) and Amur-Aššur (G59) (Fig. 5.5). According to the AKL Šamši-Adad ruled Assur for 33 years. The interval between Amur-Aššur and Ṭab-šilli-Aššur is 31 years for Position 1 which could be extended to 32 or 33 years and 36 years for Position 2 which could be reduced to 35 years (see Fig. 5.5). If the death of Šamši-Adad occurred at the beginning of the eponym year after Ṭab-šilli-Aššur and the count included the eponym year of Atamar-Ištar/Amur-Aššur,⁵¹ the length of his reign in Assur might have been reckoned to be 33 years according to Position 1. The historical note attached to the eponym Amur-Aššur begins with the name of Šamši-Adad as do the notes to the previous and following eponyms. One of these might have recorded his conquest of Assur. The uncertainty of the identifications of the eponyms

⁵¹ Or if Ahiyaya was included in the eponym list.

in the Assyrian King List as well as the dubious method of reckoning the interval should make one cautious about accepting this as evidence in support of Position 1.⁵²

Michaël Guichard (2014) has expressed reservations about the accuracy of some of the historical information in the MEC noting that the compilation of the chronicle probably served a political purpose connected with the dynasty of Šamšī-Adad and this could have influenced not only the topics selected but also their verisimilitude. Furthermore, the references to kings of Ešnunna in the MEC appear to contradict the information in an Old Babylonian treaty. This is discussed in *V MEC* §6.2 where possible explanations are proposed. The available sources are not sufficient to prove which, if any, of these explanations are correct and there remains a lingering doubt that the MEC may not be entirely reliable.

The various editions of the MEC were compiled some years after the events recorded in them had taken place and *the* original version was composed in a city or region where years were not dated by Assyrian eponyms (GUICHARD 2014: 23) such as the seat of the dynasty of Šamšī-Adad in or near the Diyala region. The conversion from the earlier dating system to Assyrian eponyms presumably occurred after Šamšī-Adad had captured Aššur and this could easily have led to an error in attributing the solar eclipse to the eponymy of Puzur-Ištar.

Furthermore, the solar eclipse is recorded on a single tablet fragment of the MEC and the reservation that ‘one witness is no witness’ applies to it. The entry recording the solar eclipse may have been misplaced or incorrect. Similarly, the order of the names recorded in lines G20 to G23 and G54 to G68 is found only on KEL G and is not confirmed by any other source. Therefore, if there were an error in the compilation by the omission of one or more eponyms or by the inclusion of names that were in error, the counts of years and the above calculations would be incorrect.

	Death of Šamšī-Adad	65 – 70 – 72 years earlier	Date of eclipse	Comment
UHC	1831	1896 – 1901 – 1903	13 March 1902	at sunrise
			3 May 1896	at sunset
LHC	1823	1888 – 1893 – 1895		
UMC	1775	1840 – 1845 – 1847	5 August 1845	after sunset
			24 March 1838	at sunrise
LMC	1767	1832 – 1837 – 1839	24 June 1833	total eclipse
			14 June 1832	at sunrise
USC	1711	1776 – 1781 – 1783	17 July 1778	before sunrise
			20 Dec. 1768	at sunrise
LSC	1703	1768 – 1773 – 1775	7 July 1769	before sunrise

Fig. 5.16. Eclipses more or less compatible with the MEC and the Venus Tablets. Dates between 65 and 68 years before the death of Šamšī-Adad could be compatible with Position 1, and those between 69 and 72 years could be compatible with Position 2. The most probable date is 70 years before the death of Šamšī-Adad (Position 2 omitting MEC 77³).

From the data in Fig. 5.1 it is possible to pick out eclipses that are more or less consistent with a reconstruction of the Assyrian Eponym List (i.e. between 65 and 72 years before the death of Šamšī-Adad) for the various chronologies compatible with the Venus Tablets (Fig. 5.16). Some of the others are unlikely candidates occurring before sunrise or after sunset. These include the eclipses compatible with the UMC and the USC.⁵³

Since on the basis of radiocarbon determinations the High and Short Chronologies can be excluded (for discussion see ROAF Ch. 13 in this volume), this leaves only the LMC for which there are three possibilities. The eclipse that took place at sunrise on the 14 June 1832 would be compatible with Position 1 if an eponym were omitted but as we have seen Position 1 is not compatible with the sequence of eponyms in M.7841: it could be compatible with the unlikely Position 1.1 omitting two eponyms. This seems impossible. The eclipse that took place at sunrise on the 24 March 1838 is compatible with Position 2, which included the dubious eponym MEC 77³. If that eponym were omitted, an additional eponym would have to be inserted. It is certainly possible that this eclipse could have been observed in the ten minutes after sunrise, but it is doubtful whether it would have been thought significant enough to have been thought worthy of inclusion in the MEC.

The only one of these possible eclipses that was visible in the daylight is that of 24 June 1833. This is not compatible with Position 2. It could be compatible with Position 1.1 or Position 1.2 omitting an eponym. Neither of these proposals

⁵² See *V MEC* §6.1 and KRYSZAT (Ch. 8 in this volume) for cogent arguments against Bloch's identifications. LACAMBRE (Ch. 7 in this volume) suggests that the names of these eponyms in the AKL are fictitious chosen for their programmatic value.

⁵³ DE JONG (2016–2017) provided additional arguments against the UMC and the identification of the MEC eclipse with the 1845 BC eclipse based on the reconstruction of the sequence of Old Assyrian intercalary months by Edward STRATFORD (2015).

are convincing. Rather than accepting one of these unlikely restorations of the MEC or the unremarkable partial eclipse five years earlier, it is, in my opinion, preferable to assume that the mention of the ‘darkening of the Sun’ was incorrectly dated to the eponymy of Puzur-Ištar and should have been attributed to an eponym four years later. It may perhaps have been inserted at this point to draw attention to the significance of the birth of Šamši-Adad and the death of Amīnum.

The eclipse on 24 June 1833 BC would have been observed and would have been remembered. So it is tempting to think that the eclipse of 1833 was the eclipse referred to in the MEC⁵⁴ but was not correctly dated to the eponym of Puzur-Ištar. The most probable reconstruction of the MEC makes the eponymy of Puzur-Ištar within four years of the 1833 BC eclipse if the Lower Middle Chronology is followed. According to the Upper Middle Chronology the most probable reconstruction of the MEC makes the eponymy of Puzur-Ištar more than ten years earlier than the 1833 eclipse. Some scholars may interpret this as support for the LMC rather than the UMC. Other scholars may see support for the LMC in the fact that a plausible reconstruction of the MEC makes the eponymy of Puzur-Ištar contemporary with the 1838 BC partial eclipse at sunrise.

The so-called Miracle of the Sun, when on 13 October 1917 a large crowd at Fátima, Portugal, reported that the Sun appeared ‘to dance or zigzag in the sky, to advance towards the Earth, or emit multicolored light and radiant colors’ for approximately 10 minutes (https://en.wikipedia.org/wiki/Miracle_of_the_Sun), suggests that eyewitness testimony of solar events may not always be reliable.

The conclusion that I draw from the previous deliberations is that it is rash to place much confidence in the reference to a ‘darkening of the Sun’ in the Mari Eponym Chronicle in the search for a precise and accurate solution to the problem of determining the absolute chronology of the period: there are too many uncertainties and too great a possibility that the association of the event with the eponym is not correct. It is better to investigate the absolute chronology of the first half of the second millennium BC through other more methodically reliable approaches (for examples see ROAF Ch. 13 in this volume).

Abbreviations

AKL	Assyrian King List (see GRAYSON 1980–1983)	REL	Revised Eponym List (see <i>U&D</i> : 91–97)
FMV	<i>Florilegium marianum</i> V (CHARPIN/ZIEGLER 2003)	SAA 10	State Archives of Assyria 10 (PARPOLA 1993)
HC	High or Long Chronology	SC	Short or Low Chronology
KEL A	Kultepe Eponym List A (see VEENHOF 2003 and 2007)	TP 1-12	Text Passages in MEC (see Fig. 5.4)
KEL G	Kultepe Eponym List G (see GÜNBATTI 2008)	<i>U&D</i>	<i>Ups and Downs at Kanesh</i> (BARJAMOVIC/HERTEL/LARSEN 2012)
LHC	Lower High or Lower Long Chronology	UHC	Upper High or Upper Long Chronology
LMC	Lower Middle Chronology	UMC	Upper Middle Chronology
LSC	Lower Short or Lower Low Chronology	USC	Upper Short or Upper Low Chronology
MC	Middle Chronology	<i>V MEC</i>	VEENHOF 2025 Chapter 4 in this volume: references are to the section numbers.
MEC	Mari Eponym Chronicle (see ZIEGLER 2025 and VEENHOF 2025 Chapters 3 and 4 in this volume)		

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⁵⁴ As already suggested by MICHEL 2000 on the basis of radiocarbon dates that have proved to be incorrect. SALLABERGER/SCHRAKAMP (2015: 8–11) also favoured the 1833 BC eclipse and realised that this provided support for the Lower Middle Chronology without being able to reconcile the date exactly with the Venus Tablets of Ammi-šaduqa.

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6. Collations and confabulations: KEL G revisited

GOJKO BARJAMOVIC

Proposed changes to the REL sequence presented in BARJAMOVIC/HERTEL/LARSEN 2012 are addressed through collation and their consequences debated for an understanding of the Assyrian commercial system in Anatolia and its fate in the 17th century BC.

Joining textual and dendrochronological data to the archaeological stratigraphy of the site of Kültepe in Central Anatolia, the first chapter of the book *Ups and Downs* at Kanesh presented a body of evidence in favor of the so-called standard Middle Chronology.¹ It argued that the low chronologies that had been the focus of much debate in recent decades are rendered unlikely by data coming out of Kültepe.

The promise of putting the absolute chronology of the Bronze Age on a less uncertain footing led to a careful dissection of its arguments. Several modifications have been proposed to the relative eponym chronology presented by the book. More fine tuning is found within the covers of the present volume, and the debate is likely to continue for some time. After a decade, however, the overall conclusion of *Ups and Downs* seems to hold: *either* the dendrochronological sequence of the East Mediterranean and/or the radiocarbon curve that ties into its formation are wrong *or* the High and Low chronologies are wrong.

Tweaking the length of the internal chronological sequence tends to be on the level of a few years and the overall system can be kept in check by astronomical observations. Yet, the backbone to all these discussions is the reconstructed sequence of Assyrian *limmums* recorded in the manuscript KEL G from Kültepe that was first published by Cahit Günbattı.² The text preserves a sequence of 143 officials, who held annual tenure in the city of Assur during the Middle Bronze Age. Their names were used to date commercial and legal records, and in some cases, they recur in commercial records across northern Syria and Iraq where *limmums* were used to date transactions.

Unfortunately, the sequence provided by the KEL manuscript is incomplete. The tablet bears tell-tale marks of an excavation tool whose impact may have shattered the bottom of columns I and II and the top of columns III and IV. The tablet also seems to have been twisted by the immense pressure it saw in the ground and a thin layer of lacquer was applied to the entire tablet during conservation to preserve its integrity. An unfortunate side effect of the varnish is that it makes reading and in particular photography exceedingly difficult due to the way the tablet now reflects light.

A close physical collation and detailed measurements were conducted in 2009 in preparation of the Revised Eponym List (REL) proposed in *Ups and Downs*. The REL proposed a reconstructed sequence of 255 *limmums* in total, using the physical dimensions of the tablet and a reconstructed sequence of *limmums* as a guideline. The physical reconstruction of what the tablet may originally have looked like is particularly controversial and high-resolution close-up photos of the tablet would be highly desirable to help decide between some of the many emendations that have been proposed since 2008.

I took new photos in 2013 with a hand-held digital camera. Hakan Erol kindly took some shots for me later in 2013. Edward Stratford and Adam Anderson spent more time on the tablet in 2015. In the same year, Fikri Kulakoğlu kindly spent an entire day with me in Ankara during his dig at Kültepe to allow me to photograph the important tablet one more time. Taha Yurttaş revisited the text in 2021 with a camera stand and raking light.

Unfortunately, none of these sessions were carried out by trained object photographers and in most cases our results did not lead to a great leap in understanding ambiguous passages. A project to have cultural heritage documentation specialist Hendrik Hameeuw work on the text was planned and funded in 2014 but rejected by the museum in Ankara.

¹ BARJAMOVIC/HERTEL/LARSEN 2012. Hereafter *Ups and Downs*.

² Kt 01/k 287 GÜNBATTI 2008.

The present chapter therefore continues to work from those imperfect photographic collations that have been undertaken since the publication of *Ups and Downs* in 2012. I am grateful to all the colleagues mentioned above for sharing their images. Owing to their work and team spirit, some progress can be made while the world waits for better documentation of this key object for our understanding the relative and absolute chronology of the Bronze Age.

1. Counting marks

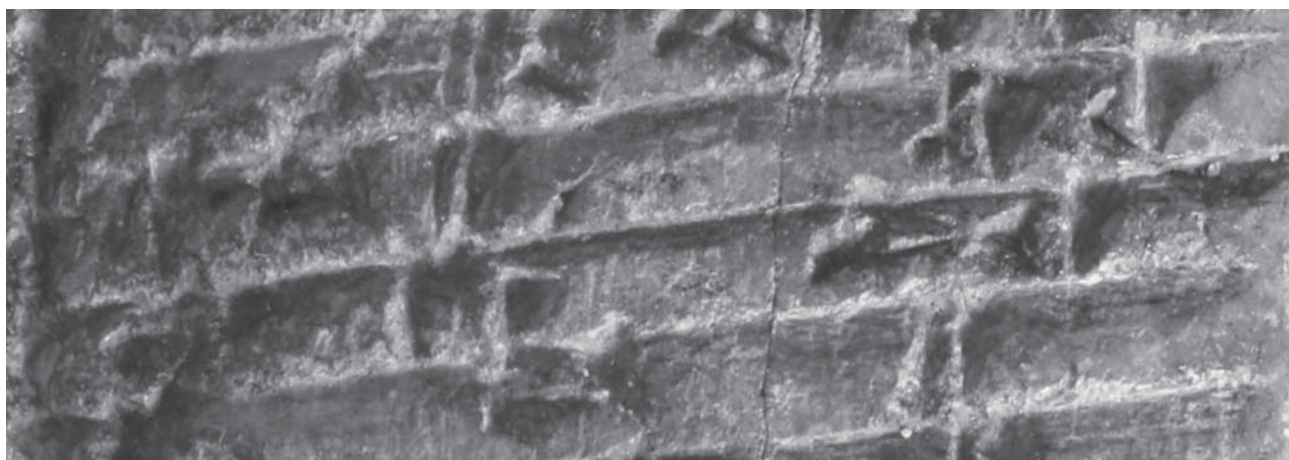


Fig. 6.1. KEL G col. IV 131–134. The proposed tally mark is the remains of an erasure (KEL G 132). Photo by T. Yurttas 2021.

An attempt to shorten the sequence proposed in *Ups and Downs* by a few years was presented by Werner Nahm as part of a longer article arguing for a lowering of the Middle Chronology. NAHM (2013: 367) proposed the existence of ‘counting marks’ added by the author of KEL G as an aid to keep track of every sixty names on the list. Where the REL sequence in *Ups and Downs* for various reasons proposed nine names missing in the break of KEL G col. III, Nahm’s proposal to identify the vertical wedge at the end of line KEL G 132 (REL 244) as a tally mark would make REL 244 into REL 240 and effectively shorten the sequence of *limmums* in the manuscript. The proposed shortening of the sequence allowed him to posit a 66-year interval between the MEC solar eclipse and the death of Šamši-Adad I and thereby link the internal chronology of KEL G to the absolute chronology of the solar eclipse.

Photos show that the author of KEL G at first mistakenly wrote the name *Pu-ha-num* – the following *limmum* in the sequence – in the line that should have contained Ahiyaya’s name. He subsequently made an erasure to write in the correct name on the line but did not completely remove the traces of the erroneous entry. The final vertical that NAHM 2013: 367 took to be a tally mark was in fact the last vertical of the previous NUM. This also confused readings for GÜNBATTI 2008 and *Ups and Downs*. There is no tally mark at the end of line KEL G 132 = REL 244 or anywhere else on the tablet for that matter (Fig. 6.1). This is a great pity, since it could potentially have linked the relative sequence of the REL to the absolute chronology once and for all.

Nahm additionally proposed to read the name *A-hi-a-a* in line KEL G 132 where he observed the tally mark. This reading (not the tally mark) is confirmed by collation and solves another crux of the discussion in *Ups and Downs* (§1.2.3 pp. 9–13). Placing Ahiyaya (Ahiyaya #3, son of Adunaya, in *Ups and Downs*) in the preserved part of KEL G col. IV removes him from the preceding break and effectively reduces the REL sequence by two by eliminating Ahiyaya in the break as well as emending the erroneously read **A-hi²-a-tal* of REL 244 in *Ups and Downs*.

The second (horizontal) tally proposed by NAHM 2013: 367 after KEL G Aššur-muttabbil (*A-šūr-mu-ta-bi₄-il₅*) turns out to be the line divider broken by the final IL₅ (Fig. 6.2).

2. *Puzur-^dUTU

In his short note in N.A.B.U., NAHM 2015 attempted further progress with KEL G and proposed a series of adjusted readings of the sequence. For KEL G 57 (REL 167) (Fig. 6.3) he suggested that the name should be read *Puzur-^dUTU against GÜNBATTI 2008 Púzur-^d{MUŠ}, although he rejected the proposal of KRYSZAT (2008: 212) to read G 124 as Puzur-Šamaš instead of GÜNBATTI’s Púzur-^d{M}[UŠ]. The passage had in the meantime been collated for *Ups and Downs* (p. 23 with n. 67) with some room for doubt. Nahm’s proposal would insert the *limmum* Puzur-Šamaš (whose name is attested

in Kt 01/k 244, 298 and 306) into the preserved part of the KEL G sequence and remove him from the possible ‘post-canonical’ *limmums*.

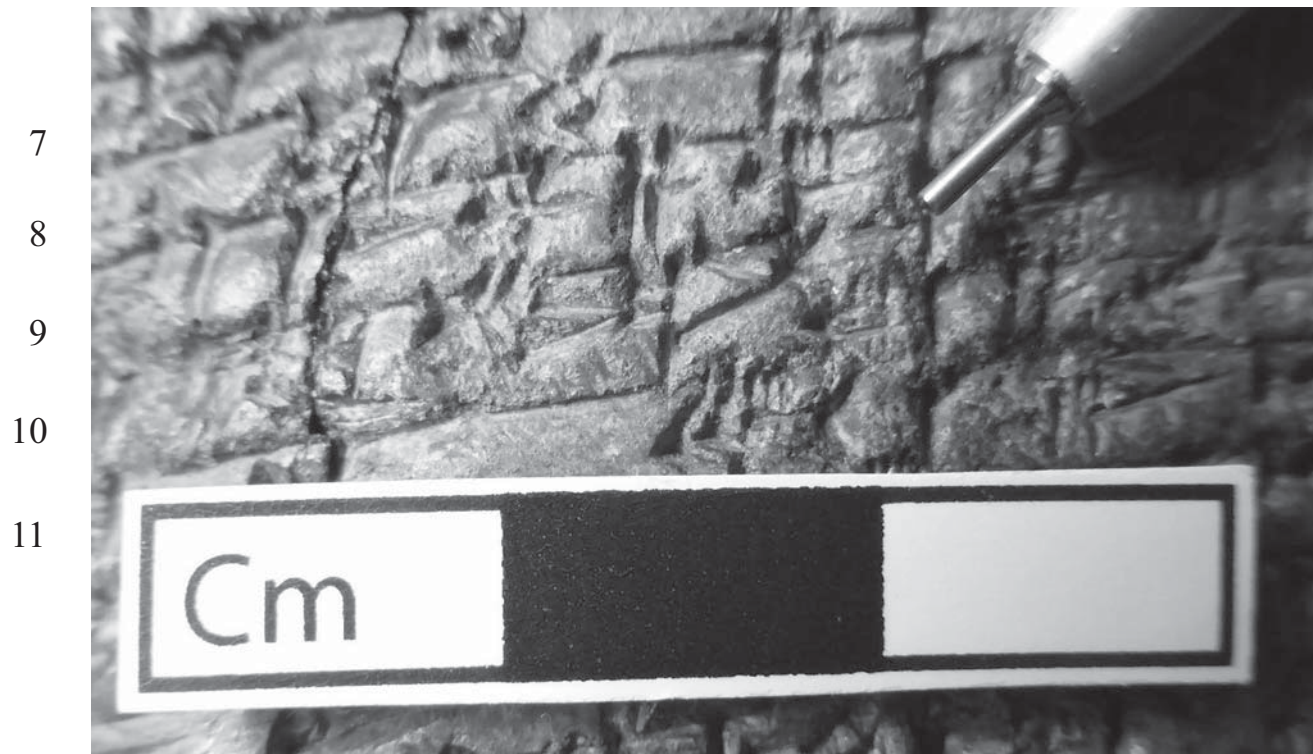


Fig. 6.2. KEL G col. I lines 7–11. Horizontal counting mark is part of the line divider (KEL G 10). Photo by author 2015.



Fig. 6.3: The ends of lines G 55 to G 59 in col. I. The proposed *Puzur-^dUTU (KEL G 57) is Puzur-^dMUŠ. Author’s photo 2015.

None of the three texts dated to the *limmum* Puzur-Šamaš have been published in the intervening years. This renders a prosopographic assessment impossible. Collation from photos suggests keeping the reading ^dMUŠ for the time being. There seem to be too many wedges for the proposed ^dUTU unless one imagines the sign being written on top of an erasure as in the example of Ahiyaya son of Adunaya. Traces fit the ^dMUŠ written in KEL G 11 (see Fig. 6.2) reasonably well.

3. Haya-malik and Šalim-Aššur

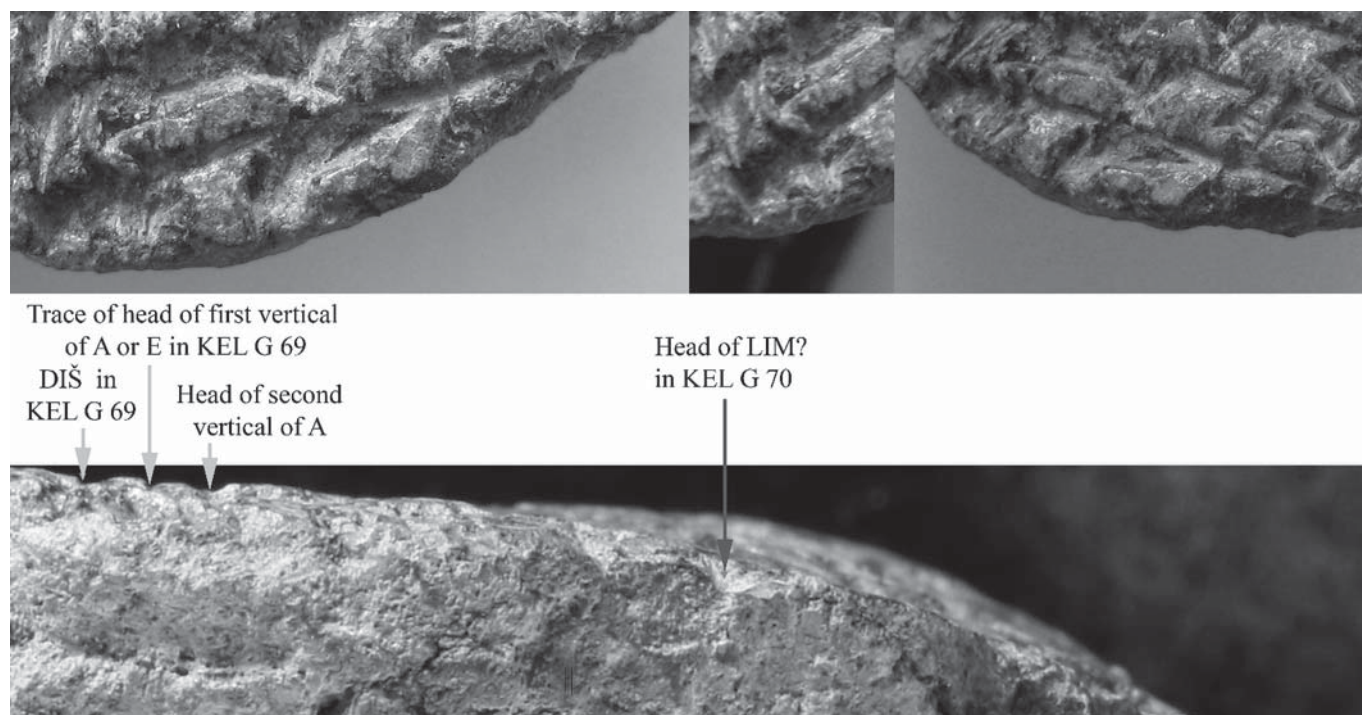


Fig. 6.4: The end of col. II. Photos by H. Erol 2013 and author 2015.

The last partially preserved line on col. II (KEL G 69) was initially read *A-ni-x¹*[...] by GÜNBATTI 2008. Charpin and Ziegler proposed *A-NI-m[a-lik]* and equated it with the *limmum* Haya-malik son of Dudānum attested at Mari.³ Collations for *Ups and Downs* suggested *E-nam-d*[...] and joined it with MEC D 2 [...] *-dA-š[ur]* for the name **Ennam-Aššur*.⁴

The proposal in *Ups and Downs* was wrong and much clearer images allowed NAHM 2015: 369 to reinstate *E/HA-NI-m[a ...]* and reject the proposed link with MEC D 2. It also allowed him to question the position of the fragment S.24-3 from Mari and to open up the chronology to make room for Išar-Lim in the sequence of historical events.

Collations show that the first sign is too broken to firmly distinguish A from E. It is unlikely to be HA. A horizontal head in front of what could be the first vertical of A is either that of a line divider (see e.g. Fig. 6.5) or the uppermost horizontal wedge of E. The second sign is also broken but the traces suggest a NI. The hand-copy published by Nahm gives a fairly precise rendering of what remains of the second and third sign on the line with the addition of a vertical at the beginning of MA.

For KEL G 70, NAHM 2015 proposed to see the head of a vertical wedge intruding into the line above and suggested reading *lim* for [¹*Ša-l*]*i[m-A-šur]*. Photos confirm that this is possible. The tiniest traces of the line divider are visible on both sides of what appears to be the head of a vertical wedge. Fig. 6.4 suggests that there might be traces of several more heads visible right on the edge of the break below NI, but an image of the tablet taken from below shows that those are merely dents.

4. Aya, *Zaya, Zizaya and the wild world of vertical wedges

GÜNBATTI 2008 read KEL G 107 (REL 221) on col. IV as *A-a-a*. KRYSZAT 2008 and NAHM 2015 working from photos preferred *Za⁽¹⁾-a-a*. Collations for *Ups and Downs* followed Günbatti and also later photos support his reading. Nothing seems to be missing on the tablet. The fact that the renditions of the sign A in KEL G 101 *A-a-a* all look different as pointed out by Nahm suggests that there was no consistent way to render the vertical wedges as compared to ZA.

The name in the following line (KEL G 108 *Zi-za-a*), which GÜNBATTI 2008 read as *Zi-zi¹(A)-a*, does in fact seem to read *Zi-za-a*. The reading is supported by the occurrence of a *limmum* Zizaya son of Abinara at Ališar. The close-up photo shown here as Fig. 6.5 showcases the degree of variability found in the same simple sign on KEL G even when immediately adjacent.

³ See KRYSZAT 2008: 211.

⁴ BARJAMOVIC/HERTEL/LARSEN 2012: 8–9, 99, 112, 114.

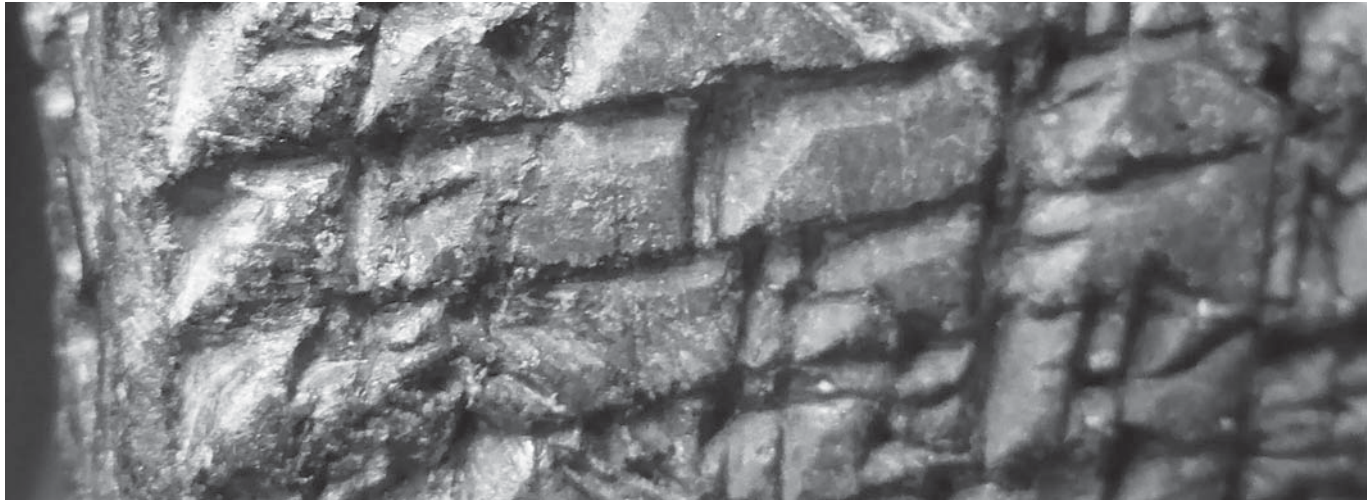


Fig. 6.5. Aya and Zizaya (KEL G 107 and G 108). Photo by author 2015.



Fig. 6.6. First names of col. IV (KEL G 123–125). Photo by author 2015.

5. First names of col. IV

GÜNBATTI (2008: 109) proposed reading KEL G 124 as $Púzur^{-d}M[UŠ]$. KRYSZAT (2008: 212) suggested *Puzur-Šamaš*. Collations for *Ups and Downs* (REL 236) did not resolve the issue and the line was simply left as *Puzur₂-^d[D]N* in the final table on p. 96 with possible readings IM and IŠTAR on p. 114. The proposal by NAHM 2015 to read *Puzur₂-Š[ú]-e[n₆]* also seems possible according to what remains after the broken determinative (Fig. 6.6). Texts dated to a *limmum* Puzur-Suen are not known from chronological contexts where one might expect them (Boğazköy, Leilan, Rimah).⁵ The issue remains unsolved for the moment.

The following name KEL G 125 (REL 237) was read *Šu-mì-x-x-x-x* by GÜNBATTI 2008 and *Šu-mì-^d1NIN1.ŠU[BUR]* (ILABRAT) in *Ups and Downs* p. 114. NAHM 2015 rejected the proposal, but photographic collation shows that NIN and ŠUBUR are correct.

⁵ BARJAMOVIC/HERTEL/LARSEN 2012: 22.

6. The last *limmums*



Fig. 6.7. The last *limmums* in col. IV (KEL G 141–143). Photo by E. Stratford and A. Anderson 2015.

The penultimate name in the sequence recorded on KEL G (G 142) is difficult to read (Fig. 6.7). The surface of the tablet is worn and there are a few parts of the surface missing. GÜNBATTI 2008 read *Ha²-da²-x-a*. *Ups and Downs* (p. 114) proposed *A-da-ri²-A-šûr* (with a typo for *šur₁*). NAHM 2015 corrected the typo, agreed to read the first sign as A instead of HA, rejected the reading of the second sign as DA, and proposed to read the following signs as A plus a divider instead of RI.

Collation shows that the first sign is indeed A (worn). The second sign is certainly DA. The third and fourth signs are A and a divider. This gives the unusual *A-da-a : A-šur*. Nahm's proposal that the signs after the divider were meant to clarify the line below seems unnecessary. The last two syllables of KEL G 143 *A-na-ak-a-na-A-šur* are clearly legible on the right edge of the tablet. His suggestion that the penultimate *limmum* may be the same as *A-ha-na-{a}-{x}* on Kt 73/5 is attractive. That text is unpublished, and the name should be collated. The name Adaya appears a handful of times in texts from Kültepe and Boğazköy; Hadaya is even more common. Being the penultimate name on the list, and presumably therefore of continued relevance for keeping track of transactions, perhaps the writing *A-da-a : A-šur* was included to disambiguate him from other individuals with the same name. This would correspond to the tendency of manuscript KEL A to attach additional identifiers by origin or title to some of the names on the list: 'the priest', 'skipper', 'of Nerabtum', etc.⁶

7. The colophon and the end of the Assyrian settlement at Kaneš

The final section of KEL G consists of a colophon and gives seven witnesses who had presumably been asked to verify the accuracy of the list. Piotr Michalowski is preparing a monograph on the Goddess Nisaba that has necessitated a reexamination of the passage. A discussion of this collation will appear in the context of his monograph.

The colophon ends with the date: 'First day of the month Ab-šarrāni (V) of the year following (*ša qá-at*) *A-na-ak-i-na¹-A-šûr*.' The variant writing of the name of the *limmum* is presumably an error, since Kt 73/k 15 likewise gives *A-na-ku-a-na-A-šûr*. The continued use of the successor (*ša qāte*) in month V is unusual,⁷ but it does not necessarily point to diminished contacts with Assur. It could also be that the lunar calendar had drifted through the solar year. The author of KEL G is clearly Anum-ennam, and not *Ennam-Sîn as given by GÜNBATTI 2008: 110 in the main text.

NAHM 2015 raised the possibility that the small number of known 'post-canonical' *limmums* (i.e. names of officials who do not appear in KEL G, and who must be placed after the end of its sequence) might indicate that KEL G was written with the end of the colony in mind.⁸ I see little reason to assume that this was the case and Nahm is surely correct in his recent observation that:

⁶ LARSEN 2015: 125.

⁷ STRATFORD 2015.

⁸ NAHM 2015.

‘For someone with statistical experience the section 227–235 on the graph on page 56 of *Ups and Downs* sticks out like a sore thumb. Nine years of eponyms with exactly one attestation is not credible as a reconstruction. The removal of Ahiyaya improves things a bit but not enough. One can be quite sure that at least two or three of those eponyms are post-KEL G.’⁹

The reconstruction of the length of the break between cols. III and IV in *Ups and Downs* is likely a couple of *limmums* too long. The breaks in KEL G cols. II to III and III to IV cause some uncertainty regarding where one moves backwards or forwards. There is every reason to assume that those breaks will be filled out by new documentation from Kültepe or elsewhere to produce the complete sequence. The settlement at Kültepe certainly continued for another couple of generations after the end of KEL G and the archaeological stratum (Lower Town Level Ia) above the one which produced it (Lower Town Level Ib) contains artefacts from Mesopotamia mixed in with local Anatolian ones.¹⁰ The suggestion that the Kanešite court moved to Hattuša under Hattušili I would entail that the settlement continued to function for as much as a century after Anāku-ana-Aššur held office.¹¹ Although there is currently no proof that Assyrian trade with Anatolia continued into the period of the early Hittite state, there is now a letter from Northern Mesopotamia that shows the main crossing used by the Assyrians on the River Euphrates at Šimala in the 19th century was an ongoing concern to its ruler in the mid-17th century BC.¹² Unless one were to posit that the Assyrian commercial network had shrunk to include only ports south of the Taurus (e.g. Hahhum, Zalpa, Uršu and Mamma), then this letter suggests that business in some form continued at least half a century beyond the sequence given by KEL G.

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⁹ Personal communication by email dated 21 Jan 2022.

¹⁰ BARJAMOVIC/HERTEL/LARSEN 2012: 40, 51–52, 75.

¹¹ KLOEKHORST 2019.

¹² GEORGE 2017: 100.

7. New data about two eponyms of the time of Samsī-Addu: the cases of Haya-malik and Ahiyaya

DENIS LACAMBRE

In this chapter, a new study of two eponyms Haya-malik and Ahiyaya and the problem of their place in KEL G is being undertaken. The first one, Haya-malik, is well attested in the archives of Mari and is the date of the conquest of this city by Samsī-Addu. Thanks to a collation of photos of KEL G (= Kt 01/k 207) and the help of *FM V 2*, the eponym Aššur-taklāku should actually be read instead of the eponym thought to be Haya-malik. Haya-malik immediately follows this last eponym in the missing part of the KEL G.

The second eponym studied here is Ahiyaya whose existence as a complete full year is now beyond doubt thanks to new data from Chagar Bazar (former Ašnakkum). For now on, even if some problems remain notably due to the problem of synchronisms between Ešnunna, Mari and ‘Samsī-Addu’ calendars, the only place for Ahiyaya is between Aššur-malik and Awīliya. This proposal is not forbidden neither by MEC nor by KEL G. Based on these new proposals, Samsī-Addu would have lived for 70 years.

The *Kultepe Eponym List G* (= KEL G; Kt 01/k 207), published by C. Günbattı in 2008 is a very important document for the chronology of the early second millennium BC. Unfortunately part of this tablet is missing and the list is especially broken between the end of column II and the beginning of column III,¹ which corresponds in part to the eponyms of the Kingdom of Upper Mesopotamia and especially to the domination of Samsī-Addu in Mari.

The place of two eponyms of the time of the Upper Mesopotamian Kingdom is still problematic. The first one, Haya-malik, is well attested in Mari and is the date of the conquest of Mari by Samsī-Addu. The second one is Ahiyaya, which is well attested in Chagar Bazar (former Ašnakkum), but whose existence as a complete full year is still subject to doubt for some researchers.

My study aims to propose or confirm the chronological placement of both Haya-malik and Ahiyaya among the contemporary eponyms of the time of Samsī-Addu. Then I will propose, on this basis, a new reconstruction of the KEL G, called ‘revised KEL G’ or R-KEL G from the beginning of the tablet until the death of Samsī-Addu in Ṭāb-šilli-Aššur, in R-KEL G 85.

1. The problem of the place of Haya-malik

1.1. *Haya-malik, the eponym dating the taking of Mari by Samsī-Addu*

The date of the conquest of Mari by Samsī-Addu in Haya-Malik was established by D. Charpin on the basis of a group of around 20 administrative texts dated by this eponym which register the booty made by Samsī-Addu.² The problem is that this eponym is mentioned neither in the *Mari Eponymal Chronicle* (= MEC) nor in the KEL G. The only mention of this eponym with other eponyms is on the tablet *FM V 2* (Archibab T6499), found in Mari but certainly written elsewhere (cf. below § 1.2).

1.2. *The tablet FM V 2, a summary of arrears from Ekallātum*

The tablet *FM V 2*, found in Mari, is a summary of arrears owed by certain people, in chronological order from Aššur-taklāku to Rīš-Šamaš:

¹ GÜNBATTI 2008: nos. 70–78. In my new chronological proposal the lacuna corresponds to a loss of 11 lines, and to R-KEL G 70–80, cf. Appendix 1 below.

² Cf. CHARPIN 1983; CHARPIN/ZIEGLER 2003: 78–79, § 2.1.1; see lastly ZIEGLER 2021b, especially 491–492 for a list of dated Haya-malik tablets from Mari.

2'	[li-mu ^d]a-šur-ták-[la-ku]	R.4'	[li]-mu ^d EN.ZU-mu-[ba-li-iṭ]
9'	li-mu ha-ià-[ma-lik]	L.E.2''	[li-mu ri-iš]- ^r dUTU ¹

The first editors of this tablet thought this tablet was written in Mari. The only argument in favor of this last hypothesis was the supposed mention of a town called Sanipātum (l. 3')³, which was known for being in the region of Mari, in the district of Saggarātum.⁴ But in fact, a collation of the published photo (see Fig. 5.6) allows instead to simply read the toponym Sanipā (l. 3': *sà-ni-pa-a*^[ki]), a town not far from Ekallātum.⁵ The passage reads: (3') 6* SAG.İR LÚ *sà-ni-pa-a* ^[ki] (4') LÁ.U *ha-mi-ta-nu* LÚ AD.[KID ...], that is: '6* male slaves (from) the 'man' of Sanipā; arrears of Hammi-tanu, the reed-[worker ...].'

In fact, I think this tablet was written outside the region of Mari. Furthermore, Lama-saga quoted in the text (L.E. 1'') is known as the steward of Yasmah-Addu in the region of Ekallātum as D. Charpin and N. Ziegler reminded us.⁶ So it seems clear that this tablet was written in Ekallātum and recorded arrears due to Yasmah-Addu, then brought by him to Mari when he was sent there by his father. It is not a coincidence that **FM V 2** is dated by Rīš-Šamaš, the supposed date of Yasmah-Addu's arrival in Mari.⁷

By way of comparison, we can quote **M.5681** (*ARM XXX*: 205–208; cf. below Fig. 7.4), a text of the same type which, according to J.-M. Durand, recorded the management of fabrics and shirts for 5 years, from Aššur-imitti to Asqūdum, in the properties of Yasmah-Addu in Upper Jezira.

Outside Mari, for this type of practice, there is a clear parallel in the archives found in Chagar Bazar. Among the tablets discovered by M. Mallowan in the 1930s, **OBTCB 25**, **OBTCB 84** and **OBTCB 90** are texts related to properties of Sîn-iqīšam (head of the region of the Habur triangle),⁸ in Ekallātum and elsewhere (Dūr-Zamurim and Haštariwe).⁹ Sîn-iqīšam therefore had at his disposal a certain number of tablets which allowed him to monitor the management of his various domains as did also Yasmah-Addu in Mari after being named king of this city.

1.3. A new reading of **KEL G 69** = *Aššur-taklāku*

It is now possible to solve the problem of the reading of **KEL G 69** (*A-ni-^rx¹-[...]*). We know now, with **FM V 2**, that Aššur-malik is the eponym just before Haya-malik. I think it is possible to read (on the photo) **KEL G 69** as: *a-šur¹*-tá[k*-lá-ku]*.¹⁰ It means that **KEL G 70**, which is broken, is Haya-malik. My proposal has the advantage to getting rid of all the problems of reading the name Haya-malik in **KEL G 69**. A reading Aššur-taklāku fits much better there. So we have now a new order for **KEL G** from **KEL G 69** onwards that I called 'revised **KEL G**' or R-**KEL G** (cf. Appendices 1 and 2 below):

KEL G 68 :	Dadaya
R- KEL G 69 :	Aššur-ta[klāku]
R- KEL G 70 :	[Haya-malik]
R- KEL G 71 :	[Šalim-Aššur]
R- KEL G 72 :	[Suen-muballiṭ], etc.

All the historical consequences of this new proposal will not be studied here, but it means that Samsī-Addu died at the age of 70 (cf. § 4 and Appendix 1 below).

2. The case of Ahiyaya

2.1. *Eponyms of the time of Samsī-Addu in Chagar Bazar*

Hundreds of new texts have been discovered in Chagar Bazar (Syria), the former city of Ašnakkum, since the resumption of archeological work in 1999 by a joint mission (*Mission archéologique de l'université de Liège en Syrie* [directed by

³ CHARPIN/ZIEGLER 2003: 72.

⁴ *MTT* I/1: 302, s.v. Sanipatum.

⁵ *MTT* I/1: 301–302, s.v. Sanipa; ZIEGLER/OTTO/FINK 2023: 167, fig. 12 and 169b–171a, § 5.E.7 suggest identification with Tell Khamira/Hamira around 90 km north of Ekallātum. In comparison Sanipātum is always written *sà-ni-pa-tim^{ki}* without an extra sign A after PA.

⁶ CHARPIN/ZIEGLER 2003: 72 and n. 258.

⁷ CHARPIN/ZIEGLER 2003: 79–80, § 2.1.2 and 145.

⁸ LACAMBRE 2019: 529–530, § 3.4.

⁹ LACAMBRE 2010: 102–103, § 1.4.

¹⁰ The sign TÁK seems quite clear on the photo. Compare this spelling with lines 60 and 94 (former **KEL G 92**). I would like to thank G. Barjamovic for sending me new photos of kt 01/k 207. However I do not follow the collations cited in BARJAMOVIC/HERTEL/LARSEN 2012: 114. Aššur-taklāku is one of the three attested eponyms of this name, cf. BARJAMOVIC/HERTEL/LARSEN 2012: 116.

Prof. Ö. Tunca] and Direction Générale des Antiquités et des Musées de la Syrie). The tablets found between 2000 and 2002 were published in TUNCA/BAGHDO 2008. The tablets found between 2005 and 2011 are still unpublished but are under study and will be published soon.

All these tablets date mostly to the time of the Kingdom of Upper Mesopotamia, as do the tablets found by M.E.L. Mallowan in the 1930s, now all published in *OBTCB* (*Old Babylonian Texts from Chagar Bazar* = TALON 1997). For this reason, the Chagar Bazar tablets are very important for studying the chronology of this period.

The eponyms attested in Chagar Bazar are indicated in bold in Fig. 7.1 below. They are all contemporary with the Kingdom of Upper Mesopotamia of Samsī-Addu (except one, Ennam-Aššur, post-Samsī-Addu):

Eponym	R-KEL G	OBTCB	CB 2000-2002 (= CB III)	CB 2005-2011 (unpublished)	Total
Aššur-taklāku	69	unattested	unattested	unattested	0
Haya-malik	70	unattested	unattested	unattested	0
Šalim-Aššur	71	unattested	unattested	unattested	0
Suen-muballit	72	unattested	unattested	unattested	0
Rēš-Šamaš	73	unattested	unattested	unattested	0
Ibni-Adad	74	unattested	unattested	unattested	0
Aššur-imitti	75	unattested	unattested	unattested	0
Ilī-elli	76	2 texts	1 text	1 text	4
Rigmānum	77	unattested	unattested	unattested	0
Ikūn-pīya	78	1 text	1 text	unattested	2
Asqūdum	79	unattested	11 texts	15 texts	26
Aššur-malik	80	1 text	unattested	unattested	1
Ahiyaya	81	3 texts	10+11 texts	4 texts	28
Awīliya	82	7 texts	20+28 texts	9 texts	64
Nimar-Suen	83	4 texts	66 texts	13 texts	83
Addu-bāni	84	68 texts	14 texts	2 texts	84
Ṭāb-šilli-Aššur	85	unattested	unattested	unattested	0
Ennam-Aššur	86	unattested	5 texts	23 texts	28

Fig. 7.1: Dated texts from Chagar Bazar¹¹ (cf. LACAMBRE/MILLET-ALBÀ 2008: 157, table 8.2 & unpublished Chagar Bazar data).

As Fig. 7.1 shows, most of the attestations date to the end of the period. The tens of texts discovered between 2005 and 2011 and still unpublished confirm this tendency.

No eponyms before Ilī-elli or after Ennam-Aššur are attested in the currently known texts from Chagar Bazar. Ennam-Aššur is dated after the death of Samsī-Addu, which is supposed to have happened in month xii* in Ṭāb-šilli-Aššur.¹²

2.2. The problem of Ahiyaya, son of Lā-qēpum (= Ahiyaya #1)

One of the eponyms which seems to cause a real problem is Ahiyaya #1. Ahiyaya is attested three times¹³ as an eponym in the Old Assyrian chronology:

- Ahiyaya #1 (son of Lā-qēpum): R-KEL G 81 (cf. Appendix 1 below).
- Ahiyaya #2 (son of Takiki): R-KEL G 91.
- Ahiyaya #3 (son of Adunaya):¹⁴ KEL G 132 (maybe now R-KEL G 134).

I will present below the published informations about Ahiyaya.

2.2.1. Ahiyaya #1 in Chagar Bazar

Ahiyaya was first attested by three texts in the archive of grain discovered by M. Mallowan (*OBTCB* 32, 52 and 73). Ahiyaya is now well attested by the new texts (CB 2000–2002 and 2005–2011) from the beer archive (especially the beer for the harem of Sîn-iqīšam, the head of the region in the time of Samsī-Addu).¹⁵

¹¹ Texts with missing eponym dates are not taken into account here.

¹² Cf. CHARPIN/ZIEGLER 2003: 136–138, § 2.9.2: see especially the quotation of the letter **M.7597** [unpublished; reference **M.7595** was given by mistake as N. Ziegler confirmed to me in August 2024] dated from the month xii*, saying that Samsī-Addu is very ill.

¹³ Cf. BARJAMOVIC/HERTEL/LARSEN 2012: 9–13, § 1.2.3.

¹⁴ Following the new reading of this line of KEL G by G. Barjamovic, quoted in LACAMBRE/NAHM 2015: 20 and n. 28.

¹⁵ Cf. LACAMBRE/MILLET-ALBÀ 2008: 161, § 8.2.2.2; LACAMBRE 2019: 529–530, § 3.4.

2.2.2. Ahiyaya #1 attested outside Chagar Bazar

Ahiyaya #1 is attested by:

- one text in **Mari** (TH 85.72, cf. CHARPIN/ZIEGLER 2003: 164; for the other Ahiyaya tablets, cf. Ahiyaya #2);
- and four texts in **Tell Bi'a** (Tuttul) (*KTT* 178, 250, 251 and 252).

2.2.3. Ahiyaya #2 and #3

- Ahiyaya is attested in the archives of **Tell Leilan** (Šubat-Enlil/Šehnā) and corresponds to Ahiyaya #2;¹⁶
- Ahiyaya #2 is attested in the Mari archives with two juridical tablets: **ARM VIII 52** (CDLI P341531) and **A.4232** and its case **M.10939** (both unpublished);¹⁷
- Ahiyaya is also attested in the archives of **Tell Rimah** (Qaṭṭarā) in one tablet (**OBTR 314**; Archibab T17544), but it is a more recent eponym, Ahiyaya #3.¹⁸

2.3. Ahiyaya #1, an eponym contemporary with the Kingdom of Upper Mesopotamia

With the new attestations of Ahiyaya in Chagar Bazar, it is no longer possible to think that Ahiyaya #1 does not exist.

2.3.1. Ahiyaya #1, a well attested eponym

Ahiyaya #1 is now well attested (22 texts) in Chagar Bazar (Ašnakum), Mari and Tuttul and moreover throughout the year. The published and unpublished data confirm that almost all months are attested (Fig. 7.2):

Month	CB	Date and Reference	Mari	Date and Reference	Tuttul	Date and Reference
i*	1	9/i* (<i>OBTCB</i> 52)	-		-	-
ii*	2	7/ii* (<i>CB</i> III 4) 1 unpublished text	1	16/ii° (TH 85.72)	-	-
iii*	-	-	-	-	-	-
iv*	-	-	-	-	1	2/iv* (<i>KTT</i> 178)
v*	1	1/v* (<i>OBTCB</i> 32)	-	-	-	-
vi*	1	25/[vi*] (<i>CB</i> III 62)	-	-	3	-/vi* (<i>KTT</i> 250) -/vi* (<i>KTT</i> 251) -/vi* (<i>KTT</i> 252)
vii*	1	21/vii* (<i>CB</i> III 63)	-	-	-	-
viii*	2	2 unpublished texts	-	-	-	-
ix*	1	[...]/ix*(?) (<i>CB</i> III 64)	-	-	-	-
x*	2	3/x* (<i>OBTCB</i> 73) 5/x* (<i>CB</i> III 65)	-	-	-	-
xi*	1	1 unpublished text	-	-	-	-
xii*	-	-	-	-	-	-
[...]	5	3[+x]/[...] (<i>CB</i> III 66) 17/[...] (<i>CB</i> III 67) 20/[...]-IM (<i>CB</i> III 68) [...]/[...] (<i>CB</i> III 69) [...]/[...] (<i>CB</i> III 70)	-	-	-	-
Totals	17		1		4	

Fig. 7.2. Tablets dated to Ahiyaya #1 in the Kingdom of Upper Mesopotamia (cf. LACAMBRE/MILLET-ALBÀ 2008: 160, table 8.3 and unpublished Chagar Bazar data).

2.3.2. The texts of the harem

Most of the tablets concerning the harem were found together in 2002 (in pit 71.1.-.1) in Chagar Bazar (in a very bad state of preservation). They are dated exclusively from Ahiyaya and Awīliya. This suggests a close relationship between these two eponyms or even an immediate succession of these eponyms given the archaeological context.¹⁹

¹⁶ Cf. CHARPIN/ZIEGLER 2003: 136–138; KOLIŃSKI 2017: 232–233. See also EIDEM 2011: 10–12, § 1.1.4 for Ahiyaya #1 not being attested in Tell Leilan.

¹⁷ Cf. CHARPIN/ZIEGLER 2003: 163, § 2.12.3. See also BARJAMOVIC/HERTEL/LARSEN 2012: 9–13, § 1.2.3.

¹⁸ Cf. LACAMBRE/NAHM 2015.

¹⁹ Cf. LACAMBRE/MILLET-ALBÀ 2008: 161, § 8.2.2.2.

The published dated harem tablets are:

- with the eponym clearly preserved: Ahiyaya (9 texts) / Awīliya (13 texts)
- after analysis of the entire file, we could add: Ahiyaya (11 texts) / Awīliya (29 texts).

Total: Ahiyaya (20 texts) / Awīliya (42 texts).

2.3.3. Ahiyaya #1 is before Awīliya

The analysis of these tablets which are all deliveries of beer for the harem of Sîn-iqīšam shows that Ahiyaya is before Awīliya in particular this is proved by the study of Undulla. At the beginning of Ahiyaya, she was still a young girl (TUR), but later in Ahiyaya (after month vii*), she had a higher position in the hierarchy of the harem. In Awīliya, she is mentioned with one, and then with two sons (TUR), that she certainly had had with the head of the Habur region, Sîn-iqīšam.²⁰

This conclusion was made before the publication of KEL G, which confirms the sequence Awīliya/Nimer-Sîn/Addu-bāni and shows that Ahiyaya is, in any case, before Awīliya.

Moreover, the two eponyms should be close to each other, due to the fact that we found many of the same people in the tablets dated to both eponyms. An equivalence of Ahiyaya with, for example, Ilī-tillatī (cf. below) five years before Awīliya seems less plausible.

2.3.4. Other texts found in pit 71.1.-.1

The other texts found in pit 71.1.-.1, together with the tablets of beer for the harem are also beer texts dated as following:

- Asqūdum (R-KEL G 79): 4 texts (**CB III 8, 11, 12 and 13**)
- Awīliya (R-KEL G 82): 3 other texts (**CB III 15, 16 and 17**)
- Nimer-Sîn (R-KEL G 83): 1 text (**CB III 55**).

So, these are all eponyms close to Awīliya (and Ahiyaya #1 in my hypothesis).

2.3.5. The ‘House of Šubat-Enlil’ tablets

Furthermore, in Chagar Bazar, the tablets of grain rations for the ‘House of Šubat-Enlil’, seem to indicate that Aššur-malik and Ahiyaya are close.

There are 8 texts in that archive: **OBTCB 73, 78, 79, 91, 93, 96, 103 and 108**.²¹ Of which two are dated by an eponym: **OBTCB 79** (-i*/Aššur-malik) and **OBTCB 73** (3/x*/Ahiyaya). In **OBTCB 78**, only the month (*kinūnum* = ii*) is preserved.

CHARPIN/ZIEGLER 2003: 164 and n. 634 indicated already that these texts were close:

‘**OBTCB 73** (du 3/x*/Ahiyaya) est très parallèle à la liste datée de l’éponymie d’Aššur-malik (**OBTCB 79**, du -i*/Aššur-malik), ce qui permet de conclure qu’elle est relativement proche dans le temps de celle-ci.’

2.4. Ahiyaya #1 in Mari

In Mari, CHARPIN/ZIEGLER 2003: 164 supposed that Ahiyaya #1 = Ilī-tillatī.

This proposition was formulated on the basis of the parallel between **TH 85.72** dated in Ahiyaya and **TH 85.65** dated in Ilī-tillatī, both texts found in Area A (‘Chantier A’ in French) and only published in transliteration and translation:

TH 85.72: (1') x KAŠ SIG₅ 2 MUNUS *mu-še-ni-qa-tum* (2') [...] x x RI GAL x (3') [...] x x PA PA (...) (1'') I[TI k]i-nu-nim U₄ 16.KAM (2'') *li-mu a-hi-ia-ia*
‘[...] good quality beer: 2 wet nurses [...]. (Date:) 16/ii°/Ahiyaya.’

TH 85.65: (1) 6 *qa* NINDA SAG (2) 1 *qa* KAŠ SIG₅ (3) 2 DUMU.MEŠ [L]UGAL (4) 4 *qa* NINDA GU (5) 6 *qa* KAŠ [SIG₅] (6) 2 MUNUS *mu-še-ni-qa-tum* (7) 4 *qa* NINDA (8) 2 LÚ.TUR-šu-nu (9) *ša iš-tu* IGI LUGAL (10) *il-li-ku-nim* (11) ITI *ki-is-ki-si-im* (12) U₄ 28.KAM (13) *li-mu i-li-ELLAT-ti*
‘6 *qa* (of) good quality bread, 1 *qa* of good quality beer: 2 princes; 4 *qa* of GU-bread, 6 *qa* of [good quality] beer: 2 wet nurses; 4 *qa* of bread: their two servants who came from the king. (Date:) 28/vi/Ilī-tillatī.’

The date of the other texts found with **TH 85.72** were also considered: Ilī-tillatī, (R-KEL G 76, 5 texts), Rigmānum (R-KEL G 77, 3 texts), Ikūn-pīya (R-KEL G 78, 8 texts). These texts discovered in 1985 are still unpublished. In this group of texts, Ahiyaya seems to be associated with older texts than in Chagar Bazar.

Other unpublished texts found in Area A are also older (between Rīš-Šamaš = R-KEL G 73 and Ikūn-pīya = R-KEL G 78).

²⁰ For a complete analysis of the case of Undulla, cf. LACAMBRE/MILLET-ALBÀ 2008: 161–162 § 8.2.2.3 and MILLET-ALBÀ 2008: 251–254, § 13.3.3.1. The new unpublished texts confirm the first analysis made in 2008. I do not follow the proposal of BLOCH 2014: 203a–204.

²¹ LACAMBRE 2010: 109–110, § 5.1.

Fig. 7.3 is a table with the data given in CHARPIN/ZIEGLER 2003: 80 which includes the texts quoted above and other still unpublished texts:

R-KEL G	Eponym	Area A (oriental palace)	Remarks
73	Rīš-Šamaš	7	
74	Ibni-Addu	26	
75	Aššur-imitti	0	
76	Ilī-tillatī	5	Including TH 85.65
77	Rigmānum	3	
78	Ikūn-pīya	13	
79	Asqūdum	0	
80	Aššur-malik	0	
81	Ahiyaya	1	TH 85.72
82	Awīliya	0	

Fig. 7.3. Eponyms attested in administrative texts in Area A
(‘Chantier A’) (after CHARPIN/ZIEGLER 2003: 80, table).

It should be noted that all these tablets are discarded texts and that chance has to be taken into account in this process.

2.5. Ahiyaya #1 in Tuttul

In Tuttul, CHARPIN/ZIEGLER 2003: 164–165 also proposed Ahiyaya = Ilī-tillatī (R-KEL G 76). This proposition was formulated in considering the date of the other texts (tags about dead cows, **KTT 183–KTT 266**)²² found with **KTT 250–252** dated by Ahiyaya (R-KEL G 81): Rigmānum (R-KEL G 77, 49 texts), Ikūn-pīya (R-KEL G 78, 12 texts). In this group of texts, Ahiyaya seems to be close to more ancient eponyms too.

Another argument for making Ahiyaya = Ilī-tillatī was the status of Yasmah-Addu in Tuttul. When did he become king of this town?

For CHARPIN/ZIEGLER 2003: 84 and 164–165, this event happened as early as Rigmānum (R-KEL G 77), because in **KTT 79** (5/i*/Rigmānum; CDLI P392714), ‘a king’ (LUGAL) was present in Tuttul. But the text is not so clear:

KTT 79: (1–2) Beer (3) *a-na ka-an-nim* (4–5) Beer (6) *a-na ni-iq* LUGAL ŠA [...] (7) Beer (Lo.E.) [blank] (R.) Reverse lost (1') ITI *ni-iq-ṣi-im*¹ (2') U₄ 5.[KAM] (U.E.3') *li-mu ri-ig-ma-/nu* (L.E.) [blank]
(1–2) Beer (3) for immediate consumption (for the 'cellar'); (4–5) Beer (6) for a sacrifice of the Lord of the land (*bēlum ša mātim*) or for a sacrifice of the great king (i.e. Samsī-Addu) (7) Beer [...]. (Date:) 5/i*/Rigmānum.

DURAND/MARTI 2004: 130 proposed to read l. 6: ‘LUGAL *ša [ma-tim]*’ or ‘LUGAL ŠA.[RA]’. They indicated that:

‘Il y a bien *ša* à la finale; il devrait donc s’agir d’une divinité ; lire *ša [ma-tim]*? Lugal se lit *bēlum* dans de telles occasions ; cf. *MARI* 4: 162, ex. gr. Cependant comme le marqueur divin manque devant lugal, il est possible que soit attesté ici l’expression *ša [ra]* qui signifie apparemment dans les textes de l’Euphrate « grand » et que cela fasse bien allusion à des sacrifices de Samsī-Addu.’

For D. Charpin and N. Ziegler, **KTT 178** (2/iv*/Ahiyaya; CDLI P392813), where Yasmah-Addu is named as such, is therefore prior to this date (i.e. Rigmānum):

KTT 178: (1) 3 qa Ḳ.ḠIŠ (2) *i-na* 1 qa ḲUTU (3) *a-na ku-uk-ki* (4) NÍG.GUB *ia-ás-ma-ah-ḲIŠKUR* (Lo.E.) [blank] (R.5) *i-na tu-ut-tu-ul*¹ [blank] (6) ITI *na-ab-ri-im** (7) U₄ 2.KAM (8) *li-mu a-hi-ia-ia* (U.E.) [blank] (L.E.) [blank]
(1) 3 qa of sesame oil (2) according to 1 qū-measure of Šamaš (3) for cakes-kukkum; (4) meal of Yasmah-Addu (R.5) at Tuttul. (6–8) (Date:) 2/iv*/Ahiyaya

But if **KTT 79** indicates that the sacrifice is not related to Yasmah-Addu (but for a divinity or for Samsī-Addu), this argument is no longer valid.

²² KREBERNIK 2001: 111–116.

The only remaining problem is the mention of the complete name of Yasmah-Addu and not the term LUGAL in **KT 178**, while a king (LUGAL) is mentioned before in **KT 83** (Aššur-malik, cf. below), which is not the usual habit if Yasmah-Addu was king of Tuttul.

To solve this problem, I could propose a new hypothesis. We know that Samsī-Addu did not trust his son, Yasmah-Addu.²³ We also know now that the proposal according to which he was king of Tuttul in Rigmānun is no longer proven by the text **KT 79** (cf. above). So I wonder if we could not assume that the king (LUGAL) mentioned in Aššur-malik (**KT 83**) is in fact Samsī-Addu. Then Yasmah-Addu is mentioned in Ahiyaya with his name because he was not king of Tuttul but only of Mari. In this hypothesis, Yasmah-Addu would have become king of Tuttul only from Awīliya onwards and therefore mentioned as king (LUGAL) in the later dated texts (**KT 82** and **KT 120**, **KT 168**).

It is interesting to note that two attestations of ‘great king’ (LUGAL GAL), i.e. Samsī-Addu, in the texts of Tuttul, are dated to the eponym of Awīliya, certainly distinguishing him from Yasmah-Addu, in charge of Tuttul at this time.

- 24/xi/Awīliya: **KT 103** (CDLI P392738): 24 (rations for messengers travelling to see the great king).
- 20/xii*(?)/Awīliya: **KT 109** (CDLI P392744): 13 (rations for messengers travelling to see the great king).

Here are all the texts of meal (NÍG.GUB = *naptanum*) found in Tuttul, following the chronological order given here (cf. Appendix 1 below):

- 28/vii*/Aššur-malik, **KT 83** (CDLI P392718): 1. 5–7, NÍG.GUB LUGAL, *ù ša-bi-im, i-na tu-ut-tu-ul^{ki}*; here LUGAL = Samsī-Addu in my hypothesis.
- 2/iv*/Ahiyaya, **KT 178** (CDLI P392813): 1. 4–5, NÍG.GUB *ia-ás-ma-ah-^dIŠKUR, i-na tu-ut-tu-ul^{ki}*; Yasmah-Addu was not yet king of Tuttul; here LUGAL = Samsī-Addu in my hypothesis.

Then Yasmah-Addu was recognised as king of Tuttul from Awīliya and in the three following years (Nimer-Sîn, Addu-bāni and Ṭāb-šilli-Aššur):

- 25/ii*/*warki* Nimer-Sîn (= Addu-bāni), **KT 120** (CDLI P392755): 1. 31, NÍG.GUB LUGAL and 1. 9 and 32, NÍG.GUB.HÁ LUGAL
- 10/i*/*warki* Addu-bāni (= Ṭāb-šilli-Aššur), **KT 82** (CDLI P392717): 1. 9, NÍG.GUB LUGAL *ù ša-bi-im*
- 3/ii*/*warki* Addu-bāni (= Ṭāb-šilli-Aššur), **KT 168** (CDLI P392803): 1. 2–3, (sheep) *a-na^{GIŠ}BANŠUR LUGAL, i-na tu-ut-tu-ul^{ki}*

Damaged date or date lost:

- 21(?)/viii*/[...] **KT 311** (CDLI P392946): 4 [NÍG].GUB LUGAL, *ù ša-bi-[im]*
- [...] /viii*/[...] **KT 310** (CDLI P392945): 5'–7' [...] *ù, NÍG.GUB.HÁ LUGAL, i-na tu-[ut-tu]-ul^{ki}*
- Date lost **KT 307** (CDLI P392942): 5 NÍG.GUB.HÁ LUGAL
- Date lost **KT 308** (CDLI P392943): 2'–3' [*a-na*]^{GIŠ}BANŠUR LUGAL, [*i-na*] *tu-ut-tu-ul^{ki}*
- Date lost **KT 312** (CDLI P392947): 5 [NÍG].GUB.HÁ LUGAL

2.6. The different proposals for placing Ahiyaya #1 in the chronology

2.6.1. Ahiyaya #1 for D. Charpin and N. Ziegler

Based on the Mari and Tuttul archives, CHARPIN/ZIEGLER 2003: 83 proposed that Ahiyaya was the same year as Ili-tillatī, due to the hypothetical death of the eponym Ahiyaya during the year.²⁴

‘L’hypothèse la plus vraisemblable est qu’Ahiyaya fut remplacé par Ili-tillati après sa mort vers le mois v*. Certains textes continuèrent d’être datés par l’éponyme défunt (Ahiyaya), tandis que les chroniques éponymales ont uniquement retenu le nom du remplaçant (Ili-tillati).’²⁵

Nevertheless the death of an eponym and his replacement by a different eponym was, if it ever occurred a very rare event and has only been proposed for Šuli son of Šalim-ahum (REL 41) being replaced by his brother Iddin-Suen, but in fact Iddin-Su may have been eponym in the following year,²⁶ so I do not think it is reasonable to propose this assumption for solving the problem of Ahiyaya.

²³ See also CHARPIN/ZIEGLER 2003: 80–82, § 2.1.3 on the difficult question of the status of Yasmah-Addu.

²⁴ Cf. CHARPIN/ZIEGLER 2003: 83 and n. 63; see also 164–166 § 2.12.3 and especially 165, n. 63.

²⁵ Cf. CHARPIN/ZIEGLER 2003: 83, n. 63.

²⁶ BARJAMOVIC/HERTEL/LARSEN 2012: 5 and n. 4 refute the suggestion that an eponym name was changed during the eponym year.

2.6.2. Ahiyaya #1 for Y. Bloch

In 2014, Y. Bloch tried to solve the problem of the dates of the conquest of Ekallātum and Aššur by Samsī-Addu, known in the AKL but not mentioned in the MEC. The alleged eponyms of these conquests (Ibni-Adad and Ātamar-Ištar) are known neither in MEC nor in KEL G. Bloch postulated that Ibni-Adad and Ātamar-Ištar were garbled versions of the eponyms Inbi-Ištar and Āmur-Aššur.²⁷ For arriving at this result, he assumed that Ahiyaya ‘held the office of the eponym for only some months of the year, which was recorded in KEL G by the name of another eponym who held the office during its concluding months.’²⁸

Firstly, all the data indicates clearly that Ahiyaya was eponym throughout a full year (cf. above § 2.3.1) and that he has to be counted among Old Assyrian eponyms.

Secondly, I wonder whether the eponyms of the AKL are fictitious and chosen by an Assyrian scribe to express the fact that Samsī-Addu (or Šamšī-Adad) whose name signifies ‘Adad (is) my Sun’ was ‘made’ by Adad (Ibni-Adad) and that he receives the favour of Ištar (Ātamar-Ištar), an important goddess for the kings of Aššur since at least the time of Samsī-Addu.²⁹

2.7. A new reading of the Mari Eponym Chronicle (= MEC) E 11

On the basis of the new informations of Chagar Bazar, I suggested that the eponym **MEC E 11**³⁰ is to be read: *[a-hi-ia-i]a* rather than *[a-wi-li-i]a*, only the final [...-I]A of the eponym being preserved.³¹

Moreover, CHARPIN/ZIEGLER noted that it is not assured that the eponym following Aššur-malik is Awīliya, ‘Il reste que la séquence Aššur-malik/Awīliya repose sur des bases très ténues.’³² and added, ‘(...) contrairement à ce qui a été indiqué dans *MARI* 4, p. 250, les textes de farine ne permettent pas de dire que l’éponymie d’Awīliya soit immédiatement postérieure à celle d’Aššur-malik (cf. *ibid.*, p. 254).’³³

The broken part of the KEL G (end of column II and beginning of column III) can be reconstructed (cf. above § 1) based on chronological data from Mari, **MEC** and two summaries, **FM V 2** (= M.7841) and **M.5681** (= *ARM* XXX: 205–208; text already quoted in CHARPIN 1985: 249 and n. 24). We obtain the following table (Fig. 7.4):

R-KEL G Eponym	MEC	FM V 2 (= M.7841)	M.5681 (<i>ARM</i> 30: 205–208)
68 <i>da-da-[a]</i>	Unattested	[...]	
69 <i>a-šūr*-tá[k*-lā-ku]</i>	Unattested	2': <i>[li-mu^d]a-šūr-ták-[la-ku]</i>	
70 <i>[ha-a-ma-lik]</i>	Unattested	9': <i>li-mu ha-ià-[ma-lik]</i>	
71 <i>[ša-lim-a-šūr]</i>	E1 : <i>[ša*-li]m*-[^d]a-šūr</i>	[...]	
72 <i>[sú-en-mu-ba-li-iṭ]</i>	E2 : <i>[^dE]N.ZU-mu-ba-al-li-iṭ</i>	R.4': <i>[li]-mu^dEN.ZU-mu-[ba-lí-iṭ]</i>	
73 <i>[ri-iš-^dUTU]</i>	E3 : <i>[r]i-iš-^dUTU</i>	T.L.2": <i>[li-mu ri-iš]-[^dUTU]¹</i>	
74 <i>[ib-ni-^dIŠKUR]</i>	E4 : <i>[i]b-ni-^dIŠKUR</i>		
75 <i>[a-šūr-i-mi-ti]</i>	E5 : <i>[^da]-šūr-i-mi-ti</i>		col. i 16: <i>li-mu a-šūr-i-mi-ti</i>
76 <i>[i-lí-ILLAT]</i>	E6 : <i>i-[i]LLAT-t[i]</i>		col. i 46: <i>li-mu i-lí-ILLAT-ti</i>
77 <i>[ri-ig-ma-num]</i>	E7 : <i>ri-ig-m[a-ni]m</i>		col. ii 5: <i>li-mu ri-ig-ma-nu</i>
78 <i>[i-ku-pi-a]</i>	E8 : <i>i-ku-u[n-pi]-ia</i>		col. ii 25: <i>li-mu i-ku-up-pi-ia</i>
79 <i>[as-qú-dum]</i>	E9 : <i>[as-qú-dí-i]m</i>		col. iii 37: <i>li-mu às-qú-du</i>
80 <i>[a-šūr-ma-lik]</i>	E10 : <i>[^da-šūr]-ma-lik</i>		

²⁷ BLOCH 2014: 209a. With my new proposal for re-reading KEL G (cf. Appendix 1), it would be Bēlānum (instead of Inbi-Ištar) which would correspond to the conquest of Ekallātum and Aššur-nīšu (instead of Āmur-Aššur) which would correspond to the conquest of Aššur, which renders Bloch's comments about these eponyms obsolete.

²⁸ BLOCH 2014: 202b; Y. Bloch thought it was Aššur-imitti (BLOCH 2014, 205).

²⁹ For Ištar in Mari, cf. DURAND 2008: 199; in Aššur, cf. MEINHOLD 2009.

³⁰ BIROT 1985: 232 and 234, copy of A.1614b. For a new photo of this fragment, cf. Fig. 3.11 in this volume (ZIEGLER, Ch. 3).

³¹ Cf. LACAMBRE/MILLET-ALBÀ 2008: 162, § 8.2.2.6 and LACAMBRE 2009.

³² CHARPIN/ZIEGLER 2003: 159.

³³ CHARPIN/ZIEGLER 2003: 157, n. 584.

	R-KEL G Eponym	MEC	FM V 2 (= M.7841)	M.5681 (ARM 30: 205–208)
81	<i>a-h[i-a-a]</i>	E11 : [<i>a-hi-ia-i</i>]a (LACAMBRE 2009)		
82	<i>a-w[i]-l[i-a]</i>	Unattested		
83	<i>ni-mar-s[ú-en₆]</i>	Unattested		
84	^d ISKUR- <i>ba-ni</i>	Unattested		
85	DU ₁₀ - <i>ši-lá-a-šur</i>	Unattested		
86	<i>e-nam-a-šur</i>	Unattested		
87	<i>a-šur-e-mu-qí</i>	G1 : [^d <i>a-šur</i>]- <i>e-mu</i> -[<i>qí</i>]		

Fig. 7.4 (cont.). Data from Mari and the reconstruction of the new order of the eponyms in KEL G (R-KEL G).

While a reading *a-š[ur-ma-lik]* has been considered for the eponym R-KEL G 81, I propose a reading *a-h[i-a-a]*, the end of the line being broken. In BARJAMOVIC/HERTEL/LARSEN 2012: 109, a collation of the tablet was made but, even if my hypothesis was accepted, the authors refute the reading Ahiyaya claiming that the second sign is not a HI but a ŠÛR, BI₄ or NA. But as long as high-resolution photos and a copy of **kt 01/k 207** are not available, uncertainty will remain.³⁴

3. The question of Ahiyaya #1 and the chronology of Ešnunna

LIEBIG 2012 thought that there is no place for introducing Ahiyaya #1 in the KEL G due to the *Distanzangaben* given in the AKL. I consider, as did BARJAMOVIC/HERTEL/LARSEN 2012: 26–28, § 1.6, that ‘the later calculations were based on a corrupt tradition.’

3.1. A well-attested eponym

One of M. Liebig’s arguments is that Ahiyaya is not very well attested:

‘Zwischen den reichlich dokumentierten Jahren des Aššur-malik (REL 192) und Awīliya (REL 194) paßt der spärlich bezeugte Ahiyaya kaum ganzjährig.’

The Chagar Bazar documentation shows that it is in fact well attested (cf. above for this point) and that Liebig’s incorrect assertion is not a sufficient argument to suggest that Ahiyaya was not an eponym.

3.2. The tablet IM 53955 and the synchronism between Mari and Ešnunna

The other point, and the most difficult one to solve for the moment, is the question of the synchronism between the chronology of Mari and Ešnunna with the addition of a new eponym:

‘Durch die Datenliste **IM 53955** aus Tell Ḥarmal steht fest, daß Šamši-Adad 5 Jahre nach der Eroberung von Qabra starb. Dem entspricht die bisher anerkannte Eponymenfolge. Die Einfügung eines zusätzlichen Eponymenjahres Ahiyaya ergibt einen Zeitraum von 6 Jahren, was nicht sein kann.’³⁵

This question was then studied by BLOCH 2014: 206–207, who agreed that it is not possible to insert Ahiyaya #1 due to this problem of synchronism.

IM 53955 (found in Tell Harmal) is an administrative text listing quantities of grain which contains the last year-name of Dāduša (‘Conquest of Qabrā’) and the first nine year-names of Ibāl-pī-El II until his ninth year (cf. Date-List 2 in BAQIR 1949: 45–46, 84 [copy], 86 [photo] and HUSSEIN 2009: 77–78; CDLI P507975). The beginning of this tablet reads as following:

	(...) MU [<i>q</i>]a-ba-ra ^{ki}	Year: (Dāduša conquered) Qabrā	= Last year of Dāduša
2	(...) MU i-ba-al-pi-el LUGAL	Year: Ibāl-pī-El (II) (became) king	= Ibāl-pī-El II Year 1
	(...) MU ^{GI} ŠGIGIR ^{GI} .RA ^d ISKUR	Year: Chariot (into the temple of) Adad	= Ibāl-pī-El II Year 2
4	(...) MU KÁ.GAL ki-kur-ra	Year: Gate of the Chapel	= Ibāl-pī-El II Year 3
	(...) MU MA.DA ma-ha-zi ^{ki}	Year: Land of Māhāzum	= Ibāl-pī-El II Year 4
6	(...) MU ^d UTU-ši- ^d ISKUR BA.UG ₇	Year: Samsī-Addu died	= Ibāl-pī-El II Year 5

³⁴ In view of the shape of the break, the existence of a join is possible among the tablet fragments from the 2001 excavation campaign at Kültepe. It would be important to search for it systematically.

³⁵ LIEBIG 2012.

If we use the reconstruction of CHARPIN/ZIEGLER 2003: 262 (followed by BARJAMOVIC/HERTEL/LARSEN 2012: 25, table 9, except for the absolute date Ṭāb-šilli-Aššur = 1776, instead of 1776–1775), and introduce a new eponym (Ahiyaya #1), here is the result (Fig. 7.5):

Date	Mari	Ešnunna	Year-name	Reconstructed event
1781	Asqūdum (viii*-bis-xii*)/ Aššur-malik (i-vii*)	Dāduša	unknown	[ii*]/(Aššur-malik): Conquest of Qabrā
1780	Aššur-malik (viii*-xii*)/ Ahiyaya (i-vii*)	Dāduša	unknown	
1779	Ahiyaya (viii*-xii*)/ Awīliya (i*-vii*)	Last year of Dāduša	Year: Conquest of Qabrā	
1778	Awīliya (viii*-xii*)/ Nimer-Sîn (i-vii*)	Ibāl-pī-El II 1	Year: Ibāl-pī-El (II) (became) king	
1777	Nimer-Sîn (viii*-xii*)/ Adad-bāni (i-vii*)	Ibāl-pī-El II 2	Year: Chariot (into the temple of) Adad	
1776	Adad-bāni (viii*-xii*)/ Ṭāb-šilli-Aššur (i-vii*)	Ibāl-pī-El II 3	Year: Gate of the Chapel	
1775	Ṭāb-šilli-Aššur (viii*-xii*)/ warki Ṭ-š-A (i*-vi*)/ZL 0 (x)	Ibāl-pī-El II 4	Year: Land of Māhāzum	xii*/(Ṭ-š-A): Death of Samsī-Addu
1774	ZL 0 (xi-xii)/ZL 1 (i-x)	Ibāl-pī-El II 5	Year: Samsī-Addu died	

Fig. 7.5. An attempted reconstruction of synchronisms between Mari and Ešnunna
(based on CHARPIN/ZIEGLER 2003: 174; 261–262).

If we apply strictly the historical reconstruction of CHARPIN/ZIEGLER 2003 and CHARPIN 2004, it does not work.

The synchronisms between eponyms and year-names here are conditioned by two data: the death of Samsī-Addu commemorated in the fifth year of Ibāl-pī-El II and the conquest of Qabrā by Dāduša commemorated in his last year.

3.3. Death of Samsī-Addu

In the fifth year-name of Ibāl-pī-El II, the death of Samsī-Addu was commemorated. It was fixed at the end of Ṭāb-šilli-Aššur, month xii*.³⁶

3.4. Campaign against Qabrā

The campaign of Samsī-Addu to the East and the conquest of Qabrā are mentioned in **MEC E 10** and in the ‘**Mardin stela**’ (**AO 2776**).

The conquest of Qabrā was also commemorated in the last year-name of Dāduša and in the Dāduša stela, **IM 95200**, giving another link with the chronology of Ešnunna: unfortunately there is no information about the date on the Dāduša stela.

MEC E 10 probably dated the victory over Qabrā in the eponymate of Aššur-malik (but the town does not appear in the preserved part of the MEC).

MEC E 10 as reconstructed by CHARPIN/ZIEGLER 2003: 97, n. 183 (cf. now N. ZIEGLER Ch. 3 in this volume), is:

- 17' *i-na* [^d*a-šur*]-*ma-lik iš-me-d*^a*gan da-am₇-da-a*-[*am ša* DUMU-^d*IŠKUR lú ya₈-i-la-nim i-du-uk*]
18' *ù* ^d*UTU-ši-IŠKUR nu-u*[*r-ru-ga-am^{ki} qa-ab-ra-a^{ki} ù NG iš-ba-at*]
19' *^lki-ib-ra-am* LUGAL [NG NP LUGAL NG]
20' *^lia-šu-ub-^dIŠKUR* LUGAL [*ah-za-yi^{ki} NP LUGAL NG*]
21' *^lia-šu-ub-li-im* LUGA[L NG NP LUGAL NG etc.]
22' *^r6* [+x] LUGAL-MEŠ *an-nu-^rtim¹ ik-s*[*u-ma te-né-še-et ma-at qa-ab-ra-a^{ki}*]
23' [*a-n*]*a da-d*[*u-ša iš-ri-ik ...*]

‘En [Aššur]-malik, Išme-Dagan [a remporté] la victoire [sur Mār-Addu le Ya’ilānum] et Samsī-Addu [a pris] Nur[rugum, Qabrā et NG]. Kibram, le roi [de NG, NP roi de NG, etc.], Yašub-Addu, roi [d’Ahāzum, NP roi de NG], Yašub-Līm, roi [de NG, NP roi de NG etc.], ces 6+ rois, il a ligoté; [la population du pays de Qabrā, il a offert] à Dādu[ša].’

³⁶ Cf. CHARPIN/ZIEGLER 2003: 136–138, § 2.9.2; cf. n. 12 above.

The text of the ‘**Mardin stela**’ (AO 2776), as reconstructed by CHARPIN 2004: 162–163, §1.3.2 (and already published by A.K. Grayson in *RIMA* 1, 1987: 63–65, A.0.39.1001), is:

col. ii' (...) (12) ITI ŠE.KIN.KU₅ (13) *i-na u₄ 20-kam-šu* (14) ¹_{i7(?)} *za-i-ba-am* (15) [*lu*] *e-bi-ir-ma* (col. iii', 1) *a-na ma-a-at* (2) *qa-ab-ra-a^{ki}* (3) *ah-ha-bi-it-ma* (4) *ma-a-tam ša-a-ti* (5) *e-bu-úr-ša* (6) *am-ha-aš-ma* (7) *a-la-ni da-an-na-ti* (8) *ša ma-a-at ur-bi-e-el* (9) *ka-la-šu-nu* (10) *i-na ITI ma-aq-ra-nim* (11) *ú-ša-ab-bi-it-ma* (12) *bi-ra-ti-ia* (13) *lu-ú áš-ta-ak-ka-an* (14) *qa-ab-ra-a^{ki}* (col. iv', 1) *e-di-[iš-ši-šu]* (2) *lu-ú [e-zi-ib]* (3) *i-na [x x x]* (4) e-BU[...] (5) ID x [...] (6) *a-lum* ¹_{šu¹}-[ú ša] (7) *i-na iti [...-KAM]* (8) *la iš-¹šu¹-[...]* (9) *a-lam ša-[a-ti]* (10) *i-na I[TI ki-nu-nim]* (11) *i-n[a ka-ak-ki-ia]* (12) *da-[an-nu-tim]* (T.1') x [...] (2') *be [...]* (3') ^d[...] (4') *lu x [...]* (5') *li [...]* (6') *a [...]*

col. ii' (...) (12–13) Au mois de *Niggallum* (viii*/[*Asqūdum*]), en son 20^e jour, (14–15) je franchis le (Petit) Zab. (col. iii', 1–3) Je me dirigeai vers le pays de Qabrā, (4–6) je détruisis la moisson de ce pays, (7–11) je m'emparai au mois de *Maqrānum* (ix*/[*Asqūdum*]) de toutes les places fortes du pays d'Arbèles, (12–13) j'y installai mes garnisons. (col. iv', 1–2) Seule restait Qabrā. (3–5) ... (6) Cette ville que, (7) pendant [*n*] mois, (8) on n'arrivait pas [*à prendre*], (9) ce[tte] ville, (10) au mo[is] de *kinūnum* (ii*)/(*Aššur-malik*)), (11–12) grâ[ce] à mes armes] pu[issantes] (T.1') [*je m'en emparai*]. (Fin intraduisible).

Three remarks about the ‘Mardin stela’:

- No eponyms are indicated in the preserved part of the stela: only days and months are indicated;
- If the first preserved part of the ‘**Mardin stela**’ (AO 2776) is dated to *Asqūdum*, it is surprising, as CHARPIN/ZIEGLER 2003: 92, n. 129 noticed, that the intercalary month *niggallum* (viii*-bis) is not mentioned:

‘On notera que la stèle ne parle pas des événements du mois supplémentaire (viii*-bis) attesté par les archives de Chagar Bazar.’

- Col. iv' 10: the date and the conquest of Qabrā, the month *kinūnum*, are reconstructed in this part of the stela.

Last but not least, it should be noted that the other tablets that shed light on these events, the archive of **Tell Shemshāra** (the former Šušarra), contain **not one dated text** (cf. CHARPIN/ZIEGLER 2003: 20 with bibliography). J. Eidem the editor of the texts, wrote (EIDEM 1992: 16a § C):

‘At Shemshāra **no texts are dated**, and consequently the diachronic sequence of the letters must serve as a base for administrative text.’

Administrative texts from **Tell Shemshāra** cannot be used for reconstructing chronology.

3.5. The problem of synchronisms between Ešnunna and Mari

With the reconstruction proposed before of the history of the Kingdom of Upper Mesopotamia, if we have Ahiyaya as a new eponym, **the conquest of Qabrā is five months too early (or four months too early if we change the month in col. iv' 10 for *tamhīrum*, month iii*)**. It is not possible to have another month in this reconstruction, (conquest of Qabrā in Aššur-malik), Yasmah-Addu was present during the conquest of the town according to the quotation of an unpublished letter from Yasmah-Addu to Išhi-Addu of Qatnā:

(13) *i-na-an-na qa-ab-ra-a^{ki}* (14) [*e-d*] *i-iš-ši-šu e-zi-ib* (...) (18) (...) *i-na-an-na iš-tu u₄ 20-KAM* (19) *iš-me-^dda-gan ù a-[na-k]u qa-ab-ra-a^{ki} la-we-nu*

À présent, seule reste Qabrā. (...) À présent, depuis 20 jours, Išme-Dagan et moi (= Yasmah-Addu) assiégeons Qabrā.³⁷

And Yasmah-Addu was in Mari in month iv* of Aššur-malik.³⁸

3.6. The question of the intercalary months in each calendar

In the so-called ‘Samsī-Addu calendar’, the year started in the month *Nikmum*, but in the Ešnunna calendar, the year started in the month *Niggallum* or ŠE.KIN.KU₅ (which is the eighth month in the so-called ‘Samsī-Addu calendar’).³⁹

Moreover, new texts in Chagar Bazar reveal an intercalary month viii* (*Niggallum* MĪN) during the eponym *Asqūdum*.⁴⁰ It was not known in the documentation of Mari.⁴¹ It is possible that other intercalary months in both calendars exist.⁴² The different calendars used in our study are listed in Fig. 7.6:

³⁷ A.2745+, cf. CHARPIN/ZIEGLER 2003: 95 and n. 160; Archibab T20269.

³⁸ Cf. CHARPIN/ZIEGLER 2003: 95, n. 169 and 147.

³⁹ Cf. LACAMBRE 2002: 510 with the bibliography; ZIEGLER 2021a.

⁴⁰ Cf. LACAMBRE 2002; LACAMBRE/MILLET-ALBÀ 2008: 156; ZIEGLER 2021a.

⁴¹ Cf. the remark in CHARPIN/ZIEGLER 2003: 190 and n. 112.

⁴² Cf. also for the question of Old Assyrian intercalary months MICHEL 2021: 83–84 (with bibliography).

‘Samsī-Addu calendar’		Ešnunna calendar		Mari calendar	
i*	<i>nikmum</i>	vi	<i>nikmum</i>	vi	<i>Hubur</i> (^d IGI.KUR)
ii*	<i>kinūnum</i>	vii	<i>kinūnum</i>	vii	<i>kinūnum</i>
iii*	<i>tamhūrum</i>	viii	<i>tamhūrum</i>	viii	<i>Dagan</i>
iv*	<i>nabrūm</i>	ix	<i>nabrūm</i>	ix	<i>līlātum</i>
v*	<i>mammūtum</i>	x	<i>mammūtum</i>	x	<i>Bēlet-bīri</i>
vi*	<i>mana</i>	xi	<i>kīškīšim</i>	xi	<i>kīškīšim</i>
vii*	<i>ayyarum</i>	xii	<i>kinkum</i>	xii	<i>ebūrum</i>
viii*	<i>niggallum</i> (= ŠE.KIN.KU ₅)	i	<i>niggallum</i>	i	<i>urāhum</i>
ix*	<i>maqrānum</i>	ii	<i>elūnum</i>	ii ^o	<i>malkānum</i>
x*	^d DUMU.ZI	iii	<i>maqrattum</i>	iii	<i>lahhum</i>
xi*	<i>abum</i>	iv	<i>abum</i>	iv	<i>abum</i>
xii*	<i>tīrum</i>	v	<i>zibnum</i>	v	<i>hipirtum</i>

Fig. 7.6. The calendars of ‘Samsī-Addu’, Ešnunna and Mari (based on CHARPIN/ZIEGLER 2003: 156 and WHITING 1987: 32, n. 16, cf. also LACAMBRE 2002: 511; see now JACQUET 2021: 138, table 2; ZIEGLER 2021a: 121). Note: for the reading of the months *nikmum* and *kīškīšim*, cf. DURAND 2008: 296 and 595–596.

3.7. An appraisal of the synchronism between Mari and Ešnunna

I consider that the synchronism between the so-called ‘Samsī-Addu calendar’ and the Ešnunna calendar is not as precise as has been suggested. D. Charpin and N. Ziegler considered that:

‘(...) on supposera que dans la période ici considérée, il y eut *globalement* le même nombre de mois intercalaires à Mari et à Ešnunna.’⁴³

But in our case (with a difference of only 4 or 5 months), due to the problem of the synchronism between calendars, it is impossible to evaluate it globally. For example, it has been noted that in Mari intercalation was very irregular.⁴⁴ The problem is that the data from the Kingdom of Ešnunna need further studies for this period and that many texts are still unpublished, especially from Tell Asmar and from Tell Harmal.

4. The lifespan of Samsī-Addu

The new reconstruction of KEL G proposed here shows that the life of Samsī-Addu lasted 70 years (cf. Appendix 1). As we know with MEC, he was born in Dādiya (R-KEL G 16) and would have ascended the throne in Šarrum-Adad (R-KEL G 31). Assyrian King List (AKL) states:

‘[Šam]šī-Adad, son of Ilu-kabkabu, went [to Karduni]aš (i.e. Babylonia) [during] the time of Narām-Suen. During the eponymy of Ibni-Adad, [Šamšī]-Adad [came up] from Karduniaš (i.e. Babylonia). He captured Ekallātum. For three years he resided in Ekallātum. During the eponymy of Ātamar-Ištar, Šamšī-Adad came up from Ekallātum. He removed Erišum (II), son of Narām-Suen, from the throne and seized the throne (for himself). He ruled as king for thirty-three years.’⁴⁵

Then he would have conquered Ekallātum in Bēlanum (R-KEL G 50) and Aššur three years later in Aššur-nīšu (R-KEL G 53). Thanks to the archives of Mari, we know that this city would have been conquered in Haya-malik (R-KEL G 70). Finally, the archives of Mari and the name of the 5th year of Ibal-pī-El II of Ešnunna tell us that he died in Ṭāb-šilli-Aššur (R-KEL G 85).

He would be one of the very few rulers whose date of birth and lifespan are precisely known in the history of the Ancient Near East.

⁴³ Cf. CHARPIN/ZIEGLER 2003: 260–261, § 3.9.4.

⁴⁴ Cf. CHARPIN/ZIEGLER 2013: 60–61, § 1.2 and n. 14; see also the remarks of KOLIŃSKI 2013 about the three intercalary months in Year 6 of Zimrī-Līm and lastly JACQUET 2021: 140–141, § 2.2 for the reign of Zimrī-Līm in general.

⁴⁵ Following GRAYSON 1980–1983: 105–106, § 11; cf. also VEENHOF 2003: 61–62. AKL is taken into account here, even if it is almost certain that it is not totally reliable, cf. §2.6.2 above for the question of Samsī-Addu's contemporary eponyms in AKL.

5. Conclusion

The new reconstruction of KEL G solves, in my view, all the problems that arose when trying to place Haya-malik in KEL G 69. From now on, KEL G 69 should be read as Aššur-taklāku. Haya-malik was the following eponym in the broken part of KEL G at the end of column II. According to this hypothesis the life of Samsī-Addu lasted 70 years if we include Ahiyaya as a full year, a question which is still debated.

Indeed the case of Ahiyaya #1 is still difficult to solve. The data from Chagar Bazar show Ahiyaya as a full attested year and I think we have the succession Aššur-malik / Ahiyaya / Awīliya even if there are still some problems when confronting this new hypothesis (based on the actual published tablets) with the chronology established until now.

I consider that in Mari the low number of occurrences of this eponym is due to chance and that it is possible to place Ahiyaya after Aššur-malik in **MEC E 11**. In Tuttul, I wonder if the question could not be solved with a very late date in Awīliya for Tuttul's control by Yasmah-Addu. Lastly, confronting the chronology of the Kingdom of Upper Mesopotamia with that of Ešnunna is a tricky exercise, especially when introducing a new eponym, Ahiyaya. The result seems for now inconclusive due to the problem of the synchronism between calendars. But the difference consists of only in five or even four months and I think this difference is not sufficient to reject once and for all the possibility of inserting Ahiyaya as a new eponym in the chronology.

About the reconstruction of the events in the Mari archives and especially for the time of Samsī-Addu and Yasmah-Addu, J. Eidem in 1992 (16a, § C) made an interesting remark:

In Mari 'a more refined sequence of the historical events mentioned in the epistolary texts is only now evolving mainly from the framework provided by dated administrative texts.'

The challenge is to marry up these two parameters. If a new chronological framework is put in place (here with the addition of a new eponym Ahiyaya), would it not be possible to reconsider its implications for the epistolary data? It would be an enormous task but will be necessary, I think, in the future, as Ahiyaya #1 seems to be a full year, that we cannot place anywhere except after Aššur-malik in view of the data available at the moment.

Appendix 1: a new reconstruction of KEL G from its beginning until the death of Samsī-Addu (with the addition of Ahiyaya after Aššur-malik).

R-KEL G is the revised KEL G with its changes starting from G 69 onwards. I follow BARJAMOVIC/HERTEL/LARSEN 2012: 25 for Samsī-Addu's death in 1776 BC.

Year BC	R-KEL G	Eponym	Year of Samsī-Addu (= SA)
1860	1	Samaya son of Šu-Bēlum	
1859	2	Ilī-ālum son of Sukkaliya	
1858	3	Ennam-Anum son of Aššur-malik	
1857	4	Ennam-Aššur son of Dunni-Ea / Dan-Ea	
1856	5	Enna-Suen son of Šu-Ištar	
1855	6	Hanna-Nārum	
1854	7	Dādiya	
1853	8	Kapatiya	
1852	9	Išme-Aššur son of Ea-dan	
1851	10	Aššur-muttabbil son of Azizum	
1850	11	Šu-Nirah son of Azuzaya	
1849	12	Iddin-abum	
1848	13	Ilī-dan son of Azuza / Azua	
1847	14	Aššur-imittī son of Iddin-Ištar	
1846	15	Buziya son of Abiya	
1845	16	Dādiya son of Šu-Ilabrat	SA 1: Birth of SA (MEC)
1844	17	Puzur-Ištar son of Nūr-ilīšu	SA 2
1843	18	Isaya son of Dagan-malkum	SA 3
1842	19	Abu-šalim son of Ilī-ālum	SA 4
1841	20	Aššur-rē'ī son of Ilī-emūqī	SA 5
1840	21	Ṭāb-Aššur son of Uzua	SA 6
1839	22	Šu-Rama son of Uzua	SA 7
1838	23	Suen-išmeanni	SA 8
1837	24	Aššur-malik son of Šu-Haniš	SA 9
1836	25	Dan-Ea son of Abu-waqar	SA 10
1835	26	Enna-Suen son of Iddin-abum	SA 11
1834	27	Aššur-balaṭ(ī)	SA 12

Year BC	R-KEL G	Eponym	Year of Samsī-Addu (= SA)
1833	28	Enna(m)-Suen/Aššur	SA 13
1832	29	Itūr-Aššur	SA 14
1831	30	Šu-Bēlum	SA 15
1830	31	Šarrum-Adad (son of Buzazu)	SA 16: Accession to the throne of SA (MEC)
1829	32	Šu-Laban	SA 17
1828	33	Aššur-imittī	SA 18
1827	34	Dadaya I (son of Šarrum-Adad?)	SA 19
1826	35	Dadaya II (son of Šarrum-Adad?)	SA 20
1825	36	Ah-Šalim	SA 21
1824	37	Ušur-ša-Ištar	SA 22
1823	38	Kataya	SA 23
1822	39	Šu-Suen	SA 24
1821	40	Abu-Šalim	SA 25
1820	41	Šudaya	SA 26
1819	42	Šu-Dādum	SA 27
1818	43	Aššur-tukultī	SA 28
1817	44	Puzur-Ištar	SA 29
1816	45	Atanah	SA 30
1815	46	Erišum	SA 31
1814	47	Aššur-ennam	SA 32
1813	48	Inbi-Ištar (son of Šin-išme'anni)	SA 33
1812	49	Aššur-bēl-malkim (son of Iddin-abum)	SA 34
1811	50	Bēlānum	SA 35: Conquest of Ekallātum (AKL)
1810	51	Sukkallum	SA 36
1809	52	Amur-Aššur	SA 37
1808	53	Aššur-nīšu	SA 38: Conquest of Aššur (AKL)
1807	54	Munawwirum	SA 39
1806	55	Idnaya (son of Aššur-imittī)	SA 40
1805	56	Dadaya (son of Šarrum-Adad?)	SA 41
1804	57	Puzur-Nirah	SA 42
1803	58	Abiya	SA 43
1802	59	Edīnum son of Bēlu-rabi	SA 44
1801	60	Aššur-taklāku (son of Illil-nādā or Ilabrat-bāni)	SA 45
1800	61	Išim-Suen (son of Iddin-Aššur, Ikūn-pīya or Haštum)	SA 46
1799	62	Adad-bāni	SA 47
1798	63	Abī-šagiš (son of Šalim-Aššur or I-a-mi-na-nim)	SA 48
1797	64	Tāb-šilli-Aššur (son of Bēliya or Ikūn-pī-Aššur)	SA 49
1796	65	Iddin-Aššur (son of Abu-Šalim?)	SA 50
1795	66	Namiya (son of Ipiq-Adad/Ipqidam)	SA 51
1794	67	Ahu-šarrī (son of Ilī-ālum)	SA 52
1793	68	Dadaya	SA 53
1792	69	Aššur-taklāku	SA 54
1791	70	[Haya-malik (son of Dudānum)]	SA 55: Conquest of Mari (Mari archives)
1790	71	[Šalim-Aššur son of Ušranum]	SA 56
1789	72	[Suen-muballit (son of Aššur-iddinam)]	SA 57
1788	73	[Rēš-Šamaš (son of Anum-pīša?)]	SA 58
1787	74	[Ibni-Adad (son of Aššur-tukultī)]	SA 59
1786	75	[Aššur-imittī]	SA 60
1785	76	[Ilī-ellitī (son of Aššur-nīšu)]	SA 61
1784	77	[Rigmānum]	SA 62
1783	78	[Ikūn-pīya (son of Šalim-Aššur)]	SA 63
1782	79	[Asqūdum]	SA 64
1781	80	[Aššur-malik]	SA 65
1780	81	Ah[iyaya (son of Lā-qēpum)]	SA 66
1779	82	Awīliya	SA 67
1778	83	Nimar-Suen (son of Aššur-nīšu)	SA 68
1777	84	Adad-bāni (son of Puzur-ilī)	SA 69
1776	85	Tāb-šilli-Aššur (son of Bēliya or Ikūn-pī-Aššur)	SA 70: Death of SA (Mari archives and name of the 5th year of Ibāl-pī-El II of Ešnunna)

Fig. 7.7. A reconstruction of KEL G from the beginning to the death of Samsī-Addu.

Appendix 2: Different KEL G reconstructions between Dadaya (R-KEL G 68) and Ṭāb-šilli-Aššur (son of Bēliya or Ikūn-pī-Aššur) (R-KEL G 85)

GÜNBATTI 2008	BARJAMOVIC/HERTEL/ LARSEN 2012	R-KEL G	Eponym	Year of Samsī-Addu (= SA)
G 68	G 68	68	Dadaya	SA 53
G 70	G 69	–	Ennam-Aššur (incorrect MEC reading)	–
–	–	69	Aššur-taklāku (instead of A-ni- ¹ x ¹ [...])	SA 54
G 69	G 72*	70	[Haya-malik (son of Dudānum)]	SA 55
–	G 73*	71	[Šalim-Aššur son of Ušranum]	SA 56
G 71	G 74*	72	[Suen-muballiṭ (son of Aššur-iddinam)]	SA 57
G 72	G 75*	73	[Rēš-Šamaš (son of Anum-pīša?)]	SA 58
G 73	G 76*	74	[Ibni-Adad (son of Aššur-tukultī)]	SA 59
G 74	G 77*	75	[Aššur-imittī]	SA 60
G 75	G 78*	76	[Ilī-ellitī (son of Aššur-nīšu)]	SA 61
G 76	G 79*	77	[Rigmānum]	SA 62
G 77	G 80*	78	[Ikūn-pīya (son of Šalim-Aššur)]	SA 63
G 78	G 81*	79	[Asqūdum]	SA 64
G 79	G 82*	80	[Aššur-malik]	SA 65
–	G 83*	81	Ah[īyaya (son of Lā-qēpum)]	SA 66
G 80	G 84*	82	Awīliya	SA 67
G 81	G 85*	83	Nimar-Suen (son of Aššur-nīšu)	SA 68
G 82	G 86*	84	Adad-bāni (son of Puzur-ilī)	SA 69
G 83	G 87*	85	Ṭāb-šilli-Aššur (son of Bēliya or Ikūn-pī-Aššur)	SA 70: Death of SA

Fig. 7.8. Comparison of the reconstructions of KEL G between Dadaya and Ṭāb-šilli-Aššur by Günbatti, Barjamovic, Hertel and Larsen and the author.

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Abbreviations

AKL	<i>Assyrian King List</i> : GRAYSON 1980–1983, 101a–115, § 3.9 King List 9. <i>Assyrian King List</i>	MEC	Mari Eponym Chronicle: BIROT 1985
ARM XXX	DURAND 2009	MTT I/1	ZIEGLER/LANGLOIS 2016
CB III	TUNCA/BAGHDO 2008	OBTCB	TALON 1997
FM V	CHARPIN/ZIEGLER 2003	OBTR	DALLEY/WALKER/HAWKINS 1976
KEL	Kultepe Eponym List	RIMA 1	GRAYSON 1987
KTT	KREBERNIK 2001	R-KEL G	revised KEL G as proposed in this study

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8. The Assyrian *līmum*-system, eponym lists and the conquest eponyms of Šamšī-Adad

GUIDO KRYSZAT ¹

This article aims to highlight the fact that the well known year eponym is only a part of a more complex political system in early Aššur. Evidence for the different dignitaries called *līmum* is collected and discussed. It becomes clear that they were all *institutional* parts of the governing system in the city. It will also be argued that there is good reason to believe that this system did not develop locally in Aššur but was rather imported by immigrants coming from a region more to the West/Northwest as seen from Aššur. Consequently a different view for the beginning of the *līmum*-system and for the understanding of some crucial passages of the Assyrian King List and the Eponym List A is offered.

In a recent article I made a critical remark on the current views on the beginning of the Assyrian eponym dating system, concluding that the whole ‘matter is surely still one of debate’.² This article aims at resuming this debate. It will be argued that the existence of an institutional *līmum*-system and the use of the *līmum* is by itself a strong argument for the existence of a large group of immigrants from north-western Syria in early Aššur. Some further remarks on early Assyrian history and the cuneiform used in Kültepe Eponym List (KEL) G will be added.

1. The Problems

In the context of Assyrian history and Upper Mesopotamian chronology the *līmum* is always treated as a pure chronological unit, i.e. the year eponym. This poses a serious problem, because to assess the character of the year eponymy and its role in early Assyrian history correctly, we have to see it as what it is: only a part, albeit an important and central one, of a larger system which further on I will call the *līmum*-system. There have been a number of *līmū* in the city of Aššur as well as in the Syro-Anatolian trading network that the Assyrian merchants kept running with its administrative centre in Kanesh. Looking at and treating the *līmum* only as year eponym limits access to the historical realities apart from pure chronological considerations. As I will try to show a closer view on all the *līmū* has consequences for the answer to the question ‘*when did the Assyrians start to date by year eponyms?*’ and secondly for the understanding of early Old Assyrian history and society in general. I hope to give answers to both issues and will consequently introduce a somewhat revised look on the political system in the early times of the city of Aššur, before and at the beginning of the early Old Assyrian period. By doing so I am at the same time continuing my recent work of a reappraisal of the early Old Assyrian history I started with my articles of 2018 and 2021.

2. The *līmum*-system 1: the *kārum-līmū*

A complete study of *all* of the *līmū* from this group is doubtless needed, but cannot be undertaken in the scope of this article. Aside from the best known meaning of *līmum* as ‘year eponym’ CAD L 195b gives also the meaning ‘2. eponym – a) an official in Aššur’ and on p. 196a it adds 2.b) ‘an official in the Anatolian colonies’. CAD L was published in 1973, and since then a good number of references to *līmū* other than the year eponym became known. The most important study on the so-called *kārum-līmū* that were not year eponyms is still Larsen’s chapter ‘The concept *limmum*’ in his *The Old*

¹ With great pleasure I remember the days and lively discussions at the conference in Lille in 2015. My warmest thanks go to the organisers Denis Lacambre and Werner Nahm. I am also extremely grateful to Werner Nahm and Michael Roaf for their efforts in preparing the present volume for publication.

² KRYSZAT 2021: 23 n. 13.

Assyrian City State and its colonies.³ There he commented on the older ideas of J. Lewy and P. Garelli and collected and discussed all the available evidence. I refer the reader in this matter to Larsen's book and will restrict myself here to some of the most significant occurrences of these officials to clarify the situation. Though there are a number of new instances for the *kārum-līmū*, credit still also has to go to Kemal Balkan, who provided us already in 1955 with the most crucial textual evidence for this phenomenon.⁴ The most interesting point here is that the word *līmum*, when designing two or three people, sometimes stands in the singular and sometimes in the plural. In those occurrences where the *līmū* appear in a group of three people we find it in the singular in BIN IV 103, 1f.: *li-mu-um Aššur-imittī Kullumāya à Hunnīya* (...), but in l. 9 the same people start a speech in the plural *umma li-mu-ú-ma*.⁵ In another example kt c/k 1338, 13–16, we have the singular again: *li-mu-um A-šūr-i-mi-ti Šu-Sú-en₆ à A-šūr-DÜG*.⁶ And there is also kt e/k 56, 6–8, where we read *ki-ma kà-ri-im I-dí-a-bu-um Û-šur-ša-Ištar à DINGIR-na-da li-mu-ú KÜ.BABBAR il₅-qé-ú* 'For the *kārum* PN 1–3, the *līmū* (plural), received the silver'.⁷ Lastly, we have the new example for a text combining a group of three *kārum-līmū* with the house of the *kārum* in kt 88/k 994, 17ff. And here again the three officials are connected with the word *līmum* in the singular: $\frac{1}{2}$ *ma-na li-mu-um il₅-qé 8 ma-na KÜ.BABBAR qá-tám ša I-dí-A-šūr²⁰⁾ à Ku-lá É kà-ri-im ša-bu-ú : li-mu-um Kà-ar-wi-a A-šūr-i-mi-ti à E-ma-a*.⁸

As an example for the appearance of two people as *kārum-līmū*, we might look at CCT I 4, which looks from ll. 4 to 13 as the copy of a debt note of normal type. But in comparison with the other debt notes of the memorandum, it seems exceptional that the debtor is the *kārum* (most likely of Kanesh) itself.⁹ And following the clause on the interest, we find the remark '*līmū* are Aḫu-waqar and Liptānum' in the plural *li-mu-ú*.¹⁰ One expects some kind of connection between the debtor and the appearance of the *līmū*. In the end it becomes clear from these examples that the term can be used as a designation for a group of people as well as for a single person as in the year eponym formula. It immediately suggests that we are dealing here with the same word as the *li'mum* we find in sources from Mari and later Emar, which Fleming chose to translate as 'tribe'.¹¹ I will come back to the discussion of the meaning of the word below. Although there are some well-known and most likely high-ranking individuals of the Assyrian *kārum* among these *kārum-līmū* discussed above,¹² only a few of them are attested as year eponyms in Aššur.¹³ As Larsen suggests, the actions these *līmū* take have most likely to be compared with the tasks of those *līmū* in the city. The question that arises here is: Does the *kārum*-system of *līmū* really mirror the one in the city and does it go back to it? And do they both have their roots in a common traditional system? As we have already seen, the first makes very much sense. As for the second, we have to approach the situation in the city. Contrary to the picture for the *kārum-līmū* given here, we never have a group of two or even three eponyms from the city of Aššur itself. For now, the evidence says that they always acted alone. But here we have to be careful not to draw too many conclusions, because we don't have enough evidence from the city itself for this period to make valid statements. But what we do have in the city, again contrary to the situation in Anatolia, are *līmū* who are specified by their areas of responsibility. I would not want to conclude that the *kārum-līmū* were not specified in their days, but rather that their area of responsibility was absolutely clear: they were the *līmū ša kārim*, though never literally called such in the known evidence. Another small text, still unpublished, adds to the evidence: kt 87/k 421 lists several amounts of silver

³ LARSEN 1976: 333–353.

⁴ BALKAN 1955: 97ff. ('List B', with a number of minor corrections that had to be done especially in LARSEN 1976; VEENHOF 1985 and 2003; KRYSZAT 2004).

⁵ ¹⁾ *li-mu-um A-šūr-i-mi-ti* ²⁾ *Ku-lu-ma-a à Hu-ni-a a-na* ³⁾ *A-šūr-ni-šu maškim iṣ-bu-tù-ni-a-ti-ma* ⁵⁾ *ta-ma-lá-ki ku-nu-ke-e* ⁶⁾ *ša kà-ri-im! Kà-né-eš* ⁷⁾ *ša-ḫe-er gal ip-qí-du* ⁸⁾ *um-ma li-mu-ú-ma*. See Hecker <https://www.hethport.uni-wuerzburg.de/altass/pdfsrc/BIN4.pdf> with references for further literature on the text.

⁶ BALKAN 1955: 98.

⁷ BALKAN 1955: 99.

⁸ See HECKER https://www.hethport.uni-wuerzburg.de/altass/html/kt_88~k_994.html.

⁹ Another albeit obscure connection between the (house of the) *kārum* and the *līmum* is found in the private note CCT I, 28c, 3f. (FAOSBh 4, 340 Nr. 425): (Textiles) *a-na É kà-ri-im a-dí i-li-mi-ni* (end of the text). Or is here meant another word? See the comment on this text in LARSEN 1976: 341 and 351.

¹⁰ *8 ma-na 18 2/3 GÍN* ⁵⁾ *ša-ru-pá-am ší-im TÚG^{ti} ša ELLAT^{at} I-ku-pi-a i-šé-er kà-ri-im* ⁴⁾ *En-lil-ba-ni i-šu iṣ-tù ḫa-muṣ-tim ša Šu-Ištar* ITU.1.KAM *qá-ra-a-tim li-mu-um En-na-Sú-en₆ DUMU Šu-Ištar* ¹⁰⁾ *a-na 6 ḫa-am-ša-tim i-ša-qú-lu šu-ma lá iṣ-qú-lu 2/3 GÍN 15 ŠE.TA a-na 1 ma-na-im ší-ib-tám i-ITU.1.KAM ú-šú-bu li-mu-ú A-ḫu-wa-qar à Li-ip-ta-num*.

¹¹ See for the use of the word in Mari FLEMING 2004, especially 209ff.

¹² One of them is the famous Pūšu-kēn, who was one of the most influential members of the Assyrian merchant community, see e.g. LARSEN 1976: 81ff. and KRYSZAT 2004: 40–50.

¹³ One such case seems to be Šu-Su'en, son of Papalum, see KTS I 50c, 12.

acquired through the selling of a number of textiles. The money is said to be deposited in the house of the *kārum*, the responsible *līmum* was one Amria.¹⁴ All these passages suggest that the *kārum-līmū* were an integral part of the *bēt kārim*. Judged by the number of texts in which the *kārum-līmū* and the *bēt kārim* appear in connection with each other there can be no doubt that we have here an institutional background. The question that has to follow immediately is of course: Were the *līmū* in the city of Aššur (aside from the year eponyms) in analogy to the situation in Kanesh likewise an integral and institutional part of the *bēt ālim*?

3. The *līmum*-system 2: the *līmū ina ālim*

As stated above, for the city we have a number of *līmū* specified by their areas of responsibility. And as I will try to show, this should also be the case for the *līmum elium* and the *līmum šaplum*. Aside from the year eponym there are four other *līmū* known to us, these are the *līmum ša kaspim*, the *līmum ša še'im*, the *līmum elium*, and the *līmum šaplum*. Unfortunately the activities of these *līmū* are very poorly attested, but nevertheless some statements may be made and Dercksen has collected all the evidence known and thoroughly discussed the different aspects of this subject.¹⁵ There is not much I can add to his detailed observations, but I would not hesitate to go one step further in the interpretation of the evidence. Concerning the *līmum ša še'im* he stated that '(...) this title can be regarded as one of two indications of institutional involvement.' Here I think we may go further regarding the significance of grain and silver, the most important commodities of commercial (and social) life in early second millennium Aššur (and not only there), being in the hands of officials called *līmū*. We may safely conclude that in the classical Old Assyrian period the *līmum*-system is an essential part of the political system of the city of Aššur. The institution of the year eponym alone makes this unmistakably clear. And there is no convincing basis for an argumentation to separate the year eponym from the other *līmū*. Even if the references for the other *līmū* are few and still give little information on what exactly they were doing, as Dercksen already stated, also the *līmum ša kaspim*, 'the eponym of the silver' was surely playing a prominent role in the financial affairs of the city's government and as he showed, likewise the *līmum ša še'im* for the grain distribution.¹⁶

As for the *līmum elium* and the *līmum šaplum*, the evidence Dercksen collected give the impression that these two were sitting in (or better constituting) the same house: *a-na É li-mi-im : e-li-im ù ša-áp-li-im : i-ši-qí-ilš* (kt n/k 511, 11-12) '(silver) was paid to the house of the upper and the lower *līmum*' and *a-na li-mi-im : e-li-im ù ša-áp-li-im : nu-ša-qí-ilš-ma* (kt 92/k 539, 13-16) 'we paid (the silver) to the upper and the lower *līmum*'. This house of the upper and lower *līmū* surely has to be part of the larger *bēt līmim*. While Dercksen shows the possibility of connecting *elium* and *šaplum* with the architectural character of a building, he stated in the end that 'it is perhaps more likely that a difference in rank is meant that was unconnected with multi-storey buildings'.¹⁷ We will not decide the matter here, but to make things more complicated I would like to add an additional possibility for the interpretation of this office. The great majority of Old Assyrian titles refer directly to their areas of responsibility, as with the *līmum ša kaspim* and the *līmum ša še'im*. The *līmū* of silver and of grain demonstrate that the tasks of these officials are important and essential ones. Why should this be any different with the upper and the lower *līmum*? The terms *elium* and *šaplum* are also often used in a geographical way. The verb *elā'um* is used for people or caravans going from Aššur *up* to Anatolia, there are plenty of examples and this point is totally undisputed. The normal word for going back *down* to Aššur is *warādum*, but there is no derivation of this word used in the meaning 'down' or a similar meaning in Old Assyrian. But we have such a formulation in AKT 3 73, 25–29, where tin from the *ma-at ša-pi-ilš-tim* is not available at the moment. The reason is found in the letter AKT 3 74, 5–10 where it is stated that a caravan from the lower land is late and only when this caravan from the lower land will arrive, will the sender of the letter be able to buy tin and textiles according to the verdict of the city.¹⁸ Now Belmonte Marín has commented intensively on a very important document published recently by George from the WGL Folio 7637 belonging to the corpus

¹⁴ kt 87/k 421, 10f.: (The Silver) ¹⁰[E2] *kà-ri-im na-ad-ú* ¹¹[I]i-[m]u-um Am-ri-a. More new connected texts could be added. I restrict myself to mention the group kt 87/k 424, 434 and 444, with no. 424, 5-12 reading: *i-li-mi-im* ⁶A-šur-[ma]-lik 4 1/2 *ma-na* KU3.BABBAR ⁷i-na [I]i-mi-im *Ú-ra-a* ⁸na-ad-a-ku ⁷šū-ru-tim ⁹1 TÚG^{1a-na-am} : *i-li-mi-im* ¹⁰A-mur-Ištar : *na-ad-a-ku* ¹¹mi-ma : *a-né-em* ¹²É *kà-ri-im*. Three *līmū*, each of them acting on his own in the house of the *kārum*.

¹⁵ DERCKSEN 2004: 52ff.

¹⁶ DERCKSEN 2004: 60–62.

¹⁷ DERCKSEN 2004: 60.

¹⁸ AKT 3 74, 5–12: *ḥa-ra-num ša ma-at ša-pi-ilš-tim ša-da-at* (...) *šū-ma ḥa-ra-num ša ma-at ša-pi-ilš-tim e-ta-ar-ba-am ma-lá dī-in a-lim^{ki}* AN.NA ù TÚG.HLA : *a-ša-a-ma*.

of the Tigunani-texts.¹⁹ Interesting as it would be, we will not discuss this text here further and notice only that the sender, obviously a previously unknown ruler of Aššur, commented on the *kārum* of the city Šimal that its *kārum* was since time immemorial an important *kārum* of the city of Aššur and that the merchants, ‘his slaves’, are going up (*e-li-iš*) and down (*ša-ap-li-iš*) constantly.²⁰ There is no reason to assume that the wording used by the king of the name Pilah-šadû is exceptional, and it is clear the ‘up’ means direction north-west to Šimal and ‘down’ back to Aššur. There is of course no proof for now, but it seems reasonable to suggest here that a *līmum elium* in the earlier Old Assyrian period could in a way be responsible for the *export* along this route and the *līmum šaplum* for the import from that direction back to Aššur. In the end we have to admit that we don’t really know and need more texts to give a definite answer to this question. And it is still the possible that *elium* and *šaplum* do indeed simply refer to rank.

Several people acted as *līmū* in Aššur without being expressly named with a specified area of responsibility, e.g. Aššur-dān and Šu-Hubur in I 489, 27–31. The latter was also year eponym in the period in question (REL 88), but the former was not. One might think of a *cursus honorum* or something similar, but the names of those non-year-eponyms do not support such an idea, with the sole exception of Šu-Hubur. Many more examples could be found, but the situation is relatively clear for now. In many cases these *līmū* are surely *līmū ša kaspim* or one of the other *līmū*. The fact is that there are persons who were not only year eponym but also *līmum* of a different kind too. And one would suggest that these two offices were not held by one and the same person at the same time.²¹ Another interesting point not to be discussed here is the life and the family background of the *līmū*. It should be quite easy to single out a number of families who more often provided *līmū* than other families did, since we have the patronyms of most of early Assyrian year eponyms. To single out the elite families of the classical Old Assyrian period is surely a desideratum and might yield surprising results.²²

There is one important phenomenon we have not yet discussed and which is likewise ignored in the preceding discussions, though it was noted, i.e. the formulation *līmum ina ālim*.²³ We have this formulation twice, both with the same eponym Puzur-Ištar, son of Sabasia (REL 60). The first and better preserved one is ICK 1 143: 16–20:

¹⁶⁾ ITI.KAM *qá-ra-a-tim* ¹⁷⁾ *i-na a-lim* ¹⁸⁾ *Puzur-Ištar* ¹⁹⁾ DUMU *Sà-ba-sí-a* ²⁰⁾ *li-mu-um*
‘Month *qarrātum*, in the city Puzur-Ištar, the son of Sabasia, is year eponym.’

The second one is TPAK 1 129, 1’–5’:

¹⁾ (...) *i-na* ²⁾ [*a-lim* ^{ki} *li*]-*mu-um* *Pu*[*zur*₄-*Ištar* ³⁾ DUM]U *Sà-ba-sí-a* : IT[I] ⁴⁾ *na-ar-ma-a*[*k-A-šur*] ⁵⁾ [*š*]*a ke-na-tim*
‘In the city, year eponym is Puzur-Ištar, the son of Sabasia, month *narmak-Aššur ša kēnātim*.’

This is one of the oldest year eponyms known from original texts, the *līmum* is REL 60, ca. 1917 BC, the 5th year of Šarrum-kēn. There are different possibilities of how to understand this formulation, with either a local or a temporal background, or maybe even both. A purely local meaning would say that there are or have been places outside the city proper where a year eponym, different to that in the city, could be found. In theory it is also possible that there was a schism in the Old Assyrian population, with some people living within the city and another group outside, but for such an explanation we surely need more evidence from this early period. I have commented on this eponym in KRYSZAT 2004: 141f.²⁴ It is also this text where we find the first instance for the week eponymy called *hamuštum*. This last observation is important because the appearance of a week eponymy makes it extremely likely that the text was not only found in Anatolia, but that it was also written there. The date-formula noticeably puts the month notation in TPAK 1 129 after the *līmum*, whereas in ICK 1 143 it is the other way round. Both differ from the normal scheme:

normal:	month	<i>līmum</i>	—
ICK I 143:	month	city	<i>līmum</i>
TPAK 129:	city	<i>līmum</i>	month

¹⁹ BELMONTE MARÍN 2021: 104ff.; GEORGE 2017, 99.

²⁰ *iš-tu du-ri-im-ma* KAR URU *Ši-ma-la-a*^{KI} | KAR.GAL URU ^d*A-šur*₄ | *šu-ú a-na* KASKAL *ša* DAM.GÀR.MEŠ | *İR.MEŠ-ia ša e-li-iš* | *ù ša-ap-li-iš it-ta-na-al-la-ka* (From WGL Folio 7637 = GEORGE 2017: 99), BELMONTE MARÍN 2021: 104ff.

²¹ Among the latest published examples known to me is the group AKT VIa 213, 214 and 215, focussing around a *līmum* Amur-Aššur, the same person as the one known to have been year eponym but here obviously not acting as such, which seems clear due to 213, 7f., cf. Larsen’s notes on the texts.

²² See also DERCKSEN’s remarks (2004: 58ff.) on ‘The place of the eponymate within the life of a merchant’ and KRYSZAT’s notes on the single year eponyms as private persons in the comments to his list (2004: 66–157).

²³ Noted in VEENHOF 2003: 31 n. 59.

²⁴ See there for further comment on the text and the acting persons.

A temporal meaning of this formulation also seems possible, if we understand the formula in a way that says: there was a time when the year eponym was not (elected/chosen or whatever) in the city (but somewhere else). As tempting as such an interpretation might appear in the light of my interpretation of the early history of Aššur, I don't believe in this solution. Indeed, the most striking point is that the *only* two occurrences for this year in original documents both give the additional note '*ina ālim*'. And this clearly indicates that whatever was different, it was different *in this year* or a period of uncertain length before, but only shortly *after* this year. The one text mentioning the year after, REL 61, year eponym Pišaḥ-ilī (but written one year later), does not give any indication that something might have been amiss. We have no texts from the years directly before the eponymy of Puzur-Ištar and cannot say anything on the matter.

For a year eponym outside of Aššur we might have another example in KTS I 49b, 5–8: *ana ḥu-bu-li-a a-na li-mi-im ša Ša-lim-A-šur* DUMU *Ku-ku-si-a* 'for my debts to the *līmum* of Šalim-Aššur (...)'. This Šalim-Aššur is not known from any other text, but one of the people mentioned in the text is Buzāzu, which must be the son of Pūšu-kēn who took over his father's business after he went back to the city in REL 79 (1876 BC) or very shortly after. We know Buzāzu was in Kanesh at least in REL 93 and after a break of some years again between REL 101 and 112 (KRYSZAT 2004: 40ff.). The use of the sign /ab/ instead of /áb/ in l. 16 speaks for an earlier date within this timespan. DERCKSEN (2004: 45) commented on this eponym 'This eponym is not mentioned in KEL and may belong to the period after it towards the end of Level II',²⁵ while Veenhof noted 'probably a *kārum-līmum*'.²⁶ The problem is the formulation *ana līmim ša* 'for the *līmum* of PN'. This does not fit with Veenhof's explanation. I can offer no clear explanation for this wording, which appears indeed several times more. But more often, in connection with payments, the wording *ana līmim ša* does mean 'to the eponymy PN = for the year PN/eponymy of PN' and does so referring to an actual year eponym. A good example is the second oldest year eponym we know from original (i.e. outside the *līmū* lists) documents, found in CCT 10a, 18–21:

iš-tù li-mi-im ša I-du-a ²⁰⁾ 10 GÍN^{līm} 1 GÍN.TA *i-na* ITI.KAM *i-lá-ak-šu-um*

'from the year/eponymy of Idua (interest,) 10 shekels per each single shekel per month, will amount for him.'

Since in KTS I 49b also a debt being repaid, it would suggest that Šalim-Aššur was indeed also a year eponym, and it is understandable that Dercksen thought so, too. But the eponym lists, which are doubtlessly complete for the period in question here, do not include his name. Could this be an example for a year eponym *outside* of the city? Is there perhaps a connection with the formulation *līmum ina ālim* discussed above, in that Šalim-Aššur was a *līmum outside the city*? We simply do not know, but we certainly have to keep an eye on those occurrences outside the norm so that we are not deprived of possible explanations for problems we can't solve yet.

4. The *līmum*-system 3: The meaning of the word *līmum*

The latest comment on the meaning of the word *līmum* was made by Larsen, who accepts the possibility of connecting the Assyrian word *līmum* with the word *li'mum* used in Mari: it 'may be related to a word of West Semitic origin that denotes a social group such as a clan or lineage'.²⁷ But note that Dercksen in his chapter '4.1.1 The word *līmum*' does not mention the possibility of this connection. He carefully states that the 'Akkadian word for eponym, *līmum*, is of uncertain origin'.²⁸ Judging by the use of the word in Old Assyrian, we come back to Larsen's statement that '*līmum* refers to the power to represent the community, and to the persons representing the community'. This definitely describes the situation correctly, and who is the community other than your people? Even without any further linguistic discussion of the word it seems likely that we are dealing here with the same word as the one that is used in Mari that was discussed by Fleming. Fleming

²⁵ DERCKSEN 2004: 45.

²⁶ VEENHOF 2003: 52 with note 90.

²⁷ 'The term *limmum*, or perhaps *līmum*, has no convincing etymology, although it may be related to a word of West Semitic origin that denotes a social group such as a clan or lineage and which is also the designation for a god attested in the texts from Mari on the middle Euphrates. The best indication of the meaning of the word in the Old Assyrian texts comes from a few documents from Kanesh which deal with business conducted by the Office of the Colony. They show how one, two, sometimes three individuals acting on behalf of the colony are referred to as *limmu*. This happens in situations where an acknowledgement had to be given for a deposit on an account in the Office of the Colony, when a loan was extended to the colony or when the colony handed over certain documents to a person for transportation; the terminology leaves no doubt about the role of the men designated as *limmu*: "On behalf of the colony (three named men) as *limmu* received the money;" or "PN *limmum* entrusted the tin to PN2 on behalf of the colony." It is fair to assume that the word denotes the power to represent the community in concrete situations where the colony had to act as a legal person, and there can be little doubt that the same conclusion can be drawn concerning the men who were *limmu* in the capital' (LARSEN 2015: 126f.). The documents mentioned are the same as the ones discussed above.

²⁸ DERCKSEN 2004: 52f., see there for his whole discussion.

describes it as a designation for the single Yaminite tribes: ‘Like the *gayum*, the *li'mum* is better translated by a term broader than “clan” when it serves this specific function, and I have chosen the word “tribe”. In both cases, other contexts probably do allow a broader semantic range’.²⁹ When I connect the Assyrian word *līmum* with the word transcribed as *li'mum* in Mari (and later in Emar and Ugarit), as I do, then I have to answer the question: Were some of the Assyrians Amorite? The answer must of course be ‘no, not at all’. There are definitely more Amorite traces especially in the Old Assyrian onomasticon than have been accepted so far, as I hope to show on a different occasion. But those traces are still few and on many occasions we will have to say ‘not *Amorite*, but *West Semitic*’.³⁰ The Assyrians, whom we may label as *newcomers* (to the city of Aššur), must have been living in close contact with West Semitic groups.³¹ A clear systemic parallel to the year eponym is found far to the west in Emar (the word *līmum* itself is not used in the dating formulae there but appears in other contexts). This also might speak for an element that was originally not at home in Mesopotamia proper but was brought to Aššur by a group knowing and using the system of old from somewhere in the West/North-west, where some more parallels to Old Assyrian culture are evident.³²

5. The *līmum*-system 4: Preliminary conclusions

So what do we learn from a closer look at the *līmū*, be they the *kārum-līmū* or the ones in the city? The use of the term *līmum* shows the markings of a well-developed and, most importantly, institutional system with regularity (year eponym) and areas of responsibility (*līmum ša kaspim*, *kārum-līmum* etc.).³³ The *līmū* in the city of Aššur and in the centre of the trading network in Kanesh must be viewed as an institutional part of Old Assyrian society at least in the late 20th and 19th centuries. A hypothesis that sees the *līmum*-system as something that had evolved locally, has to find an explanation for how a structure like the *līmum*-system could establish itself or could be established by whomsoever, naming the protagonists of such a system with a noun *līmum* that the Akkadian language in Mesopotamia proper obviously did not use, and maybe largely even did not know.³⁴ And it does not explain the many shortcomings of the current views on early Assyrian history, in that they do not deal with central aspects of Old Assyrian culture as e.g. the god Šarra-mātān, the ‘King of the two lands/people’, or the *līmū* outside the office of the year eponym, or the immense Old Assyrian onomasticon, which is for a large part clearly rooted in a strong Northern Mesopotamian tradition, but which also is in other parts something very different with unexpected ties to the much later onomasticon from Emar in the late Bronze age. The *līmum*-system is by itself a very strong argument in favour of newcomers to Aššur in the late third millennium.

Viewing the *līmum*-system as something institutional has also important consequences for the question of the beginning of the office of the year eponym in Aššur. It makes it very unlikely that this system of dating started with Erišum, because then one should give him credit for inventing the *whole* system. It is not impossible but rather unlikely that Erišum should invent only a part of this system, especially such an important one as the year eponymy. It is much easier to conceive that the system was introduced as a whole, with most of the other *līmū* already present in the system. A system that of course had to adapt to the new circumstances in the city, *after* the immigrants took over the power in Aššur under the lead of Puzur-Aššur.

Accepting a close connection between Mari *li'mum* and Assyrian *līmum*, we get the chance to build another hypothesis for the understanding of the introductory formula of the early eponym lists. The beginning of KEL A reads:

[iš-tù SAG k]u-sí-im [š]a I-ri-ší-im [UGULA be]-lī-ni / iš-tù / li-mu-um [i]-šī-ik-nu-ni

The first part is clear: ‘From the beginning of the reign of Erišum, the overseer, our lord’. Following this we have two possibilities for the understanding of the second part. The first, proposed by Veenhof, is the obvious one, since we are dealing with a list of year eponyms: ‘after the *līmum* (i.e. the year eponymy) had been instituted/appointed’.³⁵ In Veenhof’s view the first statement would connect the beginning of the reign of the king and the first eponyms of the list with the

²⁹ FLEMING 2004, especially 209ff., see above.

³⁰ See the remark by Kogan ‘An interesting but rather complicated topic is that of the ‘West Semitic connections’ of the OA lexicon. Several of the OA lexical features with no precedent in Babylonia have transparent parallels in Hebrew, Aramaic and other WS languages’ (KOGAN 2006: 213).

³¹ See KRYSZAT 2021: 251 and the reasoning in the following sections of the present chapter.

³² See KRYSZAT 2021: 244ff.

³³ It fits in this context that other officials e.g. like the *laputtā'um* seem to have different tasks in Aššur than their known counterparts from the south. This can easily be explained by assuming that in Aššur the *līmū* fulfilled part of the tasks that other officials did in the Akkadian cities of the south, among them also the *laputtā'um*. See the discussion in DERCKSEN 2004: 65ff.

³⁴ With the only exception *līm* ‘one thousand’, should it be etymologically connected to *līmum*.

³⁵ VEENHOF 2003: 20.

moment the list was written down. The second statement would connect the beginning of the year eponym system again with the moment the list is written. This is not really conceivable and it seems strange to use such a rare double *ištu*-construction for such a simple statement. It makes much more sense to me to take the double *ištu* more seriously and see three points in time connected with each other instead of only two. We may try this simply by taking *līmum* here in the meaning ‘tribe’ and translate accordingly the second part ‘since the tribe was placed’ or, equally possible with an N-stem: ‘since the tribe settled down’. This interpretation would pretty much explain why we find the same formula in the beginning of the Mari Eponym Chronicle, which begins not with Erišum but with Narām-Su’en. It is an *ab urbe condita* formula which always refers to the same point in time, while the second point in time, here the beginning of Erišum’s reign, but in the Mari Eponym Chronicle the beginning of the reign of Narām-Su’en, can change. This means we could well have here the description of three moments in time instead of two: First the moment the tribe settled down, then the beginning of Erišum’s reign (and with it the beginning of the tradition of writing down the names of *līmū*, see below), and third the moment the text was written down. This possibility seems unexpected at first, but it is certainly possible and we should not carelessly ignore it. Such an interpretation of the first line of KEL A would even add a new aspect of early Assyrian *Erinnerungskultur*, comparable to the paragraph notes of the Assyrian King List.

Accepting the arrival of a large group of newcomers and also accepting that they took over the power in the city at the latest with Puzur-Aššur I gives a completely new basis for the understanding of the situation in Aššur. We may view some of the unclear phenomena under a new light: The need to create unity. Šarra-mātān, the ‘King of the two lands/people’ was invented because a common religious focus was urgently needed and the god Aššur could not fully fill this role in the early Old Assyrian periods. Consequently, this invented deity lost his relevance with the progress of the process of unification. The King List’s first version must have had its origin also in this early stage, to give the population of the city a common historical fixed point via the ancestors of their rulers. For the same reason the ancestors of Šamšī-Adad’s lineage were added some hundred years later.

6. The *līmū* and the Assyrian King List

So if we accept the hypothesis that Erišum I was not the inventor of the *līmum* system in Aššur, how then should we understand the paragraph of the King List that corresponds with the beginning of Erišum’s reign? The decisive words are at the end of the passage with the six kings beginning with Šulê and ending with Ilu-šuma: *ša li-ma-a-ni-šú-nu la u-TU-ni* ‘whose eponyms are ...’. The discussions of this passage are extensive and shall not be repeated here. I refer the reader to the summary in VEENHOF 2003: 21f. There are two main possibilities to explain the verbal form. One goes back towards a suggestion made by Freydank and was widely accepted, because it seems to make good sense. But it forces us to take the /TU/ in the text as a mistake for /DU/ to reach the verbal form *lā uddūni*, a stative D of the verb *idā’um*, which could be translated ‘(whose year eponyms) were not identified, marked, registered as such’.³⁶ Before going to the second possibility I will shortly comment on the meaning of the D-stem of *idā’um*. The best text passage known to me to enlighten its meaning is the letter KTS 2 9: 9–13. It runs as follows:

i-na šī-ba-at tup-pi-im a-wi-lu a-ni-ū-tum wa-du-ū a-šar a-wi-lu a-ni-ū-tum da-a-nu wa-du-ū : ma-e : šī-qī-ma ḥa-li-iq

which is to be translated as:

‘in the annex of the tablet those (above mentioned) people are recorded. Where those people are recorded as judges, there wet (the tablet) with water and wipe (the names) out!’

So, it is unmistakably clear that we are talking here about writing cuneiform on clay. Applied to the passage of the King List, we have to understand ‘whose eponyms were not written down’.³⁷ This can only mean that before the time referred to by the passage in the King List, year eponym lists were *not* recorded in written form. Since we are talking about a city with scribes around it is not too far-fetched to say that this passage might well refer to an environment where writing was *not* taken for granted.³⁸

The second possibility for understanding the verbal form would be *lā ūṭūni* (<*waṭiuni*) ‘were not/could not be found’, without any need for emendation. What would this then mean for the understanding of the passage in question? In the end

³⁶ FREYDANK 1975: 173f.

³⁷ In his most recent treatment of this passage VEENHOF (2017: 58) translates ‘(...) whose year eponyms have not been marked/found’.

³⁸ Within the discussion on the disputed verbal form VEENHOF notes that neither TU nor ṬU were used in Old Assyrian. This is not quite true, there are a number of instances for TU, e.g. TC 3 219, 16 IGI *Tu-ra-am-i-li* or in the logographical spellings ⁴MAR.TU and SA/SA₁₀-TU/DU.

the result would be nearly the same: The *līmū* were not found because at the time when this passage of the King List was first written, the old eponyms of the time before Erišum could not be found. But this would also mean that the dating with *līmū* was already in use, otherwise such a note would not make any sense. And this applies to an interpretation as *lā uddūni* as well. So, however we understand this passage in the King List, it always implies that the dating with *līmū*, and most likely the complete *līmum*-system, already existed. This result stands in opposition to Veenhof's statements that 'the institution (of the year eponym) did not yet exist'³⁹ and that 'the conclusion that the office of eponym started with the reign of Irišum may shed new light on Aššur's commercial policy ...'.⁴⁰

So, if one rejects the idea that the eponym-dating (and hence the beginning of the *līmum* system in Aššur) started with Erišum, what kind of alternative can one offer? In accordance with the hypothesis of a group of immigrants bringing this system with them to Aššur, the answers are: the *līmum* system came with this group (which most likely called itself a *līmum*) and it became institutional when these immigrants took the power in Aššur in the person of Puzur-Aššur or shortly after. The existence of the *līmum*-system in Aššur is a strong argument for the immigration of a larger group of people from a north-western region to the city of Aššur. It is hard to imagine how such a system could find its place under Old Akkadian or Ur-III rule, without any evidence for such a system anywhere close to this part of Mesopotamia.

The end of this discussion seems to be a good place to remind us of the form of the Old Assyrian script, with its few logograms and so definitely easy to learn, even for a broader audience. It could well be designed exactly for introducing a new, large, and untrained group into the mysteries of cuneiform writing. I suggest that this script was introduced at the latest at the beginning of Ilu-šuma's reign, since the only inscription we have for his predecessors does not feature the typical Old Assyrian script.

Accepting the hypothesis of a two-part population in Aššur in the early Old Assyrian period we have to change the view we have on the city's society during that period. We must look at it as a society in transition, with its leaders trying to forge the city's population into one to become strong enough to survive in an often war torn environment.⁴¹ This is already evident in the King List, which I see in its origins as a list of the ancestral rulers and the descendants of the two ethnically different groups – native Akkadians and the *līmum*-people, who at least in part must be the people of the tent-dwellers of the King List.⁴² After the arrival of a large number of Šamši-Adad's followers more than 200 years later, history in Aššur was going to repeat itself, as the King List in the form it survived shows. Now the need to create an internal unity is great again, the problems are demonstrated in the Puzur-Sîn inscription. The struggle between the different fractions in the city finds its echo even in the name of the Late Old Assyrian (King) Bēlu-bāni, who chose his name for legitimation, being the son of a usurper (Adasi) himself. He wanted and needed to take a throne name that had strong connections to older Assyrian traditions and choose the name of the old family god of the Puzur-Aššur Dynasty as a theophoric element 'Bēl is (my) creator'.⁴³ There is even a chance, however small, that Bēlu-bāni was indeed a descendant of the Erišum-family and that Bēl was still his family god.

7. The *līmum*-system 5: *bēt ālim* and *bēt līmim*

With an etymology for *līmum* approaching 'clan, tribe', we would not be very far away from *ālum* 'city', if we see both essentially as a large body of people. But there is a difference that is best again made clear by the idea of Larsen as recorded by DERCKSEN (2004: 53): 'Larsen noted that *līmum* refers to the power to represent the community, and to the persons representing the community'. I fully agree, but how can one be identical with the other if one is *representing* the other? Because the community as such can only be the people of the city state. Whatever *līmum* is, it is in the Old Assyrian context most likely more restricted than *ālum*. By this reason alone it seems rather unlikely that these two should be completely identical. Therefore I would like to suggest the hypothesis that the *bēt līmim* must be an *institutional part* of the *bēt ālim*. And this situation is mirrored in Kanesh, where more and more *kārum-līmū* appear in the context of the *bēt kārim* (and not acting against it), which can hardly be an accident. This situation seems to find a preliminary end with the reign of Šamši-Adad. In the texts from the later Old Assyrian period we don't find any other *līmum* than the year eponym.

³⁹ VEENHOF 2003: 21.

⁴⁰ VEENHOF 2003: 22.

⁴¹ An indirect hint to the situation might be found in the reign of Šarrum-kēn, who ruled for 40 years, but is in later royal inscriptions remembered for having worked on the city wall of Aššur.

⁴² JANSSEN 2019 sees an Amorite king list as the origin of the Assyrian King List.

⁴³ For Old Assyrian Bēlum see now KRYSZAT 2021: 245–247.

8. The *līmum*-system 6: a *līmum*-system outside of Aššur before Šamšī-Adad?

For the Middle Bronze Age we mentioned already Emar as a place where an eponym system similar to the Assyrian one was used, albeit without using the word *līmum* or *li'mum* for it. But now we have occurrences of the word in texts from illegal excavations in the ancient town of Tigunani, well known from ancient sources. Since many of the relevant texts are still unpublished, a discussion of the Tigunani-texts is not the focus of the present study.⁴⁴ But in one aspect the *līmū* mentioned in those texts are of importance for us: the Tigunani *līmū* are often listed in pairs, sometimes as year eponyms (in connection with month names) and sometimes obviously not. The latter phenomenon leads to the question: How and when did these different *līmū* arise? From all we know, Šamšī-Adad was able to spread the Assyrian system of year eponyms in his Kingdom of Upper Mesopotamia. But did he also bring the system of *līmū* that were not year eponymies? One would think that the wide acceptance of dating by year eponyms by the cities under Šamšī-Adad's rule is hardly conceivable if a similar system had not been known in large parts of his kingdom *before* his reign. To say it more drastically, the fact that we have a double-*līmū* in Tigunani makes it very likely that the system there is *not* derived from Šamšī-Adad's spreading of the eponym-system, which only involved dating with single eponyms. But it seems well possible that there we have a *līmum* system similar to the one in Aššur *before* Šamšī-Adad.

9. Appendix: Some notes on the eponym lists and Bloch's theory of the Conquest Eponyms⁴⁵

KEL A and G are eponym lists of a special kind: they are provided with colophons.⁴⁶ KEL G even features a list of 7 witnesses, confirming the correctness of the eponym series listed on the tablet. Maybe they are not witnesses but more likely judges to judge the work of the young scribe? If this should be the case, then one feels reminded of the seven divine judges in the building called *mušlālum*. Judges and the name of the building are found in the royal inscriptions of Irīšum I, and the judges bear names like *Pūšu-kēn* 'his word is (always) true' or *Aššur-ḫablam* 'Protect the innocent one'. I do not believe that the number 'seven' in these two cases is purely accidental. KEL A has no witnesses, but it features a very different phenomenon: a prayer hidden in the spelling of the names of the three year eponyms named Enna/um-Aššur 'Mercy, oh Aššur' or 'Aššur have mercy on me' (KRYSZAT 2015). Both texts are extraordinary pieces of cuneiform writing. KEL G, written close to the end of the 18th century, has an atypical 'wrong' order of the columns (1-2-3-4) and the writing itself is not very nice, the scribe seems to have had some problems. But this affects mostly the appearance of the tablet, there are not many mistakes in the text itself, indeed very few, and some of them are still debated. See some of the contributions to this volume dealing with the problems with entries of the individual eponyms. It is simply the script itself that is not really beautiful. There are some very *non-Assyrian* features in the text, such as

- the use of *šur*₄, which came to Aššur with Šamšī-Adad, but note that *šur* and *šūr* are also used,
- the use of *ti*₁, which was originally discarded from the Old Assyrian Syllabary of the lower town level 2 texts, along with its colleagues *ab*₁, *la*₁ and some others; these signs were given up in favour of their smaller and easier to write counterparts *tī*, *áb* and *lá* (KRYSZAT 2013),
- also unusual for Old Assyrian writing, even from the later Old Assyrian period, is the spelling with an isolated *-i* as in *I-sà-i-a*, against normal Old Assyrian *I-sà-a*
- we meet for the first time in Old Assyrian the spelling /30/ for /Sîn/
- and the spelling (Aššur-) *ba-lá-ti* for classical Old Assyrian (Aššur-) *ba-lá-aṭ*; the name written with a final *i* also occurs in the Mari Eponym Chronicle, which in addition, instead of *Ilī-ālum* has the name *Ilī-ennam*, which is unknown in Old Assyrian.⁴⁷

It seems justified to think of the young scribe of KEL G as a non-Assyrian, because Aššur-*balati* is hard to understand from an Assyrian perspective. In Old Assyrian, this name is always written without the final /i/. Compared to the writing in the much older list KEL A, this may easily count as a reflection of the evolution of Old Assyrian scribal habits. See VEENHOF 2003 and KRYSZAT 2013 for further comments on the text.

Other exemplars of the eponym lists are just copies for use in a merchant's office or, like KEL F, just school texts. This becomes important when we use the eponym lists to verify the famous entries with the so called 'conquest eponyms of Šamšī-Adad' in the contemporary section of the eponym list. The entries do not fit, and BLOCH (2014) went a long way

⁴⁴ For the geographic scope of the Tigunani texts see now BELMONTE MARÍN 2021.

⁴⁵ The last comprehensive comment on the eponym lists is MICHEL 2012, see there for the details.

⁴⁶ See the comments on the colophon of KEL G by DERCKSEN 2008 and KRYSZAT 2008: 212f.

⁴⁷ For a different view of the Assyrian name *Ilī-ālum* (*Ilī-Anum*) see DERCKSEN 1991.

trying to explain the entries in the King List. Here I will not enter the discussion whether Bloch was right or wrong, because everything that needs to be said is found in VEENHOF's paper in this volume (Ch. 4). There is one thing though that I might add: I am sure Bloch is wrong not only because of the many striking arguments given by Veenhof but also because it seems hardly conceivable to me that texts of the quality of KEL A and G would have to be so corrupted if Bloch's argumentation were true. The lists would be absolutely unrecognisable should Bloch be right. And the mistakes would all have to be in the same section of the lists, too. I do not believe this. As Werner Nahm suggested in the discussion in Lille, the eponym lists could well be part of the scribal curriculum in Aššur, a hypothesis that makes a lot of sense.

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9. New information on Kish in the early Old Babylonian Period

RIENTS DE BOER

This paper reassesses the political history of Kish in the early Old Babylonian Period. Specifically, it looks at Kish' situation from the fall of the Ur III empire (ca. 2004 BCE) until its incorporation into the Old Babylonian kingdom by Sumu-la-El around 1869 BCE. At the end of the article four early Old Babylonian tablets and an envelope fragment from Kish kept in the British Museum are fully edited.

Kish is one of those sites in southern Mesopotamia that has been inhabited for millennia.¹ The site, which has yielded evidence of settlement from the Ubaid period (ca. 5500–4000 BCE) down to Islamic times, actually has two main mounds: Ingharra (ancient Ḫursagkalama, main deity: Ishtar) and Uḫaimir (ancient Kish, main deity: Zababa). Both sites together formed an urban conglomeration in Antiquity.

1. Kish in the third millennium BCE

In the Jemdet Nasr and Early Dynastic period (ca. 3100–2334 BCE), Kish was one of the most important cities in the region. Its kingship came to represent a kind of universal kingship over Mesopotamia. This is reflected in the Akkadian word *kiššatum* ('Kish-dom') which came to signify 'totality' or the 'world/universe'. Kish features prominently in the Sumerian King List with four dynasties that purportedly ruled over Mesopotamia.² The prominence of Kish in the third millennium BCE has led to a number of archaeological excavations in the early 20th century,³ but they have not yielded the anticipated results. Only the so-called 'Chariot-Burials' could perhaps attest to third-millennium royalty in Kish.⁴

According to the Sumerian King List and the literary text 'Sargon and Ur-Zababa',⁵ Ur-Zababa was king of Kish with Sargon as his cupbearer before the latter took power.⁶ Kish was ruled by the Old Akkadian kings from ca. 2334 to 2150 BCE. Noteworthy is an episode of rebellion against Sargon's grandson Narām-Sîn led by a Kishite ruler with the conspicuous name 'Iphur-Kish'.⁷ Until the reign of the Ur III kings we have little information on Kish. However, a clay bulla from Kish bears a seal inscription with the name of the last Old Akkadian king Šu-Turul (ca. 2168–2154 BCE). This suggests that Kish was not part of the Gutian sphere of influence in southern Mesopotamia.⁸

¹ The Trustees of the British Museum are kindly thanked for the permission to (re)publish these tablets, they were first provisionally published in my thesis, DE BOER 2014: 450–456. I am indebted to Prof. M. Stol and Dr. S.A. Moore for their comments on this chapter. Dr. C.B.F. Walker was kind enough to share with me his information on the provenance of the tablets.

For previous studies see GIBSON 1976–1980, DONBAZ/YOFFEE 1986: 3–22, YOFFEE 1977, CHARPIN 2004: 88–89 and 429–431. The most elaborate history of Kish is MOOREY 1978: 164–181.

² GLASSNER 2004: 117–127.

³ The first extensive official excavations were carried out by DE GENOUILLAC in 1911 and 1912 (DE GENOUILLAC 1924 and 1925). Between 1923 and 1933 an American-British expedition was led by LANGDON (Four reports were initially written: MACKAY 1925, LANGDON 1924, WATELIN and LANGDON 1930 and 1934; MOOREY 1978 gives a synthesis of the 1923–1933 excavations). GIBSON 1976–1980: 613–614 and 1972: 67–920 provides an overview of the excavations.

⁴ GIBSON 1976–1980: 616–617 and MOOREY 1978: 167.

⁵ AFANAS'EVA 1987, COOPER/HEIMPEL 1983, WESTENHOLZ 1997: 51, the ETCSL has an online edition: t.2.1.4: <http://etcsl.orinst.ox.ac.uk/cgi-bin/etcsl.cgi?text=t.2.1.4#> [accessed 4 January 2024].

⁶ The Sumerian King List does not list Ur-Zababa immediately before Sargon, several rulers from Kish and Lugal-zagesi of Uruk separate Ur-Zababa from Sargon.

⁷ EDZARD 1976–1980: 610–611, MOOREY 1978: 171–172, WESTENHOLZ 1997: 221f.

⁸ MOOREY 1978: 173, the text can be found in FRAYNE 1993 RIME 2: 215 as E2.1.11.2001.

Under the Ur III empire (2112–2004 BCE) Kish was a province with a provincial governor (ENSI₂), it is attested in administrative texts from Šulgi 43 until Ibbi-Suen 2.⁹ The names of several *ensis* are known: Ugula,¹⁰ Ilum-bānī,¹¹ and Aḫum-bānī.¹² However, the most interesting one is called Šu-Enlil, he figures as the *ensi* of Kish in one of the letters from the ‘Correspondence of the Kings of Ur III’.¹³ The historicity of the letter is disputed,¹⁴ but it mentions that Šu-Enlil had switched sides from Ibbi-Suen of Ur to Išbi-Erra of Isin. The exact year of Šu-Enlil’s defection is debated.¹⁵

2. Kish under Isin in the early Old Babylonian Period (ca. 2004–1900)

With the reign of Išbi-Erra (2017–1985 BCE)¹⁶ we enter the early Old Babylonian Period (ca. 2004–1792 BCE). The kings of Isin did everything to give the impression that they were the true heirs of the Ur III empire, but it is now clear that many other cities had regained their independence and that Isin’s sway over the former Ur III heartland was not as absolute as the Isin rulers would have us believe.¹⁷

What was the situation in Kish? Was it ruled by the aforementioned former Ur III governor Šu-Enlil or did Išbi-Erra of Isin rule it? Unfortunately, there are no textual sources from Kish, but we do have a source from Isin: the so-called Isin Craft Archive.¹⁸ This text group contains ca. one thousand texts documenting the activities of a royal workshop. The officials of the workshop regularly issued goods to people from within and outside the kingdom such as the Amorites, but also, in one instance to two soldiers from Kish for a campaign to Karaḫar in the Zagros mountains.¹⁹ Unfortunately, this does not tell us anything directly about Kish’s situation. We might speculate that it fell within Isin’s orbit, but hard evidence is lacking. A liver omen from Mari indicates that the army of Išme-Dagan, king of Isin from 1953 to 1935 BCE, was defeated near Kish.²⁰ Under the same king of Isin the ‘Nippur Lament’, a text about the rebuilding of Nippur after a catastrophe, was probably composed. The lament mentions Kish with the epithet ‘Kish, surrounder of Sumer and Akkad, its dominion superlative’.²¹

3. Kish under attack from Sumu-El of Larsa (1894–1866 BCE)

The textual sources are silent about Kish for more than a century when we see that the king of Larsa Sumu-El dedicates a string of year names to a conflict with Kish. The year name Sumu-El 11, commemorating an event around 1885 BCE, mentions the defeat of an army of Kish. The same event is mentioned in his year names 12, 13, and 14.²² Unfortunately, we do not know about the ramifications: when Kish’s army was defeated, was Kish itself also seized? Why was Kish’s defeat so important to Sumu-El that four of his year names were dedicated to the event? Does it perhaps indicate a long standing feud between the two cities? etc.

There are four possibilities for the identity of the defeated Kishite ruler: Abdi-..., Ašduni-iarim, Agānuḫi/Agāniḫi, or another ruler altogether.²³ To start with the first: Abdi-.... This king is known from a fragmentary royal inscription where one Abdi-[El or Erah], son of Ḫuzu-... is described as ‘King of Kish(?)’.²⁴ There are at least two early Old Babylonian rulers by the name of Abdi-Erah: a ruler from the Diyala region,²⁵ and one of the so-called ‘Mananā’ dynasty

⁹ MICHALOWSKI 2011: 198.

¹⁰ TCL 2 5490: 7 (Amar-Suen 4).

¹¹ PDT 2 959 r iv:22 (Amar-Suen).

¹² YOS 5 78:6 (Amar-Suen 8).

¹³ The letter from Puzur-Numušda to Ibbi-Sin, MICHALOWSKI 2011: 192–200, 439–462.

¹⁴ HUBER 2001 argued for this, but a lot of scholars believe the correspondence holds at least a kernel of truth.

¹⁵ MICHALOWSKI 2011: 192 pleads for the last years of Ibbi-Suen’s reign (2028–2004 BCE).

¹⁶ Išbi-Erra was the first king of Isin and much has been written about his origins and accession, see MICHALOWSKI 1995 and 2011: 182–185.

¹⁷ We can mention for example the independence of Malgum starting early in the reign of Ibbi-Suen, see OZAKI *et al.* 2021: 35.

¹⁸ The archive is studied by VAN DE MIEROOP 1987.

¹⁹ BIN 10 149: 7-8 (dated to IE.23).

²⁰ RUTTEN 1938: 44, MOOREY 1978: 174 writes about this: ‘... the presence of this record in the liver-omens’ tradition of Mari suggests that Kish was caught up in a conflict between the rulers of Isin and Mari’.

²¹ TINNEY 1996: 115 and 171.

²² SIGRIST 1990: 18–19 and FITZGERALD 2002: 57–69.

²³ Sumu-ditāna of Marad is often mentioned as a king of Kish. In DE BOER 2013: 87–88, I showed that he could not have been a king of Kish.

²⁴ FRAYNE 1990 RIME 4 E4.10.2.1 (p. 662): *ab-di*-[...], DUMU *ḫu-zu*-[...], LUGAL K[*i*ški], *ša-am-š*-[...], BÀ[D...], *a*-[...], *x*-[...], *i*-[...].

²⁵ DE BOER 2014: 203.

kings.²⁶ The latter one might be the same king that is known from our fragmentary royal inscription. The other two kings, Ašduni-iarim and Agānuḫi/Agāniḫi, will be discussed below.

4. Ašduni-iarim, king of Kish

The first attestations of a named, independent ruler of Kish in the Old Babylonian period are provided by the three royal inscriptions of Ašduni-iarim.²⁷ All three discuss the same events, but there are long, short, and very short versions. Ašduni-iarim, who carries a unique Amorite name,²⁸ first gives his titles as king of Kish and then recounts how he fought against an unnamed foe for eight years. Eventually, he prevails after which he builds a city wall and digs a canal. For the second time hostilities erupt against Ašduni-iarim and he builds another wall and dams up a canal. The Ašduni-iarim inscriptions contain a few oddities that are not easy to interpret.²⁹

The ‘four quarters’ became hostile and Ašduni-iarim fought for eight(!) years, after which his adversary returned (*lu i-tu-úr*) to ‘SÚ.A’. There is a possibility that SÚ.A denotes the Šimaški polity in Iran. The name Šimaški is spelled either syllabically or it is written rebus-like as LÚ.SÚ.A (LÚ = *šu/ši*, SÚ = *mašku*).³⁰ The problem, of course, with equating SÚ.A with LÚ.SÚ.A lies in the fact that LÚ is an integral part of the rebus-like spelling. Others have interpreted SÚ.A as IM.A.SU ‘clay’, however, as far as I know there are no other instances in Old Babylonian royal inscriptions where an adversary was ‘turned to clay’.³¹ Military encounters between the Šimaškians and Mesopotamian city-states are attested for at least one other early Old Babylonian ruler: Anum/Illum-mutabbil of Dēr.³²

Ašduni-iarim says that he took food and went on an expedition of only a day (*ūmakka*) after which he subjugated the enemy country ‘for forty days’. Another interpretation is to read here ‘within forty days’, despite the lack of the preposition */ina/* in the text.

It is impossible to tell whether the Ašduni-iarim inscriptions deal with a longstanding conflict with Larsa that resulted in Sumu-El’s victory. Ašduni-iarim’s unnamed adversary will have to remain unknown for now.

5. Agānuḫi/Agāniḫi: a new king of Kish

We can add another king to the short list of independent Old Babylonian kings of Kish: Agānuḫi/Agāniḫi. This ruler has already been known for quite a while, but only now can we present a satisfactory reading for his name.



Fig. 9.1. An impression of the Nanna-kiag seal, with the Aganuḫi servant line from BALL (1899: 20, public domain).

The first occurrence of Agānuḫi/Agāniḫi is on a cylinder seal published by C.J. Ball in 1899 (Fig. 9.1). Ball read the last line on the seal (‘the servant line’) as ‘*arad abīnū shamshi*’ ‘The servant of our Father the Sun’.³³ This anachronistic

²⁶ DE BOER 2014: 239–240.

²⁷ One clay cone is in the British Museum, one in the Louvre (these were re-edited by FRAYNE 1990 RIME 4 as E4.8.1.1 and E4.8.1.2) and a clay nail is in the Vorderasiatisches Museum, edited by MARZAHN 1999. See earlier discussions by GODDEERIS 2002: 253 and CHARPIN 2004: 88–89.

²⁸ MARZAHN 1999: 271, a variant is Ašduni-erim.

²⁹ See the bibliography in FRAYNE 1990 RIME 4 p. 654, add GELB 1961: 270.

³⁰ STEINKELLER 1988, 2007, and 2008.

³¹ A reference to ‘people turning into clay’ known to me comes from the Flood Story of the Gilgamesh epic, cf. GEORGE 2003 tablet XI: 135 p. 711, here it denotes the death of the people who did not survive the Flood.

³² FRAYNE 1990 RIME 4 E4.12.2.

³³ BALL 1899: 20, this means that he understood: IR *a-bi-nu* UTU.

reading is incorrect. The seal was again published by Langdon in 1927. Langdon, apparently unaware of Ball's earlier publication, read the complete seal inscription as: ^dŠEŠ-KI.ÁG, DUMU *ma-nu-um-ša-ni-in-šu*, ÌR *a-bi-nu-ḫi*.³⁴ He correctly identified the owner of the seal as one Nanna-kiag, son of Mannum-šaninšu, but I differ on his reading of the ruler's name. Frayne (1990) RIME E4.0.6 p. 815 reads the seal as: ^dŠEŠ-KI.ÁG, DUMU *ma-nu-um-ša-ni-in-š[u]*, ÌR *a-bi-nu-x*, he was not aware, however, of Langdon's 1927 article. I propose now to read ÌR *a-ga-nu-ḫi*.

The same royal name is found in the year name of RSM 49, a tablet kept at the National Museum of Scotland (number: 1911.203.21). A photo can be consulted online through the CDLI website.³⁵ This text was first published by Langdon (1911) who read the year name as MU *a-bi-a-ni-šu*.³⁶ Dalley published a new copy of the text in 1979, but she provided no details on the year name. I suggest to read here MU *a-ga-a-ni-ḫi*, 'Year: Agāniḫi'.

In my 2014 thesis I published a text containing a year name with this king, but there I read the name as *a-bi-x-x-x* and *a-bi-a²-nu²-uh²*. The text is now fully edited below (no.1, BM 108915), together with a number of related texts from Kish. It is now better to read the year name as MU *a-ga-a-nu-ḫi* LUGAL.E 'Year: Agānuḫi the king'.

The name of the king has two similar versions: Agānuḫi and Agāniḫi. He ruled for at least one year, given the fact that we have only an accession year name. The name Agānuḫi/Agāniḫi is without precedent in the early Old Babylonian period, even though it resembles names like *a-ga-nu¹-[x]*, occurring in YOS 14 82: 8 and *a-ga-nu-um* in Edubba 7 115: 25–26. We cannot be certain whether Agānuḫi/Agāniḫi is an Amorite or Akkadian name (or Hurrian, Elamite etc.), but the two attested writings show an alternation between /i/ and /u/, a phenomenon well known for certain Amorite names from the later Mari texts.³⁷

6. Yawium, the last independent king of Kish

Agānuḫi/Agāniḫi probably ruled Kish before Yawium. This is based on the group of four early Old Babylonian Kish texts published below: one of them has the Agānuḫi/Agāniḫi year name, two have Yawium year names, and one a probable Yawium year name. Yawium must have been the last independent ruler of Kish. We have no royal inscriptions for Yawium, but at least eight of his year names are known chronicling his activities. Almost all of them occur in the private archive of Šissu-nawrat, an inhabitant of Kish.³⁸ The list of year names for Yawium is shown in Fig. 9.2:

	Year names of Yawium	Translation	Oath by	Text
1/a	MU <i>ia-wi-um</i> LUGAL.E	Year: Yawium is king		BM 108925
b	MU.ÚS.SA ^{GIŠ} GIGIR <i>ia-wi-ú-um</i> MU.DÍM	Year after: Yawium fashioned a chariot	Zababa and Yawium	RSM 29
c	MU URUDU.ALAN.LUGAL	Year: a royal copper statue		OECT 13 281
	MU URUDU.ALAN.LUGAL		Zababa and Yawium	OECT 15 377
	[MU URU]DU.ALAN.LUGAL		Zababa and Yawium	BM 103192
d	MU <i>ḫi-ri-tum</i> KIŠ ^{KI} BA.BA.AL	Year: the canal of Kish was dug	Sin and Ḫaliyūm & Zababa and Yawium	YOS 14 111
	MU <i>ḫi-ri-tum</i> KIŠ ^{KI} BA.BA.AL			YOS 14 167
e	MU BĀD {KI} KĀ.DINGIR.RA ^{KI} BA.DŪ	Year: the wall of Babylon was built		BM 108918
	MU. ¹ BĀD ² [...] BA.DŪ			BM 108928
f	MU.ÚS.SA BĀD KĀ.DINGIR.RA< ^{KI} > BA.DŪ	Year after: the wall of Babylon was built	Zababa and Yawium	RA 8 4
	MU.ÚS.SA BĀD KĀ.DINGIR.RA ^{KI}		Zababa and [...]	RSM 45
g	MU KUŠ.Á.LÁ [...]	Year: Yawium fashioned an <i>alū</i> drum for the temple of Zababa	Zababa and [...]	RSM 43
	MU KUŠ.Á.LÁ X		[...]	OECT 13 288
	MU KUŠ.Á.LÁ		Zababa and Yawium	RSM 59
	MU KUŠ.Á.LÁ <i>i[a-wi-um]</i> É ^d za-ba ₄ -ba ₄ .RA MU.NA.AN.DÍM			RSM 55
h	MU <i>su-mu-di-ta-¹na¹</i> BA.UG ₇	Year: Sumu-ditāna died	Zababa and Yawium	RA 8 3
	MU.ÚS.SA [...] ALAN ² .A.X [...]	Year after: a statue(?)	Zababa and Yawium	RSM 41
–			Zababa and Yawium & Nanna and Mananā	RSM 36

Fig. 9.2. Overview of all known Yawium year names (adapted from DE BOER 2014: 423).

³⁴ LANGDON 1927: 43–44. Langdon also mentions that the seal is made of black diorite, 25 mm long and in the collection of one Mr. Casson, New College, Oxford. This must have been Stanley Casson (1889–1944), a well-known classical archaeologist; the current whereabouts of the cylinder seal are unknown.

³⁵ <https://cdli.mpiwg-berlin.mpg.de/artifacts/453226> [accessed 7 January 2024].

³⁶ LANGDON 1911: 238 n. 50, he assumed a variant of the 'year name' Sumu-abum 10.

³⁷ E.g. Bunu-Ištar and Bina-Ištar (king of Kurdā) and Sumḫu-rabi/Simhu-rabi (governor of Saggarātum).

³⁸ See DE BOER 2014: 146–148 on this archive with previous bibliography.

As one can see, they mostly concern cultic activities: the making of a chariot (b), royal statue (c) and a drum for the temple of Kish's city god Zababa (g). Two concern building activities: a channel for Kish (d) and the building of the wall of Babylon (e and f).³⁹ One year name mentions the death of a neighboring ruler, Sumu-ditāna of Marad, (h). Noteworthy is the fact that Yawium occurs twice with a 'Mananā'-dynasty king in the oaths: once with Mananā and once with Ḫaliyūm.

In addition to the year names, Yawium occurs as the writer of two letters in the Ikūn-pīša Letter Archive (DE BOER 2021, *IPLA* 12 and 13), but they give us no useful political information. The Ikūn-pīša archive can tentatively be dated to ca. 1885–1880 BCE. If correct, this means that Yawium ruled from ca. 1885/1880 until – at the latest – 1869 BCE when Kish was annexed by Babylon. The eight Yawium year names fit well in this timespan and this allows for additional Yawium year names (or ephemeral rulers) to surface.

Kish lies only 17 km east of Babylon and it comes as no surprise that Sumu-la-El, Babylon's first king, took possession of Kish during his conquests of the surrounding area. Sumu-la-El's 13th year name commemorates an event that took place in 1869 BCE: 'Year: Kish was destroyed', the year name is repeated for years 14, 15, 16, and 17.⁴⁰ It is not completely certain that Yawium was the ruler of Kish during Sumu-la-El's conquest and CHARPIN (2004: 89) even argues for the kings of Damrum (the so-called Mananā dynasty) as rulers of Kish before Babylon. The current state of research allows for both possibilities. Sumu-la-El's 19th year name states that Kish's city walls were torn down: 'Year: the city wall, reaching the heavens, of Kish was destroyed'. However, he also (re)built the temple of Kish's warrior-god Zababa (the Emeteursag 'House worthy of the Hero')⁴¹ as is evidenced by his 30th year name.

Concluding Remarks

After the annexation by the Babylonians in 1869 BCE, Kish would never regain its autonomy again and it remained an integral part of the Babylonian heartland for the rest of Mesopotamian history. There is no doubt that this sketch of Kish's history in the early Old Babylonian period is incomplete and that new information, when published, will necessitate new conclusions. Doing research on the myriad of kings in the early Old Babylonian period is an exciting, but also frustrating undertaking. We do not have a 'Mari archive' detailing for us the countless intricacies of political life. Nevertheless, *au fur et à mesure* we can make progress. This contribution, giving the name of another king of Kish, Agānuḫi/Agāniḫi, as well as the edition of four early Old Babylonian Kish texts is only another piece to the puzzle that is Kish's history.

Text editions

It is crucial to mention that the BM numbers of the Kish tablets edited below come from the 1914-4-7 group. This group of texts entered the British Museum on the seventh of April 1914. According to a list kindly provided by Dr. C.B.F. Walker, it was acquired from a dealer called Amor and was previously in the hands of a man called Maimon. The current BM numbers from the 1914-4-7 collection are 108854–108978, a total of 124 objects. Not only internal data from the tablets published below indicate Kish as its provenance, but also the fact that the Ašduni-iarim clay cone mentioned above stems from the very same group of texts: its museum number being BM 108854 1914-4-7 20. According to further indications from the archives of the British Museum (again I am indebted to Dr. C.B.F. Walker), a large part of the 1914-4-7 group stems from Uḫaimir (Kish), including all of the tablets edited below.

³⁹ On the interpretation of year names concerning events from neighboring rulers: CHARPIN 2004: 85–86 n. 301.

⁴⁰ See DE BOER 2018: 75–80 for the year names of Sumu-la-El.

⁴¹ GEORGE 1993: 125 and SALLABERGER 2017: 167–168.

1. BM 108915 (1914-4-7 81)

Dimensions (cm): 6 × 4.2 × 2.4

Agānuḫi 1/X

Surety. Sîn-naši accepts the liability for a claim of 16 shekels of silver owed by Ahuni to Yamukum.

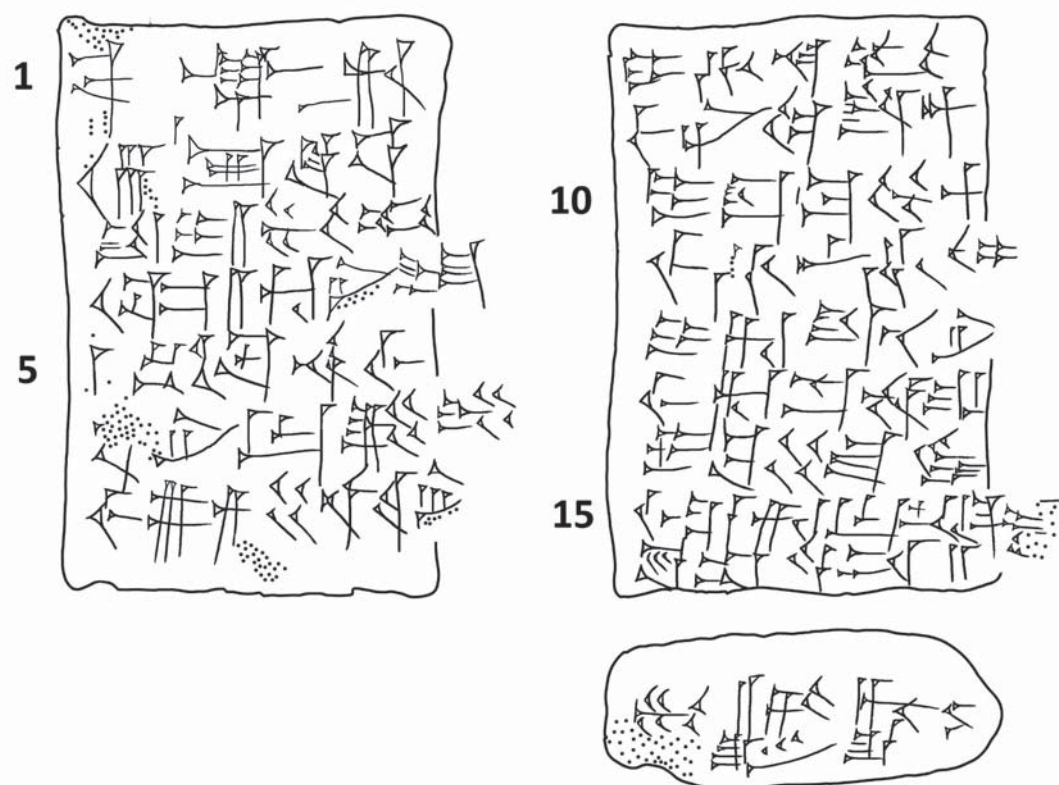


Fig. 9.3. BM 108915 (1914-4-7 81).

1	<i>ba-aq-ri</i> 16 GÍN KÙ.BABBAR <i>ša ia-mu-kum</i> UGU <i>a-ḫu-ni i-šu-<ú></i>	10	DUMU <i>iš-ma-aḫ-DINGIR</i> IGI <i>ḫa-an-ḫa-nu-um</i> DUMU <i>ri-iš-be-lī</i> IGI <i>ri-iš-ēr-ra</i> DUMU <i>ba-tu-lum</i>
5	^{ld} EN.ZU- <i>na-ši</i> BA.NI.IB.GI ₄ .GI ₄ IGI <i>ú-ṣi-na-wi'-ir</i>	15	IGI <i>e-ri-ib-^dEN.ZU DU[B]./SAR¹</i> ITI AB.E.A
R.	DUMU <i>ḫa-na-kum</i> IGI <i>nī-di-it-DINGIR</i>	U.E.	MU <i>a-ga-a-nu-ḫi</i> LUGAL.E

Illegible seal impression on the left edge.

Translation

¹⁻² (Concerning) a claim of sixteen shekels of silver ³ that Yamukum ⁴ has on Ahuni: ⁵⁻⁶ Sîn-naši will be responsible. ⁷⁻¹⁵ Witnesses: Uši-nawir, son of Ḫanakum; Nidit(ti)-ilim, son of Išmaḫ-ilum; Ḫanḫanum, son of Rīš-bēlī; Rīš-Erra, son of Batūlum; Erīb-Sîn, scribe.

¹⁶ Month X, ¹⁷⁻¹⁸ 'Year: Agānuḫi the king'

Notes

- 1-4 The formular 'Amount of X, PN₁ UGU/*eli* PN₂ TUKU/*išûm*' is interpreted by STOL 2016 as an obligation (*Verpflichtungsschein*) to pay the remainder of a debt (*Restschuld*).

2. BM 108918 (1914-4-7 84)

Dimensions (cm): 5.8 × 4.3 × 2

Yawium e (= Sumu-la-El 5)/XI

Double loan. Kunūtum lends 12 ⅓ shekels of silver against the normal interest rate (20 %) and 360 SILA₃ of barley at an interest rate of 33⅓ % to Wēr-tillatī. During the harvest he will pay back with barley.

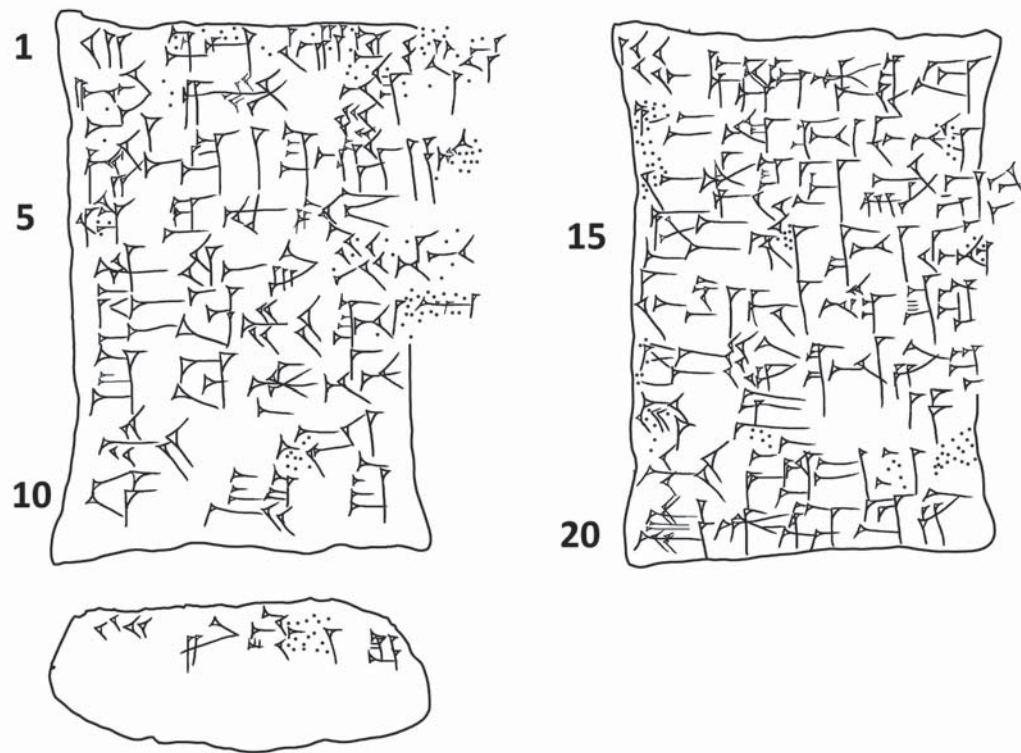


Fig. 9.4. BM 108918 (1914-4-7 84).

1	12 ḡIN ¹ IGI.6.GÁL ḡKÙ.BABBAR ¹ MÁŠ GI.NA 1.1.0.0 ŠE MÁŠ 1 GUR 0.1.4.0.TA ¹ .ḡAM ¹	L.E. ŠE ḡ.ḡAG ¹ .E R. [IG]I pu-ḡur _x - ^d EN.ZU DUMU na-da-ši-na ḡIGI ¹ ^d EN.ZU-pi-la-aḡ
5	KI ku-nu-tum ^d we-er-ILLAT-ti DUMU sú-mu-da-ḡre ¹ ŠU.BA.AN.TI MU.TÚM	15 DUMU na-da-ši-na IGI iš ² -ḡu-na-ku-um DUMU in-da-ni-ra ² ITI ZÍZ.A MU BĀD ^{ki}
10	U ₄ .EBUR.ŠÈ	20 KÁ.DINGIR.RA <ki> BA.DÙ ¹

Illegible seal impression on the upper and left edge with a presentation scene.

Translation

¹ Twelve and one-sixth shekels of silver, ² the normal interest; ³ 360 SILA₃ of barley, ⁴ the interest is 100 SILA₃ per 300 SILA₃, ⁵ from Kunūtum, ⁶ Wēr-tillatī, ⁷ son of Sumu-darē, ⁸ took. ⁹⁻¹¹ During the harvest he will measure out the barley. ¹²⁻¹⁷ Witnesses: Puḡur-Sîn, son of Nadāšīna; Sîn-pilaḡ, son of Nadāšīna; Išḡunakum, son of Indanira. ¹⁸ Month XI, ¹⁹ 'Year: the wall of Babylon was built'.

Notes

3 The amount of barley is squeezed between lines 2 and 4.

12 The sign is actually BĀD, similar to the one in line 19. BĀD can have the reading dur₈, because the sign was often read in Akkadian as *dūrum* 'fortress, wall', by analogy, I assume that it was read here with the syllabic value ḡur.

13, 15 On the name Nadāšīna: see below.

16-17 Both of these names are unusual, and I am unaware of other attestations.

3. BM 108925 (1914-4-7 91)

Yawium 1/III

Dimensions (cm): 5.6 × 4.2 × 2

Loan of silver (*hubuttātum*). Ea-mālik lends a large amount of silver (1 mina and 6 shekels) to Šubā-Ilān. Repayment of the loan seems to be in month VIII.

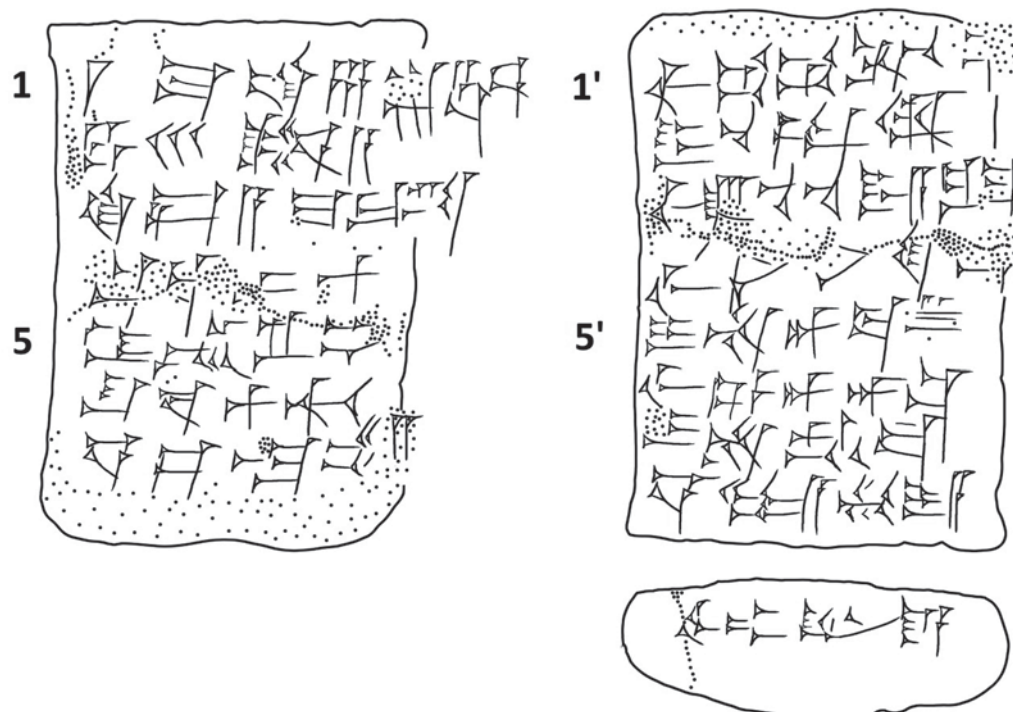


Fig. 9.5. BM 108925 (1914-4-7 91).

1	1 MA.NA 6 GÍN KÙ.BABBAR KÙ EŠ.DÉ.A KI é-a-ma-lik.TA [šú-ba ¹ -DINGIR-DINGIR	R.1'	IGI qá-qá-da-nu-u[m] DUMU nu-úr-iš ₈ -tár IGI [bur ¹ -nu-nu DUMU a-ku-ku IGI [i-lī-ki-[ti ¹
5	DUMU en-si-bi ² ŠU.BA.AN.TI ITI G ¹⁸ APIN.DU ₈ .A L.E. [KÙ i.LÁ.E] [....]?	5'	DUMU ir- ^d BA.Ú IGI dan ^{an} -DINGIR-šú DUMU ir- ^d EN.ZU ITI SIG ₄ .A MU ia- U.E. wi-um LUGAL.E

Illegible traces of a seal impression on the right, left, and upper edges.

Translation

¹ One mina and six shekels of silver, ² the silver of a *hubuttātum* loan, ³ from Ea-mālik, ⁴ Šubā-ilān, ⁵ son of Ensibi, ⁶ took. ⁷ In month VIII ⁸ [he will weigh out the silver].

^{1'-7'} Witnesses: Qaqqadānum, son of Nūr-Ištar; Būr-Nunu, son of Akuku; Ilī-kittī, son of Ir-Bau; Dan-ilišu, son of Warad-Sîn. ^{8'} Month III, ^{8'-9'} 'Year: Yawium the king'.

Notes

- 2 The EŠ.DÉ.A/*hubuttātum* loan is traditionally understood as a loan with interest discounted in the main sum, cf. GARFINKLE (2012: 48–59, for Ur III), SKAIST (1994: 33–56), KIENAST (1978: 60–64), and CHARPIN (2014: 118–120). This would mean that the original loan would have comprised fifty-five shekels, with a 20% interest (the regular rate for silver) of 11 shekels.

4. BM 108925b (1914-4-7 91b) part of the envelope of BM 108925

Dimensions (cm): $4.8 \times 5 \times 3.2$

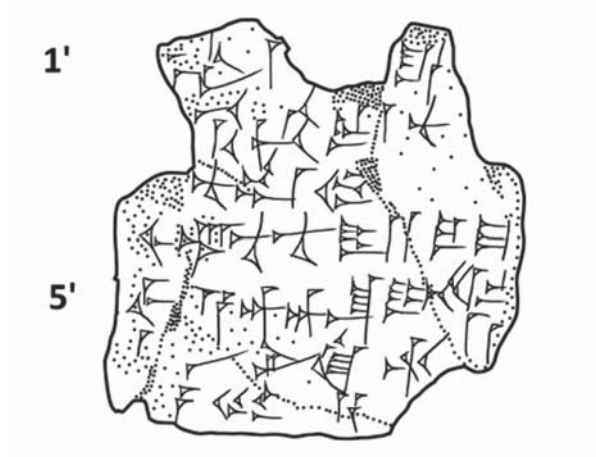


Fig. 9.6. BM 108925b (1914-4-7 91b) part of the envelope of BM 108925.

- 1' [K]Û Ì.L[Á].E
 [q]á-qá-da-nu-[um]
 [DUMU] nu-úr-iš₈-tá[r]
 ʾIGI bur¹-nu-nu DUMU a-ku-ku
 5' ʾIGI ʾdam^{1an}-DINGIR-šu DUMU ʾIR-^dE[N.ZU] / x
 ʾIGI ʾl¹-l¹i-ki-ti x [(x)]
 [IT]I ʾSIG₄¹.A

Illegible traces of a seal impression on the lower and left edges.

Translation

See translation of 3. BM 108925.

N.B. BM 108925a is a fragment of an envelope that has nothing to do with BM 108925 or its envelope BM 108925b.

5. BM 108928 (1914-4-7 94)

? (probably Yawium e)/I

Dimensions (cm): 7 × 4.2 × 2.4

Sale of a field. Anna-ḥaqbum sells a 4 IKU field to Aḥuni for [x] shekels of silver.

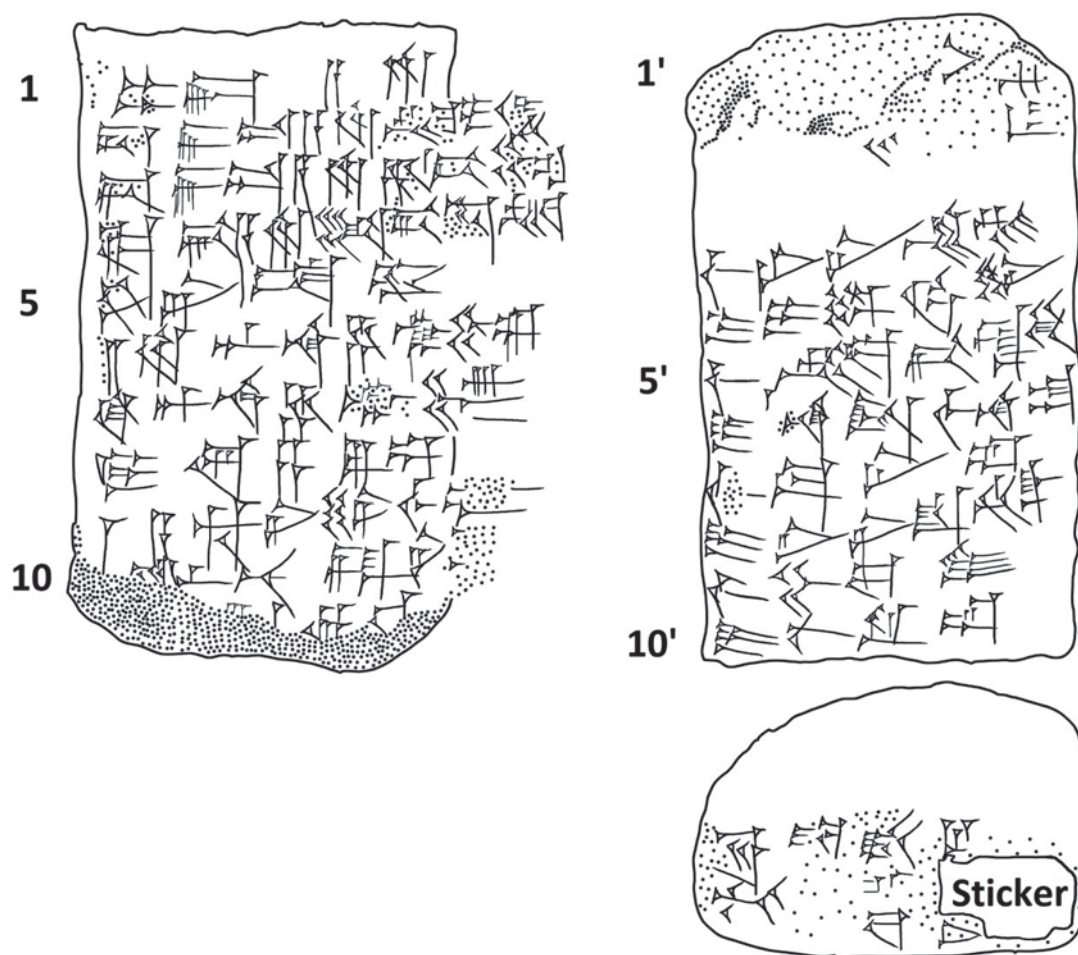


Fig. 9.7. BM 108928 (1914-4-7 94).

1	4 IKU A.ŠÀ ÚS.SA.DU A.ŠÀ ^d EN.ZU- <i>i-mi-/ti</i> ÚS.SA.DU A.ŠÀ <i>ḥa-bi-bu-um</i> SAG.BI A.ŠÀ <i>li-pí-it-^dEN.ZU</i>	R.1'	[...] 'x x' [...] 'x' [...] 'x' IGI <i>i-lí-en-nam</i> DUMU <i>i-šar-di-in</i>
5	<i>ḥi-ir-še-tum</i> A.ŠÀ <i>an-na-ḥa-aq-bu-um</i> KI <i>an-na-ḥa-aq-bu-ú</i> DUMU <i>zu-za-lu-um</i> ¹ <i>a-ḥu-ni</i> IN.ŠI.ŠÁM ¹	5'	IGI ^d EN.ZU- <i>pí-la-aḥ</i> DUMU <i>na-da-ši-na-at</i> IGI <i>ma-ni-um</i> DUMU <i>i-lí-iš-ti-kál</i> IGI PU-SÚ-NÚM
10	[Š]ÁM.TIL.LA.NI.Š[È] [x] 'GÍN ¹ KÙ.BABBAR [IN.NA.AN.LÁ]	10'	DUMU <i>nu-ru-um</i>
L.E.	broken	U.E.	ITI BARA ₂ .ZAG.GAR MU.'BÀD ² [...] BA.DÙ

Traces of a BUR.GUL seal impression on the obverse and reverse: [*an-na-ḥa*]-*aq-bu-ú*, DUMU *zu-za-[lu-um]* ([Anna-ḥ]aqbû, son of Zuza[lum])

Translation

¹ A four IKU field, ² next to the field of Sîn-imittī, ³ next to the field of Ḥabibum, ⁴ its topside is the field of Lipit-Sîn. ⁵ ‘A field that has been cut’. ⁶ The field of Anna-ḥaḳbum, ^{7–8} from Anna-ḥaḳbū, son of Zuzalum, ⁹ Aḥūni bought. ¹⁰ As its full price, ¹² [he will weigh out] ¹¹ [x] shekels of silver. [...]

^{1'–2'} ... ^{3'–10'} Witnesses: Ilī-ennam, son of Išar-dīn; Sîn-pilāḥ, son of Nadāšinat; Manium, son of Iliš-tikal; Pussunum, son of Nūrum.

^{11'} Month I, ^{12'–13'} ‘Year: ...was built’.

Notes

5 On the *hirṣētum* field: STOL 1988: 173–174.

6–7 It is noteworthy that Anna-ḥaḳbum’s (Amorite) name is once written with mimation and once with an elongated /ú/.

10 One expects ŠÁM.TIL.LA.BI.ŠÈ.

1'–2' One would expect the oath to be somewhere in these lines.

6' The name is written here as *na-da-ši-na-at*, in BM 108918: 13 and 15 it is written as *na-da-ši-na*. This name only occurs in these two texts for the early Old Babylonian period. It means either ‘Praise them’ or ‘They are praised’ (‘them/they’ are probably goddesses). This type of name was studied extensively by GELB 1961: 148–150, he interprets *na-da* as a stative based on names like *a-ḥu-na-da* (the nominative of *a-ḥu* invalidates the interpretation of *na-da* as an imperative). If we only had the attestation *na-da-ši-na*, Gelb would be correct, however, his idea seems to be incompatible with *na-da-ši-na-at*. On the other hand, for the interpretation ‘Praise them’, a more correct OB form would be *na-da-ši-na-ti*.

12'–13' Part of the year name is obscured by an old sticker on the tablet. It looks like it was applied by museum staff in the past to catalogue it. I did not try to remove it for fear of damaging the tablet. Given the tablet’s prosopographical connections to BM 108918 (Witness Sîn-pilāḥ, son of Nadašina(t) above, it seems probable to me that the year name is the same as on BM 108918: Yawium e (= Sumu-la-El 5): MU BĀD^{ki} KĀ.DINGIR.RA^{ki} BA.DÙ ‘Year: the wall of Babylon was built’.

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10. Troubles in the Kingdom of Larsa

BAPTISTE FIETTE

The main objective of this contribution is to focus on some issues of relative chronology, relating to political troubles and succession problems within the kingdom of Larsa, as they can be observed through the analysis of specific year names. This study allows us to make some critical remarks on the establishment of the chronology of Larsa kings, based on the lists of kings and lists of their year names. Thus, it can be observed that certain periods of unrest, of indeterminate duration, are deliberately omitted by ancient scribes. This phenomenon must be taken into account by historians working on synchronisms between dynasties, or on the absolute chronology of the Amorite period. Therefore, the conventional Middle Chronology is used for convenience.

Amorite kingdoms of Mesopotamia used a particular eponymal system in which a date formula was given to a new year, commemorating an outstanding event of the previous year. The Upper Mesopotamian Kingdom and other little kingdoms derived from its breaking up were an exception, as each new year received the name of a high official from Aššur, called *limmum*.¹

Date formulae were written in Sumerian in the kingdom of Larsa.² As elsewhere, they most often commemorated victorious military campaigns, constructions such as building walls and temples or digging canals, offerings to divinities or appointments of temple officials. According to the kings, these royal actions constituted outstanding events worth to be celebrated through year names, or other royal inscriptions. Moreover, they never commemorated an adverse event for them. The historian must be careful not to reconstitute an ideal history of their reigns based on memories favourable to these kings.

Furthermore, the historian cannot establish a political history of their reigns on the basis of these date formulae. This is asserted for example through the choice of Zimri-Lim's twelfth year name (*nīb šattim*).³ Two letters (DURAND 1997: *LAPO* 16 90 and 157) relate discussions between the king of Mari, Šu-nuhra-halu his secretary, and Yasim-Sumu his minister of finance (*šandabakkum*). Their purpose is to make a choice between two events to commemorate: either the sending of reinforcements to Babylon in order to help Hammu-rabi in his war against Larsa, or the offering of a great throne to the god Dagan. Finally, the offering was preferred against the military event. This example demonstrates that a year, or a succession of years, without date formula related to some politico-military events, does not in any way indicate a period without military activity.

Moreover, some events seem to have been so important that they were celebrated through several consecutive years. The most striking example is the capture of Isin by Rim-Sin, king of Larsa (1822–1763 BC), in his twenty-ninth year, which was commemorated during his thirty-one following years, until the Babylonian conquest led by Hammu-rabi (1792–1750 BC). This victory over Isin was actually decisive for Rim-Sin and his kingdom, after which he established his sovereignty on the whole southern Mesopotamia. Also, the battle won by Sumu-El (1894–1866 BC) against Kiš in his eleventh regnal year was celebrated through his year names 12 to 15, and Eduru-Nanna-isa's capture in his sixteenth year was commemorated by his year names 17 to 21. However, some royal activities in the religious field were also commemorated through several consecutive year names, see below the example of Sumu-El's year names 23 to 29 related to the appointment of the high priestess of Nanna.

Some elements shall be taken into account concerning the date formulae dealing with successions of Mesopotamian kings. In principle, a year kept its date formula until its end, even if the king died during it. So, the first year name of his successor corresponded to his first complete year of reign, even if his reign has begun during the previous year.

¹ CHARPIN 2004: 45–49.

² All year names formulae of kings of Larsa are available online: www.archibab.fr > Year names > Larsa.

³ Charpin/Ziegler 2013: 58–59.

There were some unofficial date formulae referring to the new king. They were composed during the last year of the former king, like this: MU *king name* LUGAL(.E) = ‘year: *king name* the king’. These formulae were not recorded on lists of year names and did not hold an official nature, but they were used in dating documents. They are called ‘years 0’.⁴ Also, the same formula was sometimes used as an abbreviation of any year name.⁵ However, within the kingdoms of southern Mesopotamia, like Isin or Larsa, the first year name of each king always commemorated his accession, with the same words: MU ‘*king name*’ LUGAL(.E), as official lists testify. But the translation of this formula must be: ‘year: ‘*king name*’ (became) the king’. This should be an own tradition of this region, because elsewhere the first year name of a new king celebrated any kind of events. There is no long version of this kind of formula, neither on lists nor on dated documents. Consequently, it means that it is impossible to distinguish a potential year 0 of any king of Isin or Larsa from the abbreviation of any other year name, if the southern scribes used this kind of unofficial formula.

First, this study will focus on two unofficial year names, called Gungunum 27-bis and Sin-iddinam 7-bis, which were composed after the death of these kings. The objective will be to link these date formulae with some historical events. Second, it will examine the political troubles, which mark the end of the Gungunum’s dynasty and the whole of the period of the so-called Nur-Adad’s dynasty. Finally, some remarks on the composition of the *Larsa King List* will allow us to discuss this important source for the history of the Larsa’s kingship and to comment on the establishment of its chronology.

1. Year name formulae after the deaths of Gungunum (1932–1906 BC) and Sin-iddinam (1849–1843 BC)

Two year names formulae stress a particular phenomenon: they are formulae-bis referring to the very last years of kings Gungunum and Sin-iddinam.

The *Larsa King List*⁶ gives the succession of all the kings of Larsa, from Naplanum (2025–2005 BC), founder of the kingdom, to Rim-Sin (1822–1763 BC), and the lengths of their reigns. Moreover, it records years of domination by Hammu-rabi (1762–1750 BC) and Samsu-iluna (1749–1738 BC), kings of Babylon, over Larsa. This list attributes respectively twenty-seven and seven years to Gungunum and Sin-iddinam. However, M. Sigrist mentions two additional formulae: Gungunum 28/Abi-sare 1 and Sin-iribam 1⁷ I shall rename them Gungunum 27-bis and Sin-iddinam 7-bis:

Gungunum 27-bis: MU ÚS.SA I₇ dBA.WÀ MU.BAL

‘Year following (the year Gungunum 27): (Gungunum) dug the canal Bawa’.

Sin-iddinam 7-bis: MU ÚS.SA BÀD MAŠ.GÁN.ŠABRA^{ki}

‘Year following (the year Sin-iddinam 7): The great wall of Maškan-šapir was built’.⁸

According to the scribes of the *Larsa King List*, these two date formulae were immediately composed after the deaths of Gungunum and Sin-iddinam. Nevertheless, one should expect that year names celebrating the accession of their respective successors, Abi-sare (1905–1895 BC) and Sin-iribam (1842–1841 BC), follow directly after years Gungunum 27 and Sin-iddinam 7. Then how can we interpret the existence of these formulae-bis referring to the very last years of Gungunum and Sin-iddinam?

1.1. Gungunum 27-bis

The territory of the kingdom expanded during the reign of Gungunum.⁹ He conquered Ur in his seventh year (1926 BC) and consequently gained access to the Persian Gulf, Adab and Zabalam before his fifteenth and sixteenth years (1918–1917 BC), Nippur (because of his title ‘king of Sumer and Akkad’), Kisurra before his twenty-third year (1910 BC). All these conquests were made at the expense of the kingdom of Isin. But Ur-Ninurta of Isin (1925–1898 BC) succeeded in

⁴ A very exceptional year name is attested in HOLMA 1914: 18–20 n° 5, a tablet dated by a composite formula referring both to the last year of Hammu-rabi and the accession of Samsu-iluna: MU UD.KIB.NUN.NA^{ki} [= Ha 43] ù MU *sa-am-<su>-i-lu-na* LUGAL [= S-i 0]. This tablet was written on the 10th day of 5th month, which means that Hammu-rabi died before this date. See HORSNELL 1999, vol. 1: 49.

⁵ CHARPIN/ZIEGLER 2013: 62–63 and n. 33.

⁶ CLAY 1915, YOS 1 32. Edition and commentaries in GRAYSON 1980–1983: 89.

⁷ SIGRIST 1990: 11–12, 26.

⁸ STEINKELLER 2004b: 135–146: an inscription of Sin-iddinam found at Abu Duwari (Maškan-šapir) commemorates the building of the great wall of Maškan-šapir, in his sixth year of reign (1844 BC). Its name is ‘Nergal destroys the enemy lands for me’ (ll. 41–43). Nergal, the netherworld god, is the chief divinity of Maškan-šapir, see STEINKELLER 2004a: 27–28.

⁹ CHARPIN 2004: 70–74.

recapturing some territories, perhaps among them the towns of Kisurra and Nippur. Furthermore, Gungunum commemorated some victories over Bašime and Anšan in Elam (year names 3 and 5) and over Malgium (year name 19).

In addition to the *Larsa King List*, two lists of year name formulae published by F. Thureau-Dangin¹⁰ and J.-M. Durand¹¹ record year names corresponding to the end of Gungunum's reign.¹² The date formula Gungunum 27-bis is not mentioned in them. In order to explain the existence of this formula, the following chronological data shall be taken into account:

- The last tablet dated to Gungunum 27 was written in the 12th month, as expected.
- The unique tablet¹³ dated to Gungunum 27-bis was written in the 1st month.
- The first tablet¹⁴ dated to Abi-sare 1 was written on the 23rd day of the 2nd month.

One can observe that Abi-sare was not yet on the throne of Larsa in the first month of 1905 BC, which was his first official regnal year. The date formula Gungunum 27-bis was used during this month. This phenomenon could be explained by a disturbed succession, for which there is no more evidence.

As M. Fitzgerald wrote: 'No cuneiform record or year name from the end of Gungunum's reign or the beginning of Abi-sare's reign would suggest that there was any turmoil at the time of Abi-sare's succession', but she did not comment on the Gungunum 27-bis formula.¹⁵ One can suggest an internal power struggle was consecutive to Gungunum's death, his sons fighting over the throne until Abi-sare's accession, or even a serious disease affecting them.

If this hypothesis is correct, I would like to suggest that the political difficulties affecting Abi-sare's reign (loss of territories in favour of Isin)¹⁶ began with an undetermined crisis at Gungunum's death.

1.2. *Sin-iddinam 7-bis*

Sin-iddinam's year names formulae are not attested by a list, but A. Goetze¹⁷ established their chronological order.

There is *a priori* no reason to consider that Sin-iddinam 7-bis corresponds with an eighth year of reign for Sin-iddinam¹⁸ because the *Larsa King List* gives him only seven years. Once more, it is interesting to observe the following chronological data:

- The last tablet¹⁹ dated by Sin-iddinam 7 was written in the 11th month, at the end of the year.
- Tablets dated by Sin-iddinam 7-bis²⁰ were written between the 1st month²¹ and the 15th day of the 6th month.²²
- First tablets dated by Sin-iribam 1 were written in the 4th month.²³

¹⁰ THUREAU-DANGIN 1918. The prism AO 7025 kept at the Louvre Museum mentions kings of Larsa from Naplanum to Rim-Sin, but it records year names only since Gungunum.

¹¹ DURAND 1977. The tablet AO 8620 has some gaps: it contained the complete list of Gungunum's, Abi-sare's and Sumu-El's year names. It should be noted that this list deletes one year from Gungunum and adds it to Abi-sare. This mistake could be imputed to the scribe who composed it retrospectively, under Warad-Sin or Rim-Sin (DURAND 1977: 19–20).

¹² The fragment U 7144 of a list found in Ur presents year names Gungunum 11 to 25. For editions and commentaries of all year names lists of Larsa's kings published before DURAND 1977, see UNGNAD 1933: 149–164.

¹³ SCHEIL 1917, text B.

¹⁴ FRANK 1928, text n° 47 pl. XIX. This tablet certainly does not come from Drehem (Puzriš-Dagan), contrary to what the catalogue indicates (p. 8). The correct transcription is in SIGRIST 1990, 12: MU *a-bi-sa-re* LUGAL.

¹⁵ FITZGERALD 2002: 47. M.A. Fitzgerald also commented on seal impressions of a servant of Gungunum which were found on tablets dated to the reigns of Abi-sare and Sumu-El (FRAYNE 1990: 119 n° 2003): '(these sealings are) suggesting a smooth administrative transition between Gungunum's and Abi-sare's reigns,' But this is not necessarily a sign of political stability: for example, two officials of the Mari kingdom, Liter-sarrušu and Hamatil, were first in the service of Yahdun-Lim and Sumu-Yamam and then in the service of Yasmah-Addu at Mari and Samsi-Addu at Ekallatum and Šubat-Enlil (VILLARD 2001: 35–37).

¹⁶ CHARPIN 2004: 74.

¹⁷ GOETZE 1950: 101. The chronological order of Sin-iddinam's year names suggested by A. Goetze has been retained in SIGRIST 1990: 24–25.

¹⁸ However, A. GOETZE (1950: 100 n. 67) did not rule out the hypothesis of the existence of an eighth regnal year for Sin-iddinam. His suggestion has been retained in FITZGERALD 2002: 103–104. According to M.A. Fitzgerald, the fact that a formula-bis of year 'The great wall of Maškan-šapir was built' could mean that either we have to invert the order of years 6 and 7 established by A. Goetze – but this appears unlikely, moreover the formula Sin-iddinam 6-bis exists too, see SIGRIST 1990: 25 – or an eighth regnal year for Sin-iddinam really did exist.

¹⁹ MEEK 1916–1917: 229 (RFH 13).

²⁰ The exact date of FLP 2665, in the Free Library of Philadelphia, mentioned in SIGRIST 1990: 26, is unknown (no photo on the CDLI database). Furthermore, the texts mentioned in footnotes 22 and 23 should be added to this unique document found by M. Sigrist.

²¹ NBC 6767: 2-i and NBC 9919: 5-i in the Nies Babylonian Collection of Yale University. See FITZGERALD 2002: 103.

²² FLP 1331 (<http://www.cdli.ucla.edu/P459884>) and 1333 (<http://www.cdli.ucla.edu/P459886>): 15th day of the 6th month. See HALLO 1976: 213 n. 29a.

²³ CHIERA 1922: *PBS* 8.2, GRICE 1919: *YOS* 5 156, SIMMONS 1978: *YOS* 14 348.

Thus, there is a hypothetical period of three months during which Sin-iribam may not yet have been on the throne of Larsa in 1842 BC, which was his first official year of reign. Moreover, FLP 1331 and 1333 testify of a period of two months during which Sin-iddinam 7-bis and Sin-iribam 1 formulae were used at the same time. It is therefore difficult to consider the existence of an eighth regnal year for Sin-iddinam, during which he would have shared the royal power with Sin-iribam. This could indicate that these months were a period of political trouble for Larsa's kingship and that Sin-iribam was only able to establish his royal authority on the whole territory of Larsa's kingdom after the middle of the 6th month.

In his short reign, Sin-iddinam celebrated successive victories through three of his year names: against Babylon in 1847 BC (year name Sin-iddinam 4), against Ibrat and Malgium in 1846 BC (variants of year name Sin-iddinam 5) and against Ešnunna in 1845 BC (year name Sin-iddinam 6).²⁴ According to M. Guichard, the treaty CUNES 49-04-176 between Sin-iddinam, Sin-kašid of Uruk, and Ibal-pi-El of Ešnunna against Sabium of Babylon and Ikun-pi-Sin of Nerebtum was promulgated after the defeat of Ešnunna.²⁵ These military successes are impressive, but it must be remembered that Larsa had several enemies at the time: some were rebels: we will return to Sabium below.

In *CUSAS* 17 37, Sin-iddinam commemorated some buildings in the city of Ur, ordered by An, Enlil and Enki.²⁶ He detailed three armed conflicts, which disturbed their achievement (iii 19–75). The first one (iii 19–34) is the repression of an uprising in the 'Oberland', according to K. Volk's translation of MA.DA IGI.NIM.MA.²⁷ The second (iii 42–53) is his military campaign against Ibrat and other localities. Lastly, the third one (iii 54–75) is the victory over troops of king (LUGAL) Warassa of Malgium.²⁸ All these conflicts happened during a single year (iii 76–77: MURUB₄.BA MU DILI.ÀM BA.RA.BA.ZAL), certainly in 1846 BC, because the two campaigns against Ibrat and Malgium are commemorated by the year name Sin-iddinam 5.

In another inscription written on several stamped bricks, Sin-iddinam commemorated the building of the great wall of Ur and mentioned a rebellion without any detail:²⁹ is it the uprising in the 'Oberland' that was mentioned in *CUSAS* 17 37: iii 19–34?

The hypothesis of times of troubles during the succession between Sin-iddinam and Sin-iribam receives important support from two letters of petition written by (if not, *a posteriori* attributed to) Sin-iddinam,³⁰ at the end of his reign, to the healing goddess Nin-Isina³¹ and the Sun-god Šamaš.³² According to these documents, the enemy from Babylon surrounded Larsa, and its people and the king himself were touched by a serious plague:

To Nin-Isina:

^{16–25} 'Asalluhi, the king of Babylon, child of Idlurugu, [...]..., While that city is plotting daily against my city, Their king

²⁴ NAHM 2017 suggests a possible synchronism between the sixth year name of Sin-iddinam and the eponymy of Idiabum in KEL 122, for which the MEC entry is ^dEN.Z[U...].

²⁵ GUICHARD (2014: 17–19) dates the treaty to the last two years of Sin-iddinam. The question of the identity of Ibal-pi-El is problematic: GUICHARD (2014: 19–24) suggests that he was a usurper, who acceded to the throne of Ešnunna after the defeat against Larsa in 1845 BC.

²⁶ VOLK 2011.

²⁷ VOLK 2011: 63. K. Volk considers that 'Oberland' could designate either some insurgent Babylonian towns captured by Larsa in 1847 BC (see year name Sin-iddinam 4), Malgium captured in 1846 BC (see year name Sin-iddinam 5), or the land of Emutbalum around Maškan-šapir. I prefer the third hypothesis. At the time of Hammu-rabi, the province of Larsa, still called Emutbalum, was divided into two districts: the upper district (*litum elitum* / AN.TA) and the lower district (*litum šaplītum* / KI.TA), see the letter of Hammu-rabi to Sin-iddinam *AbB* 13 47 (VAN SOLDT 1994). One could think that their capitals were respectively Maškan-šapir and Larsa: two Babylonian dignitaries, Sin-iddinam (the governor of Emutbalum) and Šamaš-hazir (the manager-*šassukum* of the royal domain, see FIETTE 2018) resided in Larsa. Maškan-šapir is obviously a royal administrative centre, see *AbB* 4 22 (KRAUS 1968): a letter of Hammu-rabi to Šamaš-hazir. See also CHARPIN 2004: 323. Moreover, the history between Maškan-šapir and the kingdom of Larsa was tumultuous. This town was integrated during the reign of Zabaya (1941–1933 BC), or virtually before: see STEINKELLER 2004a: 27, and STEINKELLER 2004b: 146–147 (Zabaya's inscription found at Abu Duwari). Then it was lost, until the king Nur-Adad (1865–1850 BC), Sin-iddinam's father, recaptured it, see his year name Nur-Adad G (FITZGERALD 2002: 90–96 = Nur-Adad L in SIGRIST 1990: 23).

²⁸ GUICHARD 2014: 15–16 n. 17, *contra* VOLK 2011: 63–64 who defends the hypothesis that Warassa was the well-known king of Ešnunna, and links this conflict with the year name Sin-iddinam 6 (victory over Ešnunna in 1845 BC). Thus Sin-iddinam should be contemporary with Warassa, *contra* VAN KOPPEN/LACAMBRE 2008–2009: 174–175: according to their results, there shall be a synchronism between Sin-iddinam 1–7 and Ipiq-Adad II 6/7–12/13. But M. Guichard suggests another reading of this passage: the army led by Warassa is not UGNIM 'MA'.AN.DI (the army of the Mandu, according to C. Wilcke in VOLK 2011: 87), but UGNIM 'MA'.AL₇.GI₅ (the army of Malgium).

²⁹ FRAYNE 1990: 174–175 n° 13: 10–17.

³⁰ MICHALOWSKI 1980–1983: 56 § 5.2, and BRISCH 2007: 75–81.

³¹ HALLO 1976, first edition. New edition in BRISCH 2007: 141–156. See also PETERSON 2016: 43–48.

³² HALLO 1982, first edition. New edition in BRISCH 2007: 158–178. See also PETERSON 2016: 48–54.

searches (to do) evil against the king of Larsa. Not being their shepherd of the land, I did not [...] their food-offerings. A young man, who came to my side at night in a dream, He stands at my head(?) and looks at me with a terrible glance, Bringing oars of the rivers(?), casting an evil spell. After that, my virility was not all right anymore, his grip had seized me. Since I became fearful, I cannot go out by myself anymore, a bitter sickness has seized me. My sickness is like a darkness that never becomes light; no one can see it. (...)’

^{37–38} ‘My sickness has turned into a disease for which I do not know of any cooling (relief)(?) (var.: My sickness has turned into a disease for which I do not know a bandage or a day of mercy (?)). At midday no one gives me food, at night I cannot sleep.’ (BRISCH 2007: 143–145)

To Šamaš:

^{12–20} ‘Evil has befallen Larsa, your chosen city. The teeming streets where games were the (favourite) pastime are now filled with silence. Your fine troops, who were united, have perished, falling apart like a reed-wall. Your young men were harvested like ripe grain, they were cut down, they were [plucked] like full-grown fruits. My people are crushed like clay figures, they perish in their fear(?). An evil storm took the little ones away from their mother’s lap. My people scratch their faces, they altered their appearance. The overpowering troops, once they were released, started grinding down the land like flour (?). Young hero Utu, like a stranger you have abandoned your city, Larsa!’

^{28–29} ‘(about impious lands of Elam, Subir and Šimaški:) (And, still, they) are not approached by death, evil, sickness, or the plague.’

³⁰ ‘For five years fighting has not subsided in my city, and death never ceases (var.: For seven years I have had battles and fighting in my city, and dying never ceases).’ (BRISCH 2007: 158–161)

alternatively

³⁰ ‘Since (or: in) the seventh year (of my reign), in my city battle and strife offer no release, death does not stay its arm.’ (STEINKELLER 2004a: 28 n. 10)³³

Whether by illness or enemies’ weapons, it could be suggested that these tragic events upset Larsa’s kingship, causing the death of Sin-iddinam, and even of his legitimate heirs. Sin-iribam, his successor, was not his son but the son of Ga’eš-rabi, according to *CUSAS* 17 50.³⁴ Later, the tradition will consider Sin-iddinam as an unlucky king: the text of an Old Babylonian liver model (*YOS* 10 1, GOETZE 1947a)³⁵ indicates that he died accidentally, because of the fall of a stone slab.

2. ‘Nur-Adad’s dynasty’ (1865–1835 BC): Times of troubles in the kingdom of Larsa

The hypothesis of a tragic end to Sin-iddinam can be strengthened by setting his reign into the historical context of the so-called Nur-Adad’s dynasty, which marked a time of political troubles in the kingdom of Larsa.

2.1. *Tragic events during the succession from Sumu-El (1894–1866 BC) to Nur-Adad (1865–1850 BC)*

Nur-Adad founded a new dynasty after Sumu-El’s death. According to an inscription of Sin-iddinam,³⁶ his father acceded to the throne of Larsa after several enemy attacks and depredations, provoking the revolt of its starving population.³⁷

^{56–76} ‘(The enemy³⁸) threw dams on its (= Larsa) watercourses. He built fortresses where the mouth was. He assigned guards in watchtowers. In plains, the vegetation did not grow anymore; cereals did not bow on their furrows. Inside the city, a massacre took place: in the streets, corpses lied down; families turned against each other. On the wide courts, the fight was raging; one killed by weapons. In the street, it was distress. Brothers ate one another.

^{101–107} ‘(Šamaš) chose a right shepherd (and) took him among the wide population: my father who created me, Nur-Adad. He

³³ STEINKELLER 2004a, 28 n. 10: ‘The conclusion that Sin-iddinam’s problems began only in the seventh (and last) year of his reign is favored both by the grammar of the Sumerian version and by the fact that his reign appears to have been generally prosperous and untroubled’. I prefer P. Steinkeller’s translation of l. 30 because the tragic events described in these two letters of petition likely led to the death of Sin-iddinam and a political crisis. But P. Steinkeller could not have known that the kingdom of Larsa was disturbed during the reign of Sin-iddinam, because this was first shown by *CUSAS* 17 37, published by Konrad Volk in 2011.

³⁴ GEORGE 2011: 106–107.

³⁵ GOETZE 1947b: 265. See also BRISCH 2007: 44.

³⁶ GOETZE 1947b: 265. See also BRISCH 2007: 44.

³⁷ See also three Nur-Adad’s inscriptions by which he proclaimed to restore Larsa and resettled its scattered population: FRAYNE 1990: 138–139 n° 1 and 146–149 n° 6–7.

³⁸ Lines 47–54 are difficult because of broken passages. The name and the home place of the enemy are lost, but one can read that he usurped the title of *sukkal*, ll. 51–52.

rose him in the kingship of Sumer.’

This attack probably caused the fall of Sumu-El.³⁹ Other events marked the political difficulties during the very last years of his reign.

First, it should be observed that, according to J.-M. Durand,⁴⁰ Ur, which had been conquered during Gungunum’s seventh year (1926 BC) at the expense of the kingdom of Isin,⁴¹ possibly did not belong to the kingdom of Larsa at the end of Sumu-El’s reign. Nanna, chief-god of Ur, is not mentioned among gods who supported Nur-Adad’s accession in Sin-iddinam’s inscription.

Moreover, Ur was lost during the seventeenth to twenty-first years of Sumu-El in favour of Isin.⁴² Then it was recaptured, and Sumu-El commemorated the appointment of his daughter, Enšakiag-Nanna, as the high priestess of Nanna in his year names 23 to 29.⁴³ That was certainly a way of proclaiming the incorporation of Ur, yet close to Larsa. This incorporation was obviously weakened by the troublesome events at the end of Sumu-El’s reign and was still unstable during the next years: inscriptions of Nur-Adad⁴⁴ and then of Sin-iddinam⁴⁵ relate the return of Ur’s scattered population, living henceforth in peace in their dwellings, as these kings proclaimed.

Second, a disaster was probably associated with the political troubles disturbing the end of Sumu-El’s reign. Its nature is unknown: some scholars have suggested that it was a natural catastrophe such as the displacement of the Euphrates and the Tigris⁴⁶ or a serious plague affecting the people of Babylon and other peoples of southern Mesopotamia.⁴⁷ That also raises the question of whether this disaster was consecutive to the troubles or provoked them.

Unfortunately, it is at the moment impossible to reconstitute the reign of Nur-Adad, because not all of his sixteen year name formulae are known and their chronological order is uncertain.⁴⁸ However, one can already discern that the kingdom was disturbed when Nur-Adad took power. Moreover, M. Van de Mieroop has observed some disruptions in archives from Ur during his reign.⁴⁹

The only military event commemorated through his year name formulae is the recapture of Maškan-šapir.⁵⁰ That means this town, integrated into the kingdom of Larsa under Zabaya (1941–1933 BC), was lost during the previous years.

After his son had become the governor-*šakkanakkum* of Ašdubba,⁵¹ Nur-Adad associated Sin-iddinam with the throne of Larsa.⁵² Did he feel too old to exercise alone the royal power during a difficult period? It is possible that Nur-Adad was still alive during the reign of Sin-iddinam, as is suggested by *CUSAS* 17 37⁵³ (iii 64, about the conflict against Warassa) and the inscription on some bricks dealing with the restoration of the Ganunmah at Ur by Sin-iddinam which was dedicated ‘to the life of my father and my own life’.⁵⁴

2.2. Eight years of troubles after Sin-iddinam’s death (1842–1835 BC)

The tragic events that provoked the death of Sin-iddinam affected durably the kingdom of Larsa, which suffered from enemy attacks between 1842 and 1835 BC. During these eight years, three kings succeeded one another. Their familial ties

³⁹ *Contra* VAN DE MIEROOP 1992: 59–60. He defends the hypothesis that this attack happened during the reign of Nur-Adad, and its adverse consequences were resolved at the time of Sin-iddinam.

⁴⁰ DURAND 1977: 19.

⁴¹ VAN DE MIEROOP 1992: 55 and n. 60.

⁴² STOL 1976: 30–31 and VAN DE MIEROOP 1992: 57–58.

⁴³ CHARPIN 2020: 193–194.

⁴⁴ FRAYNE 1990: 140–144 n° 3–4 and 146 n° 6.

⁴⁵ *CUSAS* 17 37: iv 31–34. See VOLK 2011.

⁴⁶ EDZARD 1957: 144.

⁴⁷ WESTENHOLZ 1983: 223 n. 22.

⁴⁸ FITZGERALD 2002: 90–96. She proposes a new succession of year names Nur-Adad G to M (retained by Archibab website), *contra* SIGRIST 1990: 22–23. However, according to RICHTER 2004: 288 n. 1167, it is not certain that all year names attributed to Nur-Adad by Fitzgerald and Sigrist really belong to his reign.

⁴⁹ VAN DE MIEROOP 1992: 59–60. He imputes these disruptions to the attack related in the Sin-iddinam’s inscription edited in VAN DIJK 1965, see above n. 38. But they could be the visible effects of some other unknown tragic event(s).

⁵⁰ Nur-Adad G, see above n. 28 and 49.

⁵¹ EDZARD 1957: 145 n. 766 (see GRICE 1919: *YOS* 5 152). See also CHARPIN 2004: 104 and n. 407.

⁵² CHARPIN 2004: 104 and n. 408.

⁵³ VOLK 2011: 65.

⁵⁴ FRAYNE 1990, 170 n° 10: 16–18. See also CHARPIN 2004: 105.

with Sin-iddinam are unknown, but one fact is certain: Sin-iribam was the son of Ga'eš-rabi (*CUSAS* 17 50),⁵⁵ meaning that the dynastic succession was disrupted after Sin-iddinam's death.

2.2.1. *Sin-iribam (1842–1841 BC)*

Sin-iribam reigned for only two years and it is possible, but not certain, that his fall was consecutive to a Babylonian's attack. Sabium, king of Babylon (1844–1831 BC), celebrated a victory over Larsa through a year name at the beginning of his reign (*CT* 45 3, PINCHES 1964):

MU ERIN₂ UD.UNU^{ki} [...] ^{giš}TUKUL BA.SÌG 'Year: he defeated the troops of Larsa (...).'

But it has not yet been established whether this victory happened in 1841 BC, the year of Sin-iribam's fall, or in 1839 BC during the reign of his son and successor Sin-iqišam.⁵⁶

2.2.2. *Sin-iqišam (1840–1836 BC)*

Sin-iqišam, son of Sin-iribam, ruled over Larsa for five years. His year names commemorated some important politico-military events and considerable offerings to divinities. At first glance, one could think that Sin-iqišam's reign was prosperous and successful, but one should remember that a king never commemorated any adverse events for him. Finally, all these events could represent ill-fated signs if one considers that Sin-iqišam reigned during times of troubles:

The name of Sin-iqišam's second year celebrated the capture of Pi-naratim and Našarum in 1840 BC. However, Pi-naratim had already been conquered by Sumu-El in 1887 BC, according to his eighth to eleventh year names. Consequently, Sin-iqišam made a recapture. Moreover, they are short-lived victories, because Rim-Sin conquered these towns once again in 1809 BC, according to his fifteenth year name.

Sin-iqišam's third year name commemorated the building of the great wall of Larsa, in 1839 BC. It must be a sign of threats hanging over the capital and the kingdom (see above the victory of Sabium against troops of Larsa), which could have happened in this same year.

In 1839 BC, Sin-iqišam brought munificent offerings to the Ebabbar, the temple of Šamaš in Larsa: eleven silver statues and a gold one (third year name) and, in 1838 BC (fourth year name), three thrones and a gold statue of the divine couple Šamaš and Šerida. Sin-iqišam also brought to Nippur – the holy city – fourteen other statues (fourth year name). The wealth of precious metal could testify to the wealth of the kingdom, nevertheless the considerable number of offerings is striking: it seems obvious that Sin-iqišam called for divine help from Šamaš, the main god of Larsa, and from Enlil the king of gods or from his son Ninurta the warrior god in Nippur.

Variants of Sin-iqišam's fifth year name⁵⁷ celebrated victories in 1837 BC over Elam, Isin, Uruk and Kazallu. His military power seems very impressive, but it should be highlighted that enemies surrounded Larsa, and these victories were the last ones for Sin-iqišam. His fall could have been consecutive to a military defeat. The ninth year name of Sabium is attested in Nippur,⁵⁸ which is evidence for a victorious offensive of Babylon over the kingdom of Larsa in 1836 BC, corresponding to the last year of Sin-iqišam.

⁵⁵ GEORGE 2011: 106–107.

⁵⁶ HORSNELL 1999, vol. I: 11–19 and vol. II: 67 n. 12. Maybe this victory was linked to the sending of Babylonian troops by Sabium to the kingdom of Uruk, mentioned in the letter of ANam king of Uruk (unknown dates of reign) to Sin-muballiṭ king of Babylon (1812–1793 BC). See FALKENSTEIN 1963: 56–71 and CHARPIN 2004: 107 n. 424.

⁵⁷ The restoration of Sin-iqišam 5 suggested in SIGRIST 1990: 29 is not certain, because Uruk and Kazallu are never mentioned together with Elam and Zambiya king of Isin. Moreover, formula 5Bg related to a victory over Babylon is excluded, see CHARPIN 2004: 107 n. 429.

⁵⁸ 2 NT 132 = IM.57851. See STOL 1976: 28.

2.2.3. Šilli-Adad (1835 BC)

His successor Šilli-Adad had a short-lived (official) reign of nine months,⁵⁹ in 1835 BC. His familial ties with his predecessors are unknown, but he was obviously a member of the royal court at the time of Sin-iqišam.⁶⁰ Some documents from the last three months of this year are remarkable because of their exceptional date formulae (SIGRIST 1990: 30):

MU *ši-lí-ḫi* ŠKUR NAM.LUGAL.TA ŠÍR.RA: ‘Year: Šilli-Adad was taken off the kingship’.

MU *ši-lí-ḫi* ŠKUR NU LUGAL: ‘Year: Šilli-Adad is no longer king’.

The fact that Šilli-Adad had explicitly lost his throne is an undeniable proof of times of political troubles. Moreover, the absence of royal authority is obvious because the unofficial formulae referring to his fall are attested many times at the end of year 1835 BC, whereas the formula celebrating his accession was used until the tenth month. At the end of this year, the throne was certainly left vacant.

2.2.4. A broken kingdom at the accession of Warad-Sin, son of Kudur-Mabuk

During this tumultuous period, an individual with a complex identity emerges: Kudur-Mabuk, a man of Elamite origin, whose political career can be traced back to the reign of Sin-iddinam (1849–1843 BCE), when he was stationed at Maškan-šapir in Emutbal. Only a decade later, he would witness his sons Warad-Sin (1834–1823 BCE) and later Rim-Sin (1822–1763 BCE) becoming kings. Kudur-Mabuk appears as a powerful figure in the kingdom of Larsa, without having been king himself. His career has recently been re-examined, with an investigation of the familial ties between the two dynasties of Nur-Adad and Kudur-Mabuk.⁶¹

Warad-Sin, son of Kudur-Mabuk, acceded to the throne of Larsa, in 1834 BC. According to SIGRIST (1990: 31, only a few tablets are dated by the year name Warad-Sin 1, and the oldest one is on the 10th day of the 5th month.⁶² It is possible that the throne of Larsa was still vacant during the first months of this year. The war had obviously spread throughout the kingdom. The Ebabbar temple had even been attacked by enemies, in spite of the wall built by Sin-iqišam:

‘(When Nanna) delivered into my hands the enemies who had thrown down the top of the Ebabbar temple (...).’⁶³

During the very first years of the new dynasty, military campaigns of pacification led by Kudur-Mabuk and Warad-Sin were commemorated:

- against Kazallu and the land of Mutiabab, whose troops attacked Larsa (and Nippur?):⁶⁴

‘Year (Warad-Sin 2): the wall of Kazallu was destroyed and Mutiabab and his army were defeated by the weapons at Larsa.’

‘(Kudur-Mabuk) who smote the army of Kazallu and Mutiabab in Larsa and Emutbal.’⁶⁵

- against the rebel Šilli-Ištar from Maškan-šapir:

Depiction of Šilli-Ištar, ruler of Maškan-šapir, enemy of Larsa, evil-doer against Emutbal (...), Kudur-Mabuk the king (...) having conquered (Šilli-Ištar) and having set his foot (on him) (...).’⁶⁶

According to P. Steinkeller, these events were correlated: while Kudur-Mabuk and Warad-Sin looked after the control of Larsa, these enemies took the opportunity to conquer the northern part of the kingdom and attacked the capital city.⁶⁷

Kudur-Mabuk’s dynasty is the last one of the kingdom of Larsa. Warad-Sin reigned for twelve years⁶⁸ (1834–1823 BC) and his brother Rim-Sin for sixty years (1822–1763 BC). Under their authority, the kingship was stabilized, and the

⁵⁹ The oldest formula commemorating Šilli-Adad’s deposition was written in the 9th month (UET 5 771, FIGULLA/MARTIN 1953).

⁶⁰ See GRICE 1919, YOS 5 167 dated to the year Sin-iqišam 4, edited in STEINKELLER 2004a: 31 n. 167: Šilli-Adad received beer among the ‘house of the women’ and Kudur-Mabuk, in a palatial context. Was he a son of Nur-Adad? See FITZGERALD 2002: 123–124.

⁶¹ FIETTE 2020 and FIETTE 2022 (see the figure ‘Nur-Adad and Kudur-Mabuk dynasties: hypothetical familial ties’).

⁶² 5 NT 379. See SIGRIST 1984: 114–115 n° 299.

⁶³ FRAYNE 1990: 214–216 n° 10: 16–18.

⁶⁴ PETERSON 2016: 3–11: edition of an *adab* of Ninurta(?) for Warad-Sin. Lines 12f. concern the successful defence of Nippur against peoples from the North. This conflict may be linked with the offensive against Kazallu and Mutiabab, or with another unknown conflict (pp. 4–5).

⁶⁵ FRAYNE 1990: 205–207 n° 3: 12–15.

⁶⁶ FRAYNE 1990: 266–267 n° 1.

⁶⁷ STEINKELLER 2004a: 33–34.

⁶⁸ SIGRIST 1990: 31–36 attributes thirteen years of reign for Warad-Sin, but this is incorrect. See ABRAHAM 2008: the formula ‘The army of Malgium has been defeated by weapons’ is corresponding to Warad-Sin 3, not 4 according to SIGRIST 1990: 31–32.

kingdom was prosperous. Rim-Sin even accomplished the unification of southern Mesopotamia, to the detriment of the kingdom of Isin, in 1794 BC. But thirty-one years later, the kingdom of Larsa was conquered and annexed by Hammurabi of Babylon, in 1763 BC.

3. Conclusion

This study on issues of succession and political troubles in the kingdom of Larsa allows us to discuss on the way the *Larsa King List* (CLAY 1915, YOS 1 32) was composed.

Ancient scribes recorded in this document the number of years of each reign, from Naplanum until Samsu-iluna. They then added together the lengths of all the reigns and obtained a total of 289 years, that may correspond to the length of Larsa kingship's existence, including the Babylonian domination. But the following elements should be taken into account:

- Ancient scribes performed an archival work, the aim of which was to restore the length of each reign. They counted every kings' year name formulae as recorded on lists. However, the further they went back in time, the higher were the risks of approximation.
- They probably used lists of year names, which could have been composed retrospectively. The ones that are well-preserved contain some variants.
- They did not take into account the fact that each reign did not start actually in the first month of the first year, nor end in the twelfth month of the last year, like the first years of Abi-sare, Sin-iribam or Warad-Sin.
- They did not mention times of troubles.

The *Larsa King List* ends with the Babylonian domination over Larsa, which should be good evidence for a composition date during the reign of Samsu-iluna. First, ancient scribes did not mention the year names of Rim-Sin II (1742–1740 BC), who took the throne between the eighth and tenth years of Samsu-iluna. It is certainly a matter of taboo: the name of the enemy must not be written. Moreover, this list attributes twelve years to Samsu-iluna. But the latest tablets dated to Samsu-iluna's reign found in the areas of Larsa and Uruk are from the very end of his eleventh year. However, a single tablet from Ur is dated to the eighth month of Samsu-iluna 12 (*UET 5 868*⁶⁹). Then, Babylon lost this region and there is no document dated after 1738 BC.

Because of the testimony of *UET 5 868* for Babylonian control over Ur in the year Samsu-iluna 12, the *Larsa King List* could have come from Ur,⁷⁰ like many other tablets belonging to the Babylonian Collection of Yale University. That should mean that Ur was a Babylonian pocket of fierce resistance in the South, up to the end of 1738 BC. I suggest that Babylonians believed in the restoration of Samsu-iluna's authority and may have ordered the writing of this list for political propaganda. The purpose could have been to legitimise Samsu-iluna's kingship over southern Mesopotamia, assuming the succession of the former kings of Larsa and adding his years of reign and these of his father with those of their legitimate predecessors, without any mention of the rebel king Rim-Sin II.

It thus appears that raw data from the *Larsa King List* are to be used with caution, especially because times of troubles were intentionally omitted. These remarks are all pitfalls that modern historians have to avoid before they will be able to establish the absolute chronology of Larsa's kingship and all other Amorite dynasties.

Acknowledgements and general remarks

The author warmly thanks Dominique Charpin for his remarks. This study was mainly carried out between 2015 and 2018, with the addition of some recent references.

⁶⁹ CHARPIN 1986: 209–210.

⁷⁰ *Contra* GRAYSON 1980–1983: 89: 'found at Senkereh (ancient Larsa)'. It is not certain that a tablet discovered at the beginning of the 20th century by illegal diggers and said to have been found at Senkereh really came from this site.

Kings and dynasties of the kingdom of Larsa

Reign lengths are according to the *Larsa King List* (CLAY 1915, YOS 1 32) recording official years of reigns and dates are according to the (Upper) Middle Chronology.

Naplanum's dynasty

Naplanum	21 years	(2025–2005 BC)
Yamšium	28 years	(2004–1977 BC)
Samium	35 years	(1976–1942 BC)
Zabaya	9 years	(1941–1933 BC)
Gungunum	27 years	(1932–1906 BC)
Abi-sare	11 years	(1905–1895 BC)
Sumu-El	29 years	(1894–1866 BC)

Nur-Adad's dynasty

Nur-Adad	16 years	(1865–1850 BC)
Sin-iddinam	7 years	(1849–1843 BC)
Sin-iribam	2 years	(1842–1841 BC)
Sin-iqišam	5 years	(1840–1836 BC)
Šilli-Adad	1 year	(1835 BC)

Kudur-Mabuk's dynasty

Warad-Sin	12 years	(1834–1823 BC)
Rim-Sin	60 years	(1822–1763 BC)

Babylonian domination

Hammu-rabi	13 years	(1762–1750 BC)
Samsu-iluna	12 years	(1749–1738 BC)
(Rim-Sin II)	(3 years)	(1742–1740 BC)

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11. Venus, volcanoes, eponyms: Assyrian chronology at the start of the first dark age

WERNER NAHM

The Venus observations in EAE 63 match the large eruption of the Aniakchak volcano in 1628/27 BC. This implies that the Lower Middle Chronology (LMC) is correct. The conclusion agrees with the archaeological evidence. The Minoan Santorini eruption was of about the same size, but more than 50 years later, perhaps in 1554 BC. There are indications that Yasmah-Addu was frightened by the annular solar eclipse of 1768 BC, shortly before the collapse of his father's empire. For the solar eclipse recorded in the Mari Eponym Chronicle there are candidates in 1833 BC and 1838 BC, but the reconstruction of KEL G favours the total eclipse of 1833 BC. For that date a note in a Neo-Assyrian eponym list can be interpreted as marking eponym 1000.

An understanding of the dark age between the well documented Old Assyrian and Middle Assyrian periods needs a firm chronology. Once a reliable frame is established, the meagre sources for this period allow more definitive insights. The renewed study of the Venus observations in EAE 63 has confirmed the restriction to the well-known High, Middle, and Short Chronologies, but with more emphasis on the possibility of shifts by eight years.¹ Here the following points will be made: First, the traditional preference given to the Upper Middle Chronology in comparison with the Lower Middle Chronology is not due to any valid argument, but to a couple of historical accidents. Second, the argument for a major volcanic eruption in the 11th year of Ammi-šaduqa is strong. For the Lower Middle Chronology the spectacular Aniakchak II eruption happened in precisely that year, for all other possible chronologies a major eruption in that year can be excluded with high reliability. This fixes the years of the Hammurapi dynasty in our calendar. Due to firm synchronisms, Old Assyrian dates after the occupation of Mari by Šamši-Adad I are fixed, too, but for earlier dates there is still an uncertainty of several years. The solar eclipse recorded for the eponymy of Puzur-Ištar in the Mari Eponym Chronicle can either be identified with the total eclipse on 24 June 1833 BC (Julian) or with a partial eclipse of the rising sun on 24 March 1838 BC. One also must reckon with the possibility that the Puzur-Ištar date of the eclipse is wrong by one year. A resolution is needed, but this uncertainty does not affect the chronology of the Hammurapi dynasty.

For a critical evaluation of new results, it often is helpful to look at their emergence. Thus the first three sections of this article consist of the text written in 2015 after the Lille chronology conference, apart from updated references to other updated texts in this volume. Sections 4–9 concern the current status and possible new insights due to the use of the Lower Middle Chronology.

1. EAE 63, past and future

The series of Venus observations reported in EAE 63 is a basic document for early history. The study of this text established firm constraints for Mesopotamian chronology in the second millennium BC. Arguments against its use were influential for some years but are invalid.² After our conference we can be sure that we are on track again, but one has to make sure that previous insights are not ignored once again. In particular, was the traditional preference for the Upper Middle Chronology compared to the Lower one based on valid insights, or should the two possibilities be compared on equal terms? To answer this question, one has to look at the history of the study of EAE 63.

¹ DE JONG Ch. 1 in this volume; NAHM 2013; ROAF Ch. 13 in this volume.

² NAHM 2013.

Already in 1860 Hincks had recognized the EAE 63 exemplar K.160 as a record of Venus observations and asked astronomers to determine the possible dates of Omens 9 and 13.³ Eventually, the text was evaluated by Schiaparelli, who found all possible observation dates in the Neo-Assyrian period.⁴ Kugler recognized the link to Ammi-šaduqa, which implied that Schiaparelli's time frame was about a millennium too late. He listed all astronomical possibilities in the range 2080–1740 BC, but due to misconceptions about the era of the Hammurapi dynasty, this range was too early.⁵ Because Venus phenomena repeat to a good approximation after 275 years, Kugler's main candidate years all lay exactly 275 years before the ones under current investigation.

The unexpected synchronism between Šamši-Adad I and Hammurapi led to a correction of this error. It took 20 years, however, before the corrected list was provided.⁶ The new rough time frame was constrained by Assyrian chronology based on king and eponym lists, by Egyptian chronology based on the slipping of the civil year with respect to the tropical year, and by synchronisms between Assyria, Babylonia and Egypt. Whatever doubt may have remained about it has been removed by ¹⁴C data. Thus the possible dates for Hammurapi's accession are 1848 BC (HC = High Chronology), 1792 BC (UMC = Upper Middle Chronology), 1784 BC (LMC = Lower Middle Chronology) or 1728 BC (SC = Short Chronology). If one wants to be on the safe side and casts a wider net, one may include 1840 BC (Lower High Chronology) and 1720 BC (Lower Short Chronology).⁷ For all other chronologies the deviations of the EAE 63 entries from the corresponding astronomical dates are much too large to be masked by observational conditions or textual corruption.

Van der Waerden deserves the credit for introducing the LMC in a systematic search for possible chronologies. Rowton was the first to popularize it in his authoritative contribution to CAH.⁸ Unfortunately, this was too late to change the common association of 'Middle Chronology' with the UMC, which was due to seminal but unsystematic work by Smith and Ungnad.⁹ The delay of a systematic search resulted from a wide-spread conviction that a direct determination of the one true chronology was possible by historical means, with an uncertainty of less than the well-known approximate Venus period of 8 years. For the Middle Chronology the arguments were as follows.

Ungnad briefly considered the LMC (Venus observations between -1637 and -1617), but argued that it is excluded for historical reasons. He felt certain that the Kassite dynasty ended in 1159 BC at the latest. Based on the duration of the Kassite dynasty given in King List A, he concluded that the first year of Gandaš came before 1735 BC. He also made the reasonable assumption that this first year came no earlier than the attestation of the Kassite army in the 9th year of Samsu-iluna. Taken together these assumptions imply that Hammurapi acceded before 1786 BC. Based on our present understanding of the end of the Kassite dynasty, i.e. ca. 1154 BC, Ungnad would have had no argument to prefer the UMC. In contrast, Ungnad could rely on EAE 63 itself to exclude a chronology 8 years higher than the UMC. He asked an astronomer to perform the corresponding calculations and discussed them in detail in his article.

In Smith's book the reason for adopting the UMC and missing the LMC was even more flimsy. Smith linked Yarim-Lim I of Alalah with Egypt, but confused him with his grandfather Yarim-Lim I of Yamhad, who is linked to Hammurapi by the Mari archive. Smith was convinced that Egypt dominated in Syria just until the end of the reign of Amenemhet III, which he firmly placed at 1793 BC. He assumed that this reign was immediately followed by the Syrian war of Šamši-Adad I late in the latter's career. To support this conjectural synchronism Smith noted that various archaeological and historical discrepancies disappear if 'Khammurabi of Babylon began to reign after 1800 by a year or two'.¹⁰ Then he stated, 'In order to test the soundness of the reasons for dating the First Dynasty between 1900 and 1600, roughly, and for placing Khammurabi's accession shortly after 1800, a date was estimated for the first year of Ammizaduga, and Brigadier-General J.W.S. Sewell was asked whether this year or one in close proximity, could be reconciled with the astronomical observations of Venus.'¹¹ Sewell noted that the approximate equality of 172 Venus periods to 3401 lunar months and 275 years allowed the transfer of the previously accepted date of 2067 BC for Hammurapi's accession to 1792 BC, which was sufficient to reassure Smith. Sewell was impressed by the fact that the UMC date of 1792 BC came so close to Smith's

³ HINCKS 1860.

⁴ SCHIAPARELLI 1906.

⁵ KUGLER 1909: 285.

⁶ VAN DER WAERDEN 1945: 414–424.

⁷ DE JONG 2012–2013.

⁸ ROWTON 1970: 231f.

⁹ SMITH 1940; UNGNAD 1940.

¹⁰ SMITH 1940: 16.

¹¹ SMITH 1940: 25.

rough date of 1800 BC and had no motivation to search for alternatives. It is unfortunate that the search for the correct chronology was hindered by such accidents.

The four to six possible chronologies yield rough agreement with the astronomical expectations. If one wants to get further on the basis of EAE 63 alone one has to cope simultaneously with textual corruption, the influence of bad weather on the observations and a lack of knowledge about the way how observational data were transformed to the published list. This is very difficult, so that it may be safer to rely on other arguments for the selection of the correct chronology.

Nevertheless, one must discuss a serious objection of van der Waerden and Huber against the Middle Chronologies. It will be seen that an evaluation of their argument requires a test of explicit hypotheses for the handling of observations under realistic weather conditions. As an experienced observer, Schiaparelli was well aware of that problem and gave it some discussion.¹² In contrast, van der Waerden treated the weather only as a source of minor variations of the *arcus visionis*, with equal chance to lead to earlier or later observations compared to the astronomical expectation. Both Middle Chronologies yield systematic deviations from this expectation. For the UMC the observations of first visibility and first invisibility are often stated to have occurred several days earlier than what is astronomically plausible, for the LMC the given dates often are several days too late. For this reason van der Waerden rejected both Middle Chronologies.¹³ The argument relies on an unrealistic treatment of the weather, but Huber justified van der Waerden's conclusions by analyzing the procedure used by Late Babylonian astronomers.¹⁴ He cited a record that stated, 'On the 18th Venus appeared in the east in the end of Taurus, brilliant and high, 38 minutes was its shining, it appeared on about the 15th.' Obviously bad weather had prevented observations for about three days, but the astronomical date could be estimated. Such estimates should have an equal chance to be early or late. If a similar procedure was used under Ammi-šaduqa, then both Middle Chronologies are indeed excluded with high probability. It seems quite possible, however, that Old Babylonian raw data just consisted of entries of type 'Venus present', 'Venus hidden' and NU PAP (no observation possible). To evaluate the results of the observations, it would be natural to just list the first date on which the new status of Venus (visible or hidden) had been observed. In that case the list must often contain a date which is delayed by several days, in agreement with the LMC.¹⁵ If one wants to defend the UMC, one must assume that the new status was supposed to have occurred when no observation was possible. That is much less natural.

The argument just discussed can be regarded as a major obstacle for the UMC, but in view of the rather turbulent chronology debate of the past two decades one should not expect that such subtleties have much impact. Much more strikingly, EAE 63 contains an entry which implies a major volcanic eruption in the second half of the 11th year of Ammi-šaduqa, somewhere in the tropics or in the northern hemisphere.¹⁶ The effect is visible right from the start of the 12th year. According to the EAE 63 text, Venus disappeared six weeks earlier than expected and reappeared seven weeks later than expected, staying invisible for five and a half months instead of the expected two months (omen 14+38). Taken at face value, the record appears to show a long-lasting turbidity of the atmosphere. On the other hand, observational deficiencies or textual corruption might have mimicked such a situation. One has to weigh the probabilities for the three possibilities.

Observational deficiencies may lead to wrong data or to no data at all. Wrong data might be caused by the misidentification of a bright comet or a supernova as Venus, but this would lead to a shorter apparent time of invisibility, not to a long one. Absence of data may have different reasons, for example bad weather during the entire period of invisibility. It may lead to the substitution of theoretical data to fill the gap. Indeed this happened in omens 9 and 15+41, where invisibility periods of 7 days are recorded that are far from astronomical expectations. The entries can be recognized as theoretical constructs, because EAE 63 itself contains a theoretical table of Venus invisibilities, which at inferior conjunctions are always assigned 7 days. The same table assigns exactly 3 months to every invisibility at superior conjunction. Thus, it seems very hard to explain Omen 14+38 by observational deficiencies, and no probability can be assigned to this explanation.

Since many dates in EAE 63 are corrupt, an explanation of Omen 14+38 by copying errors is in principle possible, but a detailed look at the history of EAE 63 makes it unlikely. Omens contain a pair of observation records with four data, namely month and day number of the disappearance and month and day number for the reappearance of Venus. Omen 14

¹² SCHIAPARELLI 1906: 19f.

¹³ VAN DER WAERDEN 1965: 42–47.

¹⁴ HUBER 1982: 12–14.

¹⁵ NAHM 2013.

¹⁶ WEIR 1972: 30f.; DE JONG/FOERTMEYER 2010; NAHM 2013.

and its duplicate 38 have the same data, apart from a change of a day number by 1. Such minor changes by 1–3 days are frequent and can be neglected. Thus the omen suffered no major changes after the archetype of EAE 63 was written and a reordered duplicate of the omen series was included. Already at that time all four of its data deviated from normal astronomical expectations.

To explore the significance of this fact, one has to look at the other omens. The series of Venus observations in EAE 63 concerns the 17 first years of Ammi-šaduqa. Of the 21 corresponding omens, two are theoretical constructs, as just mentioned, and Omen 14+38 is under discussion. Of the 18 remaining omens, 13 corresponded to the astronomical expectations when the archetype was written. At a later stage, 5 of the 13 were affected by major errors (the change of a month name or the change of a day number by 10) in some but not all copies of the text. Five of the 18 omens appear to have suffered a major change before the archetype was written (a change by 10 may have been present in Omens 5+40 and 6+54, a change of a month name had occurred in Omens 16+55 and 17+50+60, and in a special case in Omen 11). For each of these five omens the archetype had only a single major error, whereas three of the four data were right. The probability that the text of Omen 14+38 came about by an early accumulation of major errors in all of its four data is a fraction of a percent and can be neglected. If the text was due to miscopying, the four changes must have been linked in some way.

Errors induced by previous errors do indeed occur. In Omen 6+54 the early error induced a later one in one copy of the text, leading to two wrong day numbers. In Omen 20+58, Omen 58 appears to have retained the correct data, but Omen 20 acquired major errors in both day numbers. Even if such occurrences are taken into account, there seems to be no convincing way to explain Omen 14+38 by textual corruption. After an extensive search only one somewhat plausible scenario was found: The original text was II 25 / V 8. Initial and final dates got exchanged to yield V 8 / II 25. A scribe wanted to correct the wrong order of the dates and replaced this text by V 8 / VI 25. Then a random additional error led to I 8 / VI 25, the text of Omen 38. An exchange of the two observation dates of an omen is rare, but something like it must have happened in Omen 16+55, where east and west are exchanged in Omen 55 and the final date of K.160 occurs as the initial date in Rm. 2,531++. Once the exchange of the observation dates and its plausible correction is assumed, there is a probability of about 10% that the month of disappearance was affected by a random error and of about 1% that the result would mimic a long-lasting turbidity.

Such a scenario cannot be excluded with complete certainty, but the chances that the omen reflects a real atmospheric turbidity are higher. It must have lasted at least for the six initial and the seven final weeks of the invisibility, and presumably for the time in between. A major volcanic eruption is the one and only plausible cause. In rare cases the turbidity is due to a long-lasting, continuous emission into the troposphere (Iceland 1783). More commonly it is caused by a major Plinian eruption, which leads to long residence times of aerosols and very fine dust in the stratosphere. Turbidities caused by such eruptions last for 1–3 years. Eruptions with the required magnitude of aerosol injection occur a bit more frequently than once a century on the average. A list for the past centuries yields 1453, 1601, 1641, 1694, 1783, 1815, 1883 and 1912 AD. After the Tambora eruption in April 1815 high turbidity endured until at least September 1817.¹⁷ In 1884, the year after the Krakatoa eruption, a similar reduction of stellar visibilities was observed. In the 20th century, the lesser Novarupta (Katmai) eruption of 1912 led to turbidities which equalled those of the Krakatoa eruption for a period of four months.¹⁸

The turbidity necessary to explain Omen 14+38 must have been larger, but it is useful to use the Novarupta eruption for comparison. The effects of the Santa Maria eruption of 1902 and the Pinatubo eruption of 1991 lasted longer, but were only about half as large and would have been invisible in noisy records like EAE 63. Altogether one can estimate that the EAE 63 series of Venus observations had a 10–20% chance of picking up a major volcanic event. Thus a volcanic interpretation of Omen 14+38 is much more likely than one by text alteration. One would like to estimate the duration of the turbidity, but the relevant omens, 15+41, 16+55, and 17+50+60, are problematic. Omens 16+55 and 17+50+60 are corrupted and must be used with caution. In Omen 16+55 the disappearance date and in one version the day number of the reappearance are the expected ones without turbidity. In Omen 17+50+60 the reconstructed day number of the disappearance and the reappearance date are the expected ones. Thus it is likely that the turbidity lasted for a maximum of two years. This is better compatible with an eruption on the northern hemisphere like Novarupta 1912 than with a tropical one like

¹⁷ STOTHERS 1984: 1194f.

¹⁸ STOTHERS 1996; 2001.

Tambora 1815 or Krakatoa 1883. Omen 15+41 has a 7 day invisibility, which shows a substitution of theoretical data for observed ones. It is completely analogous to Omen 9, which certainly is unrelated to observations. The same may well be true for Omen 15+41.

DE JONG/FOERTMEYER 2010 proposed instead that only the disappearance day is substituted, whereas the reappearance date records a real observation at a time of high turbidity. If they are right, a very rapid decrease of the turbidity must have happened over eight months, which looks unlikely. They explain it by large fluctuations of the background turbidity of the troposphere. They derive the existence of such unexpected fluctuations from the Venus observations themselves, but make no allowance for bad weather and minor textual corruption.

All volcanic eruptions that increase the turbidity to an extent compatible with Omen 14+38 can be seen in polar ice-core records and in tree-rings from suitable locations. If the interpretation of the omen is correct, one expects to see an effect for 1691 BC (HC), 1683 BC (Lower HC), 1635 BC (UMC), 1627 BC (LMC), 1571 BC (SC) or 1563 BC (Lower SC), and possibly in the following year. Ice-core dating is not yet precise enough to allow a discrimination, since there are candidate events for all possibilities within the limits of uncertainty. The widths of tree-rings yield precise dates, however. Trees at suitable sites are quite sensitive to cooling by stratospheric aerosols and show effects from all known major eruptions. Nevertheless, tree-rings from a single site cannot prove a volcanic eruption in a given year, since from time to time they have large fluctuations due to local circumstances. Wide rings in given years can exclude the occurrence of a major eruption, however.

Here are the data from Yamal, north-western Siberia:¹⁹ The average tree-ring width is normalized to 100. All major eruptions are visible: Between 1451 and 1455 AD one has widths of 41, 39, 7, 61, 31 (eruption in 1453 AD, Kuwae?). Between 1599 and 1603 AD one has 159, 133, 99, 53, 109. One sees some effect of the 1600 AD Huaynaputina eruption in Peru, in spite of its southern location. Between 1639 and 1643 AD one has 82, 60, 49, 29, 54. The full effect of the Mount Parker eruption of January 1641 AD is only seen in 1642 AD, because for tropical eruptions the biennial oscillation of local stratospheric winds may keep much of the aerosols close to the equator for up to a year.²⁰ For 1692 to 1696 AD one has 88, 90, 38, 163, 124 (eruption in 1693/4, unknown location). For 1781 to 1785 AD one has 156, 196, 37, 110, 190 (Laki and Grímsvötn, June 1783). For 1813 to 1818 one has 76, 59, 22, 0, 29, 0 (Tambora, April 1815). The effect on the tree widths lasted longer than the turbidity. For 1881 to 1886 AD one has 84, 29, 45, 23, 28, 74 (Krakatoa, August 1883). The tree-ring width of the Yamal larches mainly reflects conditions in June and July. The low ring width in 1882 AD is accidental. For 1910–1915 AD one has 87, 138, 47, 76, 78 (Novarupta, June 1912). The width reduction is clearly visible, but minor.

Thus a major eruption in the 11th year of Ammi-šaduqa has to be visible in the Yamal data, by tree-rings of reduced width in the 12th and possibly the 13th year. The data for the years 11–14 in the six candidate chronologies are shown in Fig. 11.1.

Ammi-šaduqa year 9	1694 BC HC	1686 BC Lower HC	1638 BC UMC	1630 BC LMC	1574 BC SC	1566 BC Lower SC
year 9	35	85	145	101	169	104
year 10	78	106	104	22	88	97
year 11	111	126	107	45	173	122
year 12	128	147	59	12	139	137
year 13	135	148	76	18	19	135
year 14	129	170	128	28	87	141

Fig. 11.1. Tree-ring widths at Yamal for years 9–14 of Ammi-šaduqa in the six candidate chronologies.

One clearly can exclude an event for HC, Lower HC, and Lower SC. For the UMC there is some width reduction in the correct year, but the effect seems to be too small to be compatible with EAE 63. For the SC there is a strong effect, but a year too late. Since stratospheric transport from the latitude of Babylon to the latitude of Yamal should be rapid, a strong turbidity in April 1571 BC in Babylon is hard to relate to good growth conditions at Yamal in June/July of that year, but

¹⁹ HANTEMIROV/SHIYATOV 2002a; data: HANTEMIROV/SHIYATOV 2002b.

²⁰ HOMMEL *et al.* 2015.

little growth in the following year. Altogether the LMC emerges as a clear favourite, though UMC and SC need special attention as near misses.

To get a clearer signal, one needs trees from different sites. Fig. 11.2 shows the average widths for the relevant three trees HAS118, HAS121 and HAS122 from Hasholme in England.²¹ They cover four of the candidate chronologies, but neither HC nor Lower HC. The numbers are rounded to integers.

Ammi-šaduqa year 9	1638 BC UMC	1630 BC LMC	1574 BC SC	1566 BC Lower SC
year 9	65	69	71	50
year 10	56	66	67	57
year 11	73	57	42	67
year 12	80	41	55	57
year 13	63	30	57	73
year 14	74	32	63	41

Fig. 11.2. Average tree-ring widths in oak trees from Hasholme, Yorkshire.

Compared to the Yamal site the fluctuations are smaller, but the years 1627–25 BC have the smallest widths for the whole lifetime of the trees. In the relevant years there is no signal for UMC, SC and Lower SC, but a clear one for LMC. When more sites are included, the only candidate years for which a signal is seen are 1627 and 1626 BC. In these years there are narrow rings and frost damage in several location in the western USA, north-western Siberia, Ireland, Britain and Germany.²² They are generally interpreted as due to a major volcanic eruption in 1628/27 BC. By contrast, for all possible chronologies except for the LMC, one can exclude a volcanic explanation for the Venus data in the 12th year of Ammi-šaduqa.

It will not take long to prove or disprove the existence of the 1628/27 BC eruption once and for all. Ice-core dates are as precise as tree-rings after the year 775 AD and soon should reach similar precision at earlier periods.²³ Tree-rings are almost as probative as ice-cores if not only the width of the tree-rings, but also the latewood density is measured. Whereas the width is determined early in the growing season, maximal latewood density responds to average temperatures during the greater part of the season. This leads to a particularly clear relation with major volcanic eruptions, where the eight major volcanic aftermaths in 1453, 1601, 1641, 1695, 1810, 1816, 1884 and 1912 stick out.²⁴

The precise date of the eruption is important, too. The eruption in the 11th year of Ammi-šaduqa must have happened in winter 1628/1627 BC or at least at the very end of the growing season in 1628 BC. Thus the width of 1628 BC tree-rings should not be affected by volcanism. For the USA and Yamal in north-western Siberia this is indeed true, but some effect in 1628 BC was claimed for Ireland, Britain and Germany.²⁵ This would result in a mismatch of one year with the Venus data. The effect is not significant, however, because the ring widths for 1628 BC are within the range of the usual fluctuations. Weather was favourable for tree-ring growth in the middle of the 1650s, 1640s, and 1630s, with downturns at the end of the decades. In the years 1651–1650 BC and 1641–1640 BC the tree-ring width were 60, 59, 52, 58, thus close to the value of 57 for 1628 BC. A common fluctuation in Ireland, Britain and Germany is not surprising either, because the records for the three countries are strongly correlated. The Yamal data given above show that at this site tree-rings were wider in 1628 BC than in 1629 BC. Since the Yamal larches react mainly to June and July temperatures, the tree-ring data imply that the eruption happened between July 1628 BC and June 1627 BC. The EAE 63 data indicate an eruption between October 1628 BC and April 1627 BC, so that there is full agreement as far as our present knowledge goes.

With additional data it should be possible to constrain the month of the eruption even further. One needs to know, however, which volcano was responsible. De Jong and Foertmeyer linked the observations in the 12th year of Ammi-šaduqa with the Santorini eruption. This was tempting, because most researchers dated this eruption to the end of the 17th

²¹ QUB DENDROCHRONOLOGY RAW DATA https://chrono.qub.ac.uk/bennett/dendro_data/dendro.

²² LA MARCHE/HIRSCHBOECK 1984; BAILLIE 1995: 76; HANTEMIROV/SHIYATOV 2002a; SALZER/HUGHES 2007.

²³ SIGL 2015.

²⁴ D'ARRIGO *et al.* 2013.

²⁵ BAILLIE 1995: 76; ROAF Ch. 13 in this volume: §2.2.

century. But even before the tentative redating to the 16th century, a major difficulty arose. Human presence at the Akrotiri town was ended by a pyroclastic surge, which baked freshly harvested *Latyrus* pulses together with their insect pest, freshly emerging weevils.²⁶ This implies an eruption in June/July, whereas the EAE 63 data show a strong volcanic signal in early May. The volcanic event may have happened in stages, with people leaving and returning, but that cannot resolve the contradiction. Small precursor events would have no strong influence on the tree-rings and an initial pyroclastic surge without atmospheric turbidity is not plausible. Agricultural activity after a heavy fall of pumice would have required an extraordinary cleanup, for which there is no archaeological evidence.²⁷

Even after the correct chronology is agreed upon, EAE 63 will remain useful for other studies. With the recognition of Omen 14+38 as well-preserved, its textual history has to be re-assessed. The insertion of the two contrived omens, 9 and 15+41, should not be called textual corruption, since it is far from certain that the omens replaced other data. When this is taken into account, the textual corruption after the writing of the archetype was more severe than the one before. The original text can be reconstructed, apart from shifts of day numbers by single units and the uncertainty whether the difference of ten days from the astronomical expectation in Omen 5+40 was due to bad weather or to textual corruption.

In particular, EAE 63 provides information to determine the position of the Babylonian months in the solar year. This can be checked against harvest times for barley and dates and other agricultural data and will yield information about the climate. Eventually, a disentangling of textual corruption and natural fluctuations might yield some information about the weather. Last but not least, the textual history of EAE 63 will allow more insights into the development of Babylonian astronomy.

2. The MEC eclipse

There are many ways to constrain the possible chronologies that do not depend on EAE 63. Radiocarbon data and Anatolian dendrochronology apparently support the Middle Chronologies,²⁸ but in view of the teething troubles of this field and the recent revisions, independent arguments may be essential to convince the wider community. Only time will tell if dendrochronology can say much about the distinction between LMC and UMC. All other sufficiently precise methods involve astronomy at some stage. Since Egyptian chronology is astronomically constrained, synchronisms with Egypt have much potential and might be a topic for another meeting. To avoid circuitous reasoning it may be better to sort out the chronological problems of both areas independently before comparing results, however. In any case, two issues merit discussion. First, the identification of the Byblos ruler Yantin with the Yantin-ammu of the Mari archives²⁹ is likely to be correct. Second, the recent find of a Hyksos type scarab in a temple from the time of Abi-ešuh at Terqa is incompatible with the High Chronology. Originally it even seemed to exclude the Middle Chronologies,³⁰ but due to the recent finds at Tell Edfu the Hyksos ruler Khayan was contemporary with Sobekhotep IV, the younger brother of Neferhotep I.³¹ The latter was overlord of the Byblos ruler Yantin just mentioned. Thus Khayan must have overlapped with Hammurapi and/or Samsu-iluna.

Huber's month-length argument for the HC is reiterated in his contribution to this conference.³² He uses documents dated to the 30th day of a lunar month and checks if the corresponding astronomical month had 29 or 30 days. For the Late Babylonian period he has verified that the argument works. The scribes of Old Babylonian economic documents had different habits, however.³³ Still one can hope that some variation of the argument will allow conclusions when precise relative dates for the huge number of Ur III documents will be available.

In view of all these uncertainties the most efficient means of discrimination between the chronologies may be the solar eclipse entry in the Mari Eponym Chronicle (MEC).³⁴ To use it, one needs a reconstruction of the MEC between the year of the eclipse and the late reign of Šamši-Adad. At present such a reconstruction must be based on KEL G.³⁵ The major

²⁶ PANAGIOTAKOPULU *et al.* 2013.

²⁷ DOUMAS 1990.

²⁸ HÖFLMAYER 2015; ROAF Ch. 13 in this volume: §4.

²⁹ ALBRIGHT 1945.

³⁰ PODANY 2014.

³¹ MOELLER/MAROUARD 2011.

³² HUBER Ch. 2 in this volume.

³³ ROAF Ch. 13 in this volume: §3.

³⁴ ROAF Ch. 5 in this volume; DE JONG Ch. 1 in this volume.

³⁵ GÜNBATTI 2008; BARJAMOVIC/HERTEL/LARSEN 2012.

difficulty is the break at the bottom of KEL G. The missing names in column I are known from the MEC, so that column I contained 36 lines, including those on the edge. To find the corresponding number of missing names in column III at the reverse of KEL G one would like to have a 3D recording of the tablet. This is not yet available, but the reconstructions in both GÜNBATTI 2008 and in BARJAMOVIC/HERTEL/LARSEN 2012 have five entirely destroyed lines at the top of column III.³⁶ The tablet may have been one or two lines shorter than in that reconstruction, however. There may have been three names on the edge at the bottom of column I instead of two. More strikingly, the missing lines in column I were reconstructed with an unusually large height, 50% higher than those in column II. Before the break, there is no such difference between the columns. Thus the number of destroyed lines at the top of column III might have been three or four instead of five. Less than three or more than five lines appear to be excluded. The reconstruction of the bottom of column II is more problematic. If one compares column I and column II where they meet at the middle of the obverse, column II contains one additional line plus a small fraction compared to column I. Thus Günbatti reconstructed column II with 37 lines (edge included), which seems possible. The lines are slanted upwards, however, which may well allow an additional line in column II. Furthermore, BARJAMOVIC/HERTEL/LARSEN 2012 argue that column I may have had two names at the lower edge compared to three names in column II. Altogether, column II may have had 37–39 lines compared to the 36 lines of column I. BARJAMOVIC/HERTEL/LARSEN 2012 opt for 41 lines, which seems excessive, but was forced by their reconstruction of MEC. Following a suggestion by Charpin and Ziegler, I proposed to identify the last partially preserved eponym name in column II with Haya-malik (more precisely Ha-NI-malik), the eponym of the year when Mari was conquered by Šamši-Adad.³⁷ Between Haya-malik and Assur-malik nine eponyms are known from Mari and it seems likely that none are missing. In KEL G their names must have been placed at the bottom of column II and at the top of column III. The count is in agreement with the previous reconstruction, with three possibilities:

1. 37 names in col. II, five entirely destroyed lines on top of col. III.
2. 38 names in col. II, four entirely destroyed lines on top of col. III, the most likely case.
3. 39 names in col. II, three entirely destroyed lines on top of col. III.

Since the upper lines of columns III and IV are well aligned, one should reckon with 9–11 missing names at the top of column IV.

The last of these possibilities together with the corresponding number of nine missing names in col. IV assigns eponym 240 to a line with extra wedges at the end. The line was interpreted as the name *A-hi-a-a* plus check marks at multiples of 60, perhaps left by one or two examiners who controlled the tablet.³⁸ Collation by Gojko Barjamovic confirmed the reading of the name Ahiyaya, but convincingly explained the two extra wedges as remnants of a name that was erroneously written one line too early, then erased and written again one line later.³⁹ This invalidates my argument, though the less precise assignment of Ahiyaya to one of the positions 240–242 in the eponym list remains valid. On the other hand, the collation confirmed and documented the reading *A-NI-m[a-lik* for the last partially preserved name in column II. This is not the same as *Ha-NI-m[a-lik*, but the close similarity is unlikely to be accidental. If the names can be identified, this fixes the number of eponyms during the life of Šamši-Adad I in such a way that the MEC eclipse year came 49 years before the accession of Hammurapi. This yields 1897 BC (HC), 1889 BC (Lower HC), 1841 BC (UMC), 1833 BC (LMC), 1777 BC (SC), 1769 BC (Lower SC). There was a total eclipse in 1833 BC and nothing comparable in the other years. The following partial solar eclipses come close. There was one of magnitude 0.84 at Assur on 3 May 1896 BC. It started at sunrise, but was not yet remarkable when the sun was close to the horizon. Moreover, one would have to assume an error of one year in MEC. A partial eclipse at 5 June 1842 BC has similar problems. The annular eclipse of 20th December 1768 BC requires an error of two years for the Lower Short chronology. Its significance for LMC is another matter.

Altogether, LMC emerges as a clear favourite. Nevertheless, there remain three obstacles for the proposed reconstruction of KEL G. Lacambre has argued that another eponym by the name of Ahiyaya should be inserted about five years before the death of Šamši-Adad.⁴⁰ In view of the dense documentation of these years it seems more plausible to follow the suggestion of Charpin and Ziegler to identify Ahiyaya's year with the year of another eponym.⁴¹ The author has proposed

³⁶ BARJAMOVIC/HERTEL/LARSEN 2012: appendix 4.

³⁷ NAHM 2013: 368f.

³⁸ NAHM 2013: 367.

³⁹ BARJAMOVIC Ch. 6 in this volume.

⁴⁰ LACAMBRE Ch. 7 in this volume.

⁴¹ CHARPIN/ZIEGLER 2003: 83–85, 163–166.

Asqudum.⁴² The second argument against the reconstruction comes from the need to find the place of the fragment S.24-3 in MEC. Kryszat has proposed a location which agrees with the reconstruction proposed above,⁴³ but Veenhof argues against it.⁴⁴ Ziegler had been able to study the tablets directly and provides their photographs in an authoritative account that is hard to improve.⁴⁵ Nevertheless, the following remarks may be useful, at least for a study of Assyriological methodology. Line by line one has the following situation.

1': The line is of little help. Only two wedges of a single sign are preserved: a candidate is DA, which could be interpreted as first sign of either the eponym name Da-da-ia without a historical note or of *da-aw-da-am* in such a note.

2': Birot read the one conserved sign as KA, which is convincing. The sign has the position expected for the eponym name Puzur-Nirah, if there was no historical entry. Note that the document uses the writing KA.ŠA for Puzur in the name Puzur-Ištar on S.24-1++.

3': This line is crucial, since Birot's reading ^{-rd1}a-šur would exclude Kryszat's placement. Birot's copies are of high and well-known quality, but occasionally he drew spurious wedges at breaks, for example at the beginning of the first preserved line of A.1288 obv. Kryszat assumed a different small error and read A-b]e-a-a, misprinted as A-b[e-a-a]. His reading -b]e- instead of ^{-rd1}- was problematic, since Birot's drawing only shows a horizontal wedge without *Winkelhaken* at the end. Birot had indeed left out a wedge, but a different one. In the line under consideration, the reading ^{-rd1}a- only was possible, because he omitted the lower of two horizontals. Ziegler reads 'ia', but 'e' may be possible. Birot's -šur has an unreliable *Winkelhaken* at a break, followed by three vertical heads over two vertical tails. For -šur one would expect a single tail, for Kryszat's -a only two heads. There is a close precedent for a spurious third head, since King drew the -a- in the eponym name Aššur-gar-u-a-ne-ri on BM 103392 with three heads (CT 33: pl. 19). Thus, the name was long read as Aššur-ša2-pat-be7-ri.⁴⁶ At the conference, Ziegler remembered that she had disagreed with Birot's reading after autopsy, but the final clarification only came with the photographs, which show an unambiguous 'ia'. On hindsight, Birot indicated some uncertainty by drawing a small line at the place of an upper horizontal of ia.

4': Ziegler reads [i-na] 'a'-ta-nim, but one can make a case for 'e'-ta-nim. It is true that the long tails of the wedges are more typical for 'a', but a look on the photo of S.24-1++: 10' (Figs. 03.05–08) shows that the e in e-ri-ši-im also has these long tails, though Birot did not draw them. The position of the sign favours 'e', since the beginnings of the eponym names were well aligned, whereas the name in line 8' would have started to the left of 'a'. The reading 'e'-ta-nim, agrees better with e-di2-num in KEL G. In view of the derivation of the name from *etinnu* the only significant difference is the vowel in the second syllable. For that there are precedents, in particular the writings Da-di-ia in A. 1288 compared to Da-da-[ia] in M. 5911 (REL 117) and Bu-ši2-e-a in KEL G compared to B]u-ša-a-ia in A. 1288 (REL 125). In both cases the reference is to the same person.

8': The name Aššur-taklaku is uncontroversial.

9': Only the damaged last sign of the eponym name survives, but still the line is the most difficult one. Depending on the reconstruction, the eponym name must have been Haya-Malik or Išim-Sin. In private discussions, Veenhof admitted that the reading ^dEN].^rZU' is somewhat better than 'LIK', but he argued forcefully that there is not enough space for [i-ši-im-] at the beginning of the line. A solution may come from the occasional exchange of I-ši-im- and Iš-me-, which is well documented, though difficult to explain. The king Išme-Addu of Ašnakkum could be called Išim-Addu,⁴⁷ while the same person is called Išim-Suen in AKT 4: 28, 29, 30 and 32, but Išme-Suen in AKT 4: 31.⁴⁸

Veenhof states that Kryszat's reconstructions pushes S. 24-3 to the top of the rev. of M.7811++ and shows that this cannot be accepted. Indeed, the correct position of S. 24-3 is very close to the bottom of the obverse. The top of this flake must have been about two lines below the bottom of S. 24-2. Thus there is much to say in favour of Kryszat's placement and nothing decisive against it.

⁴² NAHM 2013: 370.

⁴³ KRYSZAT 2008: 214.

⁴⁴ VEENHOF Ch. 4 in this volume.

⁴⁵ See now ZIEGLER Ch. 3 in this volume, with photographs Figs. 3.8–3.9.

⁴⁶ STRECK 1916: 462.

⁴⁷ CHARPIN 1993: 189–191.

⁴⁸ ALBAYRAK 2006.

The third argument against the proposed reconstruction of KEL G is due to M.7841, which appears to show an eponym Aššur-taklaku before Haya-malik (CHARPIN/ZIEGLER 2003: 70–73).⁴⁹ The format of the tablet is rather irregular and the reconstructed [*li-mu*] before the name Aššur-taklaku is uncertain, but quite plausible. During the conference, Lacambre noted that the tablet seems to be an inventory of possessions of Yasmah-Addu in Ekallatum. One wonders whether they go back to an endowment shortly after his birth. Alternatively, Ekallatum might have had its own eponyms. The eponyms Ibni-Adad and Atamar-Ištar in King List A might have the same source, an idea mooted by Kryszat on 4 December 2012 on the internet, in the Ancient Near Eastern Chronology Forum. Texts from Ekallatum itself would be helpful.

3. Eponyms and the Assyrian King List

Once the issue of Mesopotamian chronology in the first half of the second millennium BC is settled, one has a better chance to understand late Old Assyrian history. Here eponyms will be compared to the AKL and to KAV 21–24. The latter text needs a dedicated new study. It shows that reliable eponym lists were available until the end of the Assyrian empire.⁵⁰ Counting back to the destroyed beginning of the tablet, one finds that its first entry concerned an unremarkable year, in the middle of the reign of Tukulti-Ninurta I.⁵¹ The calculation should be redone based on a new edition of KAL 21–24, but this result is likely to be confirmed. Thus the tablet probably was the second of a pair that listed all eponyms from the beginning of the records under Erišum I. Following Ungnad's count, line 10 of column V it contains an unexplained entry [x-]MU, underlined by a short line between lines 10 and 11.⁵² Schroeder's copy of the latter line is not quite exact. If the LMC is correct and if the MEC eclipse happened in 1833 BC, year 1000 counted from Erišum I happened 1000 - 127 years after the eclipse, thus in 960 BC. Line 10 of column V corresponds exactly or almost exactly to year 1000, counted from Erišum I. The only uncertainty is the exact date of the eponym year of Tiglathpileser II, which is recorded in line 5 of column V.

Since the number of eponyms could be used as a check, one expects a nearly correct total year count in the Assyrian King List (AKL). This cannot be checked yet, because the reign lengths of rulers 65 and 66 have not been recovered so far. Another difficulty is the uncertainty about the reign length of ruler 61, Puzur-Aššur III (14 or 24 years). Nevertheless, one can estimate the total throne tenure of the four generations from ruler 61 to ruler 68. According to Rowton's statistics one expects 85 to 125 years for this period.⁵³ If one uses the LMC and the year count of the AKL up to ruler 60 and the standard Assyrian chronology for ruler 69, the tenure from ruler 61 to ruler 68 can be calculated. With a reign of Aššur-dan I of 46 years, one obtains 125 years, which means that the year count may fit. For a reign of 36 years instead one would need about 135 years for the four generations, which is not impossible but not very likely.

What happened in Assur in the final years of Hammurapi and during the reign of his son is still a mystery. More than 30 eponyms are known for this period, but they have not yet been matched with any political developments in Assur. One would expect that the men named as rulers 41–47 in the AKL were active at this time. Some of them might have been eponyms in some stage of their political career. A candidate is Ipqi-Ištar (no. 45 in AKL), who might be identical with the eponym Ipqu-Ištar of the year 1739 BC. Landsberger stated that names composed with Ipqu- did not exist in Assyria and treated Ipqi-Ištar as a foreign ruler.⁵⁴ This can hardly be maintained, but the name is remarkable and the identity of the two men is quite likely.

4. The Volcano of 1628/1627 BC: Aniakchak

The text above was written in 2015. Most of it needs no change, but its main message can be stated with greater confidence: For the chronology of the Old Babylonian period the volcanic eruption of 1628/1627 BC, reflected in EAE 63 for observations in the 12th year of Ammi-šaduqa is a secure anchor point. In this sense it is fully comparable to the solar eclipse of 763 BC, recorded in the eponymate of Bur-Sagale in the Neo-Assyrian period. Much of the greater confidence is due to the identification of the volcano and traces of its ash in ice layers. Currently, just two volcanic eruptions in the 17th and 16th century BC with large calderas are known, namely the Minoan Santorini event and the second eruption of

⁴⁹ For further discussion see ROAF Ch. 5 in this volume; §7 and LACAMBRE Ch. 7 in this volume; §2. For photographs of the tablet see Fig. 5.6.

⁵⁰ SCHROEDER 1920.

⁵¹ SCHROEDER 1921.

⁵² UNGNAD 1938: 436; VAT 11254 photographs taken by N. Morello, email 30.6.2021.

⁵³ ROWTON 1958.

⁵⁴ LANDSBERGER 1954: 38.

Aniakchak in the Aleutian Range in Alaska. As has been explained above, the Santorini eruption happened in the wrong season. The Aniakchak caldera was discovered to science 100 years ago.⁵⁵ It is harder to visit than Santorini but is the centre of the Aniakchak National Monument created in 1978. The caldera has a diameter of 10 km and an average depth of 500 m, thus it is one of the largest on Earth. The eruption certainly had an explosive strength comparable to the Minoan Santorini event, with an obscuration of the sky that matches the EAE 63 record.

Based only on radiocarbon, the date of the eruption was 1780–1640 BC for one standard deviation, 1920–1600 BC for two.⁵⁶ A more precise date was determined due to glass shards with the chemical signature of Aniakchak that had been found in ice-cores from Greenland. Counting the layers appeared to yield a date close to 1645 BC.⁵⁷ It turned out, however, that the precise counting of ice layers is more difficult than for tree-rings. Missing or doubled years occur in both cases, but the small number of ice-cores makes it much harder to correct mistakes. As a result, errors of one year or more in the count of ice layers happen once in one or two centuries. Adolphi/Muscheler measured the mismatch between ice-layers and tree-rings, using cosmogenic Carbon and Beryllium. For the averaged mismatch they found an approximately linear increase over the past 5000 years, which implies that the ice layer with Aniakchak tephra should have a true age of about 1625 ± 5 BC, where the error refers to two standard deviations.⁵⁸

Many volcanic events from the second millennium AD have well-known historical dates which allow a correction of the small miscounts and a firm dating of the corresponding ice layers. For earlier periods, the initial layer counts were wrong by seven years in the first century BC.⁵⁹ A firm identification of the tree-ring event from 1627 BC with the Aniakchak ice layer event became possible, because the tree-rings showed that it was preceded by another probable volcanic event in 1653 BC. Two independent ice-cores from Greenland showed that the Aniakchak ice layer event also was preceded by a volcanic event that happened 26 years earlier. Moreover, a severe tree-ring event happened in 2036 BC, 383 years before the 1653 BC event. This distance is closely matched by a count of 382 layers between the Aniakchak eruption and a previous volcanic event.⁶⁰ Consequently, the date 1628/27 BC for the Aniakchak eruption has been accepted, but there is still an uncertainty of about one year. Based on the tree-rings, a recent volcanological study dates the two salient events to 1654 BC and 1628 BC.⁶¹ Eventually it should be possible to proceed from annual to seasonal data and to check for an eruption time in winter 1628/27 BC.

5. The Santorini Eruption

The date of the Minoan Santorini eruption still is an open problem. Before the radiocarbon recalibration in 2020, a date in the second half of the 17th century BC appeared to be certain. Now this has become far less certain, though dates after 1540 BC remain excluded. The interpretation of the archaeological data is controversial, but the arguments for a date in the early 18th Dynasty of Egypt are quite strong.⁶² Current volcanological estimates focus on the range 1561–1538 BC.⁶³ Tree-ring data from Yamal have a strong signal in 1554 BC.⁶⁴

The Egyptian chronology for this time is uncertain by about 30 years, but in any case the year 1554 BC must have been close to the accession of Pharaoh Ahmose. His so-called ‘Tempest Stela’ may have recorded effects of the Santorini eruption, but the discussion is controversial.⁶⁵ The stela carried largely identical texts on recto and verso, but much is destroyed. It reports an atmospheric event in all of Egypt. If it was a tempest, one has a choice between a localized storm, which contradicts the text, a storm in all of Egypt, which contradicts science, or a symbolic interpretation, which is arbitrary and has no precedent. The rainstorm interpretation is based on the word for rain, of which only the first letter is preserved, and the word for floods, which is unambiguous but may have a different context. It would be better to talk about the ‘Boom Stela’, because of its report of a loud noise heard in all of Egypt.

⁵⁵ SMITH 1925.

⁵⁶ BACON *et al.* 2014: 14.

⁵⁷ PEARCE 2004.

⁵⁸ ADOLPHI/MUSCHELER 2016: 26.

⁵⁹ BAILLIE 2010; BAILLIE/MCANENEY 2015; SIGL *et al.* 2015.

⁶⁰ BAILLIE/MCANENEY 2019.

⁶¹ PEARSON *et al.* 2024.

⁶² HÖFLMAYER 2012.

⁶³ PEARSON *et al.* 2024.

⁶⁴ HANTEMIROV/SHIYATOV 2002a; HANTEMIROV *et al.* 2011.

⁶⁵ WIENER/ALLEN 1998; RITNER/MOELLER 2014; with references to previous articles.

According to RITNER/MOELLER 2014, after the introductory titles one reads from line 2 recto: ² ‘His Majesty dwelt in the town of Sedjefa-tawy ³ [in the district just to] the south of Dendera.’ Indeed, the predecessors of Ahmose had their residence about 30 km north of Thebes, though the old city remained the religious centre. At some time during Ahmose’s reign the residence was shifted to Thebes, but not before the event under consideration. According to lines 3–5 recto, Ahmose went to Thebes to bring offerings to Amun, which were well received. When he left, the god objected. At this point the text becomes difficult. According to Allen (p. 3), the god desired that the king return to him, but the phrase translated as ‘return to him’ is in a lacuna and was restored by Allen: Ritner (p. 6) states that other restorations are possible but does not provide any. To enforce his desire, the god sent a terrifying atmospheric event to the whole country. This led to widespread looting, ‘the entering of the sacred estates, the dismantling of tombs, the hacking up of mortuary enclosures, and the toppling of pyramids’ (translation by Allen, line 15, recto). Ahmose interpreted the events as unintended collateral damage: ‘How much greater this is than the wrath of the great god, [than] the plans of the gods!’ (translation by Allen, line 10, recto). He deployed his top administrators and his army, gave help to the victims and managed to restore order. He satisfied the wish of the god, which probably implied that he moved the residence to Thebes.

As far as the terrifying event is concerned, the text explicitly states that it concerned the whole country. The event is described in three parts (lines 7–8, verso). The first part is fragmentary. The second part reads *p.t šn .tj nn wn.[t 3bw]*, translated by Allen as ‘[with]... the sky beclouded without [stop]’. The translation of *šn* ‘as ‘becloud’ has been criticized as too limited, because the verb expresses something more energetic.⁶⁶ The determinative of the verb carries additional information.⁶⁷ On the recto it is the recumbent Seth animal above the sky with rain, on the verso the same animal above the normal sky without rain. Schneider calls attention to the fact that ‘once Auaris was taken over, [we] find Ahmose (and the early 18th Dynasty) as strict observers of the worship of [Seth-]Baal’ and to EA147, where Abimilki of Tyre says that the pharaoh ‘gives forth his cry in the sky like Baal, and all the land is frightened at his cry.’⁶⁸ The verb *šn* ‘clearly needs more discussion among Egyptologists. Schneider has drawn my attention to an association with illness and seething.’⁶⁹ Thus ‘the sky seething without [stop]’ may come close to the intended image, but ‘oppressive sky’ would cover another aspect of the verb.

The third part is the longest and its end is well preserved. It describes an unprecedented noise ‘on the mountains louder than the sound of the underground source of the Nile that is in Elephantine.’ This is what one would expect from the Santorini eruption, as stressed by Moeller.⁷⁰ Coming back to the first part, Allen’s translation ‘storm of r[ain, with dark]ness in the western region’ is conjectural. Only the word for storm-wind is clear, of the subsequent word ‘rain’ only the first consonant is preserved. The following ‘darkness’ is based on the determinative. One needs more input from Egyptologists, but something like *ḏ n ḥ[3t jb m jḥḥw]*, ‘a storm-wind of moaning at dawn’ might capture Amun’s complaint and the resulting terror. To say more one also needs a sufficiently detailed modelling of the effect of the Santorini boom in the Nile valley, in parallel to what de Jong has done with his calculations of a large volcanic event on the Babylonian Venus observations.⁷¹ In particular, echoing effects are important, because the text states that the effect went as far as the mountains in the west. A statement concerning some time after the event is translated by Allen ‘no torch could give light over the Two Lands’. This cannot be true in a literal sense. Schneider translates this phrase as ‘the luminary (= Re) could not shine over the two lands.’⁷² This may well describe the turbidity due to stratospheric aerosols. A proper translation of the whole text needs input from science and Egyptology, but one preliminary remark is necessary. The first action of the king against the chaos is the conducting of the floods (line 13, recto). The Santorini eruption happened in June/July, thus at the time of the annual Nile flood. It appears possible that the civil unrest had prevented the usual agricultural response.

Altogether, the Santorini eruption is crucial for dating Egyptian and Mediterranean events, but currently of little direct importance for Mesopotamian history. Nevertheless, it will be important to find traces of its tephra in Greenland, in order to test our understanding of the relation between volcanism, tree-rings and ice layers.

⁶⁶ RITNER/MOELLER 2014: 6 n. 40.

⁶⁷ SCHNEIDER 2010: 406.

⁶⁸ SCHNEIDER 2010: 409.

⁶⁹ T. Schneider, email 25/1/2022.

⁷⁰ RITNER/MOELLER 2014: 18.

⁷¹ See DE JONG Ch. 1 in this volume.

⁷² SCHNEIDER 2010: 406.

6. The date of the MEC eclipse: the current status

This section considers the MEC eclipse record in the context of the LMC. For possibilities to reconcile this eclipse with other chronologies, see Roaf's article.⁷³ The relative chronology between Old Babylonian and Old Assyrian dates before Šamši-Adad's conquest of Mari still has an uncertainty of a few years. It depends on the number n of eponyms between Dadaya, the last certain eponym in column II of KEL G (REL 178), and Haya-malik, the eponym of the conquest. An extra eponym Ahiyaya during Šamši-Adad's rule at Mari also would contribute to n . In his article in this volume, Veenhof points out that there are arguments for n in the range 0–6. From the reconstruction of the tablet, there 'appears to be no room for an additional line' beyond $n=4$.⁷⁴ In section 2 I have argued that even 4 is too high, whereas 0–2 is possible without problems.⁷⁵ If $n=0$, the solar eclipse in the year of REL 127 was the total eclipse of 1833 BC. If $n=1$, the solar eclipse must remain the same, but its correct eponym was REL 128. If $n=5$, one obtains the partial eclipse at sunrise in 1838 BC. If $n=4$, the correct date of the latter eclipse was REL 126. Errors of one year weaken the argument, but are quite common.

In section 2 three problems with $n=0$ have been described. One of them is resolved. Birot's copy of S.24-3 had a minor mistake in line 3', which made its placement problematic.⁷⁶ The text is important, but of no relevance for the eclipse date. The Mari tablet FM V 2 (= M.7841) has been treated by D. Lacambre in this volume.⁷⁷ It appears to summarize arrears stemming from the time when Yasmah-Addu was still in Ekallatum. There is a strong case for a sequential listing of arrears from the eponymates of Aššur-taklaku and Haya-malik, whereas the reading of two further eponym names cannot be relied upon. Yasmah-Addu must have been born around eponym Aššur-taklaku = KEL 170. Indeed, his father arranged a diplomatic marriage, plausibly at the age of 17, five years after installing him as king of Mari, plausibly at the age of 12. A transfer of endowments from Ekallatum to Mari at the occasion of his accession is probable, but a nine-year interval between the years of Aššur-taklaku and Haya-malik is unexpectedly large. Ekallatum's own dating system may become known soon, because the city has just been identified.⁷⁸

Concerning the final problem for $n=0$, Veenhof has argued that the similarity between *A-NI-m[a-lik]* and *Ha-NI-ma-lik* is deceptive, because the latter name would look different in Assyrian orthography. This argument is not very strong, because Kryszat has shown that KEL G has Babylonian orthographic features.⁷⁹ Alternations A/Ha in proper names do occur, but *Ha¹(A)-NI-m[a-lik]* remains problematic.

Lacambre gets rid of both problems by introducing another eponym Aššur-taklaku between Dadaya (REL 178) and Haya-malik, written *A-šur¹(NI)-tá[k-lá-ku]*.⁸⁰ The readings *-m[a-* and *-tá[k-* for the damaged sign are both possible, indeed the only possible ones. The emendation *-šur¹(NI)-* looks far less likely than *Ha¹(A)-*, but otherwise Lacambre's solution is elegant. If his eponym sequence is correct, an alternative interpretation of the KEL G text would be that its scribe started with the initial sign of Aššur-taklaku, but got confused and continued with Haya-malik. All of this is speculative, but $n=1$ is a possibility.

With Ahiyaya, Lacambre wants to add a further eponym in the gap, so that $n=2$. A mistake of two years for the solar eclipse is problematic, but perhaps possible. Still, I prefer to regard Ahiyaya as an alternative name for Asqudum, as argued above. The identification is compatible with all the temporal criteria worked out in Lacambre's article.

Veenhof first read the name as *A-lí-m[a-lik]*, but switched to *A-ni-m[a* in the final version contained in this volume. Neither name is known from elsewhere. On the other hand, Veenhof's case is supported by the distance number of 159 years between Erišum I and Šamši-Adad, in an inscription of Shalmaneser I. If one interprets it as a count from the last year of Erišum I to the last year of Šamši-Adad and adds 159 years to the 40 year reign of Erišum I, this places the death of Šamši-Adad at eponym year 199, equivalent to $n=6$.⁸¹ For this volume, Veenhof hesitatingly reduced the count to $n=5$, but this is still one more than the upper limit stated by BARJAMOVIC/HERTEL/LARSEN 2012. Moreover, the distance number may have a more natural interpretation as a count between initial years of the two rulers, with an error of one year. If $n=0$

⁷³ ROAF Ch. 5 in this volume.

⁷⁴ BARJAMOVIC/HERTEL/LARSEN 2012: 27.

⁷⁵ NAHM 2013: 366.

⁷⁶ ZIEGLER Ch. 3 in this volume.

⁷⁷ LACAMBRE Ch. 7 in this volume. For a photograph of this tablet see Fig. 5.6 in this volume.

⁷⁸ OTTO/ZIEGLER 2022.

⁷⁹ KRYSZAT Ch. 8 in this volume.

⁸⁰ LACAMBRE Ch. 7 in this volume.

⁸¹ VEENHOF 2003: 51f.

and if Šamši-Adad ruled for 33 years in Assur, his accession happened in REL 160, one year off the expected REL 159. It is true that Veenhof's version is supported by the alternative distance number of 126 years given in an inscription of Esarhaddon, but Esarhaddon's control of ancient history was shaky and the subtraction 159–33 may have been based on a misunderstanding.

For now, the total eclipse of 1833 BC and the partial eclipse at sunrise of 1838 BC remain as candidates for the eclipse in the Mari Eponym Chronicle. The line count of KEL G is hard to reconcile with 1838 BC, but it seems that a resolution will need stronger arguments. In the next section, a different solar eclipse will be discussed. It is not affected by the uncertainties just studied, but by our incomplete knowledge of the rotation of our planet.

7. An annular solar eclipse in 1768 BC observed at Mari?

Any chronology can be tested by searching for direct or indirect attestations of solar eclipses. It is easy to calculate the days when the Moon's shadow fell on the Earth at a particular latitude, but the precise place depends on the Earth's rotation angle around the polar axis. For the period around 1800 BC it is not yet possible to calculate this angle to an accuracy better than about 10°. The uncertainty is expressed by a clock correction time ΔT , where one day corresponds to a full circle of 360° and one hour to 15°. When the shadow of the Moon moved parallel to the equator the uncertainty is of minor importance, except for eclipses at sunrise or sunset, but when the path of the Moon's shadow is narrow and inclined against the East-West direction it does become important. Still, if current best estimates are correct, there was an impressive annular eclipse at Mari on 20 December 1768, a ring of fire in the sky shortly after sunrise.⁸²

In the LMC, this was less than a year before the death of Šamši-Adad. His eldest son Išme-Dagan tried hard to keep things together, but the end of the extended kingdom was imminent. His brother Yasmah-Addu was ruler of Mari. His capital would fall soon and he must have felt the pressure. In that situation a ring of fire in the sky may have driven him to desperate means. Two of his actions can be interpreted in this sense. He wrote a letter to the god Nergal and he lit a large fire for no apparent reason. The letter starts with the statement that Nergal had talked to him. At the end Yasmah-Addu wishes nothing more than life and progeny. The previous sentence may have explained in which way Nergal had acted, but it is very fragmentary: '[N]A[?] (or UD[?]) KU *it-ti-[i]a mi-na[m]*'.⁸³ I am unable to make sense out of it. CHARPIN/DURAND obtain a plausible interpretation, but at the high price of emending KU to DUMU. Because Nergal was the god of plague, it is well possible that Yasmah-Addu had lost his oldest, maybe even his only son due to the epidemic of 1771 BC.⁸⁴ But Yasmah-Addu was still young. The experience was not uncommon, as attested by many personal names that thank some god for a replacement of a son lost in infancy. In the months after the death of Šamši-Adad, four palace women bore children.⁸⁵ Thus it makes sense to look for a different interpretation. The annular eclipse may have been linked to Nergal, because it took place close to the winter solstice. At least in later times, this period was dedicated to Nergal.⁸⁶ On the other hand, Nergal is connected with the sunset and its only connection with sunrise occurs in an unclear context.⁸⁷

A second action of Yasmah-Addu caused an explicit criticism of his panic. He lit a double fire in the night, which might have been regarded as a call for military help. Išme-Dagan asked Yasmah-Addu to send a fast messenger to various districts with a counter-order and a made-up explanation (DURAND 1998, LAPO 502):

'Dis à Yasmah-Addu: ainsi parle Isme-Dagan, ton frère. Il ne faudrait pas que le Pays vienne tout entier au secours parce que tu as levé dans la nuit un double feu. Fais rédiger des tablettes pour le pays tout entier, pour le pays d'Andarig, y compris le district de Hâsidānum et celui du Nurrugûm, et fais-les porter par des serviteurs à toi rapides, disant: «L'armée lourde de l'ennemi est sortie du pays pour une opération de pillage. Voilà pourquoi 2 feux ont été levés. Ne venez pas au secours.»'

Yasmah-Addu's response to Išme-Dagan is not known, but it hardly was close to the truth, judging by Išme-Dagan's annoyed reply (LAPO 503). Otherwise it seems that Yasmah-Addu followed the advice of his brother. He received a disparaging reply (LAPO 504):

⁸² NAHM 2013: 368.

⁸³ DURAND 2000: 72 n. 931, line 23f.

⁸⁴ CHARPIN/ZIEGLER 2003: 111, n. 294.

⁸⁵ CHARPIN/ZIEGLER 2003: 154f.

⁸⁶ WIGGERMANN 1998: 222b.

⁸⁷ ARTEMOV 2012: 23.

‘Dis à mon Seigneur Yasmah-Addu: ainsi parle Meskînum, ton serviteur. Mon Seigneur m’a écrit que l’on a levé deux torches. Dans le Haut Pays, on ne montre pas d’empressement pour la torche et on n’en a pas levé. Que mon Seigneur prévienne bien en ce qui concerne l’usage de la torche: si jamais il se produit quelque chose d’inquiétant, ce n’est pas sur nous que s’abattrait la panique.’

This rather bizarre event can be explained, if Yasmah-Addu tried to contact a deity, either Nergal as in his letter or someone else. That Meskînum saw no reason to pay attention to the signal must mean that it was lit outside of the war season, most likely in winter. The solar eclipse had a narrow range of about 80 km, and people outside of this range would not have been impressed. Finally, one may ask if the oil for the king and the troop delivered on 16/iv of that eponym year was intended for an expiatory ceremony.⁸⁸ The interpretation of the events has to remain speculative until more data are available, either on history or on ΔT . If ΔT differs significantly from the currently preferred value, it falls immediately.

8. Further evidence for the Lower Middle Chronology

Radiocarbon dates strongly support the Middle Chronologies.⁸⁹ They are crucial, but suffer from a history of repeated contradictory statements, all called definitive. On the other hand, the data never supported the High or the Short chronologies. A detailed discussion of the wrong announcements and the remaining potential errors should restore confidence in the precision of the results. At present, one cannot expect a decision between UMC and LMC by this method.

Another argument for the Middle Chronologies is the statement of the text BM 17175+ that the spring equinox lies at day 15 of the last month of the year. Due to the lunisolar nature of the Babylonian calendar, the position fluctuated from year to year, but the statement should be true on average. For the reign of Ammi-šaduqa and almost certainly for the last two decades of the reign of Ammi-ditana this is correct for the LMC and grossly wrong for HC and SC.⁹⁰ For the UMC it is wrong by a few days. With further data, this might become a strong argument.

In a Middle Babylonian school text, a time interval of 500 years is given as the period from an unspecified event at an earlier date to the beginning of the reign of Marduk-šapik-zeri. If the time interval of 418 years given in the Bavarian inscription of Sennacherib is correct, the beginning of the reign of Marduk-šapik-zeri's predecessor Marduk-nadin-ahhe can be dated to 1106 BC or 1105 BC, with minimal uncertainty.⁹¹ Since Marduk-nadin-ahhe ruled for 18 years, the date of the unspecified event can be calculated as very close to 1588 BC or 1587 BC. For the Upper Middle Chronology, Poebel found no candidate for the earlier event.⁹² For the Lower Middle Chronology, however, one arrives at a date close to the fall of Babylon if it is assumed that this event coincided with the end of the Hammurapi dynasty and that Ammi-šaduqa and Samsu-ditana really reigned for the 21 and 31 years given by King List 7.⁹³ This has been put in doubt.⁹⁴

No specific arguments supporting the Upper Middle Chronology have turned up after the Lille conference, neither firm nor speculative ones.

9. New Perspectives with the MEC

The known fragments of the MEC appear to list three natural events: The eclipse in the eponymate of Puzur-Ištar, here dated to 1833 BC, a flood in a distant country in 1824 BC, and what may have been a meteorite (*ki-ir-ba-na-a*, cf. *kirbānu* 2b lump (of stone, metal or slag), CAD K 403b) in 1800 BC. If the annular eclipse of 1768 BC was observed, it is likely that it got an entry, too. The two eclipses would have provided an approximate frame for the life of Šamši-Adad. Nevertheless, the author of the MEC kept events separate; the earlier eclipse was recorded in the year after the birth of Šamši-Adad, at the same time the year before the death of Aminum. The event of 1800 BC would have preceded the accession of Šamši-Adad in Assur by one year, but the flood was followed by three years without recorded events. Unfortunately, the country is not named, one wonders if the event was a tsunami.

The MEC gives no hint of manipulating data. It gives equal attention to Ešnunna and to the house of Šamši-Adad and ends with a victory of Zimri-Lim a year or two after the death of Šamši-Adad. The text seems to be precocious, anticipating

⁸⁸ CHARPIN/ZIEGLER 2003: 153.

⁸⁹ MANNING 2020; PEARSON 2022.

⁹⁰ NAHM 2023a; NAHM 2023b.

⁹¹ BRINKMAN 1968: 84.

⁹² POEBEL 1955.

⁹³ NAHM 2023a.

⁹⁴ ROAF 2012: 169–170; ROAF Ch. 3 in this volume: §1.

the type of annals that became popular more than a millennium later, from China to Rome. A comparison between the MEC and Confucius's Chunqiu Annals would be of interest. In the intervening years the record of partial solar eclipses had become routine and Confucius had far greater resources. Nevertheless, there are common features. The texts share the terse diction, the type of events recorded and the absence of bias. The comparison suggests an MEC author like Confucius; an independent state servant, ready to look for employment in various places. There were such persons in Old Babylonian time. One of them was Napsuna-Addu, brother of queen Iltani, the wife of Aqba-Hammu. They communicated frequently. Iltani's archive preserves 37 letters from him, even short ones that only ask about her well-being. OBTR 42 appears to be one of the relaxed family letters, but it has a serious undertone, expressed by a political joke. In the translation given by DALLEY/WALKER/HAWKINS (1976: 53):

'Speak to Iltani, thus Napsuna-Addu. May Šamaš and Marduk grant you long life. Concerning what you wrote saying: "I have sent you small fishes which Aqba-Hammu loves (ira'ammu)" – just as your husband Aqba-Hammu knows (lamdu) little fishes from Qaṭara and Karana, I myself have from of old liked (ara'am) the big fish from Šubat-Enlil, Ekallatum, Mari and Babylon. If indeed there is no big fish, you will send me little ones, but who will eat them?'

Small fish are food for big fish, *grandibus exigui sunt pisces piscibus esca*. I don't know if such a proverb existed in the Old Babylonian period, but the idea could easily arise spontaneously. Napsuna-Addu copied Iltani's verb, but he clearly expressed his interest in learning about the big fish residing in the capitals. The author of the MEC would certainly have added Ešnunna.

The transfer of events dated by different local calendars to the MEC could easily lead to errors of a year or so, but so far no example of a wrong date has been found. Of particular interest is a potential synchronism with an event in the fifth year of Sin-iddinam of Larsa.⁹⁵ His sixth year name, his fifth full year of reign is: mu a₂-dam / ma-da eš₃-nun-na^{ki} ba-an-hul ('year: the dwelling / the land of Ešnunna was destroyed'). This year is said to be 1845 UMC, equivalent to 1837 LMC. The event referred to in that year name would have occurred in 1838 LMC, i.e. five years before 1833 BC.⁹⁶ MEC records a victory of Elam over Ešnunna 14 years after the eclipse, so that one might expect an entry for the victory of Larsa, too. Five years before the eclipse there is an entry [*i-na l-di*]-a-bu-um (d)EN.Z[U- (M.7481:20). Nothing more is preserved, but because there are only two MEC entries starting with Sin-, this is still quite a strong argument for a synchronism and supports the eclipse date of 1833 BC. In any case, a definitive determination of the chronology of the reign of Sin-iddinam would help. Some uncertainty is due to four attestations of a year 7-bis, which is usually identified with the first year of his successor Sin-iribam.⁹⁷ Note, however, that the first tablets of Sin-iribam are dated to the fourth month (PBS 8 105, YOS 5 156) and to the fifth month (CUSAS 15 173, YOS 5 155, YOS 14 294), whereas two tablets from the year Sin-iddinam 7-bis are dated to vi/15 (FLP 1331 and 1333). HALLO regarded them as possible evidence for an eight-year reign.⁹⁸

Another expected MEC entry is the accession of Šamši-Adad at Assur. Since he ruled Assur for 33 years, it should have been recorded for eponym 58 of the MEC. Indeed, there was an entry starting with his name. This is not a strong confirmation, however, since in the relevant decade every second entry started like that.

Since many MEC events concern Ešnunna, one may expect to find further synchronisms, but the reconstruction of the chronology of Ešnunna before Ibal-pi-El II is not straightforward.⁹⁹ In any case it seems that a date of the Puzur-Ištar eclipse at 1838 BC is very hard to reconcile with what is known about reign lengths at Ešnunna. For the date of 1833 BC one finds potential synchronisms.¹⁰⁰ With the publication of more texts from the territory ruled by Ešnunna it certainly will be possible to make progress.

Altogether, there is much progress in Mesopotamian chronology, but one wishes for more text editions from Kanesh Ib and Ešnunna and of course for new texts from Ekallatum.

⁹⁵ NAHM 2017b.

⁹⁶ I thank M. Roaf for a clarification of this issue.

⁹⁷ FIETTE 2020: 279 n. 21; FIETTE Ch. 10 in this volume.

⁹⁸ HALLO 2010: 340 n. 29a.

⁹⁹ LACAMBRE/VAN KOPPEN 2009.

¹⁰⁰ NAHM 2017a.

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12. Chronology of the second millennium BCE in the Lower Middle Euphrates region: the evidence from Hana

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The kingdom of Hana existed in the Lower Middle Euphrates region from the Late Old Babylonian period to at least 1300 BCE, to judge from comparable documents and archaeological evidence. Two corpora of cuneiform texts, found in Areas C and E at Terqa, the possible capital city of Hana, are key to determining the chronology. The dates of tablets from each area do not overlap; the earliest documents in a jar archive from Area E (beginning with the reign of Ammi-šaduqa of Babylon) almost immediately follow the latest texts from Area C (which span the period from Samsu-iluna to Ammi-ditana of Babylon). Area E texts then continue through the reigns of Samsu-ditana of Babylon and 16 subsequent local kings. Prosopographic evidence and frequent instances of father-son royal succession indicate that the kings named in the Hana texts, from earliest to latest, represent about 20 generations, or as much as 500 years (assuming an average father-son generation of 25 years). The Hana documents therefore lend support to a Middle Chronology, and almost certainly include some tablets that were written during the elusive 16th century BCE.¹

For more than a century, the chronology of the kingdom of Hana has been the subject of considerable debate. Hana was located in the Lower Middle Euphrates region of Syria around the confluence of the Habur and Euphrates rivers and is attested largely from the excavations at the site of Terqa (modern Tell Ashara), the kingdom's probable capital city. In spite of uncertainties around the dating of particular kings' reigns, it is clear that a kingdom, usually called Hana, existed in this region from the reign of Samsu-iluna of Babylon, after the conquest of Mari by Hammurabi (late 18th century BCE, by the Middle Chronology [MC]), to the Middle Assyrian period (at least the 14th century BCE). In this paper I will argue that documents found in stratigraphic contexts in Areas C and E at Terqa were deposited at least 300 years apart and the dates of the tablets in each area do not overlap. The dozens of kings named in them represent about 20 generations and are spread across the entire period from ca. 1740 BCE to ca. 1300 BCE. The Hana documents therefore lend support to a Middle Chronology, and almost certainly include some tablets that were written during the elusive 16th century BCE.

1. Documents from Areas C and E at Terqa

Formal excavations at Terqa began in 1974 and were initially led by Giorgio Buccellati and Marilyn Kelly-Buccellati. In 1987 Oliver Rouault took over as director and excavations continued until 2010. Over these years, several hundred cuneiform tablets and tablet fragments were recovered, especially from two areas of the excavation, Areas C and E.

1.1 Area C

Area C, which was excavated for the most part during the first six seasons, constituted a neighborhood of a few private houses, some narrow streets, and a small shrine to the goddess Ninkarrak. An archive belonging to a man named Puzurum was uncovered in one house in this area,² while other documents referring to Puzurum's neighbors and their families came from the area of the Ninkarrak shrine and the buildings and street adjoining it.³

¹ An earlier version of this chapter was presented in 2015 for the conference called 'Perspectives on the chronology of the early second millennium BC in the Near East and Egypt', at Université Lille III and has been cited elsewhere under a previous title: 'Chronology in the Middle Euphrates region: a continuous sequence of kings'.

² ROUAULT 1984.

³ ROUAULT 2011.

The dating of the documents found in Area C is uncontroversial. Several of the tablets in Puzurum's archive bore year-names of a king named Yadih-abu.⁴ Rouault proposed that this was the same king who was identified as an enemy of Babylon who had been defeated in the 28th year-name of Samsu-iluna of Babylon (1723 BCE [MC]).⁵ The two Hana kings who preceded Yadih-abu and who were also attested in Puzurum's archive, namely Yapah-Sumu-abu and Iši-Sumu-abu, must therefore also have been contemporary with Samsu-iluna of Babylon. The latest document in Puzurum's archive dated to a king with the Kassite name Kaštiliašu, who must have ruled shortly after Yadih-abu, because many people recorded in documents from Terqa lived through both reigns.⁶ Kaštiliašu therefore was approximately contemporary with Abi-ešuh of Babylon (1711–1684 BCE).

Three Hana-style documents from the reign of Kaštiliašu had appeared on the antiquities market long before the excavations of the 1970s, as had a few from the reigns of two later Hana kings, Šunuhru-Ammu, and Ammi-madar, son of Šunuhru-Ammu.⁷ Scholars in the early 20th century rightly concluded that all three of these kings ruled during the Late Old Babylonian period. Other documents from the reigns of the kings known from Area C have been uncovered at the sites of Tell Taban, to the north on the Habur River (dating to kings Iši-Sumu-abu and Yadih-abu);⁸ and Harradum, south of Terqa on the Euphrates (Iši-Sumu-abu),⁹ both of which must have been ruled by the Early Hana kings. A letter found at Tell Sakka, near Damascus, seems to have been sent there by the Hana king Kaštiliašu.¹⁰

More documents from the reign of Kaštiliašu were excavated from the Ninkarrak shrine and adjoining buildings in Area C, as was one from the reign of Šunuhru-Ammu. This document provides the date of the latest stratigraphic level in Area C.¹¹ Although no documents were found there dating to Ammi-madar, his reign must have succeeded that of Šunuhru-Ammu because Ammi-madar's only known year-name is that in which he 'ascended to the throne of the house of his father'.¹² A prosopographical study of four local families mentioned across all the tablets from the reigns of these late Old Babylonian / Early Hana kings shows that they ruled for four generations, or approximately 100 years.¹³ The documents from Area C therefore date to a period from approximately 1740–1640 BCE, contemporary with Babylonian kings Samsu-iluna, Abi-ešuh, and Ammi-ditana (1683–1647 BCE), and perhaps extending into the reign of king Ammi-šaduqa (1646–1626 BCE). Many aspects of the material culture in Area C, along with the physical traits and contents of the tablets themselves, confirm this conclusion.

One curious group of artifacts seemed, however, to belie these dates. These were nine scarabs found in the Ninkarrak shrine in a level apparently dating to the reign of Kaštiliašu. A. Ahrens noted in 2010 that they were of a Levantine Hyksos type that was found in MB IIB and MB IIB/C sites in Syria no earlier than the 15th Dynasty in Egypt, ca. 1650/40 BCE.¹⁴ This seemed too late, given that other evidence from Terqa made it clear that Kaštiliašu ruled soon after the reign of Samsu-iluna in Babylon. Ahrens argued that Kaštiliašu must have ruled at the middle, or possibly the end of the 17th century BCE. (This evidence helped persuade me at the time that the Low Chronology might be a better fit for the Hana evidence, as I noted in a 2014 article.)¹⁵ However, a more recent discovery at Edfu in Egypt of seal impressions of the 15th Dynasty king Khayan in the same context as those of 13th dynasty king Sobekhotep IV, might raise the date of the earliest Hyksos finds. This would allow for an early 17th century date for Kaštiliašu, in keeping with the Middle Chronology.¹⁶

⁴ TFR 1: tablets 1, 2, 3, 4, 5, 6, and 7, some with accompanying envelopes: ROUAULT 1984: 6–42.

⁵ ROUAULT 2004: 4.

⁶ TFR1 10: ROUAULT 1984: 54–55, 90.

⁷ Kaštiliašu: LH 1, 2, 3; Šunuhru-Ammu: LH 6, 7, 8; Ammi-madar: LH 9: PODANY 2002. Original publication dates of these texts ranged from 1909 to 1929: LH 1: THUREAU-DANGIN 1909; LH 2: SCHORR 1910; LH 3: BAUER 1928–1929; LH 6: THUREAU-DANGIN/DHORME 1924: 269–270; LH 7: THUREAU-DANGIN/DHORME 1924: 271; LH 8: THUREAU-DANGIN/DHORME 1924: 272–273; LH9: UNGNAD 1909: 26–32.

⁸ Iši-Sumu-abu: Tab T05B-42, Tab T05B-43; Tab 05B-39: YAMADA 2011b: 140–144; Tab T06-4: YAMADA 2012: 593–595. Yadih-abu: Tab T07-3: SHIBATA/YAMADA 2009: 89–90.

⁹ JOANNÉS 2006: text 16.

¹⁰ ABDALLAH/DURAND 2014; PODANY 2016: 81–82.

¹¹ TFR2 Terqa 5-2, ROUAULT 2011: 10–11.

¹² LH 9: 57: PODANY 2002: 117–120.

¹³ PODANY 2002: 27.

¹⁴ AHRENS 2010: 434–436.

¹⁵ PODANY 2014: 52.

¹⁶ MOELLER 2011. My thanks to W. Nahm for alerting me to this reference.

1.2 Area E

1.2.1 Middle Hana Period tablets from Area E and elsewhere

In the years during which Olivier Rouault directed the excavations at Terqa, more tablets were discovered, especially in Area E. This is at the highest point of the tell and was home to a large public building that is now right at the edge of the escarpment caused by river erosion. Two jars containing cuneiform tablets were discovered there during the 1989 excavation season, one of them complete, the other broken and perhaps missing some of its original contents. The texts found in the jars, along with a few tablets that were uncovered in subsequent seasons, have not yet been published but have been described by Rouault in several publications.¹⁷ The tablets in the jar archive largely consist of land grants, and they date to the reigns of 16 kings (4 of whom seem to have been overlords of local kings), none of whom are the same as the kings who are attested in tablets from Area C.

These kings are as follows: Ammi-šaduqa of Babylon (TQ 12-13); Samsu-ditana of Babylon (TQ 12-18); Zimri-Lim, son of king Idi-abu (TQ 12-17, TQ 12-20); Kasapan (TQ 12-11); Kuwari (TQ 12-05, TQ 12-07, TQ 12-08, TQ 12-12, TQ 12-22, TQ 12-23); Hanaya, who ruled with another king named Yausa (TQ 12-14); Qiš-Addu, who apparently was the son of Siniya and who ruled with three kings named Sausadatra (TQ 12-06, TQ 12-21), Sa'itarna (TQ 12-19), and Paratarna (TQ 12-09, TQ 12-10, TQ 12-15, TQ 12-16); Iddin-Kakka (TQ 12-24); Išar-Lim (TQ 12-03); Isi(h)-Dagah, son of Iggid-Lim (TQ 12-02); (H)ammurapi(h), son of Ahuni (TQ 12-01, TQ 12-04, TQ 12-25, TQ 12-47); and Pagiru (TQ 12-26, TQ 12-29). In addition, two more tablets from the reign of Qiš-Addu were included, though the name of the relevant overlord was missing (TQ 12-30, TQ 12-43), and one tablet dated to the reign of a son of (H)ammurapi(h), whose name is broken (TQ 12-33).¹⁸

Almost all the known rulers of Hana are included among the tablets from Areas C and E, but the lack of overlap between the two groups is striking. Other than the two kings of Babylon, the kings of the Area E archive seem all to have ruled during the Middle Hana period, which, I have argued, lasted from the end of the First Dynasty of Babylon to the reign of Pagiru.

Tell Taban has produced a document from this period as well, an adoption contract dating to and sealed by Ahuni, king of Hana, who is otherwise known only from the seal inscription of (H)ammurapi(h), where he was listed as that king's father.¹⁹ A fragmentary tablet sealed by (H)ammurapi(h), king of Hana was found built into an ancient wall at Dura-Europos,²⁰ and an undated tablet fragment from Dur Katlimmu (also, coincidentally, built into a wall) seems likewise to be from this era.²¹ In unstratified levels at Terqa were found fragments of a contract from the reign of King Iggid-Lim of Hana, who is known to have been the son of Išar-Lim (TPR7-4), and of another contract, this one from the reign of Isih-Dagan of Hana (TPR7-5).²² All these locations were within the kingdom of Mari in the Old Babylonian period and seem to have continued to be subject to the kings of Hana.

Some other Middle Hana period documents have no known archaeological provenance, having been acquired by museums or collectors mostly before the beginning of formal excavations at Terqa. These include one contract each from the reigns of Išar-Lim,²³ Iggid-Lim,²⁴ and Isih-Dagan,²⁵ and a contract and a duck weight from the reign of (H)ammurapi(h).²⁶ Each of these contracts was sealed by the relevant king with his own seal. The seal inscriptions reveal that Išar-Lim was the son of Iddin-Kakka (known from the jar archive), Iggid-Lim was the son of Išar-Lim, and Isih-Dagan was the son of Iggid-Lim. (H)ammurapi(h) was the son of Ahuni, known from the adoption contract from Tell Taban. These father-son transfers of power suggest that the dynasty was stable and that their reigns may each have lasted a generation.

¹⁷ ROUAULT 1992a: 251–254; ROUAULT 1993–1994: 285–286; ROUAULT 2001: 11–12; ROUAULT 2004: 54–57; ROUAULT 2017. The most comprehensive list of the archive tablets is found in ROUAULT 2021: Table 1, 153–154. The only published images of any of these tablets are in ROUAULT 1993–1994: 285–286 (TQ 12-05), and ROUAULT/MASETTI-ROUAULT 1993: 338 (TQ 12-06).

¹⁸ ROUAULT 2021: Table 1, 153–154.

¹⁹ Tab T09-47: YAMADA 2011a.

²⁰ LH 14: PODANY 2002: 139–143 original publication STEPHENS 1937.

²¹ This tablet reflects Hana scribal practice, and Frahm has argued that it might date to the reign of Iggid-Lim. RADNER 2002: no. 1; FRAHM 2020: 37–38.

²² ROUAULT 1979.

²³ LH 10: PODANY 2002: 122–125; original publication THUREAU-DANGIN 1897.

²⁴ LH 11: PODANY/BECKMAN/COLBOW 1991–1993; PODANY 2002: 126–129.

²⁵ LH 12: PODANY 2002: 130–132; original publication NOUGAYROL 1947: 42–46.

²⁶ LH 13: PODANY 2002: 133–138; original publication JOHNS 1907. LH 16: PODANY 2002: 149–150, original publication THUREAU-DANGIN/DHORME 1924: 275.

An additional unpublished document from the reign of Isih-Dagan of Hana, again bearing a royal seal impression, is in private hands. It is described by Walther Sallaberger as being a fragmentary royal grant of a house dating to the third day of the month Belet-beri in the year when Isih-Dagan ascended to the throne of his father.²⁷ It is intriguing to note that the divine owners of the house included Šamaš and Dagan. The rest of the line is broken but the other owners are likely to have been the god Itur-Mer and the king, as in a royal bequest from the reign of Isih-Dagan's grandfather Išar-Lim (LH 10), among others. Although unfortunately the findspots of both this tablet and another from the reign of Isih-Dagan (LH 12) are unknown, a man named Išsur-Adad is listed as a scribe on both.

1.2.2 The date of the jar archive in Area E

The building in Area E in which the tablet jars were found was large and impressive. The excavators could uncover only a narrow sliver of it, but it was undoubtedly public (possibly a palace, temple, or administrative structure) with thick, well-built walls that were rebuilt generation after generation on the same plan.²⁸ The stratigraphic level where the jar archive was found has been dated to the 14th century BCE on the basis of the documents found there,²⁹ but the dates of the documents themselves are not self-evident. Instead, the latest among them can be dated based on parallels from elsewhere for objects found in the building.

Rouault asserts that the great building must have been destroyed violently and that the ceilings of the rooms collapsed, trapping all the objects underneath.³⁰ These objects include seals and sealings, along with a clay chariot and a female clay centaur that has no parallels outside Terqa.³¹ Whole vessels, a bone needle, and a gold earring were also found.³²

The ceramics from these final levels in Area E seem to date to the 14th and 13th centuries BCE. M.T. Pieri wrote in 2008 about button-based cups with a fine texture that were discovered in these levels at Terqa, noting that parallels for these cups come from later Mittanian levels at Tell Bderi, and Middle Assyrian levels at Tell Sheh Hamad and Tell Brak.³³ ROUAULT (2009: 136) noted that the 'based button jars' found in the building 'are found during several centuries without any visible change and resemble some aspects of the later Middle Assyrian ceramic'.

The room adjoining the one with the tablet jars housed a workshop for fashioning glass objects. The glass itself was not made there – no kiln was found. Instead, the archaeologists uncovered a large ingot of 5.1 kg of raw glass (one of the largest known in the Middle East from the second millennium BCE), along with a number of glass and vitreous objects, such as beads, pendants, and cups.³⁴ A small glass head found in the building has direct parallels with figurines found at Megiddo and at several other sites in Syria, Mesopotamia and elsewhere.³⁵ A comparative study of the heads by G. Dardeniz shows that the earliest example cannot be any earlier than the 14th century, and she proposes a date of 1350–1200 BCE for these types of figurines.³⁶

An adjoining room contained a bronze sword lying on the floor near a doorway. The sword has a straight section near the handle and a curved section towards the tip. It is known as a *harpe* or sickle sword.³⁷ Several of the seals of the kings of Hana feature the king holding a sword and, where the shape of the sword is clear, it is often in this *harpe* shape, certainly in the reigns of kings Išar-Lim and Ahuni, though not in the earlier reigns of Hanaya and Qiš-Addu.³⁸ Swords like this often signaled authority and were carried by both kings and gods. The ones with the gentle curve like the one found in Area E, rather than a more semicircular curve, seem to date no earlier than the late fifteenth century BCE. The Terqa sword is very similar in its manufacture, shape, and style to a sword in the Metropolitan Museum,³⁹ which has an

²⁷ SHIBATA/YAMADA 2021: 171 and SALLABERGER personal communication. Sallaberger plans to publish this tablet in future.

²⁸ Reports of the excavations in Area E are given in ROUAULT 1992b, ROUAULT 1993, ROUAULT 1993–1994, ROUAULT 1997, and ROUAULT 1998. The descriptions given here are from these publications.

²⁹ See e.g. TENU 2022: 137.

³⁰ ROUAULT 1998: 314.

³¹ ROUAULT 1997: 100.

³² ROUAULT 2007: 63.

³³ PIERI 2008: 284–285.

³⁴ MATOŖAN/BOUQUILLON 2007.

³⁵ MATOŖAN 2007.

³⁶ DARDENIZ 2018.

³⁷ MASETTI-ROUAULT/ROUAULT 1997: 181; ROUAULT/MASETTI-ROUAULT 1993: 340.

³⁸ PODANY 2014: 58, Fig. 1.

³⁹ Metropolitan Museum 11.166.1: <https://www.metmuseum.org/art/collection/search/322443>; MAXWELL-HYSLOP 2002: 210.

inscription dating it to the reign of the Assyrian king Adad-nirari I, who ruled from 1305 to 1274 BCE. This seems to be the closest parallel to the sword from Area E.

All of this indicates that the level in Area E where the tablet jars were found was contemporary with objects that were mostly made from the mid-14th century to the early 13th century BCE elsewhere. This would therefore also be the time of the latest kings named in the jar archive, (H)ammurapi(h) and Pagiru. Pagiru might have even been the son of (H)ammurapi(h). A sole witness to a contract from the reign of (H)ammurapi(h) was named Pagirum, and the first witness in extant Middle Hana Period texts was often a prince.⁴⁰

The latest level preserved in the large building in Area E, that is, the level in which the tablet jars were found, was therefore occupied at least 300 years after the latest level in Area C. Remarkably, though, the tablets found within the jars bridge that entire period, from the earliest document – the contract of Ammi-šaduqa (1646–1626 BCE), who would have been a contemporary of local kings Šunuhru-Ammu and Ammi-madar – to the latest contract of Pagiru, which written around 1350 BCE at the earliest, and, as I will argue below, possibly in the early 13th century BCE.

1.2.3 Documents from the Late Hana Period, not from Terqa

The three latest documents written in the Hana style were not recovered from excavations. They do not mention Hana, but all share many features in common with the earlier tablets and pertain to lands that had previously been within the Hana kingdom. These are from the Late Hana period. One of these is dated to a king named Ammirapi, who, as I will argue below, is probably a different man from (H)ammurapi(h) of Hana,⁴¹ and the other two are dated to eponyms of Middle Assyrian kings Tukulti-Ninurta I (1243–1207 BCE),⁴² and Aššur-reša-iši (1132–1115 BCE).⁴³ These last two clearly derive from a time when the former land of Hana was within the Middle Assyrian empire.

1.3 Characteristics of Hana tablets

The documents from Hana are potentially useful for our understanding of the wider chronology of the ancient Middle East and, in particular, for determining which chronology under discussion in this volume accurately represents the absolute dates for the second millennium BCE. Some reasons for focusing on the Hana documents for their chronological interest are as follows:

A. Although the corpus is limited in size, at least 77 of the Hana documents name a king who was ruling the region when the tablet was written. Most of these comprise contracts that are dated or mention the reigning king in the oath. We therefore currently know the names of 21 or 22 local kings and 4 kings who ruled the region from a distance, along with 4 kings who seem to have served as overlords to the local kings.

B. The tablets are distributed surprisingly evenly across the many centuries through which these 30 kings ruled, with between 1 and 9 tablets known from each reign.⁴⁴

C. Many of the Hana contracts are distinctive, employing legal clauses that were apparently used nowhere else, so that even documents without a clear provenance can be identified as coming from Hana if the scribes included these clauses. The most notable of these was the identification of property being transferred as *našbum ša lā baqrim u lā andurārim* ('the relevant property, legally established, is not subject to claims or release').⁴⁵ In addition, through many centuries, Hana legal documents often included a punishment to be inflicted on anyone who broke the contract: hot asphalt would be poured or smeared on that person's head.

D. It has become clear that scribal tradition in the region was highly conservative, with some of the same legal clauses appearing on real estate contracts for more than 500 years.⁴⁶ Although the title 'king of the land of Hana' is only known

⁴⁰ LH 13:28; PODANY 2002: 133–138; PODANY 2014: 64–65 and n. 110.

⁴¹ LH 15; PODANY 2002: 144–148, original publication: NOUGAYROL 1960.

⁴² LH 17; PODANY 2002: 151–153, original publication: KÜMMEL 1989.

⁴³ TSUKIMOTO 2003; TSUKIMOTO 2011 (the latter is a translation of the 2003 article from Japanese into German). For the eponym Aššur-kena-šallim, which appears in this text dating to the reign of Aššur-rēša-iši rather than Tiglath-Pileser I as had previously been thought, see JEFFERS 2013: 99–100.

⁴⁴ The reigns of 13 of the kings are each represented by only 1 document, 5 kings by 2 documents, and 3 kings by 3 documents. The reigns of only 7 kings produced 4 or more documents. These kings are not clustered in one period. The king whose name was spelled variously as Hammu-rapih or Ammu-rapi (and associated variants) was one of the latest of the Hana kings and is known from 7 documents, but the two kings each known from 9 documents, Yadih-abu and Kaštiliašu, ruled centuries before him, in the Late Old Babylonian period.

⁴⁵ YAMADA 2011a: 66.

⁴⁶ PODANY 2016.

to have been used during the Middle Hana period, I have used the term Hana for all three eras, for convenience. One hopes that at some point we will learn the name of the kingdom for the Late Old Babylonian period. Since its predecessor, under King Zimri-Lim of Mari was known as ‘Mari and the land of Hana’ it seems likely that the kingdom continued to be called Hana even in the Early Hana Period.⁴⁷

E. Although a gap of about 75 years exists between the reigns of Tukulti-Ninurta I (1243–1207 BCE) and Aššur-reša-iši I (1132–1115 BCE), from which no Hana-style documents are known, the other Hana tablets attest to the existence of between 22 and 24 reigns (depending on whether the father of Ahuni was a king himself or only father of a king, and whether Siniya proves to have been a king).⁴⁸ Therefore, some of the kings of Hana probably ruled during the otherwise almost silent 16th century BCE (MC), and could potentially provide support for one of the chronologies under discussion in this volume.⁴⁹

1.4 Scholarly disagreements about the sequence of Hana kings

Until about thirty years ago, scholarly consensus held that all the Hana tablets were written during the Late Old Babylonian period. In 1993 I published an article proposing that some of the tablets (particularly those mentioning ‘kings of the land of Hana’) were, instead, from the Middle Babylonian period.⁵⁰ I then elaborated on this chronological study in my 2002 book, *The Land of Hana: Kings, Chronology, and Scribal Tradition*. The sequence of kings, along with possible dates for those kings, that I proposed at that time were based on gradual changes that could be observed in many aspects of the tablets, both as physical objects, and with regard to their contents and the scribal practice of their writers.⁵¹ Many of these aspects also found parallels in datable tablets from other regions. Dominique CHARPIN elaborated on this sequence and provided additional evidence in a 2002 article.⁵²

In 2011, Shigeo Yamada published an article concerning an adoption contract from the reign of Ahuni, king of Hana, found at the site of Tell Taban.⁵³ In it, he proposed a sequence for the Hana kings using the same criteria I had established previously, but also adding newer information from tablets he had found at Tell Taban, and still-unpublished tablets from Area E at Terqa. These included gods named in the oaths on contracts, month names, tablet shape, and sealing practice. Based on these, he moved the ‘period of Mittanian domination’ to a time before that of the ‘kings of the land of Hana’.

In 2014 I published an article in which I agreed with YAMADA’s (2011a) revised sequence for the kings, based on more supporting evidence, including changes over time in the iconography of royal seals.⁵⁴ I still believe that Yamada’s sequence for the kings is essentially correct, and I am therefore using a modified version of his sequence here. In my 2014 article, though, I made a case for the Low Chronology possibly being better supported by the Hana tablets than the Middle Chronology, a view that I no longer support. With the increasing evidence for the accuracy of one of the Middle Chronologies, my sense now is that the documents from Hana do not stand in the way of the Middle Chronology and, indeed, support it. Note that Yamada changed his reconstructed sequence for the Hana kings in 2019, when he accepted Rouault’s assertion that Zimri-Lim was probably the son of Yadih-abu.⁵⁵ Therefore he now includes Zimri-Lim and Kasapan in the Late Old Babylonian / Early Hana period, whereas I continue to maintain (for reasons discussed below) that these kings ruled after the Babylonian interregnum at Terqa in the time of Ammi-šaduqa and Samsu-ditana.

⁴⁷ Although the name of the kingdom is not known for the Late Old Babylonian Early Hana Period, I will assume for now that it was already called Hana for the following reasons: 1. This region was known as ‘Mari and the land of Hana’ in the time of Zimri-Lim; it is unclear whether Mari still featured in the name of the kingdom, but it seems likely that Hana would have remained. 2. Yapah-Sumu-abu was called UGULA Hana in AIT 56 from Alalah before he (presumably) became king of Hana (PODANY 2002: 32–32); it is likely that the term Hana continued to be attached to his people and therefore to his kingdom. 3. When the dynasty apparently begun by a man named Iddin-Kakka took power, the kings seem to have consciously emulated the earlier kings who had ruled in the same region, using the old gods in their oaths, taking archaic royal names, and maintaining ancient legal practices. It makes sense that they would have given their kingdom (Hana) the same name that it had borne in those past times.

⁴⁸ For the argument that Siniya may have been a king, see PODANY 2014: 64–65.

⁴⁹ The growing evidence for the accuracy of the Venus tablets suggests that only the possible chronologies that are drawn from them should be considered.

⁵⁰ PODANY 1991–1993.

⁵¹ The sequence of kings is found in PODANY 2002: Fig. 1, 10–11.

⁵² CHARPIN 2002.

⁵³ YAMADA 2011a: see Table 1, 76.

⁵⁴ PODANY 2014: see Table 1, 54–55.

⁵⁵ YAMADA 2019: 1119.

In order to try to anchor the Hana material in time, debated synchronisms need careful examination, i.e.:

- Was Idi-abu/Yadi-abu, the father of King Zimri-Lim, the same man as the Yadih-abu from Area C, or did he (like his son) share a name with an earlier king? (There is no doubt that Zimri-Lim of Mari was an earlier king than the Zimri-Lim of Hana known from the tablets at Terqa.)
- Was Kaštiliašu, known from Early Hana Period documents, the same king as any of the known Kassite kings named Kaštiliašu?
- Was the period when the Babylonian kings Ammi-šaduqa and Samsu-ditana controlled Terqa (as attested by documents from their reigns found at the site, which are said to be clearly Babylonian) earlier or later than the reigns of local kings Zimri-Lim, Kasapan, Kuwari, and Hanaya (ruling with Yausa)?
- Were the individuals with Indo-Aryan names (Sausadatra, Sa'itarna, and Paratarna) in fact Mittanian kings? If so, can we be sure which Mittanian kings their names refer to, or were they previously unknown? They all seem to have served as overlords to the local king Qiš-Addu.
- Was the king Ammirapi, in the first year of whose reign land sale LH 15 was written and whose title is unknown, the same king as King (H)ammurapi(h) of Hana?

<i>King</i>	<i>Number</i>	<i>Source of tablets</i>
Yapah-Sumu-abu	1	Area C
Iši-Sumu-abu	2	Area C, Taban, Harradum
Yadih-abu	3	Area C, Taban
Kaštiliašu	4	Area C, Tell Sakka, other
Šunuhru-Ammu	5	Area C, other
Ammi-madar	6	Other
Ammi-šaduqa	7	Area E
Samsu-ditana	8	Area E
Idi-Abu, father of Zimri-Lim	9	Area E
Zimri-Lim, son of Idi-Abu	10	Area E
Kasapan	11	Area E
Kuwari	12	Area E
Hanaya/Yausa	13	Area E
Siniya?/Sausadatra	14	Area E
Qiš-Addu/Sausadatra	15	Area E
Qiš-Addu/Sa'itarna		Area E
Qiš-Addu/Paratarna		Area E
Iddin-Kakka	16	Area E
Išar-Lim	17	Area E, other
Iggid-Lim	18	Other
Isih-Dagan	19	Area E, other
father of Ahuni	20	Taban
Ahuni	21	Taban
(H)ammurapi(h)	22	Area E, other
Pagiru	23	Area E
Ammirapi	24	Other

Fig. 12.1. Sequence of the kings of Hana.

Not only are absolute dates unclear for most of the Hana kings, not everyone agrees with the sequence that is given here (Fig.12.1). A similar but not identical sequence was proposed by YAMADA (2011a: Table 1 on pp. 76). Olivier ROUAULT (2021: 153–154) maintains that kings Zimri-Lim, Kasapan, Kuwari, and Hanaya (ruling with Yausa) all ruled between the reigns of Yadih-abu and Kaštiliašu, rather than after the period of rule from Babylon represented by tablets dated to the reigns of Ammi-šaduqa and Samsu-ditana. He also believes that the texts in which Qiš-Addu is listed along with one of three kings (who might have been his overlords) were written almost at the end of the Hana era, right before

the last known king, Pagiru,⁵⁶ whereas Yamada and I believe that the Qiš-Addu documents were written considerably earlier, between the Early Hana (Late Old Babylonian) period and the time of the dynasty begun by Iddin-Kakka. In a 2019 article, Yamada changed his 2011 sequence of kings slightly, now placing the reigns of Zimri-Lim and Kasapan between those of Yadih-abu and Kaštiliašu.⁵⁷

2. Synchronisms

2.1 *Idi-abu = Yadih-abu?*

The Hana king named Zimri-Lim who ruled Terqa was certainly not the man of the same name who ruled Mari in the 18th century BCE; everything about him and the documents dating to his reign confirms this, as Rouault has convincingly shown.⁵⁸ Zimri-Lim's place among the sequence of Hana kings depends, however, on whether his father, whose name was written as both Idi-abu (*I-di-a-bu*)⁵⁹ and Yadi-abu (*Ia-di-a-bu* LUGAL),⁶⁰ was the same man as King Yadih-abu who was defeated by Samsu-iluna. Rouault has long asserted that the two were indeed the same. If correct, this would mean that some of the jar archive tablets were from within the time period of the Area C documents, even though none of the relevant kings is attested in Area C.

This Hana Zimri-Lim (no matter who his father was) must have been immediately succeeded by three other kings, because several tablets in the jar archive pertain to a single father and son who lived through all four reigns. This was a man named Sin-imagurrani, son of Ištar-išmešu. His documents were dated to Zimri-Lim, Kasapan (probable son of Zimri-Lim), and Kuwari. Sin-imagurrani's son lived in the combined reign of Hanaya and Yausa. The possible equation of Zimri-Lim's father with Yadih-abu seems to be the main reason why Rouault has always placed the kings Zimri-Lim, Kasapan, Kuwari, and Hanaya/Yausa in the Early Hana Period, between the reigns of Yadih-abu and Kaštiliašu.⁶¹

In 2017, Rouault published an article on Zimri-Lim of Terqa in the *Reallexikon der Assyriologie*, in which he provided additional evidence for his assertion that Idi-abu was the same man as Yadih-abu, and therefore that Zimri-Lim and his three successors ruled between kings Yadih-abu and Kaštiliašu.⁶² Rouault noted that a scribe named Sin-nadin-šumi wrote a tablet in the time of Zimri-Lim and that a scribe with the same name was active in the time of Yadih-abu, who is known from the archive of Puzurum.

Nonetheless, evidence of several generations of Terqa families in documents from Area C at Terqa allows no chronological space between Yadih-Abu and Kaštiliašu that would accommodate two additional generations and four additional kings, none of whom are attested in tablets from Area C. Zimri-Lim and his successors were more likely to have lived in a later period, after the time of Samsu-ditana of Babylon. Had these four kings ruled during the time that the community in Area C was thriving, one would expect at least a few documents dating to their reigns to have been found in the excavations there.

Particularly striking is the fact that Rouault mentions that oaths in some contracts from the reigns of kings Zimri-Lim, Kuwari, and Hanaya were sworn in the names not of the local gods Šamaš, Dagan, and Itur-Mer (as in the contracts from Area C), but of Marduk, Almuš, and others.⁶³ It seems highly unlikely that a local king would have introduced the Babylonian god Marduk into the oath formula. This is always an indication of Babylonian influence. Just as odd would be the quick reversion to the old gods during the reigns of Kaštiliašu and his successors.

As for the name of Zimri-Lim's father, Idi-abu/Yadi-abu, since Zimri-Lim of Terqa must have been named after Zimri-Lim of Mari, his father could just as well have been named after the earlier Yadih-Abu. Finally, Sin-nadin-šumi was not a rare personal name. A search on the Archibab database generated 124 Old Babylonian texts that include the name,

⁵⁶ See the third column of Table 1 in ROUAULT 2021: 153–154, in which the numbers represent his sequence for the kings. Kaštiliašu, though not represented among the jar archive texts, would be king number 8 in his sequence.

⁵⁷ YAMADA 2019: 1199.

⁵⁸ ROUAULT 2017.

⁵⁹ TQ 12-20: ROUAULT 2017: 299.

⁶⁰ TQ 12-11: ROUAULT 2017: 299.

⁶¹ See ROUAULT 2021: Table 1, 153–154, in which he continues to list Zimri-Lim as king number 4, Kasapan as 5, Kuwari as 6, and Yausa and Hanaya as 7. Kaštiliašu would then be 8, Šunuhru-Ammu 9, and Ammi-madar 10 (these kings do not appear in the jar archive, so they are not listed on the table). Ammi-šaduqa is listed on the chart as 11.

⁶² ROUAULT 2017. Note that YAMADA (2019: 1199) partially agrees with this, placing Zimri-Lim and Kasapan immediately after Yadih-abu, but not Kuwari or Hanaya.

⁶³ ROUAULT 2004: 55.

identified by a great many different patronyms.⁶⁴ Four of these men named Sin-nadin-šumi (from sites other than Mari and Terqa) were scribes.⁶⁵ Several scribes could well have had the same name across the many generations of the Hana texts. The scribe from the time of Zimri-Lim and the scribe from the time of the earlier Yadih-abu might well simply have shared a common name. (I should note that a similar assumption about the name Pagirum – that all references to that name in Hana texts were to the same man – caused earlier chronological mistakes. Some scholars believed that a Pagirum who lived in the time of (H)ammurapi(h),⁶⁶ now known to be one of the latest Hana kings, was the same man as Pagirum who lived in Terqa perhaps 300 years earlier, in the Late Old Babylonian period, and therefore mistakenly concluded that (H)ammurapi(h) of Hana must have been an Old Babylonian period king.)

The identification of Idi-Abu, father of Zimri-Lim, with Yadih-Abu known from Area C is therefore unproven. The evidence favors two separate kings with similar names, one of whom (Yadih-Abu) was succeeded by Kaštiliašu, and the other (Idi-Abu) was succeeded by his son Zimri-Lim, all of them probably being local kings of Hana who ruled from Terqa.

2.3 *Kaštiliašu at Terqa = any of the Kassite kings named Kaštiliašu?*

Kaštiliašu seems to have been a local king with an adopted foreign name, rather than having been identical with one of the Kassite kings of the same name.⁶⁷ In support of this conclusion, one notes that (a) his published year-names all suggest local activities near Terqa; (b) a tablet listing offerings that was found in a shrine in Area C at Terqa dates to Kaštiliašu, and lists ‘the king’ at the top; he presumably was present at the shrine in Terqa along with the other people named in the list; (c) a letter found at Tell Sakka, written from a leader named Kaštiliašu to yet another Zimri-Lim, is written in the style typical for Terqa in this period;⁶⁸ (d) a civil servant named Gimil-Ninkarrak, who resided in Terqa, was the recipient of an elaborate seal from King Kaštiliašu, which was impressed on a number of his documents; and (e) none of the other names from texts of Kaštiliašu’s reign are Kassite. It is also notable that, in the Tell Sakka letter and throughout the Hana corpus, the name of king Kaštiliašu was written differently from the spelling of the homonymous Kassite kings: the Hana king was *ka-aš-ti-li-ia-šu*, not *kaš-til-ia-šu*.⁶⁹ He therefore does not seem to have been a Kassite king ruling an early Kassite stronghold. He seems more likely to have been a local king whose name was borrowed from a prominent foreign leader. Later kings of Hana continued periodically to adopt the names of earlier prominent kings and dignitaries: other examples include Iggid-Lim, Išar-Lim, Iddin-Kakka, and (H)ammurapi(h).

2.4 *Ammi-šaduqa and Samsu-ditana: before or after Hana kings from Zimri-Lim to Hanaya?*

For reasons discussed below, I will argue that the documents excavated from Area C at Terqa were written during an era that is chronologically distinct from, but right before, the era of the documents from Area E, and that Ammi-šaduqa and Samsu-ditana of Babylon are the earliest kings attested in Area E. The region around Terqa must have been under external rule from Babylon for a period that began after the reign of the local kings found in Area C, which ended before the reigns of Zimri-Lim and subsequent local kings known from Area E, although Babylonian influence continued.

2.4.1 *Characteristics of texts from Area C at Terqa*

As discussed above, the Early Hana Period kings who are known from texts excavated in Area C at Terqa were Yapah-Sumu-abu, Iši-Sumu-abu, Yadih-abu, Kaštiliašu, and Šunuhru-Ammu.

The personal names and month names used in the Early Hana tablets are closely matched by those found in documents from Mari in the reign of the original Zimri-Lim of Mari.⁷⁰ This is to be expected since Yapah-Sumu-abu’s reign began only a short time after the end of the reign of Zimri-Lim, and the Early Hana kings ruled the same region.

⁶⁴ 113 of the tablets mentioned men whose name was written ^dEN.ZU-na-di-in-šu-mi, and 11 had the alternate spelling, ^d30-na-di-in-šu-mi.

⁶⁵ These scribes are found in AbB 7 153 (KRAUS 1977: 128–133), AJSL 33 RFH 40 (MEEK 1917: 244), and YOS 13 207 and 211 (FINKELSTEIN 1972: plate LXIX). Note also that one tablet from the reign of Ammi-ditana included two witnesses named Sin-nadin-šumi, each with a different patronym.

⁶⁶ LH 13, PODANY 2002: 133–138.

⁶⁷ PODANY 2019: 132–134. Note that, although these Kassite kings are usually referred to in the literature as Kaštiliaš, their name was routinely spelled with the last sign ŠU: BRINKMAN 1976: 186–187. This is in contrast to other Kassite kings whose names ended with signs ending in the sound ‘aš’. The kings’ name must have been pronounced Kaštiliašu, like the Hana king.

⁶⁸ This point was originally made by ABDALLAH/DURAND 2014: 236–237.

⁶⁹ PODANY 2016: 81–82, citing BRINKMAN 1976: 186–187 for the writing of the Kassite names.

⁷⁰ Personal names: PODANY 2002: 207–229; month names: SHIBATA/YAMADA 2021: 170–174.

The prosopographic evidence is supported by a diplomatic study of these contracts, which tells the same story. The earliest ones (TFR1 8 and TFR1 9) closely resemble texts from the period of the Mari kingdom (which is also attested in earlier levels at Terqa). They also have many of the same features as other contracts from around the time of Samsu-iluna at other sites, such as Alalah and Sippar. These include their ratios of proportions, rectangular shape, lack of margins, sealed envelopes and so on.⁷¹

Subsequent contracts gained left hand margins, and gradually seem to have dispensed with the need for envelopes. Their proportions became taller and more angular. At this point, the Hana contracts began to develop differently from those found in the rest of Mesopotamia. Whereas seals were impressed in the wide margins of Hana contracts from the time of Yadih-abu (that is, the time of Samsu-iluna in Babylon), in imitation of the envelopes, it wasn't until considerably later that this change took place in Mesopotamia. Also, seals were usually rolled from top to bottom of a tablet in Mesopotamia, whereas the Hana practice was to roll the seals around the left edge from front to back. The multiple seal impressions on each tablet were sometimes identified with by-scripts, which show that they belonged to civilians. Only one seal on a tablet from this era is identified as having belonged to a king.⁷² A hiring contract from the reign of Šunuhru-Ammu (LH 6) includes many clauses that were found in equivalent contracts no earlier than the reigns of Abi-ešuh and Ammi-šaduqa in the south.⁷³

One feature of scribal tradition that seems to have been uniform across Mesopotamia and Syria in the Old Babylonian period is that scribes almost never employed the personal name determinative in any context except when a man's name appeared right at the beginning of a line. The DIŠ sign was not really a determinative at all at that time, because most personal names were written without it. This was the case at Terqa in all the documents from Area C and in other tablets from the same reigns.

All these features confirm that the documents found in Area C at Terqa, dating to kings Yapah-Sumu-abu, Iši-Sumu-abu, Yadih-abu, Kaštiliašu, and Šunuhru-Ammu, are contemporary with kings Samsu-iluna, Abi-ešuh, and Ammi-šaduqa of Babylon. It is unlikely that any additional Hana kings ruled during this era (unless perhaps for a very brief time) because no generations are missing from the local families at Terqa whose names appear in the documents dating to these kings.

2.4.2. *Characteristics of texts from Area E at Terqa*

The second group of kings comprises those whose names appeared on the 31 documents in two jars found in Area E. As noted above, no overlap occurs between the two groups of texts – no king is attested in stratified levels in both Area C and Area E.⁷⁴

After the Ammi-šaduqa contract, the second earliest text from Area E seems to be a contract from the subsequent reign of Samsu-ditana of Babylon (TQ 12-18). It is said to have been drawn up in the style of previous Hana contracts (and was therefore a local creation), and to be from the beginning of his reign.⁷⁵

A few details have been published about other documents. For example, Kuwari named two of his years after battles against Hatti.⁷⁶ This might well have been soon after the reign of Samsu-ditana of Babylon, when the Hittites were in the area.⁷⁷ Hanaya would then have ruled after the end of the First Dynasty of Babylon. It is noteworthy that, whereas the Early Hana kings through Ammi-madar continued to use month names from the earlier kingdom of Mari, kings Ammi-šaduqa, Samsu-ditana, Zimri-Lim, Kuwari and Qiš-Addu used Babylonian month names. The later kings of Hana of the

⁷¹ PODANY 2002: 171–185.

⁷² The king's seal on Taban T06-4: YAMADA 2012: 595.

⁷³ PODANY 2002: 105.

⁷⁴ Rouault noted in 2004 that a letter found in Area C was addressed to Isih-Dagan, and he asserted at the time that this was probably the king of the same name, and that therefore Terqa was still probably the capital city of Hana during his reign: ROUAULT 2004: 52. If so, this would mean that a document from the time of Isih-Dagan, a member of the Iddin-Kakka dynasty, was stratigraphically associated with the kings of the Early Hana Period and there would be an overlap between the two groups of texts. However, when Rouault published a full edition of this text (TFR2 5–8: ROUAULT 2011: 14–15), he did not repeat this assertion. The letter was from a man named Hišni-Dagan to his brother, whose name was written Yassi-Dagan, and it appears in all ways to be an Early Hana text, contemporary with the documents found near it in the house of Puzurum (Area C stca 2) which are from the time of Kaštiliašu. Hišni-Dagan and Yassi-Dagan are both mentioned in one of the contracts from Puzurum's archive (TFR1 5): two witnesses are named Hišni-Dagan (lines 26 and 31), and the father of another witness was named Yassi-Dagan. There seems to be no reason therefore to suppose that the recipient of the letter was the later king who had a similar name.

⁷⁵ ROUAULT 2001: 11.

⁷⁶ ROUAULT 1992a: 252.

⁷⁷ FORLANINI 2009: 55–57.

Middle Period used distinctive local names for the months.⁷⁸ The Babylonian influence that began with domination of the region by Ammi-šaduqa seems to have continued until the end of the period of so-called ‘Mittanian’ influence, after the reign of Qiš-Addu.

The same phenomenon is seen in oaths. Only during the reigns of Samsu-ditana, Zimri-Lim, Kuwari, Hanaya, and Qiš-Addu was Marduk included in the oaths on any known contracts.⁷⁹ During this era, towards the beginning of the period covered by the jar archive, Babylon was powerful enough in the Hana region for Marduk to be worshiped and invoked when oaths were sworn.

Although most of the contracts from the jar archive are as yet unpublished, photographs of two of the texts were published in 1993, one from the reign of Kuwari (TQ 12-05, to judge from the number painted on the tablet),⁸⁰ the other from that of local leader Qiš-Addu and the Mittanian-sounding Sausadatra (TQ 12-06).⁸¹ Over time, the Hana tablets grew squatter and more pillow-shaped after the Old Babylonian period. The published plates show that these two tablets from the jar archive have intermediate dimensions and characteristics that bridge the gap between the tall, rectangular Old Babylonian/Early Hana contracts, and the squarer ones known from the Middle Hana period.

The number of seal impressions on Hana tablets also seems to have decreased over time. On Old Babylonian/Early Hana contracts, many different seals were impressed in the margins and under the script of each text, whereas known contracts from the kings of the dynasty founded by Iddin-Kakka bear just one seal, that of the king, and it was often rolled twice. Of the two tablets from the jar archive that have been published in photographs, the Kuwari text (TQ 12-05) has wide margins for seal impressions, but it is impossible to tell from the photograph how many impressions there are (or even if the tablet has any seal impressions at all). The Qiš-Addu/Sausadatra text (TQ 12-06) bears four seal impressions. Unlike in the Early Hana texts, they are not of civilians but of two gods, the probable king, and the crown prince-governor.⁸²

This evidence adds to the argument that the kings from Zimri-Lim to Hanaya ruled after the end of the Late Old Babylonian period (rather than between the reigns of Yadih-abu and Kaštiliašu, as discussed above), and therefore after the reigns of Ammi-šaduqa and Samsu-ditana.⁸³ This is also supported by the fact that the seal of King Hanaya⁸⁴ shares features in common with the royal seal of Qiš-Addu and later kings of Hana, making it unlikely that it was created in the Early Hana period.⁸⁵ Once the tablets from the jar archive are published, other features of the tablets will be available for analysis and the sequence of kings will become clearer.

2.5 Hana-attested kings Sausadatra, Sa’itarna, and Paratarna = Mittanian kings Sauštatar, Šuttarna, and Paratarna?

The possible synchronisms of some of the Terqa texts found in the jar archive with known kings of Mittani could shed some light more broadly on the chronology of the second millennium BCE, but these synchronisms have complications that are not yet possible to resolve.

2.5.1 Complications in equating Terqa kings with Mittanian kings

The first, and obvious, limitation in any analysis of this type is that the tablets naming these apparent Mittani kings, from the 12th season of excavations at Terqa, have not yet been published, so it is difficult to say anything definitive about them.⁸⁶

Second, the dates of the Mittanian kings to whose reigns these synchronisms would apply (Parattarna (I or II), Šuttarna (I or II), and Sauštatar) are poorly understood, since no Mittanian king list survives and there are few syn-

⁷⁸ SHIBATA/YAMADA 2021: 170–173. Note that the only exception to this pattern is that a document from the reign of Kasapan (TQ 12-11) was dated to the Mari month of Kinunum.

⁷⁹ ROUAULT 1992a: 253–254. Note, however, that Rouault did not believe that these kings ruled across a contiguous period.

⁸⁰ ROUAULT 1993–1994: 288.

⁸¹ ROUAULT/MASETTI-ROUAULT 1993: pl. 301, p. 338.

⁸² PODANY 2014: 61–65.

⁸³ I first made a case for this in PODANY 2002: 42–43.

⁸⁴ GUALANDI 1998: 134.

⁸⁵ PODANY 2014: 61–63.

⁸⁶ The publication of the TQ 12 texts is in progress; they will provide much more information for this question than we currently have available: ROUAULT private communication.

chronisms for the early kings of Mittani. Scholars vary widely in their determinations of absolute dates, but the sequence and general dates of Mittanian kings recently published by Eva von Dassow are persuasive.⁸⁷

If each of the Terqa kings with a Mittanian name is equivalent to the first king of Mittani with a similar name, these would be Sa'itarna = Šuttarna I, Paratarna = Parattarna I (both mid-15th century BCE), and Sausadatra = Sauštatar (late 15th century BCE).⁸⁸ However, the kings attested at Terqa apparently ruled in a different sequence than their Mittanian equivalents. It is clear from the published images of one text mentioning Sausadatra (TQ 12-06), that Qiš-Addu was described as the governor in his seal inscription.⁸⁹ The local king (perhaps Siniya) seems to have been Qiš-Addu's father.⁹⁰ In the other texts Qiš-Addu is said to have been the king. This means that this text naming Sausadatra must have been written before the others and therefore that Sausadatra ruled before Paratarna and Sa'itarna. It also means that all three were in power within Qiš-Addu's lifetime. This seems to rule out their having been identical to Šuttarna I, Parattarna I, and Sauštatar.

VON DASSOW (2022: Table 29.1, 471) suggests that Sausadatra could in fact have been equivalent to the Mittanian king Sauštatar but that Sa'itarna must be a previously unknown king who succeeded Sausadatra⁹¹ and that Paratarna could be the same man as Mittanian king Parattarna II (late 15th century BCE). This is possible, and would allow Qiš-Addu to have been an adult during all of their reigns, if the earliest Terqa texts were written towards the end of Sauštatar's reign and the latest was written at the beginning of Parattarna II's reign. The apparent problem of the synchronism does seem to be resolved in von Dassow's proposed chronology, but other evidence points to the Hana-attested kings possibly having ruled earlier than their Mittanian counterparts.

2.5.2 Evidence calling into question the assertion that Sausadatra was the same king as Sauštatar

So far, the only one of the co-called 'Mittanian period' tablets from Terqa available for study is one mentioning Sausadatra (TQ 12-06).⁹² Is there any supporting evidence from the published photographs of this tablet that this was indeed Sauštatar?

Some of the earliest documents from Syria, after the 'Dark Age' of the sixteenth century BCE, are those from Level IV at Alalah that can be ascribed to the reigns of the Mittanian kings Parattarna I and Sauštatar, along with a tablet from Tell Bazi also from the reign of Sauštatar.⁹³ If Sausadatra and Sauštatar were the same man, then TQ 12-06, which mentions Sausadatra, might be expected to resemble the early Mittani texts. In fact, the tablets' shapes, scripts, and sealing practices are quite distinct; they are not from the same scribal tradition.

One possible point of similarity is that a royal seal is found on all these texts. But this continued to be true in later contracts from Mittani and Hana that named the king, and also in other Syrian and Hittite contracts. It was not a purely 15th century or Mitannian phenomenon but a Late Bronze Age phenomenon, so this parallel cannot be used to find a date for TQ 12-06 and therefore for Sausadatra.

Was Sausadatra a king? His name appears three times in TQ 12-06 but never, apparently, with the designation LUGAL.⁹⁴ Although no images of the left or right edge of the tablet have been published, the sign LUGAL does not seem to appear at all in the photos of the obverse or reverse. Sausadatra's name is followed each time by 'and Qiš-Addu'. Qiš-Addu was a governor, according to his seal impression, perhaps of Terqa. He apparently later became the local king.⁹⁵

⁸⁷ VON DASSOW 2022: Table 29.1, 471.

⁸⁸ VON DASSOW 2022: Table 29.1, 471. Earlier dates for these kings are given by MAIDMAN 2010: xx and FREU 2008: 8. Note though that NOVAK 2007: 396 n. 42 suggests that Parattarna was a contemporary of Thutmose III, who was later than both kings, according to von Dassow.

⁸⁹ ROUAULT/MASETTI-ROUAULT 1993: Pl. 301, p. 338; GUALANDI 1998: 134.

⁹⁰ PODANY 2014: 62–65. Qiš-Addu's seal inscription notes that he had the title of governor (ENSÍ) rather than king (LUGAL) and was DUMU s[i?...] (perhaps 'son of S[iniya]'). The apparently royal seal impressed in the margin belonged to Siniya, according to its by-script. It had granulated caps and bore the standard scene of a king standing opposite a suppliant goddess.

⁹¹ VON DASSOW (2022: 473) notes that this was proposed by MLADJOV 2019, who showed that Sa'itarna could not be one of the known kings named Šuttarna.

⁹² ROUAULT/MASETTI-ROUAULT 1993: 338.

⁹³ Alalah: AIT 3 (Parattarna); AIT 13 (Sauštatar): NIEDORF 2008: 239–244; AIT 14 (Sauštatar): NIEDORF 2008: 245–247; Bazi: Bazi 1 (Sauštatar): SALLABERGER *et al.* 2006.

⁹⁴ ROUAULT/MASETTI-ROUAULT 1993: 338.

⁹⁵ ROUAULT 1992a: 254; Rouault did not mention this change from governor to king, just that Qiš-Addu, who was a local king, was also listed as a governor in one text. TQ 12-06 must be that one text, and the images of the text show that in this instance he was not the king.

In TQ 12-06, Sausadatra is named as an owner of property, alongside a list of gods (Šamaš, Addu, Marduk, Almuš and Annunitum) and Qiš-Addu. The absence of LUGAL after Sausadatra's name is striking in lines 8 and 9 (the end of the line is clearly visible in this case), because in other similar Hana texts where a list of gods and the king are named as the owners of property, the king's name is always followed by his title.

The same gods, along with Sausadatra and Qiš-Addu, are listed in the oath formula on the tablet, again with no title. It is typical of both the published and unpublished Hana texts that the same gods and kings who were listed as owning land that was being alienated were also the focus of the oath. In the second millennium BCE, oaths were generally sworn by gods of the local kingdom (if it was independent) or by those of the prevailing overlord (if it was a vassal state). In Hana, the fact that these same gods also seem to have been the 'owners' (along with the king) of land being granted suggests that that land was also perceived as belonging to the gods of the prevailing power. The presence of Marduk among the gods listed strongly suggests that Babylonia had a role in governing Hana at this time.

Even though he did not have the designation LUGAL in TQ 12-06, Sausadatra does seem to have held some sort of power (his name appears where one would expect a king's name to appear) and he might indeed have been the overlord of the local king. Rouault notes that, generally, the title 'king' did appear after the second name in these 'Mittanian' documents,⁹⁶ so these men with Mittanian-sounding names were all likely to have been kings.

Sausadatra did not seal TQ 12-06. It was sealed by the gods Addu and Marduk, the governor Qiš-Addu, and the man named Siniya who had a very royal-looking seal and who, as I have argued previously, may have been the local king at the time.⁹⁷

Although Sausadatra was probably a king, there is nothing in TQ 12-06 to prove that he was from Mittani. The word Mittani does not appear on the contract. Indo-Aryan names are known from several sites during the Late Bronze Age.⁹⁸ Sausadatra might even have been the ruler of a different Syrian state whose leaders bore Indo-Aryan names. Others are known, such as rulers of Kizzuwatna and smaller principalities in Syria.⁹⁹ Or he may have been an otherwise unknown king of the land that later became Mittani. Most of the evidence from the tablet suggests that Sausadatra was an earlier king than Sauštatar. And since he lived during the same generation as the men named Sa'itarna and Paratarna who are named in the Terqa texts, the same may be true for all of them.

In several ways, the features of TQ 12-06 link it more to Babylon than to Mittani.¹⁰⁰

- As YAMADA (2011a: 73) noted, TQ 12-06 is dated to the month Ulūlu (ITI.KIN-dINNIN) – a Babylonian month name not otherwise known at Terqa. Other documents in the same group of supposedly 'Mittanian' Hana tablets also date to the Babylonian months, namely Simānu (TQ 12-09) and Tašrītu (TQ 12-16).¹⁰¹
- The text is not only sealed by the god Marduk, but the same god is named in the oath, as noted above.
- The agreement includes three individuals who have Marduk as the theophoric element in their names. Prior to the reign of Ammi-madar there was no mention of Marduk in any known Hana texts. His marked presence here seems to suggest a significant Babylonian influence.
- The seal impressions of Qiš-Addu and Siniya are among the earliest to show the similarities to Kassite glyptic that have long been noted for the later kings of Hana.

TQ 12-06 is not alone among the Terqa texts in having Marduk in its oath formulation. Olivier Rouault has noted that Marduk was named in the oaths of many of the contracts from the jar archive in Area E.¹⁰² These probably reflect periods of strong Babylonian influence, for example when Samsu-ditana was apparently ruling Terqa directly, and during the reign of Zimri-Lim soon after that. The Babylonian influence is seen again in the reign of Kuwari, lasting right through to Qiš-Addu and his three Mittanian-sounding overlords, so TQ 12-06 is actually typical of many of the jar archive texts in this regard. As CHARPIN (2011: 48–51) has noted, the names of gods that appear in the oath formulas seem to reflect political realities that cannot be ignored.

⁹⁶ ROUAULT 1992a: 254. Rouault does not mention on which tablets the title of 'king' was found in the oath formula.

⁹⁷ PODANY 2014: 64–65.

⁹⁸ COTTICELLI-KURRAS/PISANIELLO 2023: 336.

⁹⁹ KOSYAN 2006: 1–2.

¹⁰⁰ These features are all visible in the plates in ROUAULT/MASETTI-ROUAULT 1993: 338.

¹⁰¹ SHIBATA/YAMADA 2021: 171.

¹⁰² ROUAULT 1992a: 251–253.

Other features of TQ 12-06, however, reflect a continuum of scribal tradition at Terqa. Many of the same typical Hana clauses appear, particularly the *našbum* clause that has always been the distinguishing feature of Hana texts, and the punishment whereby hot asphalt would be applied to the head of anyone breaking the contract.

Also, particular signs that changed over time appear in the form that would be expected for this era at Terqa, falling between the forms seen in the Old and Middle Babylonian documents. One such sign is AZ, seen most often in the word *našbum*. In the Old Babylonian (Early Hana) period it had no infixed ZA sign. TQ 12-06 is the earliest published example of AZ with the infixed ZA. The sign DUB changed as well, at the same rate. In the Early Hana period it had two vertical wedges; starting with TQ 12-06 it had three. The sign LUGAL changed at a different moment. Throughout the Early and Middle Hana periods it had no vertical lines dissecting the bottom horizontal, but these had been added by the time of (H)ammurapi(h). Although LUGAL is not found on TQ 12-06, it does appear on the Kuwari text, TQ 12-05, and exhibits the expected earlier form.

Therefore TQ 12-06 shows strong connections to other Hana texts and typologically fits between the Old Babylonian Early Hana texts and the later Middle Hana texts. It was therefore perhaps written during the so-called Dark Age of the 16th century BCE.

Is it possible to say, based on the evidence of this document, that Sausadatra was definitely the same man as Sauštatar? It seems not. His name is barely similar enough even to be considered a homonym. Sausadatra's name (*sa-ú-sa-da-at-ra*) was written in a different way from any previously attested spelling of Sauštatar (*sa-uš-ta-at-tar* on his seal impression),¹⁰³ though an alternate spelling, Saussatatar, (*sa-us-sa-ta-at-tar*), found in texts from Alalah, is slightly closer to the Terqa version.¹⁰⁴

Once the tablets from the 12th season at Terqa are published, they will no doubt provide a host of new clues about the identities of the mysterious men with Mittanian names. For now, though, it seems best to assume that none of them can be considered to be a known king of Mittani, even Paratarna. Based on currently available evidence, if it were not for their names, one might never propose that they were anything other than local rulers (ones, in fact, who seem to have been under the influence of Babylon rather than Mittani). Many examples of homonymy exist among Mesopotamian and Syrian kings, so caution is in order. Although one or more of them may still prove to have been a known king of Mittani, and the synchronism proposed by von Dassow may prove to be correct, for now this evidence is not strong enough, and it does not allow the assertion that Terqa was within the kingdom of Mittani.

2.6 (H)ammurapi(h), King of the Land of Hana = Ammirapi in LH 15?

Two published tablets and a stone duck weight were clearly produced during the reign of King (H)ammurapi(h) of Hana, who was one of the latest of the kings to hold this title. The 'h' sounds in his name seem to have been optional or unpronounced. The name was sometimes spelled with the h (*ha-am-mu-ra-pi-ih*, LH 13, year-name; *am-mi-ra-pi-ih*, LH 14, year-name; *ha-am-mu-ra-[pi²]*, LH 14, seal inscription), and sometimes without (*am-mu-ra-pi*, LH 16, votive weight). The inscriptions on his royal seal (which appears on both tablets) and on the votive duck weight call him 'king of the land of Hana' (LUGAL KUR *ha-na*). Four more tablets – the ones from the jar archive at Terqa – also were written during his reign. In one, he was Ammirapih, son of Ahuni (TQ 12-01), and in others, according to Rouault, his name was written Hammurapi (TQ 12 T4), Ammirapih (TQ 12-25), and 'Hammu/Ammi'rapih (TQ 12-47).¹⁰⁵ All these documents seem to derive from the reign of a single king.

LH 15 is different from these texts in many ways, although it, too, dates to a year-name of a king named Ammirapi.¹⁰⁶ It is small and square with no margins, unlike the pillow-shaped documents from the reign of (H)ammurapi(h). No seal of the king was impressed on it, and the name of Hana doesn't appear. The text begins part way down the obverse, as in documents written in Alalakh, Emar, and Ugarit in the Late Bronze Age, rather than at the top of the obverse, as in other Hana texts. The field transaction that occasioned the tablet being written was a sale by a private individual to two brothers rather than a grant from the king, in contrast to most of the Middle Hana texts. The contract does, though, include the characteristic Hana clause guaranteeing that the land is *na-aš-bu-um ša la ba-aq-ri ù la AMA.GI-ri*. Also, the month to which it was dated, Teritum, was in the Hana calendar.

¹⁰³ STEIN 1993: no. 711, p. 184.

¹⁰⁴ AIT 3 and AIT 14: *Sa-uš-sa-ta-at-tar*: KLENGEL 1978: 93.

¹⁰⁵ ROUAULT 2021: Table 1, 153–154.

¹⁰⁶ Louvre Museum AO 29701. PODANY 2002: 144–148, original publication: NOUOGAYROL 1960. See also plates in CHARPIN 2002: 80–81.

LH 15 (Ammirapi)	LH 17 (Tukulti-Ninurta I)
IGI.4.GÁL.LA KIRI ₆ A.GÀR URU <i>Qa-tu-na</i>	6 IKU A.ŠÀ <i>lu-ú ši-qu ù lu-ú</i> TU.ŠUM A.GÀR É <i>I-ba-la</i>
ÚS AN.TA ^m <i>I-ri-im-^dDa-gan</i>	ÚS AN.TA ^{id} <i>a-tap-pa maš-qi-ti ù ba-li-tum</i>
ÚS KI.TA ^m <i>Ib-bi-ia</i> DUMU DINGIR.ZAG-ti	ÚS KI.TA ^{id} <i>Hu-bur-GAL</i>
SAG.KI AN.TA ^m <i>Ia-qu-ub-DINGIR</i>	SAG AN.TA ^{id} <i>Hu-bur-ma</i>
SAG.KI KI.TA ^{id} <i>Hu-bur GAL</i>	SAG KI.TA ^m <i>Šil-lí-^dMe-er</i> DUMU ^m <i>Sa-na-ka[?]</i>
KIRI ₆ ^m <i>A-bi-ra-pi</i> DUMU <i>Gi-mil-^d30</i>	A.ŠÀ ^m <i>Bu-ni</i> DUMU ^m <i>Ta-^{hi}-ia</i>
KI <i>A-bi-ra-pi</i> EN-li KIRI ₆	KI-i ^m <i>Bu-ni</i> EN A.ŠÀ
^m <i>I-din-^dÁš-ku-ur ù ^mHa-al-mu-ú</i>	^m <i>Pi-ra-te</i> DUMU ^m <i>Ru-a-bi</i>
DUMU.MEŠ <i>Sa-ti-i</i>	^{md} 30-MU.SUM-na DUMU ^{md} <i>ISKUR-ú-ma-i</i>
ŠEŠ.MEŠ <i>la zi-zu-tum</i>	ŠEŠ.MEŠ NU <i>zi-zu-tum</i>
KIRI ₆ ŠU BA.AN.TI	A.ŠÀ
IN.ŠI.ŠÁM.E	IN.ŠI.ŠÁM
<i>a-na</i> ŠÁM.TIL.LA.BI.ŠÈ	<i>a-na</i> ŠÁM.TIL.LA.BI.ŠÈ
26 GÍN KÙ.BABBAR <i>šar-pa</i>	
	[Š]U.BA.AN.TI-ù
IN.NA.AN.SUM	1/2 MA.NA KÙ.BABBAR <i>šar-pa te-di-nu</i>
KIRI ₆ <i>na-aš-bu-um</i>	<i>tup-pu na-aš-bu</i>
<i>ša la ba-aq-ri ù la</i> AMA.GI-ri	<i>ša la-a ba-aq-ri ù l[a]-a</i> AMA.AR.GI
<i>ba-qir-rù i-ba-qa-rù</i>	<i>ba-qir i-ba-QIR</i>
<i>ša i-na be-ri-šu-nu</i>	
<i>ib'-bal-ki-it</i>	
MU DINGIR ù LUGAL <i>ú-ma-al-[l]a[?]</i>	MU DINGIR ù LUGAL <i>ú-ma-la</i>
10 MA.NA KÙ.BABBAR	10 MA.NA KÙ.BABBAR
<i>a-na</i> É.GA[L] Ì.LÁ.E	<i>a-na</i> É.GAL- <i>lim</i> Ì.LÁ.E
[8 witnesses]	[3 witnesses and scribe]
[date]	[date]

Fig. 12.2. Comparison of Hana-style land sale contracts LH 15 and LH 17.

No findspot is known for this tablet, but the field being sold, which abutted the ‘Great Habur’ (the Habur River),¹⁰⁷ was far to the north, up the Habur, in Qaṭuna (Old Babylonian Qaṭṭunan, Qaṭun in a Middle Assyrian itinerary from the time of Tukulti-Ninurta I, Qaṭni in the Neo-Assyrian period). In contrast, the texts from the Middle Hana king named (H)ammurapi(h) were largely found in the south of the Hana region, at sites on the Euphrates.¹⁰⁸

Whereas LH 15 has many differences from the documents from the reign of (H)ammurapi(h) of Hana, it is almost identical in formulation to LH 17, which was written during the reign of Tukulti-Ninurta I (see Fig. 12.2). Both pertain to land next to the ‘Great Habur’, and both plots were purchased by men described as ‘indivisible brothers’ (ŠEŠ.MEŠ *la zi-zu-tum*).¹⁰⁹ In both contracts the silver paid for the purchase was described as ‘refined’ (*šar-pa*). None of these features were found in earlier texts. The Hana-style contract from the reign of the later Middle Assyrian king Aššur-reša-iši records the purchase of a field in Qaṭuna, like LH 15, though this one lacks the phrases about ‘indivisible brothers’ and ‘refined silver’, and the field was not next to the ‘Great Habur’.

¹⁰⁷ RECULEAU 2010: 206.

¹⁰⁸ The tablets from the jar archive were found in Terqa, as was the duck weight (LH 16), though it was taken to the Louvre from Terqa in the 1920s, and LH 14 was found in Dura-Europos. The provenance of LH 13 is unknown, however, and no geographical names appear in it other than in the year-name: JOHNS 1907: 177.

¹⁰⁹ The buyers in LH 15 had the same father, but this expression was also used in LH 17 even though the men who bought the fields were not, in fact, brothers.

The year-name of LH 15 is that in which the king ‘ascended to the th[rone of the house of?] his [fath]er’. The many late features of this tablet argue for it having been written after the documents from the reign of (H)ammurapi(h) of Hana. This Ammirapi seems more likely to have been a later king, ruling in the northern part of the Habur region, where the Hana scribal tradition was still used for real estate contracts and months were still called by their Hana names, but which might not have been within the kingdom of Hana. Given the strong similarities to LH 17, he may well have ruled shortly before Tukulti-Ninurta I took control of the region. Variations on the name Hammurabi had been adopted by rulers in several kingdoms since the time of Hammurabi of Babylon. Ammurapi, the last king of Ugarit, ruled perhaps a century after this Ammirapi, who might have been named after the Middle Hana king (H)ammurapi(h). It is interesting to note that, even under Middle Assyrian kings, the scribes in the region around Qaṭuna still wrote up contracts for real estate using clauses that had been in use in the region for more than 600 years.

3. Hana kings across five centuries

In order to determine whether any of the Hana kings ruled during the missing 16th century of the Middle Chronology, it would be helpful to determine whether the list of Hana kings who are currently attested is close to complete.

As seen in Fig. 12.3, the dated Hana texts represent approximately 21 generations.¹¹⁰ Some of these are based on times when a son followed his father to the throne; others are determined based on family relationships of men named in the texts. For Area C, I have listed the families of men named Yašub-Dagan and Sin-nadin-šumi, but several other examples exist.

It is notoriously difficult to estimate chronology from generations, unless (as in the case of the Area C texts) several different families can be traced through the same era. A father-son generation within a family could be as short as 18 years or as long as 60. The average reign length for the kings of the First Dynasty of Babylon (who followed one another father-son) was 26.5 years. In Fig. 12.3 I have estimated approximate dates for kings using a more conservative average generation length of 25 years (except when exact dates are known for Old Babylonian and Middle Assyrian kings). In this chart known synchronisms are highlighted in grey. Of course, some reigns could have been much shorter than 25 years, especially in cases when a son did not take over the throne from his father, so it is entirely possible that several kings of Hana are still unknown. However, Fig. 12.3 shows that the sequence of known Hana kings is unlikely to be missing the 16th century BCE as is the case in so much of the ancient Middle East.

The phases of Hana history can be reconstructed, tentatively, as follows:

Soon after Hammurabi conquered Mari in 1761 BCE, Yapah-Sumu-abu took the throne in Terqa. Over the course of four generations, five more local kings ruled the Hana region (including Tell Taban in the Habur region), issuing their own year-names, but continuing to use the same names for the months that had been traditional at Mari.¹¹¹ These are the kings attested in documents from Area C. The people swore oaths in the names of the local gods Dagan and Itur-Mer.¹¹² Although Samsu-iluna claimed to have conquered Yadih-abu, there is, to date, no evidence at Terqa of direct rule from Babylon during his reign. This era lasted until perhaps 1650 or 1640 BCE.

At some point during the reign of Ammi-šaduqa of Babylon, Terqa was brought under direct control by the kings of Babylon, and for about 7 generations (perhaps 175 years) the people of Hana used Babylonian month names and swore oaths that often included the name of Marduk. This Babylonian influence was not interrupted by the end of the First Dynasty of Babylon; it continued into the reigns of local kings Zimri-Lim, Kasapan, and Kuwari (all of whom reigned within a single generation), along with Hanaya, Siniya, and Qiš-Addu. Based on generation length, these are likely to have been the kings who ruled in Terqa during the 16th century BCE. Although a Hana month name was used at one point during the reign of Kasapan,¹¹³ and a contract from the reign of Zimri-Lim included an oath in the names of the

¹¹⁰ I have allocated a generation to Idi-abu, because he is apparently listed as having had the title of ‘king’ (and to the father of Ahuni, since Ahuni’s seal inscription features the name of his father, which cannot be read), in the third line, which would be unlikely for a man who had not ruled as king. See YAMADA 2011a: 68 for Ahuni’s seal inscription. Siniya, the probable father of Qiš-Addu, also warrants a generation since Qiš-Addu’s seal inscription notes that his father was a king, and the seal impression of Siniya on tablet TQ 12-06 shows all the characteristics of a royal seal; PODANY 2014: 61–65.

¹¹¹ See YAMADA 2019 for additional details of this Early Hana period.

¹¹² Šamaš was usually included in the oaths as well, but Dagan and Itur-Mer were never omitted.

¹¹³ TQ 12-11, SHIBATA/YAMADA 2021: 170–173.

King	Genera- tion no.	Evidence for generations	Month names	Dagan or Marduk invoked in oaths	Possible dates, all BCE (MC)	
Kings in tablets from Terqa Area C + Ammi-madar						
Yapah-Sumu-abu	1	Yašub-Dagan	Sin-nadin-šumi	?	1740–1726	
Iši-Sumu-abu	2	sons of Yašub-Dagan		Mari	1725–1701	
Yadih-abu				Mari	Samsu-iluna year 28: 1723	
Kaštiliašu	3	son of Sin-nadin-šumi	Mari	Dagan	1700–1676	
Šunuhru-Ammu	4	grandson of Sin-nadin-šumi	Mari	Dagan	1675–1651	
Ammi-madar, son of Šunuhru-Ammu			Mari	Dagan		
Kings in tablets from Terqa Area E						
Ammi-šaduqa	5	Ammi-šaduqa	Babylon	?	1646–1626	
Samsu-ditana	6	Samsu-ditana	Babylon	Marduk	1625–1595	
Idi/Yadi-Abu (no known texts)					1625–1601	
Zimri-Lim	7	Sin-imguranni	Babylon	Dagan and Marduk	1600–1576	
Kasapan			Mari	?		
Kuwari			Babylon	Marduk		
Hanaya/Yausa	8	son of Sin-imguranni	?	Marduk	1575–1551	
Siniya? (no known texts)	9	Siniya? Qiš-Addu (son of Siniya?)			1550–1526	
Qiš-Addu/Sausadatra	10		Babylon	Marduk	1525–1501	
Qiš-Addu/Sa'itarna						
Qiš-Addu/Paratarna						
Iddin-Kakka	11	Iddin-Kakka	Hana	?	1500–1476	
Išar-Lim	12	son of Iddin-Kakka	Hana	Dagan	1475–1451	
Iggid-Lim	13	grandson of Iddin-Kakka	?	Dagan	1450–1426	
Isih-Dagan	14	great-grandson of Iddin-Kakka	Hana	Dagan	1425–1401	
father of Ahuni (no known texts)	15	father of Ahuni			1400–1376	
Ahuni	16		?	Dagan	1375–1351	
[H]ammurapi[h]	17		son of Ahuni	Hana	DINGIR ù LUGAL	1350–1326
son of [H]ammurapi[h]	18		grandson of Ahuni			1325–1301
Pagiru	19	Pagiru	?	?	1300–1276	
Kings of the Late Hana Period (tablets not from Terqa)						
Ammirapi	20		Hana	DINGIR ù LUGAL	1275–1250?	
Tukulti-Ninurta I	21		Babylon	DINGIR ù LUGAL	1243–1207	
Aššur-reša-iši			Babylon	DINGIR ù LUGAL	1132–1115	

Fig. 12.3. Calculations of generations and possible dates given a 25-year average generation. Synchronisms with Old Babylonian and Middle Assyrian kings are highlighted in grey.

traditional local gods,¹¹⁴ the more prevalent practices of the time, with regard to month names and the gods to whom oaths were sworn, seem to have been Babylonian.

The three kings with Mittanian-sounding names, who seem to have been successive overlords of Qiš-Addu, would be too early (if the calculated generations in Fig. 12.3 are even close to reality) to have been the same men as any of the known Mittanian kings. This chronological discrepancy might also account for the fact that the Hana documents seem to have no Mittanian features beyond the kings' names.

Qiš-Addu's seal inscription on TQ 12-06 includes the earliest published example of the title 'King of the land of Hana',¹¹⁵ though the title referred to his father, rather than to Qiš-Addu himself, who was still a governor when he used the seal. Four generations of the dynasty of Iddin-Kakka followed suit, using the same title (though it is unknown whether Iddin-Kakka was related to Qiš-Addu), but these kings broke with the Babylonian influences that had dominated the previous seven generations. The kings of Iddin-Kakka's dynasty returned to swearing oaths in the names of local kings, Šamaš, Dagan, and Itur-Mer, and they adopted a monthly calendar unrelated to the Babylonian one that had been in use previously. They also regularly impressed their seals on contracts, not only for royal land grants, but also for a will (LH 13),¹¹⁶ and two adoption contracts (LH 11 and T09-47).¹¹⁷ The royal seal appeared alone.

¹¹⁴ TQ 12-17, ROUAULT 1992a: 251.

¹¹⁵ GUALANDI 1998: 134; CHARPIN 2002: 78; PODANY 2014: 63–64.

¹¹⁶ LH 13: PODANY 2002: 133–138.

¹¹⁷ LH 11: PODANY/BECKMAN/COLBOW 1991–1993; PODANY 2002: 126–129, Tab T09-47; YAMADA 2011a.

These kings were followed by another sequence of four kings of Hana who also seem to have passed the throne from father to son. These were the father of Ahuni, whose name cannot be read, Ahuni, (H)ammurapi(h), and a son of (H)ammurapi(h) whose name is broken. It is not impossible that this son might have been the same man as the king named Pagiru who seems to have been the latest king known from Area E at Terqa. These eight or nine father-to-son generations, from Iddin-Kakka to Pagiru (with a possible break between Isih-Dagan and the father of Ahuni), might well have ruled for at least 200 years, from around 1500 to 1300 BCE. This last date coincides nicely with the archaeological evidence of the level in Area E where the jar archive was found.

Why and how the two jars came to contain documents from so many reigns reflecting such a long period of time – from Ammi-šaduqa of Babylon to Pagiru, who lived perhaps 350 years later – is impossible to determine at this point. The fact that many of the tablets in the jars recorded royal grants of land may provide a clue; perhaps they were kept together because of the need to prove land ownership over long periods of time. They presumably would have been consulted periodically, which also might help explain the remarkable conservatism in legal language seen in Hana real estate contracts: very old documents were still in circulation and could provide models for generations of scribes.

What exactly the relationship might have been, during these years, between Babylon and Hana is unknown. By 1350 BCE, around the time, perhaps, of (H)ammurapi(h) of Hana, the Great Kings of the region, including Tušratta of Mittani and Burna-buriaš II of Babylon, had forged close ties with Egypt, and Aššur-uballit I of Assyria was at the start of his long reign. Hana was located north of Babylonia, south of Mittani, and west of Assyria, and it is hard to believe that the region was fully independent of all its powerful neighbors.

One year-name of (H)ammurapi(h) notes that he ‘opened the Habur-ibal-bugaš canal between Dur-Išar-Lim and Dur-Iggid-Lim’.¹¹⁸ Dur-Iggid-Lim was later renamed Dur-Katlimmu by Middle Assyrian leaders, perhaps to disguise the original name, since it paid homage to Iggid-Lim who had been a non-Assyrian king.¹¹⁹ Dur-Katlimmu is at the modern site of Tell Sheikh Hamed, which has been excavated extensively. Although Hana levels have not been reached, the city must have been occupied during the 14th century and before.

The Late Hana Period of Ammirapi and the Middle Assyrian kings was a time when Hana scribal traditions continued in the northern Habur region, but Terqa seems not to have been occupied. Maria Grazia Masetti-Rouault has recently published an article reconstructing the history of the Lower Middle Euphrates region after the 14th century BCE.¹²⁰ She notes that some Middle Assyrian documents from Dur-Katlimmu refer to a ‘high’ (AN.TA) land of Hana that was under Assyrian control,¹²¹ though little occupation is to be found at this time in the ancient region of Hana along the Euphrates. She also observes that the ‘Land of Hana’ was not referred to in Assyrian texts after the reign of Tukulti-Ninurta I.¹²²

This takes us beyond the scope of this paper, though the memory of a ‘land of Hana’ lived on, perhaps even as a real toponym, into the 11th century, when a king of Hana named Tukulti-Mer dedicated an object inscribed with his name at the temple of Šamaš in Sippar.¹²³

4. Conclusion

The period covered by the Hana texts lasted at least 500 years. During that time, the court of every successive king must have produced thousands of documents (to say nothing of private letters, contracts and so forth that were written outside the royal courts). Of all those tens of thousands of tablets, a mere 77 dated ones have been found. We are remarkably lucky that those 77 include the names of so many different kings, along with year-names and other clues about the chronology and the history of the kingdom. But it is much easier to generalize from a small number of tablets than it would be if we had tens of thousands of them. At that point a vast population would be revealed in which dozens of different men had the same names, apparent trends in scribal tradition might be upended, and tablets that we now think of as typical might prove to have been anomalies. There is currently no way to know.

For now, though, it is clear that the name of Hana was associated for centuries with the region of Syria where the Habur and Euphrates Rivers come together, and that documents were produced there from the Old Babylonian to the

¹¹⁸ LH 14: 30–34, PODANY 2002: 135–136.

¹¹⁹ RECULEAU 2010: 207.

¹²⁰ MASETTI-ROUAULT 2022.

¹²¹ MASETTI-ROUAULT 2022: 39.

¹²² MASETTI-ROUAULT 2022: 43.

¹²³ BM 93077: PINCHES 1885.

Middle Assyrian period, including the 16th century BCE. In spite of the apparent lack of a chronological overlap between documents from Area C and Area E, the earliest documents in the jar archive in Area E seem to have been written right after the end of documentation from Area C, with little, if any gap between them. The sheer number of kings and, more importantly, of generations attested in Hana documents provides support for the Middle Chronology, though not yet in much detail. One hopes that future discoveries in the region will provide more evidence for the poorly understood era between the end of the First Dynasty of Babylon and the middle of the 15th century BCE.

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13. Towards the Lower Middle Chronology but not quite there yet

MICHAEL ROAF

This chapter investigates the chronology of the first half of the second millennium BC in Mesopotamia by examining the Venus Tablets of Ammi-šaduqa and their possible association with the so-called 1628 BC Event, 30-day months in the reigns of Ammi-ditana and Ammi-šaduqa, and the results of radiocarbon and tree-ring dating in Anatolia and the Northern Levant. The study of the Venus Tablets confirms that only the High, Middle and Low (Short) Chronologies are possible. The evidence of the 1628 BC Event supports the Lower Middle Chronology. The analysis of 30-day months is in favour of the High or the Low Chronology. The revised radiocarbon dating of palaces in Anatolia and the recent radiocarbon dating of the Middle Bronze Age destruction level at Zincirli are difficult to reconcile with the High and Low Chronologies. There is still doubt about the absolute chronology, but the bulk of the evidence favours the Middle Chronologies with the Lower Middle Chronology slightly in front.

1. Introduction

Numerous attempts have been made to elucidate the chronology of the first half of the second millennium BC (PRUZSINSZKY 2009). In recent years, a consensus has been building in favour of one of the two so-called Middle Chronologies. In 2012 I published an article in *Akkadica* entitled ‘The Fall of Babylon in 1499 NC or 1595 MC’¹ outlining my views of the then current situation regarding the absolute chronology of the first half of the second millennium BC in Mesopotamia.² In that article I discussed the radiocarbon and dendrochronological dating of palaces in Turkey and their relationship to the Assyrian Eponym List, The Venus Tablets of Ammi-šaduqa, various ‘historical omens’ mentioning eclipses, *Distanzangaben* in Assyrian royal inscriptions, the solar eclipse in the Mari Eponym Chronicle, and the lengths of the reigns of Ammi-šaduqa and Samsu-ditana.

Because the ‘historical omens’, the *Distanzangaben*, and the solar eclipse in the Mari Eponym Chronicle³ are disputed and give rise to numerous interpretations, I considered that they do not provide reliable evidence in favour of a

¹ In ROAF 2012a: 147 I adopted Pruzsinsky’s abbreviations NC for the New or Ultra-Low or Gasche Chronology and MC for the Middle Chronology. In this chapter instead of NC, I use the abbreviation NLC for the New Low Chronology as proposed by its promoters and defenders (ARMSTRONG/GASCHE 2014: 1). The other abbreviations used in this chapter for chronological schemes are HC for High or Long Chronology, LHC for Lower High Chronology, LMC for Lower Middle Chronology, LSC for Lower Short Chronology, SC for Short or Low Chronology, UHC for Upper High or Long Chronology, UMC for Upper Middle Chronology, and USC for Upper Short or Low Chronology.

² It was written in order to rebut an ill-considered and disappointing article by David WARBURTON (2011), in which he attempted to defend the untenable NLC (GASCHE *et al.* 1998). WARBURTON (2013) published a ‘rejoinder’ to my article in which he avoided confronting the arguments that I had presented and offered no sensible reasons in support of the NLC.

³ In a separate contribution to this volume (Chapter 5), I have investigated whether it is possible to reconstruct the Mari Eponym Chronicle (MEC) so that the ‘darkening of the Sun’ mentioned in it coincides with the most impressive total solar eclipse of the first quarter of the second millennium BC (24 June 1833 BC). Relying on the convincing study by Klaas VEENHOF Ch. 4 in this volume, I examined whether placing fragment S.24-3 parallel to Kültepe Eponym List G 57-60 (as suggested by KRYSZAT [2008: 214] and also hinted at by GÜNBATTI [2008: 119]) could lead to a chronology consistent with the Lower Middle Chronology (LMC). It transpires, however, that such a reconstruction is very problematic and, if the MEC refers to the 1833 BC eclipse, it has been assigned to the wrong eponym year being about 4 years too early. Since 1833 BC is closer to the date of the eclipse in the MEC according to the LMC than that according to the UMC (Upper Middle Chronology), this might be thought to support the former. See DE JONG Ch. 1 in this volume and NAHM Ch. 11 in this volume for possible alternatives.

DE JONG (2016–2017) provided arguments in favour of the LMC rather than the UMC based on the plausible but not certain reconstruction of the sequence of Old Assyrian intercalary months by Edward STRATFORD (2015) and on the various other premises including the assumptions that the MEC can be securely reconstructed and that the eclipse in the MEC can be securely identified with the 1845, 1838, or 1833 BC eclipse and concluded that the 1845 eclipse which is compatible with the UMC can be rejected and that the 1838 and 1833 eclipses are compatible with the LMC and concluded that the LMC is the correct chronology.

particular chronological system and excluded them from my investigations. Similarly, because scholars have had different degrees of confidence in the analyses of the Venus Tablets of Ammi-šaduqa, I was overly sceptical about the accuracy of the dates recorded on the Venus Tablets, many of which were certainly based on actual observations of the planet Venus,⁴ and I wrongly questioned their attribution to the reign of Ammi-šaduqa, which is confirmed by the sequence of intercalary months known from his reign (see below).

My principal aim was not to promote a particular view, but to demonstrate that, by choosing what evidence to believe and what to discount, arguments can be found that support or refute any chronological scheme. I thought, however, that the radiocarbon dating of the Kültepe and Acemhöyük palaces in Turkey was compatible with neither the High nor the Short Chronology and supported both the Upper Middle Chronology (UMC) and the Lower Middle Chronology (LMC).⁵ Because of the remaining uncertainties, I recommended that we should continue to cite dates according to the (Upper) Middle Chronology, labelling them as MC rather than BC, if it was thought desirable to indicate the remaining uncertainty.

Moreover, because the lengths of the reigns of the last two kings of the Old Babylonian Dynasty, Ammi-šaduqa and Samsu-ditana, are still uncertain, I concluded that it is not sensible to label the various different chronological schemes that have been proposed for the first half of the second millennium BC with the name of the ‘Fall of Babylon’⁶ assuming that this coincided with the end of the Old Babylonian Dynasty (ROAF 2012: 169–170).⁷ The only evidence in favour of the traditionally accepted lengths of the reigns of Ammi-šaduqa and Samsu-ditana, 21 and 31 years respectively, is King List 7.⁸ Since this King List gives manifestly false lengths for the reigns of the previous five rulers, lengths which are incompatible with the numbers of year-names attested on dated documents, it is likely that the lengths recorded for the last two reigns of the dynasty are also inaccurate (Fig. 13.1). The latest dated tablet of the reign of Ammi-šaduqa, of which I am aware, dates to the tenth month⁹ of his 19th year and the only list of year-names of Samsu-ditana contains only 26 names.¹⁰

king	number of year-names attested	accepted length of reign	length of reign in King List 7
Sîn-muballiṭ	20	20	30
Hammu-rabi	43	43	55
Samsu-iluna	38	38	35
Abi-ešuh	28	28	25
Ammi-ditana	37	37	25
Ammi-šaduqa	19	21	21
Samsu-ditana	26	31	31

Fig. 13.1. The dates and the lengths of the reigns of the last seven kings of the First Dynasty of Babylon.

Since the publication of my article, several new studies have led me to revise my views. I will outline some of them in the following pages and indicate how they modify my earlier conclusions.

⁴ This is the view of all those scholars who have studied the evidence seriously. See DE JONG 2012–2013 and 2013 and NAHM 2013 for convincing arguments in favour of accepting the reliability of some of the observations.

⁵ As discussed below, my faith in this analysis was not entirely justified, though the main conclusions based on the radiocarbon dating are still valid.

⁶ ‘The Fall of Babylon’ is a striking phrase that has often been used to characterise the end of the First Dynasty of Babylon, even though it is more frequently associated with the capture of Babylon by Cyrus in the sixth century BC. Different chronological proposals have often been identified with the putative date of ‘the Fall of Babylon’ and it is not surprising that scholars are reluctant to abandon this familiar, convenient, memorable, but imprecise nomenclature. I, myself, have been guilty of this practice citing ‘The Fall of Babylon’ in the title of my 2012 article, though the sin may be somewhat mitigated by the fact that it was repeated from the title of the article by David WARBURTON (2011) and my suggestion that an alternative and more accurate title would have been ‘The Fall of Babylon at some time between 1506 and 1499 NC or at some time between 1602 and 1595 MC’ (ROAF 2012: 169–170).

⁷ Furthermore, it is possible that the supposed incursion of the Hittites into Babylonia did not bring an end to the Old Babylonian Dynasty and that Samsu-ditana continued to rule Babylon after the Hittites had withdrawn.

⁸ Also known as Babylon King List B, King List 7 is only attested in a Neo-Babylonian copy (GRAYSON 1980–1983: 100).

⁹ Unless otherwise stated all months referred to in this chapter are lunar months. Previously uncertain year-names of Ammi-šaduqa, 17+a, 17+b, 17+c, 17+d, 17+e, and 17+f, can now be assigned to Ammi-šaduqa Years 17, 18, 1, 19, 5, and 15 or 18, respectively (PIENTKA 1998: 92–127; HORSNELL 1999: 86–91; RICHARDSON 2010: 65; SIGRIST/DAMEROW 2001).

¹⁰ For discussions of the end of the reign of Ammi-šaduqa and the beginning of the reign of Samsu-ditana see RICHARDSON 2005: 285 and 2010: 65–66 and VAN KOPPEN 2017.

For the possible year-names of Samsu-ditana see PIENTKA 1998: 122–144, HORSNELL 1999: 93–119, and SIGRIST/DAMEROW 2001. The tablet with the year-names of Samsu-ditana is a student’s copy (see FEIGIN/LANDSBERGER 1955). The website ARCHIBAB (<http://www.archibab.fr/>) follows HORSNELL 1999 and lists exactly 21 and 31 year-names for the reigns of Ammi-šaduqa and Samsu-ditana.

I now think that the most important sources, which might contribute to the establishment of a precise absolute chronological system, are:¹¹

1. The Venus Tablets of Ammi-šaduqa
2. 30-day month length analyses of the reign of Ammi-šaduqa and perhaps that of Ammi-ditana
3. Radiocarbon determinations and tree-ring dating

Other possible approaches in addition to those mentioned above, such as dates of harvests, synchronisms with Egypt and the Levant, pottery assemblages, seal styles, etc., may give information about the approximate dating, but are not helpful in determining an accurate and precise absolute chronology.

2. The Venus Tablets of Ammi-šaduqa and the motion of the planet Venus

The Venus Tablets have been central in discussions of the chronology of the first half of the second millennium BC for over a century (Fig. 13.2).¹² The text was incorporated into the series *Enūma Anu Enlil* and contains information about the conjunctions of the planet Venus in the reign of the penultimate Old Babylonian king Ammišaduqa. 21 fragments have been identified belonging to at least 9 separate tablets which were attributed to three manuscript traditions by Erica Reiner.¹³ The find spots of most of the fragments are uncertain, but many probably came from Nineveh and others from Babylon. One piece was excavated in Kish and dates to the reign of Sargon II at the end of the 8th century BC. None of the other pieces can be shown to be earlier than this fragment.

The principal edition of the text is REINER 1975. The text is organised in four sections (I–IV) each followed by a note indicating the contents of the preceding section.¹⁴ Most of the omens in Sections I (Omens 1–21) and III (Omens 34–37) are repeated in Section IV (Omens 38–60) but rearranged in month order.¹⁵ The omens in Section II (Omens 22–33) have a different form with fixed lengths for the periods of invisibility, 7 days at the inferior conjunction and 3 months at the superior conjunction and therefore are not actual observations.¹⁶ Thus, the potentially useful astronomical information is contained in Omens 1 to 22 and 34 to 37, sometimes corrected by data from Omens 38 to 60.

The omens in Sections I, III, and IV have a standard structure: the first part (the protasis) records the dates of the disappearance of Venus in the west or east and its re-appearance in the east or west as well as the length of the period of invisibility; the second part (the apodosis) contains the prediction as in Omen 9:

Omen 9 DIŠ ina ITI.ŠU UD.25.KAM ^dNin-si₄-an-na ina UTU.ŠÚ.A it-bal È UD.7.KAM ina AN-e uh-ha-ram-ma ina ITI.NE UD.2.KAM ^dNin-si₄-an-na ina UTU.È IGI.DU₈ ŠEG.MEŠ ina KUR GAL.MEŠ ub-bu-tú GAR.MEŠ
If on month IV day 25 Venus disappeared in the west, 3 days in the sky it stays away, and on month V day 2 Venus became visible in the east: there will be rains from the sky, there will be famine.

Only one of the omens in Sections I and III has a different form:

Omen 10 DIŠ ina ITI.ŠU UD.25.KAM ^dNin-si₄-an-na ina UTU.È it-bal MU GIŠ.DÚR.GAR KÙ.GI.GA.KAM
If in month XII 25th day Venus disappeared in the east: year of the golden throne.



Fig. 13.2. The reverse of one of the Venus Tablets (British Museum K. 160).

¹¹ See also the contributions of Peter Huber, Teije de Jong, and Werner Nahm (Ch. 1, 2 and 11) in this volume, who, to a greater or lesser extent, rely on these three sources and reach similar conclusions.

¹² For a summary and references to earlier studies of the Venus Tablet data see PRUZINSZKY 2009: 70–72.

¹³ For the fragments see REINER 1975: 11 Table 1a to which a fragment published by OSSENDRIJVER (2013: BM 32107) can be added. For the tablets and traditions see REINER 1975: 12 Table 1b.

¹⁴ Several of the fragments indicate that they are copies from tablets from Babylon (REINER 1975: 8).

¹⁵ Omens 38 to 59 in Section IV are rearrangements of the omens in Sections I and III with three omissions. Omen 60 repeats Omen 50, which itself is a correction of Omen 17 in which east and west were interchanged in the protasis.

¹⁶ HUBER (1982) and NAHM (2013) treat disappearances of 7 days and of 3 months elsewhere in the Venus Tablet with scepticism and omit them from their analyses.

Here only the date of the disappearance is recorded and, instead of the other observations and the prediction, there is a year-name, ‘year of the golden throne’, which was recognised by Father Francis X. KUGLER (1912: 280–281) as an abbreviation of the name given to the 8th year of Ammi-šaduqa, king of Babylon.

The dates on the Venus Tablets are recorded as the days of the lunar month, which started in the evening with the observation of the first crescent of the new moon and was either 29 or 30 days long. Since 12 lunar months are about 11 days less than a solar year, the Babylonian calendar was kept synchronised with the seasons by inserting an additional intercalary month about once in three years (STEELE 2011).

As the planet Venus moves round the sun, it is sometimes between the Sun and the Earth and sometimes on the far side of the Sun. So-called conjunctions occur when it is in line with the Sun as seen from the Earth, an inferior conjunction when Venus is closer to the Earth than the Sun and a superior conjunction when it is beyond the Sun. The time between similar conjunctions (the synodic period), that is the time when it returns to the same position relative to the Sun as seen from the Earth, is 583.92 days. Close to the conjunctions it is not possible to see Venus because it is hidden by the glare of the Sun. After an inferior conjunction Venus is first visible in the east as the Morning Star and then remains visible for 8 or 9 months until it disappears in the east and reappears in the west after the superior conjunction as the Evening Star. Because Venus is six times further from the Earth at a superior conjunction, it appears to be much smaller than at an inferior conjunction (Fig. 13.3) and because it appears to be moving more slowly than at an inferior conjunction it reappears after a much longer interval, about 2 months instead of about a week. Venus has phases like the Moon being full at the superior conjunction but because it appears to be much larger close to the inferior conjunction appears much larger and so, although only a crescent, after several days it is brighter and easier to see (Fig. 13.3).

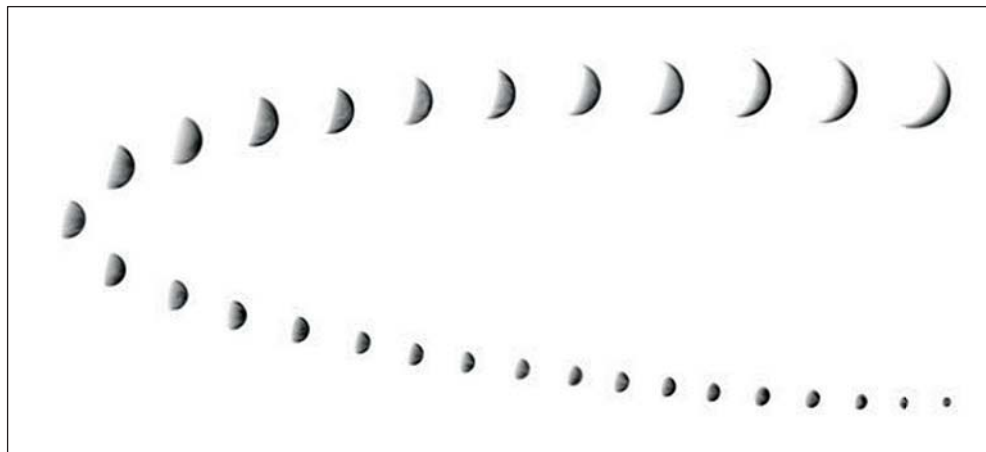


Fig. 13.3. The phases of Venus from close to the inferior conjunction (top right) to close to the superior conjunction (bottom right) with light and dark inverted (<https://skyandtelescope.org/astronomy-news/see-phases-venus/>).

Venus takes eight years to rotate five times round the Sun. Since for each rotation there are two periods of invisibility and two periods of visibility, five rotations correspond to 20 observations recorded in 10 omens on the Venus Tablets. The dates of these observations should be only about 2.4 days less than 8 years in the Julian calendar and about 4 days less than 99 lunar months (Fig. 13.4).

Lunar months 29.530587981 days		Venus cycles 583.921 days			Julian years 365.25 days			Gregorian years 365.242 days		
No.	length	No.	length	difference	No.	length	difference	No.	length	difference
99	2,923.528	5	2,919.605	− 3.923	8	2922	− 1.528	8	2,921.936	− 1.592
692	20,435.166	35	20,437.235	+ 2.069	56	20454	+ 18.834	56	20,453.552	+ 18.386
791	23,358.694	40	23,356.840	− 1.854	64	23376	+ 17.306	64	23,375.488	+ 16.794
1483	43,793.862	75	43,794.075	+ 0.213	120	43830	+ 36.138	120	43,829.040	+ 35.178

Fig. 13.4. Lengths in days of periods measured in lunar months, Venus cycles, and solar years. The differences measure the movement within the lunar month. A negative number means that the observation occurs earlier within the lunar month, a positive number later.

Because five Venus cycles are close to 8 years, the dates of the disappearances and appearances of Venus should repeat after 20 observations but should be about 4 days earlier in the lunar month. After 7 or 8 of such cycles (56 or 64 years) the change in the lunar date will have added up to about one lunar month (28 or 32 days compared with 29.53 days) and the phenomena should recur on approximately the same day of the lunar month with an error of 2 or 3 days. Such an error is comparable or less than the expected inaccuracies due to difficulties in the sighting of the first crescent of the new moon (to fix the beginning of the lunar month) and in the observation of the last and first appearances of Venus.

2.1. *The reliability of the observations on the Venus Tablet*¹⁷

None of the surviving Venus Tablets can be shown to be earlier than the Neo-Assyrian period and, if the text was based on actual observations made in the reign of Ammi-šaduqa, it would have been copied and perhaps edited on several occasions.¹⁸ This is indicated by the variants on different tablets, which are probably the result of the text being copied inaccurately. Some of the recorded dates are not compatible with the calculated dates for the visibility of Venus: some of these may be incorrect observations or dates on which Venus was not observed but the observer thought it should have been and others might be due to incorrect copying. Nevertheless most of the dates are close to what can be predicted using modern astronomical theory.

Observations 1–10				Observations 21–30				Differences				Number of intercalary months
No.	day	month	year	No.	day	month	year	month	day	day+4	± 29.5	
1	15	xi	1	21	11	xii	9	+1	–4	0	0	2
2	18	xi	1	22	15	xii	9	+1	–3	1	1	2
3	11	viii	2	23	10	viii	10	0	–1	3	3	3
4	19	x	2	24	16	x	10	0	–3	1	1	3
5	23	vi	3	25	26	vi	11	0	+3	7	7	3
6	13	vii	3	26	7	vi ₂	11	–1	–6	–2	–2	4
7	2	iv	4	27	9	i	12	–3	+7	11	11	6
8	3	vi	4	28	25	vi	12	0	+22	+26	–3.5	3
9	2	ii	5	29	5	ii	13	0	+3	7	7	3
10	18	ii	5	30	12	ii	13	0	–6	–2	–2	3

Fig. 13.5. The first two sequences of Venus observations on the Venus Tablets. Months are labelled with lower case Roman numbers. Intercalary months have a subscript 2. The right-hand column records the number of intercalary months between the observations in the first and second sequence. The months in Observations 7 and 21 have been corrected from vii to iv and from iii to xii. Observation 27 is anomalous and does not fit with the other observations. Data from HUBER 1982: 16–17 with correction by WALKER 1984: 66. A recently discovered fragment gives the date of Observation 4 as 18.x instead of 19.x (OSSENDRIJVER 2013).

When we look at the dates of observations made five Venus cycles apart (i.e. close to 8 years apart) (Fig. 13.5), we see that most of the observations took place in the same lunar month in both sequences. The first two observations, however, are a month later in the second sequence than in the first, meaning that instead of the expected average of three intercalary months in 99 lunar months there were only two intercalary months in the interval between the sequences. In the first 8 years only two intercalary months are attested on dated tablets, in Years 4 and 5 (4.xii₂ and 5.vi₂). The 23rd observation in Year 10 took place in the same month as the 3rd in Year 2, and a further intercalary month is attested in Ammi-šaduqa's tenth year (10.vi₂). The 6th observation was in the month after that of the corresponding 26th observation in the second sequence (month vii instead of month vi₂) showing that in the interval between the two observations there were four intercalary months, the fourth of which was recorded on the tablet as month vi₂ in Year 11.

The periods of invisibility at the conjunctions vary: when Venus is between the Earth and the Sun (inferior conjunction), it disappears for a period of between 2 and 20 days (average ca. 6.5 days) and, when it is on the far side of the

¹⁷ I am very grateful to Peter Huber and Teije de Jong for their lively correspondence and for their assistance and patience in explaining to me different aspects of the chronological problems.

¹⁸ For reconstructions of the redactional history of the text see REINER 1975: 7–12 and 21–25 or MEBERT 2010: 68–69.

Sun (superior conjunction), for a period between of two and three months (average ca. 69.5 days 2 months 10.5 days).¹⁹ The corresponding periods of visibility are between 8 and 9 months (average ca. 254 days = 8 months 18 days).

Fig. 13.6 gives estimated dates for the superior conjunctions (when Venus is furthest from the Earth on the far side of the Sun) assuming that the superior conjunction occurred close to the midpoint between the last observed sighting of Venus as the Morning Star and the first sighting of Venus as the Evening Star. The periods of disappearance between Observations 27 and 28 and between Observations 31 and 32 are 5 months 16 days and 1 month respectively instead of the expected 2 to 3 months. In these cases, therefore, no estimated date has been calculated.

Observation Nos.	last sighting of the Morning Star			first sighting of the Evening Star			Estimated date for the superior conjunction		
	day	month	year	day	month	year	day	month	year
3–4	10	viii	2	19	x	2	14/15	ix	2
7–8	1	iv	4	3	vi	4	2	v	4
11–12	24	ix	5	29	xi	5	26/27	x	5
15–16	20	v	7	2	viii	7	26	vi	7
19–20	25	xii	8	missing					
23–24	9	viii	10	16	x	10	12/13	ix	10
27–28	9	i	12	25	vi	12			
31–32	21	x	13	21	xi	13			
35–36	19	v	15	5	viii	15	27	vi	15
39–40	14	xii	16	25	iii	17	4/5	ii	17
43–44	missing			missing					
47–48	24	iii	20	24	vi	20	9	v	20
51–52	27	x	21	28	xii	21	27/28	xi	21

Fig. 13.6. Estimated dates of superior conjunctions (the midpoint between the last sighting of Venus as the Morning Star and the first sighting as the Evening Star) according to the Venus Tablets with the month of Observation 7 emended. Months are reckoned as 30 days long. The dates in the shaded entries are outside the normal range for the period of invisibility at a superior conjunction. In these cases and in those cases where dates are missing, no estimated date has been calculated.

Fig. 13.7 gives the intervals between the estimated dates for the superior conjunctions given in Fig. 13.6.

from estimated date			to estimated date			interval between conjunctions	period length per cycle	difference less than 19m 23d	attested intercalary months
day	month	year	day	month	year				
14/15	ix	2	2	v	4	19m 17.5d	19m 17.5d	5.5d	
2	v	4	26/27	x	5	17m 24.5d	17m 24.5d	2m – 1.5d	4.xii ₂ , 5.vi ₂
26/27	x	5	26	vi	7	19m 29.5d	19m 29.5d	– 6.5d	
26	vi	7			8		19m 23d		
		8	12/13	ix	10	38m 17.5d	18m 24.5d	1m – 1.5d	10.vi ₂
12/13	ix	10			12		18m 23.25d	1m – 0.25d	11.vi ₂
		12			13		19m 23d		
		13	27	vi	15	57m 9.5d	18m 23.25d	1m – 0.25d	13.xii ₂
27	vi	15	4/5	ii	17	19m 7.5d	19m 7.5d	15.5d	
4/5	ii	17			18		19m 11.5d	11.5d	17.xii ₂
		18	9	v	20	39m 4.5d	19m 23d		19.vi ₂
9	v	20	27/28	xi	21	18 m 18.5d	18 m 18.5d	1m 4.5d	

Fig. 13.7. Intervals between superior conjunctions using the estimated dates from Fig. 13.6. The expected interval per cycle (synodic period) is 19 lunar months 23 days (584 days). The intervals have been calculated assuming that each year consisted of 12 lunar months with no intercalary months and each month lasted 30 days. Where estimated dates are missing, the interval to the next estimated date has been calculated. The resulting value has been divided into two or three period lengths per cycle (in italics). These may include the synodic period.

¹⁹ The different periods of invisibility depend on the distances between the Earth, the Sun, and Venus and on the difference between the orbit of Venus and the ecliptic (the plane in which the Earth moves). The major factor is the height of Venus above the ecliptic and so the variation in the period of invisibility at the inferior conjunction is more than that at the superior conjunction. At present the height of Venus above the ecliptic is highest in March, which leads to shorter periods of invisibility from February to April. Because the seasons vary making a complete cycle in ca. 24000 years, in the Old Babylonian period the shortest periods of invisibility were probably reached between December and February (months viii to xi in the Babylonian calendar). I am grateful to Werner Nahm for this information.

The interval between the superior conjunctions in Years 2 and 4 is 19 lunar months and 17.5 days close to the expected value of 19 lunar months and 23 days. The interval between the superior conjunctions in Years 4 and 5 is 17 lunar months and 24.5 days, approximately two months less than the expected value.²⁰ Two intercalary months are required and these are Year 4 month xii₂ and Year 5 month vi₂ which are attested on dated tablets. Several of the intervals between the cycles are close to one or two months shorter than expected. These match the known attested intercalary months from the reign of Ammi-šaduqa.²¹ The intercalary month 17.xii₂ compensates for the sum of the two shorter intervals between Years 15 and 18, while the intercalary month 19.vi₂ appears earlier than might have been expected.

Observation Nos.	last sighting of the Evening Star			first sighting of the Morning Star			Estimated date for inferior conjunction			Period of invisibility
	day	month	year	day	month	year	day	month	year	in days
1–2	14	xi	1	18	xi	1	16	xi	1	3
5–6	22	vi	3	13	vii	3	2/3	vii	3	20
9–10	1	ii	5	18	ii	5	9/10	ii	5	16
13–14	27	viii	6	1	ix	6	29	viii	6	3
17–18	25	iv	8	2	v	8	28/29	iv	8	6
21–22	10	xii	9	15	xii	9	12/13	xii	9	4
25–26	25	vi	11	7	vi ₂	11	1	vi ₂	11	11
29–30	4	ii	13	12	ii	13				
33–34	9	vii	14	27	viii	14				
37–38	4	iv	16	20	iv	16	12	iv	16	15
41–42	9	xii	17	14	xii	17	11/12	xii	17	4
45–46	0	vi ₂	19	17	vi ₂	19	9/10	vi ₂	19	16
49–50	26	i	21	3	ii	21	29/30	i	21	6

Fig. 13.8. Estimated dates of inferior conjunctions (the midpoint between the last sighting of Venus as the Evening Star and the first sighting as the Morning Star) and the periods of invisibility according to the Venus Tablets with the date for Observation 21 emended. Months are reckoned as 30 days long. The intervals between the previous and succeeding dates in the shaded entries are outside the normal range for the synodic period of Venus. In these cases no estimated date has been calculated.

The same procedure can be carried out on the inferior conjunction when Venus lies between the Earth and the Sun (Fig. 13.8). The lengths of the periods of invisibility vary between 3 days and 20 days as expected at inferior conjunctions. Most of the shorter periods of invisibility lie between months viii and xii and the longer periods between months ii and vii.²²

As in Fig. 13.7 the pattern of intercalary months in Fig. 13.9 fits the known pattern of intercalary months from the reign of Ammi-šaduqa attested on economic and administrative documents: 4.xii₂, 5.vi₂, 10.vi₂, 11.vi₂, 13.xii₂, 17.xii₂, and 19.vi₂. Furthermore, no additional intercalary months are to be expected between 1.xi and 20.v, thus confirming that the data on the Venus Tablets was based on knowledge of the pattern of intercalary months in the reign of Ammi-šaduqa.²³ Thus the pattern of intercalary months confirms that the years correspond to the regnal years of Ammi-šaduqa, as had originally been proposed because of the abbreviated form of the year-name of the eighth year of Ammi-šaduqa appearing in *Omen 10* (Year 8). Thus the majority of the dates on the Venus Tablets could be records of actual observations, and these observations began in the first year of the reign of Ammi-šaduqa and continued for 21 years.

²⁰ It is easier to observe Venus immediately before it disappears than when it becomes visible again because it would be not far distant from where it was seen on the previous night. Therefore the precise date of the conjunction would be closer to the last sighting than to the first sighting. In this case, since we are interested in the interval between observations of the same phenomenon, this is not a major concern.

²¹ The interval between the superior conjunctions in Years 20 and 21 is shorter than the synodic period and an intercalary month is expected. This could have been at the end of Ammi-šaduqa's reign or more likely in the first or second year of the reign of Samsu-ditana. Fewer intercalary months from the reign of Samsu-ditana are attested than one might expect, only four in the 25 years of his reign. None of those so far attested occurs before the sixth year of his reign.

²² Corresponding roughly to the height of Venus above the ecliptic. See footnote 19.

²³ HUBER (2017: 58–59) calculated that the probability that five intercalary months chosen by chance (but excluding those that made the beginning of the year too far away from the spring equinox) were compatible with the Venus Tablet information for Years 2 to 16 was less than 1 in 1500. Including the two intercalary months in Years 17 and 19 reduces the probability to less than 1 in 30,000.

from estimated date			to estimated date			interval between conjunctions	period length per cycle	difference less than 19m 23d	attested intercalary months
day	month	year	day	month	year				
16	xi	1	2/3	vii	3	19m 16.5d	19m 16.5d	6.5d	
2/3	vii	3	9/10	ii	5	19m 7d	19m 7d	16d	4.xii ₂
9/10	ii	5	29	viii	6	18m 19.5d	18m 19.5d	1m 3.5d	5.vi ₂
29	viii	6	28/29	iv	8	19m 29.5d	19m 29.5d	– 6.5d	
28/29	iv	8	12/13	xii	9	19m 14d	19m 14d	9d	
12/13	xii	9	1	vi ₂	11	18m 18.5d	18m 18.5d	1m 4.5d	10.vi ₂
1	vi₂	11			13		<i>19m 23d</i>		11.vi₂
		13			14		<i>18m 25d</i>	1m – 2d	13.xii ₂
		14	12	iv	16	58m 11d	<i>19m 23d</i>		
12	iv	16	11/12	xii	17	19m 29.5d	19m 29.5d	– 6.5d	
11/12	xii	17	9/10	vi ₂	19	18m 28d	18m 28d	1m – 5d	17.xii ₂
9/10	vi₂	19	29/30	i	21	19m 20d	19m 20d	3d	19.vi₂

Fig. 13.9. Intervals between inferior conjunctions using the estimated dates from Fig. 13.8. The expected interval per cycle (synodic period) is 19 lunar months 23 days (584 days). The intervals have been calculated assuming that each year consisted of 12 lunar months with no intercalary months except 11.vi₂ and 19.vi₂, which are attested on the Venus Tablets and in bold in the figure, and each month lasted 30 days. Where estimated dates are missing, the interval to the next estimated date has been calculated. The resulting value has been divided into three period lengths per cycle (in italics): two of these are assumed to have been the average synodic period and the third is then calculated and turns out to be one month less than the synodic period. Intercalary months recorded on the Venus Tablets are in bold.

2.2. The Venus Tablets and absolute chronology

If the dates on the Venus Tablets were originally based on actual observations, it should be possible to compare them with calculated dates for the rising and settings of Venus as the Evening or Morning Star and to see which set of values fits them best. As discussed above, the risings and settings of Venus occur 3.9 days earlier in the lunar month after 99 lunar months (ca. 8 solar years) and after 1483 lunar months (75 Venus cycles or ca. 120 years) 0.2 days later. After 692 or 791 lunar months (approximately 56 or 64 years respectively), Venus rises or sets 2.1 days later or 1.9 days earlier in the lunar month (Fig. 13.4).

Analyses of the Venus Tablets have produced a large number of possible alternative solutions (see, for example, WEIR 1972, HUBER 1982, 2000, and 2011; GURZADYAN 1998, MEBERT 2010, DE JONG 2012–2013, 2013, LAPPIN 2012, NAHM 2013, VICKERY 2021). There are five main reasons for these variations: (1) different interpretations of the types of observation recorded on the Venus Tablets;²⁴ (2) different selections of which observations should be compared with the calculated dates (including on occasion emendations of the dates recorded on the Venus Tablets);²⁵ (3) different assumptions about the conditions under which Venus would have been observed;²⁶ (4) alternative methods of assessing the compatibility of the observations on the Venus Tablets with the calculated dates;²⁷ and (5) blunders.²⁸ It is not my

²⁴ MEBERT (2010) proposed that the date recorded for the ‘disappearance’ of Venus was the date of ‘last visibility’. Others have not followed him (HUBER 2011, DE JONG 2013).

²⁵ Some of the observations on the Venus Tablet are clearly not compatible with the motion of Venus leading to impossibly long or short periods of visibility or disappearance. Alternative suggestions for the exclusion or inclusion of certain dates (as well as different emendations of the dates) can lead to different absolute dates. VICKERY (2021) knowing that the daily movement of Venus at the inferior conjunction is much greater than at the superior conjunction and so the dates at the inferior conjunction would be more precise chose to use 22 observations at the inferior conjunction.

²⁶ WEIR (1972: 40–45) considered the possibility that the observations were not made in Babylon but at a site further north. HUBER (1982) assumed that the conditions under which Venus was visible to Old Babylonian observers were comparable to those pertaining to observations recorded in the Late Babylonian astronomical diaries and that the *arcus visionis* (the vertical angular distance between Venus and the Sun) determined the visibility of Venus. DE JONG and FOERTMEYER (2012–2013: 144) computed the visibility of Venus from its brightness in contrast to the brightness of the morning/evening twilight sky using the sensitivity of the human eye as well as the visual extinction due to absorption and scattering within the atmosphere. (see also DE JONG 2012). VICKERY (2021) chose different values of the *arcus visionis* and selected the one that he thought fitted the observations on the Venus Tablets best.

²⁷ HUBER (1982: 20–23, 43) compared the distributions of the observations on the Venus Tablets for different chronologies with those in the Late Babylonian Astronomical Diaries and concluded that the High Chronology was correct. DE JONG (2013: 369) stated that although Huber’s ‘sophisticated statistical methods’ ... ‘are mathematically sound and allow one to choose between different chronologies on the basis of computed probabilities, they are complicated and not very suitable to convince the Assyriological community’ and asserted that the best-fitting chronology

intention to adjudicate between these various scholarly approaches. Suffice it to say that there is no easy answer, but at the same time the difference between the various approaches (apart from those based on blunders) is not great, amounting to a couple of days, enough to tip the balance between the 56 or 64 year cycles but not to allow more than a handful of possible solutions. Solid arguments by HUBER (1982) and DE JONG (2013), combined with the widely accepted view that the accession of Ammi-šaduqa took place after 1740 BC and before 1540 BC,²⁹ limit the number of plausible chronologies to six (Fig. 13.10).³⁰

Chronology		Hammurabi	Ammi-šaduqa 1	‘Fall of Babylon’	
				‘traditional’	‘corrected’
HC	High or Long	1848–1806	–1701(1702)	1651	1658
LHC	Lower High	1840–1798	–1693(1694)	1643	1650
MC, UMC	(Upper) Middle	1792–1750	–1645(1646)	1595	1602
LMC	Lower Middle	1784–1742	–1637(1638)	1587	1594
SC	Low or Short	1728–1686	–1581(1582)	1531	1538
LSC	Lower Short	1720–1678	–1573(1574)	1523	1530

Fig. 13.10. Six possible chronological systems according to the Venus Tablets. Dates are BC except for the astronomical dates which are prefixed by a minus sign. The dates for the Fall of Babylon assume that it occurred at the end of the reign of Samsu-ditana and the ‘corrected’ date is calculated assuming that the lengths of the reigns of Ammi-šaduqa and Samsu-ditana were 19 and 26 years respectively (see Fig. 13.1).

Peter HUBER (1982, 2000, 2011) has been convinced for decades that the data from the Venus Tablets support the High Chronology, while Teije DE JONG (2012–2113: 153) asserted that the same data are not sufficient to decide between the various possibilities, although soon afterwards DE JONG (2013: 368) claimed that they supported the LMC, and Werner NAHM (2013) using a different method to evaluate the data opted for the Lower Middle Chronology. The degree of disagreement does not allow us to accept unconditionally any one of these six possibilities.

2.3. The Venus Tablets and the 1628 BC Event³¹

WEIR (1972: 30–31) and DE JONG/FOERTMEYER (2010) have suggested that the longer than usual period of invisibility of Venus recorded on the Venus Tablets for Year 12 might have been caused by dust in the atmosphere resulting from a

can already be recognized from a simple comparison of the average lunar calendar shifts and their standard deviations.’ He clearly meant the LMC with its mean of 0.3 and its standard deviation of 3.0 thereby contradicting the previous statement of FOERTMEYER/DE JONG (2010: 153) ‘analysis of the Venus observations does not allow us to choose between the four Venus chronologies.’

NAHM (2013) discussed possible procedures of the Old Babylonian observers under adverse weather conditions: either they could have made an educated guess as to when Venus would have been visible under optimal conditions (as was done in the Late Babylonian period) or they could have recorded the actual dates when Venus was first not observed or was first observed in this case on average the dates both for first and for last visibility would be later than the calculated dates. Based on Huber’s results (HUBER 1982: Table 4.3.b on p. 21) he asserted that the LMC was the only chronology that matched this method of recording.

VICKERY (2021: 16) compared the observed dates for the inferior conjunctions with his calculated dates using the method of Least Squares. In the current version online he used an out-of-date list of observations and reduced the differences between the observed and the calculated dates by two days, reaching the conclusion that the UMC was clearly the correct chronology. Further analysis by Bruce VICKERY (pers.com.), using the same inferior conjunction observations as HUBER 1982, concluded that applying the root mean square deviation the order was UMC, HC, SC and LMC and applying the mean absolute deviation SC, HC, UMC, and LMC. In each case there was not much difference between the first three but the LMC clearly came fourth. Comparing the means of pairs of observations either side of an inferior conjunction with the calculated dates of the conjunction gave the sequences HC, UMC, SC, LMC and HC=SC, UMC, LMC.

²⁸ For example, GURZADYAN (1998) in assuming that the dates in a lunar month calendar for the rising and setting of Venus repeated every 8 years clearly showed that he did not understand the nature of the phenomenon (see for example, NAHM 2013: 356–357). COLE (2014) did not mention Gurzadyan’s error.

²⁹ These limits are set by radiocarbon determinations: in fact, as discussed below, the High and the Short Chronologies are not compatible with the current interpretation of the radiocarbon determinations.

³⁰ Since both the Long and the Low Chronologies might be abbreviated to LC, I refer to them as the High and the Short Chronologies even though this might be thought inconsistent.

³¹ I am very grateful to Mike Baillie for information and for advice about the dendrochronological evidence. The views expressed here, however, are my own and do not necessarily reflect those of Prof. Baillie. I also thank Sturt Manning for his useful comments on an early draft version of this section of my chapter and David Brown of Queen’s University Belfast who provided relevant information. See also the contribution of Werner NAHM (Ch. 11 in this volume) for further discussion of the 1628 BC Event.

volcanic eruption. DE JONG/FOERTMEYER (2010) suggested the Santorini/Thera eruption³² and NAHM (2013) suggested that it could be identified with what has been called the 1628 BC Event (BAILLIE 1990: 165). The 1628 BC Event is now thought to have been caused by the eruption of the Aniakchak volcano in Alaska³³ and not the Santorini/Thera eruption.³⁴

Several different strands of evidence from ice-cores, tree-rings, and the Venus Tablets have been interpreted as the result of a volcanic eruption around the year 1628 BC. Volcanic eruptions are accompanied by local lava flows, volcanic ash fallout extending for as much as several hundred kilometres from the volcano, and dust particles and gasses entering the atmosphere that may persist over a complete hemisphere for months or even years.³⁵ There is general agreement that phenomena noted around the year 1628 BC were due to aerosols in the atmosphere. These might have had four main effects: first, a reduction of visibility, especially in the observation near the horizon of the rising and setting of stars and planets such as Venus, second, a reduction of temperatures due to dust blocking solar radiation, third, an increase of acidity in precipitation due to the presence of sulphates and carbon dioxide in the atmosphere, and fourth, an increase in the quantity of unusual particles derived from tephra falling to the ground or being absorbed when tree-rings were formed (PEARSON/MANNING *et al.* 2005, PEARSON/DALE *et al.* 2009).

The reduction in temperature may result in abnormally narrow tree-rings and at high altitudes in the formation of ‘frost-rings’ in bristlecone pines. Narrow rings and frost-rings are attested widely over the northern hemisphere in 1627/1628 BC (PEARSON/DALE *et al.* 2009: 1206).³⁶

DE JONG and FOERTMEYER (2010) and NAHM (2013) are not the first to have suggested that a longer than usual period of invisibility of Venus recorded on the Venus Tablets should be associated with a particular volcanic eruption. C. Leroy Ellenberger suggested to Mike Baillie that the period of invisibility of 9 months and 4 days in Year 9 of the reign of King Ammi-šaduqa might have been because of dust in the atmosphere resulting from a volcanic eruption (BAILLIE 1995: 86).³⁷ Some scholars may not have much confidence in a suggestion made by a once fervent follower of Immanuel Velikovsky, who later recanted and became an equally passionate opponent, but BAILLIE (1995: 86; 1999: 70–71) accepted the suggestion and identified this unusually long period of disappearance in Year 9 with the 1628 BC Event. This could be interpreted as providing support for the Lower Middle Chronology (LMC), but would contradict the interpretation of the long disappearance in Year 12 as being due to volcanic dust in the atmosphere.

According to the Venus Tablets (Fig. 13.11, Observations 21 and 22), Venus disappeared in the West on the 11th day of the 3rd lunar month of the 9th year of Ammi-šaduqa and re-appeared as the Morning Star on the 15th day of the 12th

³² Teije DE JONG (Ch. 1 in this volume) has withdrawn this suggestion and argues that the Santorini eruption is incompatible with the period of disappearance of Venus before the 9th day of the first month of Ammi-šaduqa Year 12 (corresponding to late April in the Gregorian calendar), since the insect remains show that the Santorini eruption occurred in early or mid-summer (perhaps June or July) (PANAGIOTAKOPULU *et al.* 2013, see also HÖFLMAYER 2021 for additional arguments). The same argument against the identification with the Santorini eruption was given by MCANENEY/BAILLIE (2019: 108). The Santorini eruption, however, is thought to have begun with an initial eruption that left a thin layer of ash that was not eroded by the winter rains (GOURNELOS *et al.* 2008: 247) and gave the inhabitants time to abandon their island settlement before the major eruption occurred. ŞAHOĞLU *et al.* (2022: 5–6) interpreted the evidence of an excavation on the Turkish Coast as the result of different tsunamis caused by eruptions on Santorini separated by an interval of days or weeks. They, however, date this event to after 1612 Cal BC, without specifying which calibration curve was used.

³³ PEARSON/SIGL *et al.* 2022 confirm that the 1628 BC Event was caused by the eruption of the Aniakchak volcano as previously suggested by MCANENEY/BAILLIE (2019: 108) based on the re-dating of the ice core sequence and the analysis of the composition of the corresponding ice layer.

³⁴ Much ink has been spilled attempting to establish the date of the Santorini/Thera eruption. Furthermore many articles and books have included mention of the Santorini/Thera eruption in their titles and have overemphasized its magnitude and global significance. PEARSON/SALZER *et al.* (2020a: 8410) suggested that the Santorini eruption may be identified with elemental markers in the Gordion tree-ring sequence dated to 1560 BC (GOR-76) and PEARSON/SIGL *et al.* 2022 conclude that the Thera eruption might have been responsible for a climate forcing event in 1611, in 1561/1558/1555, or in ca. 1538 BC.

³⁵ Volcanic eruptions may also cause tsunamis (ŞAHOĞLU *et al.* 2022).

³⁶ Although volcanoes are normally associated with narrow tree-rings, sometimes they may lead to wider tree-rings (KÖSE *et al.* 2013). Such an event was proposed for an exceptional growth ring (POR RY 854) in trees from Porsuk in south-west Turkey (KUNIHOLM *et al.* 1996: 781), and this appears to be confirmed by high levels of zinc and hafnium and increased quantities of sulphur, calcium, and some rare earth elements in subsequent rings (PEARSON/DALE *et al.* 2009). KUNIHOLM *et al.* (1996: 780–781) identified this exceptional growth with the 1628 BC Event and thereby proposed a precise date for the floating Anatolian tree-ring sequence and PEARSON/DALE *et al.* (2009) identified it with the Santorini eruption, but neither proposal is convincing. A revised dating of the Porsuk dendrochronological sequence dated this event to 1681–1673 BC (68.3%) (MANNING/GRIGGS/LORENTZEN/BARJAMOVIC *et al.* 2016: 1), but as discussed below this date is not reliable.

³⁷ I have been unable to find a reference to this suggestion in Ellenberger’s publications.

month. Since the normal period of invisibility at the inferior conjunction when Venus lies between the Earth and the Sun is between 2 and 20 days, Venus disappeared between 8 and 9 months earlier than expected.

The interpretation of the Venus Tablets at this point is complicated by the fact that the date of the preceding observation (Observation 20) is not recorded, and instead the text has an abbreviated form of the year-name for Ammišaduqa's 8th year. The observation before that is for the disappearance of Venus in the East on the 25th day of the 12th month of Year 8. This would be the superior conjunction when Venus is on average absent for a period of between two and three lunar months. So, in normal circumstances, the date when Venus re-appeared would have been in the first half of the 3rd month. If the date for Observation 21 on the Venus Tablets is correct, very soon after Venus had risen above the horizon and as Venus waxed, on the 11th day of the 3rd month the planet, suddenly disappeared and remained invisible until it re-emerged after the inferior conjunction on the 10th of the 8th month after 9 months – 5 days (= ca. 8 months 25 days), the normal length of time that Venus is normally visible as the Morning Star.

Omen	Obs.	Event	Babylonian date			Disappearance	Julian date		
			Day	Month	Year		Day	Month	Year
8	15	DE	21	v	7	3 months – 19 days (= 2 m 11 d)	11	9	1632 BC
	16	VW	2	viii	7		20	11	1632 BC
9	17	DW	25	iv	8	1 month – 23 days (= 7 d)	5	8	1631 BC
	18	VE	2	v	8		12	8	1631 BC
10	19	DE	25	xii	8	3 months – 14 days (= 2 m 16 d)	20	3	1630 BC
	20	VW	11	iii	9		12	6	1630 BC
11	21	DW	11	(iii) xii	9	(9 months +) 4 days	6	3	1629 BC
	22	VE	15	xii	9		10	3	1629 BC
12	23	DE	10	viii	10	2 months + 6 days	3	12	1629 BC
	24	VW	16	x	10		30	12	1629 BC
13	25	DW	26	vi	11	1 month – 18 days (= 12 d)	1	10	1628 BC
	26	VE	8	vi ₂	11		12	10	1628 BC
14	27	DE	9	i	12	5 months + 18 days	9	5	1627 BC
	28	VW	25	vi	12		20	10	1627 BC
15	29	DW	5	ii	13	7 days	24	5	1626 BC
	30	VE	12	ii	13		31	5	1626 BC

Fig. 13.11. Dates of disappearance and first visibility of Venus with proposed emendations (in bold) of the dates in Observations 20 (missing to 11.iii) and 21 (11.iii to 11.xii) with the equivalent Julian dates according to the Lower Middle Chronology.

Most commentators have assumed that the date for Observation 21 (11.iii) is an error: it could either be the date of Observation 20 giving a period of disappearance of 3 months – 14 days (= 2 months 16 days [approximately what is expected at a superior conjunction]) or the month in Observation 21 should be 'corrected' to the 12th month giving a disappearance of 4 days (approximately what is expected at an inferior conjunction in winter). It could be that both corrections are correct, because, if the day of the month were the same, it would have been easy for the scribe to have made a dittographic error in the month name (Fig. 13.11).³⁸ While the suggested restoration of the date of Observation 20 is very plausible, there is more uncertainty about how the date of Observation 21 should be emended.

The possibility that the extended disappearance of 9 months and 4 days in Year 9 could be due to dust in the atmosphere is not widely accepted, not only because the re-emergence of Venus after the inferior conjunction does not appear to have been delayed but also because the date of Observation 20 is easily explained as a scribal error when the text was copied.

If Observation 21 was correct and the early disappearance of Venus in Year 9 was due to dust in the atmosphere from a volcano, the dust must according to the Lower Middle Chronology have entered the atmosphere after 20 March 1630

³⁸ I am grateful to David Lappin for discussions of the Venus Tablets and for reminding me of his suggestion that 'the scribe of K.160, when he inserted the year-name ('year of the golden throne') between year 8 and year 9, seems to have transposed information on the superior conjunction to the inferior conjunction' (LAPPIN 2012: 17). Thus the result of the replacement of Observation 20 with the partial year-name and the consequent omission of the date for the first visibility in the West led to the date of Observation 20 being recorded as Observation 21.

BC according to the Julian calendar³⁹ when Observation 19 was made and before 12 June 1630 BC.⁴⁰ But this does not fit with the dating of the 1628 BC Event.

The frost damage to bristlecone pines in 1627 BC is reported by LAMARCHE/HIRSCHBOECK (1984: Table 3 on p. 125) where it is stated that over 50% of the trees examined showed frost damage.⁴¹ The exceptional temperatures that cause these frost-rings are notoriously unpredictable, but they are more likely to occur within one or two years after an eruption when the dust in the atmosphere is at a maximum and its cooling effect is also at a maximum (BAILLIE/MCANENEY 2014: 1802; 2015: 105, SIGL/WINSTRUP *et al.* 2015: 546–547).



Fig. 13.12. 'Ring width plots of four trees from Garry Bog Co, Antrim. Shaded areas indicate reduced growth in the 1620s BC. The mean curve is derived from nine trees which all show similar growth reduction.' After BAILLIE 1990: 162 fig. 1.

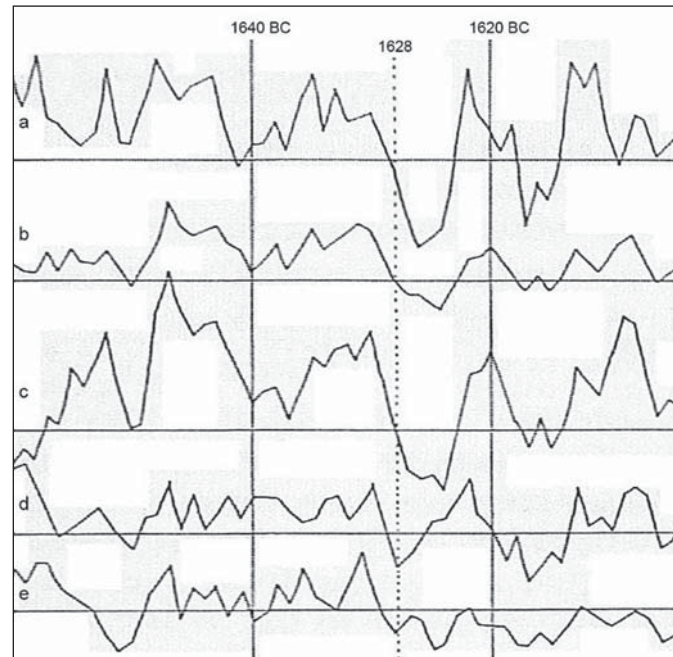


Fig. 13.13. Ring width plots for (a) Hasholme, England and (b–e) four sites in Germany. The vertical dotted line indicates the year 1628 BC. After BAILLIE 1990: 162 fig. 2.

The most detailed discussion of the European tree-ring data is still the paper by M. BAILLIE published in 1990. Here he presented ring width plots for four trees from Garry Bog in Northern Ireland (BAILLIE/MUNRO 1988: fig. 1, BAILLIE 1990: fig. 1, here Fig. 13.12) and for Hasholme in England and four sites in Germany (BAILLIE 1990: fig. 2, here Fig. 13.13). The Irish data show reduced ring-widths for 1627 BC when compared with 1628 BC, while the English and German data show reduced ring-widths for 1629 and 1628 BC when compared with 1630 and 1629 BC which could be interpreted as indicating that the event that caused the minimum ring-width in 1624 BC when six trees on four bogs indicate the narrowest growth rings (indicated by arrows in Fig. 13.12) had only started in 1628 BC.⁴² BAILLIE (1990: 165) summed up the European tree-ring data as follows:

So while the visible effects in the European trees suggest something happening to the trees around 1628 BC it has to be recognised that the actual date of the eruption could be either in that year or one or two years before.

³⁹ 6 March 1630 according to the Gregorian calendar which stays in line with the seasons. BC dates are conventionally given according to the Julian calendar. In the 17th century BC Julian dates are about 14 days earlier than Gregorian dates.

⁴⁰ If the observations in Year 9 or Year 12 were associated with the 1628 BC Event, they are only compatible with the LMC, since Years 9 and 12 correspond to 1638 and 1635 BC according to the Upper Middle Chronology and thus the UMC is too early. Similarly the Short Chronology is too late.

⁴¹ The date given in this article of 1626 BC must be corrected to 1627 BC as there is no year 0 BC or 0 AD (as pointed out by HUGHES 1988). Unfortunately no further details are given. According to LAMARCHE/HIRSCHBOECK (1984), the frost damage caused by exceptionally cold temperatures must have occurred during the growing season and thus roughly between April and October 1627 BC.

⁴² Baillie suggested that the reason why the minimum width rings occurred in 1624 BC, long after one might have expected the cooling effects of the atmospheric dust to have finished, was due to waterlogging resulting from the cooler and wetter conditions following the volcanic eruption.

GRUDD *et al.* (2000: 2958–2959) offer a similar interpretation noting that the growth rings for 1628 BC were already reduced in the European tree-ring sequences (Fig. 13.13). They wrote:

the European Oak tree-ring width chronologies [BAILLIE 1990] and the bristlecone pine frost-ring record [LAMARCHE/ HIRSCHBOECK 1984], are both absolutely dated, i.e. there is no uncertainty associated with the dates of individual rings. Therefore, we can be certain that oak trees in different parts of central Europe and the British Isles were affected by a period of stressful growth conditions starting in the year 1628 BC. We can also be certain⁴³ that pine trees at the timberline in the White Mountains of California were affected by severe cold in the year 1627 BC. These lines of evidence point to a climate cooling in the mid-latitudes of the Northern Hemisphere starting in the year 1628 BC.

If these events were caused by dust in the atmosphere that was reflected in the unemended dates of Observations 20 and 21 on the Venus Tablets (from 11 iii to 15 xii of Year 9 of Ammi-šaduqa, i.e. Julian date 12 June 1630 to 10 March 1629), one might expect that the growth rings for 1629 BC would have been noticeably narrower than normal, but, although the widths of the rings decrease from 1630 to 1628 BC, the absolute widths of the rings for 1630 and 1629 are not unusually narrow.

Nevertheless, Baillie thought that the dating of the 1628 BC Event could be consistent with the proposal that dust in the atmosphere had interfered with the observation of Venus in Year 9 of Ammi-šaduqa. For this to be correct, the eruption must have occurred before 11 iii Year 9 (= 12 June 1630 LMC) when Venus disappeared prematurely, that is three years before the Californian frost-rings and three years before the reduction in the width of the Northern Irish oak tree-rings (Fig. 13.12). In fact, the ring widths for three of the four trees shown in Fig. 13.12 increase in the years 1630 to 1628 and this is also true for the mean from 9 trees. Similarly in two of the three sequences from Southern Ireland (BAILLIE 1990: 164 fig. 3), the width of the ring for 1628 BC is greater than for 1629 BC, and in all three cases the 1627 ring is narrower than the 1628 ring. The sharp reductions in ring width occur after 1628 BC and, if this reduction was due to dust entering the atmosphere, this had not happened before the growing season of 1628 had already begun.⁴⁴

If we conclude that neither the evidence of the Venus Tablets nor the current tree-ring data offer significant support for Ellenberger's proposal that in Year 9 of Ammi-šaduqa there was dust in the atmosphere deriving from a volcanic eruption, we should consider the alternative possibility that the longer than normal period of invisibility in Year 12 was due to dust in the atmosphere deriving from a volcanic eruption.

According to the Venus Tablets, Venus disappeared on Day 9 of Month 1 in Year 12 and reappeared on Day 25 of Month 6, thus being invisible for 5 lunar months and 16 days instead of the expected 2 months and a few days. According to the LMC, Venus was visible from 13 October 1628 BC, disappeared from 10 May 1627 BC to 18 October 1627 BC and then stayed visible until 24 May 1626 BC (Obs. 29) about 13 days after it was too close to the Sun to have been seen when it disappeared for 7 days (Fig. 13.14).⁴⁵ The following observation (Obs. 31) of the last visibility of the Morning Star is 4 days late, while the first sighting of the Evening Star (Obs. 32) is 34 days too early: Observations 29, 31, and 32 are impossible.⁴⁶ The following early disappearance of the Evening Star (Obs. 33) might be explained by dust from the volcano still being present in the atmosphere.⁴⁷ From the end of 1625, the observations are within a few days of what is expected. If the previous unexpected dates for the visibility of Venus were caused by dust in the atmosphere, by the end of 1625, this had dissipated and the observation of Venus was not impeded.

As discussed above, the tree-ring data indicates serious effects in 1627 BC, but in the Northern Irish oak sequence there is no indication of abnormal conditions in 1628 BC. The picture is less clear for the other sequences from Southern Ireland, England and Germany, but because of the contradictory indications, it is likely that local conditions in 1628 BC

⁴³ The repetition of 'no uncertainty', 'we can be certain', and 'we can also be certain' might raise doubts in a sceptical reader's mind (see RAHTZ 1975).

⁴⁴ The interpretation of tree-rings is a specialist preserve and even amongst specialists there can be divergences of opinion. The differences in ring widths in different trees from the same locality in the same year (Fig. 13.12) indicate some of the difficulties: for example, the ring widths for years 1632 to 1629 form a peak in Tree 1 but a trough in Tree 2. More samples are needed before a definite conclusion can be reached.

⁴⁵ Since this is the standard period for disappearance at the inferior conjunction, this may be a calculated value, and too much reliance should not be put on this observation: see above footnote 16. It is possible that the original date of the disappearance recorded in Observation 29 was 5.i.Year 13 about 27 days earlier than normal, giving a period of disappearance of 1 month and 7 days and that when the text was copied the scribe realised that this was longer than expected for an inferior conjunction and changed the date to fit with a disappearance of 7 days.

⁴⁶ I see no easy way to explain these anomalous dates other than assuming that the observations were in error or that the text is corrupt.

⁴⁷ DE JONG/FOERTMEYER (2010: 152) wrote: 'because they require improbable or impossibly small or large values of the visual atmospheric extinction ... Observation nrs. 10, 17, 18, 27, 28, 29, 32 and 33 were discarded as corrupted for all four chronologies. These happen to be exactly those that were characterized as 'bad' by HUBER (1982).'

rather than a widespread intercontinental phenomenon were responsible. The evidence is most naturally interpreted as being the result of volcanic dust entering the atmosphere between October 1628 and May 1627 BC, thus affecting the growing season of 1627 BC and not that of 1628 BC.⁴⁸

Obs.	Phase	Day	Month	Year	Julian Date	Δ day
24	EF	16	x	10	29 Jan. 1628 BC	2
25	EL	25	vi	11	30 Sept. 1628 BC	7
26	MF	8	vi ₂	11	13 Oct. 1628 BC	-2
27	ML	8	i	12	9 May 1627 BC	-41
28	EF	25	vi	12	19 Oct. 1627 BC	48
29	EL	4	ii	13	24 May 1626 BC	13
30	MF	12	ii	13	1 June 1626 BC	7
31	ML	20	x	13	31 Jan. 1625 BC	4
32	EF	21	xi	13	1 Mar. 1625 BC	-34
33	EL	9	vii	14	9 Nov. 1625 BC	-40
34	MF	27	viii	14	28 Dec. 1625 BC	2
35	ML	19	v	15	12 Sept. 1624 BC	4
36	EF	5	viii	15	25 Nov. 1624 BC	3

Fig. 13.14. Observations 24 to 36 of Venus according to the LMC from 1628 to 1624 BC. E is the Evening Star, M the Morning Star, F the first visibility, L the last visibility, and Δ day the number of days after or before the expected date of visibility. Data from DE JONG/FOERTMEYER 2010: 147 Table 3, where further details can be found. They selected the date of Observation 27 as day 8 as in Omen 38 rather than day 9 as in Omen 14 (REINER 1975: 19). The details for Year 12 are in bold. In this year Venus disappeared about 41 days early and reappeared about 48 days late. The numbers of days before or after the expected date of visibility sometimes differ by a couple of days for the other chronologies, but the pattern is very similar.

Ammi-šaduqa 9	Ammi-šaduqa 12	Frost-rings	Closest tree-ring minima
HC 1694	HC 1691		1695, 1693
UMC 1638	UMC 1635	1653	1652, 1649–1648
LMC 1630	LMC 1627	1627	1626–1624, 1619–1617
SC 1574	SC 1571		1597

Fig. 13.15. Years 9 and 12 of the reign of Ammi-šaduqa according to the different chronologies and the dates of frost-rings and tree-ring minima arranged so that they are closest to the chronologies. Data from BAILLIE 2013 Table 7 on p. 73 with 1626–1624 instead of 1628–1626 as tree-ring minima (see footnote 48).

If the longer than usual disappearance of Venus in either Year 9 or in Year 12 was caused by dust in the atmosphere resulting from a volcanic eruption, we might expect such an eruption to have taken place in or before 1694 HC, 1638 UMC, 1630 LMC, or 1574 SC for Year 9 or three years later for Year 12 and to be represented in the tree-ring sequences either as frost-rings or as narrow rings (Fig. 13.15). Frost-rings are a reliable indicator of volcanic events, but they do not occur with most volcanic eruptions and are rather rare events. In the 17th century BC they occurred only in 1653 BC and 1627 BC: the next earlier one was in 1815 BC and the next afterwards in 1419 BC. The frost-ring in 1627 BC is compatible with the LMC. There are no suitable candidates in the tree-ring data for frost-rings or tree-ring minima compatible with Ammi-šaduqa Year 12 according to the Upper Middle⁴⁹ and Short Chronologies (Fig. 13.15). The 1695 BC and 1693 BC tree-ring minima might perhaps be compatible with the High Chronology and with Venus being obscured

⁴⁸ BAILLIE (2013: Table 1 on p. 66, Table 2 on p. 68, and Table 7 on p. 73) listed 1628 as a tree-ring minimum as well as 1626 but not 1624. This seems to be a misprint as the references given for the tree-ring minima for these tables are SALZER/HUGHES 2007, which in Table 2 on p. 63 listed only 1626 as a tree-ring minimum and BAILLIE/MUNRO 1988, which in fig. 1 on p. 344 has 1626 and 1624 as minima but not 1628. Attached to 1628 in Table 1 is an asterisk which according to the key indicates ‘tephras located and analysed in NGRIP at Belfast’: As this is not repeated in Table 2, I assume that this too is a mistake as NGRIP is an ice core sequence and in 2013 could not be exactly correlated with the tree-ring sequence.

⁴⁹ According to the Upper Middle Chronology, the 1628 BC Event might have affected the visibility of Venus in Ammi-šaduqa Years 18 and 19. Unfortunately, the two observations in Year 18 (43 and 44) are missing on the Venus Tablets and the following observations (45 and 46) at an inferior conjunction (1.vi₂.19 and 17.vi₂.19), although not compatible with astronomical theory, offer no evidence for dust in the atmosphere from a volcanic eruption.

through atmospheric dust in Year 9 (1694 HC) and the 1693 BC tree-ring minimum with Venus being obscured through atmospheric dust in Year 12 (1691 HC), although the dust in the atmosphere that might have obscured Venus in Year 12 is likely to have been even denser a few years earlier, since the tree-ring widths were already reduced by 1693 BC.

We might also expect that a volcano that released sufficient aerosols to obscure Venus would have left traces in the ice core records. At least the major volcanoes should be recognisable in the ice cores.⁵⁰ Unfortunately different ice core samples dated to the 16th and 17th centuries BC give different dates for volcanic events (indicated by increased acidity and presence of tephra). In general researchers have been over optimistic about the accuracy of the dates derived from ice cores. SALZER/HUGHES (2007: Table 2 on p. 62), for example, accepted the then proposed accuracy for volcanic events recorded in the ice cores of ± 5 years. Comparison with tree-ring data has shown that the error may be greater. BAILLIE/MCANENEY (2014: 1800) demonstrated ‘that in the 6th and 7th centuries of the current era, and presumably for all earlier dates, the key European ice chronologies from the North Greenland Ice Core Project, namely Dye3, GRIP, NGRIP and NEEM appear to have been wrongly dated by 7 years, with the ice dates being too old.’⁵¹ More recently MCANENEY/BAILLIE (2019: 102–103) have proposed a correlation between the 26 year interval between the 1653 and 1627 frost-rings and the same interval between maxima of sulphates in the Greenland ice core record⁵² and proposed that in the 17th century BC the GICC05 dates are 14 years too old and the GISP2 dates are 42 years too old.⁵³ They also observed that the composition of the tephra in the ice cores that they associated with the 1627 BC frost-ring is characteristic of the Aniakchak volcano in Alaska and could not have been derived from the Santorini (Thera) eruption.⁵⁴

The available tree-ring evidence indicates that a volcanic eruption took place after the growing season of 1628 and before the growing season of 1627 which is therefore consistent with the longer than normal invisibility of Venus in Ammi-šaduqa Year 12. Nevertheless before accepting this conclusion unconditionally, a more detailed examination of a larger sample of trees is required.⁵⁵

3. 30-day month length analyses of the reign of Ammi-šaduqa and Ammi-ditana

In my 2012 article I completely ignored the method of dating by month lengths: partly because different scholars reached different conclusions⁵⁶ and partly because I did not understand the method. In the following four years I wrestled with this method: Peter Huber, Teije de Jong, and Jane Galbraith have kindly and patiently helped me to understand the method and Frans van Koppen has provided me with information about additional 30-day months in the reigns of Ammi-ditana and Ammi-šaduqa. Most readers will find this section difficult to follow and will need to consult HUBER 1982 or 2017 for the details of the method.

The method was devised by Karl Schoch (FOTHERINGHAM 1928: 76) and elaborated by Peter HUBER (1982 and 2017). Huber established a master list of first crescents, dates when the first observation of the new moon could have been observed,⁵⁷ and counted the number of mismatches for different chronological systems between the attested 30-day

⁵⁰ ZIELINSKI *et al.* (1994: 948) claimed that the GISP2 sequence ‘provides a complete record’ of moderate high-latitude Northern Hemisphere and larger equatorial or Southern Hemisphere eruptions.

⁵¹ This re-dating has been confirmed by comparing rapid increases in Beryllium-10 in ice-cores and of Carbon-14 in tree-rings (SIGL *et al.* 2015: Fig. 1). The spikes in the production of Beryllium-10 and Carbon-14 are both thought to be the result of exceptional solar activity.

⁵² BAILLIE (2013) had already used this method to correlate the ice core sequences with the dendrochronological sequence.

⁵³ MCANENEY/BAILLIE (2019: 103) also noted that ‘ADOLPHI and MUSCHELER (2016) compared concentrations of the cosmogenic isotope Beryllium 10 in Greenland ice cores with the IntCal13 radiocarbon calibration curve. They found that, by the seventeenth century BC, the seven-year offset in GICC05 had grown to around 20 $\pm$ 5 years.’

⁵⁴ COLE-DAI *et al.* (2021: 10 Table 4) have identified traces of a volcanic eruption in layers in an ice core from the Antarctic dated to 1629 BC (± 11 years) by matching particular ¹⁰Be concentrations in the ice core with ¹⁴C concentrations in tree-rings. They suggest that these traces resulted from the same volcano that left traces in the Greenland cores in layers dated to 1627–1626 BC.

⁵⁵ As discussed above, the observations following the longer than usual disappearance of Venus in Year 12 (Observations 29, 31 and 32) are clearly in error, especially if there were longer periods of disappearance due to dust in the atmosphere. Given such inconsistencies, it would be imprudent to accept at face value the dates given in Observations 27 and 28 without considering the possibility that the dates of these observations too might have been erroneous.

⁵⁶ FOTHERINGHAM 1928, HUBER 1982, WEIR 1982, MITCHELL 1989–1990, MEBERT 2010, and LAPPIN 2012.

⁵⁷ DE JONG has also produced a list of first crescents: this is almost identical to that of Huber. I am grateful to both scholars for giving me access to these lists. A further list by ANDERLIĆ/FIRNEIS (2003) is available on the internet: 4° must be added to the elevations in this list to give a close approximation to the lists of Huber and De Jong. Antoine JACQUET (2021: 133–136) has argued strongly that the month in Mari in the reign of Zimri-Lim began on the day after the disappearance of the moon at the end of the lunar month. Clearly if this was the case for texts dated to the 30th day of the month, the analysis would require a list of last crescents. Similarly such a practice would invalidate the currently accepted chronologies based on the Venus Tablet.

months and those expected from the master list.⁵⁸ The number of mismatches gives a measure of the correctness or incorrectness of the different chronologies. Assuming similar procedures to determine when a new month began were used in the Old Babylonian period as those used in the Late Babylonian period, he devised statistical methods to evaluate which of the chronologies indicated by the Venus Tablets was more likely to be correct.⁵⁹ The proportion of mismatches for an incorrect chronology is 47% (on average 53% of lunar months have 30 days) and for a correct chronology is ca. 33% (the average of mismatches of 30-day months on Late Babylonian economic tablets). Given a large enough sample it should be possible to distinguish between these two alternatives.

The number of available 30-day months from the reign of Ammi-šaduqa has increased from 21 in 1982, to 27 in 2010, 32 in 2013, and 38 in 2014 (Fig. 13.16).⁶⁰

1982	2010	2013	2014	*2014
1.vii	1.vii	1.vii	1.vii	1.vii
1.viii	1.viii	1.viii	1.viii	1.viii
1.xii	1.xii	1.xii	1.xii	
	3.iv	3.iv	3.iv	3.iv
3.vi	3.vi	3.vi	3.vi	3.vi
4.xi	4.xi	4.xi	4.xi	4.xi
4.xii ₂	4.xii ₂	4.xii ₂	4.xii ₂	
		5.viii	5.viii	5.viii
5.xii	5.xii	5.xii	5.xii	
6.vi	6.vi	6.vi	6.vi	6.vi
7.xii	7.xii	7.xii	7.xii	
11.ii	11.ii	11.ii	11.ii	11.ii
		11.vii	11.vii	11.vii
			11.ix	11.ix
12.iv	12.iv	12.iv	12.iv	12.iv
		12.vii	12.vii	12.vii
12.viii	12.viii	12.viii	12.viii	12.viii
		13.i	13.i	13.i
13.ii	13.ii	13.ii	13.ii	13.ii

1982	2010	2013	2014	*2014
			13.vi	13.vi
			13.x	13.x
13.xii	13.xii	13.xii	13.xii	13.xii
		13.xii ₂	13.xii ₂	
	14.iv	14.iv	14.iv	14.iv
14.vi	14.vi	14.vi	14.vi	14.vi
14.viii	14.viii	14.viii	14.viii	14.viii
15.ii	15.ii	15.ii	15.ii	15.ii
15.x	15.x	15.x	15.x	15.x
15.xii	15.xii	15.xii	15.xii	
16.i	16.i	16.i	16.i	16.i
16.xi	16.xi	16.xi	16.xi	16.xi
16.xii	16.xii	16.xii	16.xii	
	17.xi	17.xi	17.xi	17.xi
	17.xii	17.xii	17.xii	
			18.iii	18.iii
	18.v	18.v	18.v	18.v
			18.x	18.x
			19.iii	19.iii

Fig. 13.16. 30-day months attested in the reign of Ammi-šaduqa known in 1982, 2010, 2013, and 2014 and 30-day months not at the end of the year known in 2014 (labelled *2014). 17.xii is not at the end of the year since there is an intercalary month 17.xii₂.

Only one month per year could be the last month of the year. Allowing for 7 intercalary months in 19 years, 8.1% (19 out of 235 months) would be last months of the year. When, however, we look at the distribution of the 38 30-day months over the year (Fig. 13.16), we find that 18.4% (7 out of 38) of the 30-day months are last months of the year⁶¹ more than twice the expected average. A similar pattern is found in the reigns of Ammi-ditana, Abi-ešuh, Samsu-iluna

⁵⁸ There are various possible reasons why an attested 30-day month did not match the calculated length of the month in the ‘master list of first crescents’: it could be that the new moon was not observed, perhaps because of poor visibility caused by the weather conditions, or a mistake in the previous month might have led to the new moon appearing on the 30th rather than the 29th day of the month, or the scribe might have made the choice to write 30 rather than 29 to save time (in cuneiform only 3 wedges are needed to write the number 30 while 11 wedges are needed to write the number 29), or the scribe might have chosen to extend the previous month rather than starting a new month (see below for discussion of the possible reasons for the final month of the year being given 30 days rather than starting the new year), or the scribe may have followed a systematic custom (an ideal Babylonian month contained 30 days and often calculations were based on this assumption [TANRET 2004]). In any particular case it is difficult to establish a reason for a mismatch.

⁵⁹ HUBER (1982: 36–39; 2017: 46–48; 2021: 5–7; 2023: 5–16) applied this method not only to the data from the reigns of Ammi-ditana and Ammi-šaduqa but also to those of Hammurabi, of Samsu-iluna, and of Rim-Sin and to those of kings of the Third Dynasty of Ur. Here I will discuss only the analysis of the month-lengths of Ammi-ditana and Ammi-šaduqa as the data for the other analyses are less well founded.

⁶⁰ The analyses carried out by FOTHERINGHAM 1928, HUBER 1982, WEIR 1982, MITCHELL 1989–1990, MEBERT 2010, and LAPPIN 2012 were based on fewer 30-day months, some of which appear to be incorrect, than are now available.

⁶¹ There were intercalary months xii in Aš 16 and Aš 17 and therefore Aš 16.xii and Aš 17.xii were not the last months of the year.

and Hammurabi, but this pattern is not found in the distribution of 30-day months found in Late Babylonian economic texts (Fig. 13.17).⁶²

	Months i-xi		Months xii and xii ₂		Total no. of months
	No.	%	No.	%	
Hammurabi, Samsu-iluna, and Abi-ešuh	71	74.0	25	26.0	96
Ammi-ditana	23	69.7	10	30.3	33
Ammi-šaduqa	29	76.3	9	23.7	38
Late Babylonian texts	131	87.1	18	12.9	149
All intercalary months last months	209	88.9	26	11.1	235
No intercalary months last months	216	91.9	19	8.1	235

Fig. 13.17. Distribution of 30-day months attested in Old Babylonian and in Late Babylonian economic and administrative texts. Also included are the numbers for when the expected seven intercalary months in 19 years occurred at the end of the year and for when none of them did. Data from Huber 1982 with additions for the reigns of Ammi-ditana and Ammi-šaduqa.

Attested 30-day months xii and xii₂ are more than twice as frequent in Old Babylonian times (44 of 167) as in Late Babylonian economic texts (26.3% as opposed to 12.9%). On average there are 7 intercalary months in 19 years making a total of 235 lunar months (8 lunisolar years). Assuming that half of these were month vi₂ and half month xii₂, 22.5 of the 235 months, i.e. 9.6%, would be either month xii or month xii₂. An underlying assumption of the method is that a similar procedure to that used in the Late Babylonian period was in use in the Old Babylonian period to determine whether a month had 29 or 30 days. Since this does not appear to be the case for the last month of the year,⁶³ in my analysis I have not used the 30-day months that occurred at the end of the year.

Accepting the assumptions and using the methods explained in Huber 1982 and Huber 2017, I analysed the incidence of 30-day months in the reigns of Ammi-ditana and Ammi-šaduqa. The results of my investigations confirm the results obtained by Peter HUBER (2017 and Ch. 2 in this volume) for the data set available in 2013 and by Teije DE JONG (Ch. 1 in this volume) for the data set available in 2014 and so here I will only present the results obtained by excluding the 30-day months that occurred at the end of the year.⁶⁴

The 11 additional 30-day months from 2010 and 2013 increased the numbers of mismatches for the HC and UMC by 6 and for the LMC and SC by only 3. The six new 30-day months from 2014 increased the number of mismatches for the HC by 4 and for the other chronologies by 3. The removal of the seven months at the end of the year reduced the number of mismatches for the HC by 1, for the LMC by 3 and for the UMC and SC by 4 (Fig. 13.18). For all chronologies, the numbers of mismatches are close to or more than the expected mean for a wrong chronology (Fig. 13.19).⁶⁵ The High Chronology is now as probable as the Lower Middle at 25%, while the most probable is the Short Chronology at 45% while the Upper Middle Chronology is unlikely at 4%.

⁶² Since the pattern of intercalation is only established for the reign of Ammi-šaduqa, it is not possible to estimate exactly which of the months xii in the reigns of other Old Babylonian kings were the last month of the year. I have therefore lumped together months xii and xii₂ in Fig. 17. The same procedure has been adopted for purposes of comparison for the Late Babylonian months in Fig. 17.

⁶³ I suspect that the reason for this difference may have been that year-names were employed in Old Babylonian dating formulae whereas regnal years were used in the Late Babylonian period. The prevalence of documents dating to the 30th day of the last month of the year may have been because the scribes were reluctant to use a new and previously unused year-name. Peter Huber (pers. comm.) and Jane Galbraith (pers. comm.) have suggested that all end-of-year months had 30 days as was sometime the custom in Mesopotamia for administrative, accounting, or ideological reasons (SALLABERGER 1993: 11, STEELE 2011: 473–474). In the absence of definite proof that some years ended on the 29th day of month xii it is difficult to disprove this proposal, but in my opinion the inclination of the scribes to continue to use the well-known year-name and to postpone introducing a novel and unfamiliar formula is a more plausible explanation.

⁶⁴ See HUBER 2017 and the contribution of Teije DE JONG Ch. 1 in this volume. I have not included the Lower High and the Lower Short Chronologies in this analysis so that the results are comparable with those presented by Huber and de Jong. A more thorough investigation would include these two chronologies as well as looking at other possibilities for intercalary months in the reign of Ammi-ditana.

⁶⁵ HUBER (2017: 29–31) showed that the distribution of mismatched 30-day months attested in Late Babylonian economic documents follows a binomial distribution. The mean is np and the standard deviation is $\sqrt{np(1-p)}$, where n is the number of observations and p is the probability of a mismatch (ca. 0.33 for a correct chronology and 0.47 for a wrong chronology).

HUBER (2017: 38–45) also examined the 30-day months in the last third of the reign of Ammi-ditana⁶⁶ and found that, if one of the four chronologies considered was correct, there was statistical evidence in favour of an additional intercalary month situated at the end of the reign of Ammi-ditana or at the beginning of the reign of Ammi-šaduqa. The revised data appears to support this proposal (Fig. 13.19).

	1982	2010	2013	2014	*2014
No. of months	21	27	32	38	31
HC	6	8	12	16	15
UMC	13	16	19	22	18
LMC	12	15	15	18	15
SC	12	13	15	18	14

Fig. 13.18. Nos. of mismatches of 30-day months in the reign of Ammi-šaduqa in analyses from 1982, 2010, 2013, and 2014 data and from 2014 data not including months at the end of the year (*2014). (After information from HUBER 1982, 2010, and 2017 supplemented by analyses by Michael Roaf August 2015.)

	Aš		Ad+0		Ad+0 + Aš		Ad+1		Ad+1 + Aš	
	Nos.	prob.	Nos.	prob.	Nos.	prob.	Nos.	prob.	Nos.	prob.
No. of months	31		18		49		18		49	
HC	15	0.25	10	0.10	25	0.16	4	0.82	19	0.75
UMC	18	0.04	7	0.61	25	0.16	10	0.02	28	0.004
LMC	15	0.25	10	0.10	25	0.16	11	0.01	26	0.01
SC	14	0.45	9	0.19	23	0.52	7	0.14	21	0.23
correct	10.23±2.62		5.40±1.99		16.17±3.29		5.40±1.99		16.17±3.29	
wrong	14.57±2.78		8.46±2.12		23.03±3.49		8.46±2.12		23.03±3.49	

Fig. 13.19. Numbers of mismatches for the reigns of Ammi-šaduqa (Aš) and of Ammi-ditana without (Ad+0) and with an additional intercalary month (Ad+1) at the end of the reign of Ammi-ditana or at the beginning of the reign of Ammi-šaduqa, using the *2014 data. The columns also record the relative probabilities for a correct chronology assuming that the correct chronology is one of the four chronologies considered. The lowest two rows record the means and standard errors of the numbers of mismatches for correct and wrong chronologies.

The numbers of mismatches for Ad+0 + Aš are more than 2 standard errors higher than the expected value for a correct chronology, suggesting that either none of the four chronologies is correct or there was an additional intercalary month at the end of the reign of Ammi-ditana or at the beginning of the reign of Ammi-šaduqa. In this case, one of the four chronologies for Ad+1 + Aš must be correct: here the calculated probabilities for the UMC and the LMC are so low (0.004 and 0.01) that it is unlikely that either the UMC or the LMC is the correct chronology and that it is probable that either the HC or the SC is correct. David LAPPIN (forthcoming) has re-examined the question looking at 12 possible chronologies from Ammi-šaduqa 1 = 1702 BC (HC) to Ammi-šaduqa 1 = 1419 BC with 14 different proposals for intercalary months.⁶⁷ He concluded that of the chronologies compatible with the Venus Tablets those with Ammi-šaduqa 1 = 1702 BC (HC) and 1483 BC are the most probable. Surprisingly (at least to me), the NLC is the third most probable of the chronologies that he examined. In all cases the Middle Chronologies are very unlikely.

As the conclusion that the Middle Chronologies are very unlikely to be correct is at odds with the results of studies based on radiocarbon determinations (see below), we should examine the assumptions underlying the 30-day month analysis.

⁶⁶ Twenty-one 30-day months are attested from the later part of the reign of Ammi-ditana in 24.iv, 24.vii, 24.viii*, 26.vi, 26.ix, 27.vii*, 27.xii, 29.ii, 30.iv, 30.vii, 31.ii, 31.xii, 32.viii, 32.xii, 33.ix, 34.viii, 34.ix, 36.i*, 36.ii, 36.xii, and 37.iv*. The four 30-day months marked with an asterisk were not used in the calculations of Peter HUBER (2017). Intercalary months are attested in Years 26, 27, 28, 32, 33, and 37 (all xii) and in Year 25 (xi): this last occurring in an unlikely month and being a single attestation was not accepted by Huber. Three 30-day months (27. xii, 31.xii, and 36.xii) occurred at the end of the year.

⁶⁷ I am grateful to David Lappin for sending me a copy of his article before publication. In his analyses he included 30-day months from almost the whole of the reign of Ammi-ditana although the number and dates of intercalary months are uncertain. He considers the data including and excluding the 30-day months at the end of the year. The 12 different chronologies include the NLC (Ammi-šaduqa 1 = 1499 BC) as well as others (e.g. Aš 1 = 1566 and 1550 BC) which he observes are not compatible with the Venus Tablet. He also investigated 14 different proposals for the insertion of intercalary months. The omission of the 30-day months at the end of the year did not make much difference to his conclusions. For example, the ranges of the proportion of mismatches for the HC with and without the end of the year months were 32%–44% and 32%–39% while the same ranges for the LMC were 49%–59% and 48%–61%.

Assumption 1. The proposed conditions for the visibility of the first crescent of the new moon used to create the 'master list of first crescents' are correct.

There are some uncertainties in the calculation of the position of the moon in the past since there is some uncertainty in the estimation of clock time. Both Peter Huber and David Lappin have investigated this question and have found that it does not affect the results greatly.⁶⁸

There are different opinions about the correct criteria for creating the 'master list of first crescents', but the criteria proposed by P.V. Neugebauer (PVN) and adopted by Huber and de Jong are widely accepted. The PVN criteria provide a closer match with the data from the Late Babylonian Astronomical Diaries than those adopted by ANDERLIČ/FIRNEIS (2003).

Assumption 2. The probability for a mismatch between the 'master list of first crescents' and attested Old Babylonian 30-day months for the correct chronology is the same as the probability for a mismatch between the 'master list of first crescents' and attested 30-day months in Late Babylonian administrative and economic tablets.

This assumption assumes that the proposed conditions for the visibility of the first crescent of the new moon were the same for the creation of the 'master list of first crescents', for the Late Babylonian period and for the Old Babylonian period. As discussed above, this does not appear to be true for the new moon following the last month of the year since in the Old Babylonian tablets there are more final months of the year with 30 days than in the Late Babylonian tablets.⁶⁹

It is also possible that in the millennium after the end of the Old Babylonian period, improvements were made in the accuracy of reckoning the beginning of the month. John Steele has proposed that the dates in the Late Babylonian Astronomical Diaries were calculated using observations of the lunar six rather than by direct observation.⁷⁰ The miss rate is only 10%. It is possible that some of the dates in Late Babylonian economic-administrative tablets were also calculated rather than observed. It is therefore likely that the probability of mismatches in the Old Babylonian period was higher than the 33% attested on the Late Babylonian tablets. This would make more chronologies compatible with a correct chronology, but would not change the relative probabilities distinguishing between the four candidate chronologies.

Assumption 3. There is a complete record of intercalary months.

For the reign of Ammi-šaduqa, the Venus Tablets confirm this. There is more uncertainty for the reign of Ammi-ditana: the number of attested intercalary months between Year 24 and Year 37 of the reign of Ammi-ditana is slightly more than the expected number, but the distribution is irregular with intercalary months attested in three consecutive years. The expected number of intercalary months in 14 years is 5. In the 14 years from Year 24 to Year 37, 7 intercalary months are attested, though that in Year 25 is in the unlikely month xi and was not accepted by Huber. Inclusion of this intercalary month could make a difference of at most 3 mismatches. Furthermore 3 of the remaining 6 intercalary months are attested by a single example. If one of these should be incorrect, a different conclusion might result from the analysis. The addition of an unattested intercalary month at the end of the reign of Ammi-ditana as proposed by Huber might increase the suspicion that one of the singly attested intercalary months is incorrect. If Assumption 3 is not correct for the reign of Ammi-ditana, there could be a problem in accepting the extended analysis which includes the 30-day months from the reign of Ammi-ditana.

Assumption 4. The sample size is sufficient to provide a reliable conclusion.

HUBER (2017: 35) discussed the required sample size. He considered the sample size needed for 'testing whether one of the four main Venus chronologies gives a better fit than the best of four random wrong chronologies' and calculated 'that one would need 60 or more month lengths for such a test to have a fair chance of being successful.' It is possible that such a test might be successful with a smaller sample but this cannot be predicted.⁷¹ This calculation is dependent on the

⁶⁸ HUBER 2017: 54–57 and LAPPIN (forthcoming).

⁶⁹ TANRET (2004) proposed that all Old Babylonian months had 30 days and that 'the use of month length data for the chronology should be abandoned'. Unfortunately, his supposition does not appear to be justified and is inconsistent with the evidence for intercalary months in the Venus Tablet.

⁷⁰ STEELE 2011: 478–481.

⁷¹ HUBER (2017: 42) calculated that the analysis with 3 mismatches in 17 30-day months from the reign of Ammi-ditana confirms 'the HC with better than 95% confidence, and if we are willing to lower ΔT by 3 minutes against the arbitrarily assumed default formula, it is confirmed even with over 99% confidence.' The proposed lowering of the change in clock time (ΔT) is a possibility but there is no evidence supporting it. This analysis was based on the 2013 dataset. Using the 2014* revised data set, with the addition of new 30-day months and the removal of the end of the year months, gives 4 mismatches in 18 30-day months, for which the confidence level is 94.4%. Furthermore this analysis was based on the

assumption ‘that the miss rate for correct chronologies is close to the NB value of 33%’: if the miss rate in the Old Babylonian period was higher an even larger sample size would be needed.

The 30-day month analysis gives an objective assessment of the wrongness of a chronology: a chronology with a miss rate close to or more than the average (47%) is likely to be wrong, while a chronology with a miss rate less than or close to the expected rate for a correct chronology (33%) may be correct. The 30-day method should be valid without the evidence of the Venus Tablets: the intercalary months are attested on tablets and those from the reign of Ammi-šaduqa are compatible with – but not dependent on – the Venus Tablets.

Radiocarbon and dendrochronological determinations combined with the reconstruction of the Assyrian Eponym List (see below) appear to limit the acceptable possibilities to a chronology with the accession of Ammi-šaduqa to between 1670 and 1620 BC. It is therefore reasonable to investigate how restricting the considered chronologies to one of the Middle Chronologies affects the results.

	Aš		Ad+0		Ad+0 + Aš		Ad+1		Ad+1 + Aš	
	Nos.	prob.	Nos.	prob.	Nos.	prob.	Nos.	prob.	Nos.	prob.
No. of months	31		18		49		18		49	
UMC	18	0.15	7	0.85	25	0.5	10	0.64	28	0.24
LMC	15	0.85	10	0.15	25	0.5	11	0.36	26	0.76
correct	10.23±2.62		5.40±1.99		16.17±3.29		5.40±1.99		16.17±3.29	
wrong	14.57±2.78		8.46±2.12		23.03±3.49		8.46±2.12		23.03±3.49	

Fig. 13.20. Numbers of mismatches for the reigns of Ammi-šaduqa and of Ammi-ditana without (Ad+0) and with (Ad+1) an additional intercalary month at the end of the reign of Ammi-ditana or at the beginning of the reign of Ammi-šaduqa using the *2014 data, assuming that the correct chronology is one of the two Middle Chronologies. The columns also record the probabilities that a particular system is a correct chronology and the lowest rows record the means and standard errors of the numbers of mismatches for correct and wrong chronologies.

If the considered chronologies are restricted to the two Middle Chronologies, the data from the reign of Ammi-šaduqa alone shows that the number of mismatches for the LMC is within two standard errors of the expected number for the correct chronology but that for the UMC is almost 3 standard errors more than the expected number for the correct chronology (Fig. 13.20). For both chronologies, however, the number of mismatches for both chronologies are more than the expected mean for an incorrect chronology. Thus, accepting the assumptions discussed above and limiting the possible chronologies to the UMC and the LMC using only the data from the reign of Ammi-šaduqa, the analysis shows that the LMC is more likely to be correct than the UMC, but a more realistic conclusion is that the assumptions are wrong: either the sample size is too small or the miss rate for the Old Babylonian period is higher than 33% or both.

If the analysis also includes the data from the reign of Ammi-ditana, the number of mismatches for both chronologies are more than the expected mean for an incorrect chronology and almost three standard errors more than expected for a correct chronology.

An additional intercalary month at the end of the reign of Ammi-ditana or at the beginning of the reign of Ammi-šaduqa is less likely, since the numbers of mismatches for Ad+1 + Aš (28 for the UMC, 26 for the LMC) are more than 3 standard deviations more than the expected number for the true chronology. Without an additional intercalary month, the probabilities for the UMC and the LMC are equal and the 30-day month analysis does not help to decide which of the two chronologies is correct. But as discussed above, there may be problems in combining the evidence from the reign of Ammi-ditana with that from the reign of Ammi-šaduqa. It should be noted that in all except one case (UMC Ad+0) including the reign of Ammi-šaduqa alone, the numbers of mismatches are more than the expected mean for a wrong chronology.

If the Lower Middle Chronology is correct, it is possible that dust in the atmosphere from the 1628 BC Event interfered with the visibility of Venus from Observation 27 to Observation 33.⁷² This same dust might have obscured the

assumption that the High Chronology was the most probable in the analysis of the month lengths from the reign of Ammi-šaduqa but this is not the case with the 2014* dataset.

⁷² If dust in the atmosphere affected the Venus observations even if it could not be associated with the 1628 BC Event, this might also have affected the 30-day months according to the UMC. In this case 5 out of 10 months would be suspect leaving a total of 13 out of 21 mismatches.

visibility of the first crescent of the New Moon, thereby making the lengths of the months from Ammi-šaduqa 12.i to Ammi-šaduqa 14.vi less predictable. This affects 11 of the 38 attested 30-day months attested from the reign of Ammi-šaduqa: 12.iv, 12.vii, 12.viii, 13.i, 13.ii, 13.vi, 13.x, 13.xii, 13.xii₂, 14.iv, and 14.vi. Of these one (13.xii₂) occurred at the end of the year and so is more likely to have been given a 30th day. The miss rate for the other 10 months would not have been the same as the supposed miss rate for a correct chronology (estimated by Huber to be 0.33) and would have been closer to the miss rate for a wrong chronology (0.47) and this proves to be the case since 4 of the 10 months (0.40) are mismatches. It might be thought safer not to include these 30-day months in the analysis. This would leave 11 mismatches among the remaining 21 months. If these were distributed as a binomial, the mean and standard deviation would be 6.93 ± 2.15 for a correct chronology and 9.87 ± 2.29 for a wrong chronology. With 11 mismatches out of 21 the LMC is less likely to be the correct chronology than with 15 mismatches out of 31, but this does not significantly alter the conclusions to be drawn from Fig. 13.20.

In conclusion, with the currently available data, the 30-day month analysis favours the High Chronology and the Short Chronology and suggests that neither of the Middle Chronologies is correct. There are, however, some reservations about accepting this conclusion unconditionally, because there are doubts about whether the assumptions on which the method is based are correct and whether the sample size is large enough.

4. Radiocarbon determinations and tree-ring dating

The astronomical methods discussed above are independent of methods based on radiocarbon determinations. As far as I am aware, there are no high precision radiocarbon determinations from North or South Mesopotamia from contexts which can be directly connected with historical figures or historical events that took place in the first half of the second millennium BC.⁷³ One might hope that radiocarbon determinations from Mari, Tell Leilan, Chagar Bazaar, or Tell ed-Der might have provided such information. Harvey Weiss informed me that the results of the analysis of seed and bone samples from the Acropolis NE and Lower Town Palace excavations in Tell Leilan (Šubat-Enlil, Šamši-Adad's capital) have proved inconclusive. Instead, until now radiocarbon determinations from Anatolian palaces have contributed to the debate in a rather circuitous fashion, but recent radiocarbon determinations from a destruction level at Zincirli which has been connected with Hittite and Mesopotamian historical events may provide a more direct connection.

The probabilities associated with the ranges in calibrated radiocarbon dates assume that the sample has been correctly excavated, processed, calibrated and analysed. They do not allow for any uncertainties arising from contamination, the type of sample, regional or seasonal variation, inaccuracies in the calibration curve and other methodological problems. Thus the probability ranges associated with calibrated radiocarbon dates can be misleading (WIENER 2012: 430–431). For example the 95% range for the construction of the Sarıkaya Palace (Fig. 13.22) proposed in 2010 was 1781–1767 BC. This interval does not overlap with that suggested in 2016 1798–1782 BC. As it turned out the 2010 95% range did not allow for a possible unknown error in the reconstruction of the tree-ring sequence. In 2020 with a new calibration curve the range (1780–1769 BC) does not overlap that proposed in 2016. This does not mean that the probabilities were wrong: they did not and could not allow for unknown eventualities. But it does mean that there is more uncertainty in the radiocarbon dates than is suggested by the published probability ranges.

4.1. Anatolian palaces and Old Assyrian texts

The relative dates for the felling of the timbers used in the construction of palaces in Achemhöyük and Kültepe in central Turkey have been established by means of dendrochronology and their absolute dates by radiocarbon dating of specific groups of tree-rings (so-called wiggle matching). The construction of these palaces may be related to inscriptions dated by Old Assyrian eponyms and to seal impressions of servants of Šamši-Adad. The Assyrian Eponym List can be correlated with the potential Venus Chronologies since the death of Šamši-Adad occurred in REL 197/198 which corresponds to 1831 HC, 1775 UMC, 1767 LMC, and 1711 SC (Fig. 13.21).

⁷³ Pers. comm. 6 June 2018.

Months	June	July	Aug	Sept	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May
REL earlier	REL 197							REL 198				
REL Š-A	REL 197				REL 198							
Eponym	Ṭab-šilli-Aššur				Ennam-Aššur							
Babylonian date	Hammurabi 18										Ha 19	
HC	1831 1775 1767 1711							1830 1774 1766 1710				
UMC												
LMC												
SC												
Death of Š-A												
Months	June	July	Aug	Sept	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May

Fig. 13.21. Correlation of the date of the death of Šamši-Adad (Š-A) with dates in the Julian, Assyrian and Babylonian calendars and with the HC, UMC, LMC, and SC. Before the reign of Šamši-Adad the Assyrian year (row REL earlier) began close to the winter solstice (ca. 5 January in 1770) but in the reign of Šamši-Adad (row REL Š-A) it began close to the autumn equinox (ca. 6 October 1771). The Babylonian year began around the spring equinox (ca. 5 April in 1770 BC). Šamši-Adad was reported to have been extremely ill in the last month of the eponym year Ṭab-šilli-Aššur (REL 197) and it is assumed that he died not long afterwards. The shaded area marks the probable interval in which he died. Because of uncertainties in the intercalary months the Assyrian and the Babylonian years might have started a couple of months earlier or later. Whenever the Assyrian eponym year began, it is probable that the death of Šamši-Adad occurred in Hammurabi's 18th year.

4.1.1. Dating the Sarıkaya Palace in Acemhöyük

Let us look first at the dating of the construction of the Sarıkaya Palace in Acemhöyük (see below Fig. 13.22). 29 bullae bearing impressions of five different seals with inscriptions mentioning Šamši-Adad were found in Room 6 of the palace (ÖZGÜÇ 2015: 37; PATRIER 2015; MANNING/GRIGGS/LORENTZEN/BAJAMOVIC *et al.* 2016: 7; VEENHOF 2017). At least one of these was Šamši-Adad's own seal. Three of these seals mention the god Aššur and so should date to the last 33 years of his reign.⁷⁴ It is probable that the bullae were sealed during Šamši-Adad's lifetime and arrived in Acemhöyük within months of having been sealed. One of the Šamši-Adad seals belonged to his servant Līter-šarrūssu and was reused by Bunuma-ili with a different legend in 1771 UMC (REL 202) (KOLIŃSKI 2021: 223).⁷⁵ Fifteen bullae were sealed by three cylinder seals mentioning Aplahanda, king of Carchemish, who was a contemporary of Šamši-Adad and died about 11 years after Šamši-Adad. Five bullae were sealed with a seal inscribed '[N]agiha[...], daughter of Yahdun-Lim, king of Mari and of the land of the Sim'[alites]'. Yahdun-Lim was succeeded by his son Sumu-Yamam two years before Šamši-Adad conquered Mari, thus about 17 years before the death of Šamši-Adad (CHARPIN 2004: 145), but this seal might have been used in the reign of Sumu-Yamam or even later (MANNING/GRIGGS/LORENTZEN/BAJAMOVIC *et al.* 2016: 7 contra ÖZGÜÇ 2015: 35). Although Nimet ÖZGÜÇ (2015: 35–36), on stylistic grounds, dated the seals impressed on bullae in Room 6 to various different periods from the 20th century BC to after 1719 BC, it is quite possible that all of the bullae were sealed within a relatively short period in the later part of the reign of Šamši-Adad (OTTO 2000: 49–50).⁷⁶

The Aegean Dendrochronology Project at Cornell University has examined numerous timbers including examples with bark from the Sarıkaya Palace at Acemhöyük which had been felled according to their dendrochronological sequence in relative ring years 730 and 731. It has therefore been suggested that the earliest date for the use of the building would likely have been a year or two later in relative ring year 732.

Normally, most of the objects left in a violently destroyed building date to the latest phase of the occupation and so, although it is possible that the inscribed bullae were heirlooms, it is to be expected that they were deposited not long before the destruction of the Sarıkaya Palace. Unfortunately the date of this destruction is uncertain.⁷⁷ NEWTON/KUNIOLM (2004: 168–169) reported that there was a repair to the palace using timbers that were at least 8 years later than the original timbers.

⁷⁴ A fourth seal has the title 'appointee of Enlil', but this may date to after Šamši-Adad's conquest of Aššur (contra ÖZGÜÇ 2015: 35).

⁷⁵ VEENHOF (2017: 254) dated the tablet (L85-115) on which this seal is impressed to the eponymy of Ikūn-pīya (REL 190 1783 UMC) instead of that of Ikūn-pī-Ištar (REL 202). If this were correct, it would introduce an additional constraint.

⁷⁶ I am indebted to Adelheid Otto for information and illuminating discussions about these seal impressions. She tells me that the publication by Nimet ÖZGÜÇ (2015) has not led her to change her opinion.

⁷⁷ According to Remzi KUZUOĞLU (2016: 693), 'Carbon-14 results of the materials used for filling of the Sarıkaya Palace ... show that it dates to the late stage of the Old Assyrian Period, around ± 1710–1700 B.C.' I have not been able to confirm this information that came from Prof. Dr. Aliye Öztan. Unfortunately the nature of the sample, the conventional radiocarbon age, and the method of calibration are not given.

Since 1993, the Aegean Dendrochronology Project has published increasingly precise and supposedly increasingly accurate dates for the felling of the trees used in the construction of the Sarıkaya Palace (Fig. 13.22). Most of these estimates were based on ‘wiggle-matching’ of selected radiocarbon determinations from timbers dated according to the relative dendrochronological sequence with the then current radiocarbon calibration curves.⁷⁸

This process culminated in 2010 with the conclusion that the trees were felled between 1778 and 1772 BC with a probability of 0.682 and between 1781 and 1767 BC with a probability of 0.956. The presence of impressions of seals probably to be dated within the reign of Šamši-Adad (died in 1775 UMC or 1767 LMC) was thought to be just compatible with the UMC, but more easily compatible with the LMC.

At the 2nd Kültepe International Meeting from 27–29 July 2015, Sturt Manning informed the participants that this precise estimate should be revised:⁷⁹ he claimed that the sequence of tree-rings derived from timbers found at Porsuk had been wrongly correlated both with the sequence from Gordion and with that from Acemhöyük, Karahöyük, and Kültepe.⁸⁰ He disentangled the three parts of the combined sequence and presented them as independent sequences. The RR (Relative Ring) numbers that had been used previously for the complete sequence were converted into RY (Relative Year) numbers in three series prefaced by POR for Porsuk, GOR for Gordion and MBA (Middle Bronze Age) for Acemhöyük, Karahöyük, and Kültepe. Conveniently the MBA-RY numbers are the same as the previous RR numbers. New wiggle-matching of the combined tree-ring sequence based on samples from Acemhöyük, Karahöyük, and Kültepe led to dates some 13 to 16 years earlier than those proposed in 2010 (MANNING/GRIGGS/LORENTZEN/BARJAMOVIC *et al.* 2016: 15) (Figs. 22 and 23). This revision changed both the absolute dating and reduced its precision. The additional constraints involved in accommodating the Porsuk section of the tree-ring sequence in the complete sequence had led to a shorter confidence interval. In this article the authors, in fact, offer several different ‘dating models’, based on different prior assumptions including treating the Kültepe and Acemhöyük samples separately or together, omitting outliers, and allowing for possible ¹⁴C offsets (MANNING/GRIGGS/LORENTZEN/BARJAMOVIC *et al.* 2016: Table 2 on p. 12). These give a range for MBA-RY 730 for the MBA sequence (Models 7a to 8c) from 1795 to 1782 (68.3%) and from 1819 to 1778 (95.4%), considerably larger than the intervals of 1795–1786 (68.3%) and 1799–1783 (95.4%) proposed in Table 3 on p. 16.⁸¹ The proposed confidence intervals do not take account of possible uncertainties resulting from the prior assumptions, the methods of analysis, and the choice of ‘dating model’.⁸²

Within a year, Manning and his colleagues (MANNING/BARJAMOVIC/LORENTZEN *et al.* 2017) revised the 2016 dating. The authors realised that the IntCal98 calibration curve fitted the wiggle matching better than the more recent IntCal13 calibration curve, giving a best fit 8 or 9 years later and a standard error of ± 2 instead of ± 8 years.⁸³ They assumed that the closer fit supports the proposals that the IntCal98 calibration curve should be used instead of the IntCal13 calibration

⁷⁸ The exact dating of 1752 BC in KUNIHOLM *et al.* 1996 was based on the assumption that an exceptional growth ring in timbers from Porsuk could be identified with the 1628 BC Event.

⁷⁹ I am grateful to Klaas Veenhof for providing me with a summary of Manning’s proposals shortly after the meeting. The results are published in MANNING/GRIGGS/LORENTZEN/BARJAMOVIC *et al.* 2016.

⁸⁰ Compare the confident assertion (MANNING/KROMER/KUNIHOLM *et al.* 2003: 2):

‘Although sample numbers are not especially large in the mid-16th century BC, we note that for the ¹⁴C wiggle-match we employed a long-lived tree, GOR-161, which grew from the 18th–10th centuries BC. It is securely cross-dated on the early end against dozens of juniper trees from Porsuk (KUNIHOLM *et al.* 1992 and ongoing work since), and then against, progressively, dozens, scores, and finally over 100 trees from Gordion and environs. There is thus no possibility of dendrochronological error in the placement of the data shown in Figure 2.’

with the contradictory statement (MANNING/GRIGGS/LORENTZEN/BARJAMOVIC *et al.* 2016: 7):

‘There are two issues here. First, with just 1 or 2 trees in the Gordion chronology – too few for any secure cross-dating following standard dendrochronological methods, which makes the best available match, whether statistical, visual, or both, highly tentative at best. Second, the supposed linkage between the Porsuk chronology and the MBA chronology is also, in dendrochronological terms, no more than tentative due to the overlapping segment of POR containing only 1 or 2 samples, again too low for secure cross-dating.’

⁸¹ These figures are said to be from Model 8a in Table 2, but, according to my calculations, the Model 8a figures in Table 2 give 1792–1785 and 1796–1772.

⁸² It would be sensible in evaluating these datings to follow BRONK RAMSEY *et al.* (2001: 388), who recommended ‘that once the analyses have been performed, an extra latitude of about 5 years either way be allowed in any interpretation’, and even this, as we will see, may be an underestimate of the uncertainty.

⁸³ The suggested confidence intervals cited in MANNING/GRIGGS/LORENTZEN/BARJAMOVIC *et al.* 2016 and MANNING/BARJAMOVIC/LORENTZEN 2017 are technically High Posterior Density (HPD) intervals. ‘Credible intervals’ in Bayesian statistics are analogous to the concept of confidence intervals used in classical statistics. The HPD is the ‘credible interval’ in which most of the posterior distribution lies and is the smallest such interval. The HPD is therefore the minimum interval. Despite the smaller HPD interval, it would be rash to assume that the 2013 calibration curve is less reliable than the 1998 curve.

curve and that the dendrochronological determinations confirmed the correctness of the LMC.⁸⁴ Given the inherent uncertainties involved in the wiggle matching of the radiocarbon determinations with the calibration curve, it may be thought unrealistic to assume a standard error of 2 years.⁸⁵

A further uncertainty about the accuracy of radiocarbon dating is the possibility of local offsets from the calibration curves.⁸⁶ This was already proposed for the 9th century BC radiocarbon determinations from Gordion (KROMER *et al.* 2001, MANNING/KROMER/BRONK RAMSEY *et al.* 2010, see ROAF 2012b: 8 for its possible implications for the dating of the destruction of Hasanlu) and has been established for later periods in the Southern Levant (MANNING/GRIGGS/LORENTZEN/BRONK RAMSEY *et al.* 2018). MANNING/KROMER/CREMASCHI *et al.* (2020) investigated this for the Gordion part of the Anatolian dendrochronological sequence and identified several periods when this sequence varied from the IntCal98, IntCal04, and IntCal13 curves by several decades. They proposed that these offsets were the result of the species of tree, its growing season, and the environmental conditions. There are, however, additional problems with the calibration using IntCal 2013.

A revised calibration curve recording the annual variation of Carbon 14 based on the analysis of individual annual rings in Californian bristlecone pine and Irish oak for the period 1700–1480 BC shows marked differences from previous IntCal curves: ‘between c. 1660 BCE and c. 1540 BCE, both species, from growth environments c. 7900 km apart, indicate a clear and sustained offset from IntCal13’ (PEARSON/BREWER *et al.* 2018: 2 and fig. 1).⁸⁷ This offset is about 20 to 40 radiocarbon years more than the IntCal13 curve and, if correct, also affects the range of calibrated radiocarbon determinations from ca. 1700 to 1500 BC by a considerable amount since it extends the plateau region of the curve. There are also smaller systematic differences between the results for bristlecone pine and Irish oak (PEARSON/WACKER *et al.* 2020: 945 fig. 1). Subsequent analysis of individual annual rings showed that there was a similar offset on the Gordion section of the Anatolian dendrochronological sequence (PEARSON/SALZER *et al.* 2020a: 8412 fig. 2).

This study (PEARSON/SALZER *et al.* 2020a) dated the beginning of the Gordion section of the sequence (RR 1764) to 745 ± 4 BC (95.4%), by comparing a sequence of radiocarbon determinations from single rings of the Gordion sequence with similar determinations from the dendrochronologically dated bristlecone pine and Irish oak tree-ring sequences. This gave a date of 1783–1775 (95.4%) for RR 730 (Fig. 13.22) assuming that the Anatolian dendrochronological sequence was correctly reconstructed from Gordion to Acemhöyük. They then compared the individual rings in the Gordion sequence with the anomalies in the master bristlecone pine tree-ring sequence caused by frost-rings following a volcanic eruption. They proposed that wider rings in the Gordion sequence corresponded with narrow rings in the bristlecone sequence and concluded that the date of the first ring in the Gordion sequence (RR 1764) was 746–744 BC giving a date of 1780–1778 for RR 730 allowing for a possible error of ± 1 in correlating the wider and narrower rings in the two sequences (Fig. 13.22). Although the authors included MANNING/GRIGGS/LORENTZEN/BAJAMOVIC *et al.* 2016 in their list of references, they did not accept or discuss the proposed division of the Anatolian sequence into three independent sequences. This omission was noted in a comment by Sturt MANNING (2020) who suggested that there were complications in accepting the conclusions of PEARSON/SALZER *et al.* 2020a. This led to a rebuttal of Manning’s comments (PEARSON/SALZER *et al.* 2020b), in which they pointed out ‘that none of the objections raised affects, undermines, or alters our core scientific conclusions’, and a minor corrections to the original article (PEARSON/SALZER *et al.* 2020c). In their reply they did not, however, discuss the validity or otherwise of the division of the Anatolian sequence into three separate sequences, but restricted their comments to confirming the date of the beginning of the Gordion sequence.

In the same year a new calibration curve, IntCal20, was published (REINER *et al.* 2020). MANNING/WACKER *et al.* (2020: 2–5) attempted to use IntCal20 to revise the date of the MBA section of the Anatolian tree-ring sequence. Unfortunately they found a poor fit. A statistical test ‘to assess whether there is systematic difference between the MBA time series and the calibration curve ... indicates ... a bimodal finding’ with maxima about 22 ± 5 ¹⁴C years later or

⁸⁴ It seems that the authors were encouraged by their conclusion that the dendrochronological results were compatible with the LMC, which at this time was gathering further adherents.

⁸⁵ They note in the footnote on p. 72 that ‘an additional 0–1 year error should be regarded as applying to the numbers quoted’, adding the statement ‘this is clearly negligible’. See also footnote 82.

⁸⁶ For unexplained anomalous ¹⁴C dates 200 years too old for skeletons dated to the sack of Nineveh in 612 BC see TAYLOR *et al.* 2010.

⁸⁷ MANNING/KROMER/CREMASCHI *et al.* (2020: 5) suggest that this may be due to ‘a laboratory or method difference’. IntCal20 also exhibits an offset between 1660 and 1540 BC (REIMER *et al.* 2020; VAN DER PLICHT *et al.* 2020) and may show that this was not due to practical, or methodological, or local conditions.

32 ± 8 ^{14}C years earlier than the best fit. These would then give the mid-point of the 68.3% highest posterior density for RY 730 values of 1822 BC, 1774 BC (+ 22 ^{14}C years) and 1854 BC ($- 32$ ^{14}C years). The authors proposed that IntCal20 is only applicable to the MBA sequence with an offset. This assumption and the omission of several supposedly anomalous readings give the estimates given in Fig. 13.22.⁸⁸ A correlation of the Gordion sequence with IntCal20 notes possible systematic differences between the IntCal20 curve and the data from accelerator mass spectrometry (AMS) of -2.3 ± 2.1 ^{14}C years and that from earlier methods, such as low-level gas proportional counting (LLGPC) and liquid scintillation spectrometry (LSS) of 11.2 ± 1.9 ^{14}C years.

suggested date	suggested confidence level	publication
1828–1754	?68.3%	KUNIHOLM 1993: 372
1752–1753	100%	KUNIHOLM <i>et al.</i> 1996: 781
1841–1743	95.4%	
1778–1767	95.4%	MANNING/KROMER/KUNIHOLM <i>et al.</i> 2001: 2534
1790–1767	99.7%	MANNING/KROMER/KUNIHOLM <i>et al.</i> 2003: 2
1779–1771	95.4%	
1777–1774	95.4%	NEWTON/KUNIHOLM 2004: 169
1778.3–1772.3	68.3%	MANNING/KROMER/BRONK RAMSEY <i>et al.</i> 2010: 1594
1781–1767	95.4%	
1795–1782	68.3%	MANNING/GRIGGS/LORENTZEN/BARJAMOVIC <i>et al.</i> 2016: 12
1819–1778	95.4%	
1795–1786	68.3%	MANNING/GRIGGS/LORENTZEN/BARJAMOVIC <i>et al.</i> 2016: 16
1799–1783	95.4%	
1784–1780	68.3%	MANNING/BARJAMOVIC <i>et al.</i> 2017: 74
1787–1778	95.4%	
1783–1775	95.4%	PEARSON/SALZER <i>et al.</i> 2020a: 8413; 2020b
1780–1778	100%	
1776–1771	68.3%	MANNING/WACKER <i>et al.</i> 2020: 4
1780–1769	95.4%	

Fig. 13.22. Suggested dates for the earliest felling of the timbers used in the construction of the Sarıkaya Palace in Acemhöyük (RR 730 = MBA-RY 730) and the resulting possible ranges according to different analyses of the dendrochronological evidence. The suggested confidence levels are either derived from the ‘wiggle matching’ of the dendro-dates with the relevant ^{14}C calibration curve or identifications of exceptional rings in the floating Anatolian sequence with exceptional rings in other absolutely dated dendrochronological sequences (labelled 100%). The date of the earliest use of the palace would have been at least 2 years later (RR 732).

In a lecture given at Columbia University on 5th November 2023 Charlotte Pearson presented the results of her further investigation of the Anatolian tree-ring sequence based on individually radiocarbon dated tree-rings. She concluded that Peter Kuniholm’s original placement of the Porsuk part of the sequence was correct even though there were only a small number of overlapping trees.⁸⁹ With the proposal that the Porsuk section can be used to link the Gordion sequence with the MBA sequence, RR 730 can be dated to 1780–1778 BC.

Until the disagreements about the integrity of the Anatolian master sequence are resolved there will be uncertainty about the reliability of the proposed dates for the construction and first use of the Sarıkaya Palace. The differences are, however, not great and both (with a confidence level of 95.4%) fall within the interval 1785 to 1765 BC. Since the Sarıkaya Palace contained seal impressions of Šamši-Adad, which probably arrived in Acemhöyük after his conquest of Aššur and before his death, and was in use for at least 8 years, this proposal gives dates for the first use of the palace (RR 732 1783–1763 BC) and its repair (after RR 738 1777–1757 BC) that are rather late for the UMC since Šamši-Adad died

⁸⁸ Given the bimodal distribution (suggesting a dating some 54 years older than those advocated by MANNING/WACKER *et al.* 2020: 2), the suggested confidence levels are over optimistic.

⁸⁹ I am grateful to Charlotte Pearson (email dated 12.2.2024) for a detailed and, in my opinion, convincing explanation of why she is not convinced by the arguments of MANNING/GRIGGS/LORENTZEN/BARJAMOVIC *et al.* 2016 and MANNING/WACKER *et al.* 2020 which reject the dendrochronological connections of the Porsuk section with the earlier and later sections of the sequence and support the separation of the MBA section of the Anatolian sequence from the Gordion section. She will publish these results in a future article.

in 1775 UMC.⁹⁰ The UMC is even more unlikely if the more precise date of 1778–1776 BC proposed in PEARSON/SALZER *et al.* 2020a is correct. The LMC is slightly better, but it implies that the Sarıkaya Palace was only occupied for about a decade. The Short Chronology fits easily, but the High Chronology is impossible if the dendrochronological sequence has been correctly dated.

4.1.2. Dating the Warshama Palace in Kültepe

The analyses by MANNING/GRIGGS *et al.* 2016, PEARSON/SALZER *et al.* 2020a and MANNING/WACKER *et al.* 2020 also changed the dating of the felling of the timbers of the Warshama Palace in Kültepe (Fig. 13.23), which took place over three years (MBA Relative Years 670–672). Assuming that the trees were cut down after the Old Palace went out of use, objects beneath the Warshama Palace must have been deposited before MBA-RY 670 (= RR 670).⁹¹ PEARSON/SALZER *et al.* (2020a) assuming a continuous dendrochronological sequence proposed the shorter intervals 1843–1835 and 1840–1838 BC. It is likely that the correct date should be between 1845 and 1825 BC.

Date (68.3%)	Date (95.4%)	Publication
1838–1832	1843–1829	MANNING/KROMER/BRONK RAMSEY <i>et al.</i> 2010: 1594
1854–1845	1858–1842	MANNING/GRIGGS/LORENTZEN/BARJAMOVIC <i>et al.</i> 2016: 16
1844–1840	1847–1838	MANNING/BARJAMOVIC <i>et al.</i> 2017: 74
	1843–1835	PEARSON/SALZER <i>et al.</i> 2020a: 8413
1836–1831	1840–1829	MANNING/WACKER <i>et al.</i> 2020: 4

Fig. 13.23. Suggested dates for the felling of the first timbers used in the construction of the Warshama Palace (RR 670 = MBA-RY 670) in Kültepe according to various estimates.

BARJAMOVIC/HERTEL/LARSEN (2012: 31) discussed ‘a group of texts coming from the burnt mudbrick debris of the early fortification wall underlying the Waršama Palace’. They also noted that ‘some of those texts can be securely dated on prosopographical grounds to the main generation of Old Assyrian trade REL 80–110.’ By this they did not mean that the texts necessarily belonged to this interval but rather that the texts mentioned individuals who were active during this period. Since, however, over 80% of the attestations of year eponyms from Kültepe date between REL 80 and 111 (Fig. 13.24),⁹² it is relatively unlikely for these tablets to have been written before REL 80 or after REL 111, but by no means impossible.

Fig. 13.24. Distributions of numbers of attestations of year eponyms and of single and double *hamuštum* dates from Kültepe between REL 60 and REL 127. REL numbers follow BARJAMOVIC/HERTEL/LARSEN 2012: 93–94. Data from KRYSZAT 2004: 68–156.

REL	Eponym name	year eponyms	<i>hamuštum</i> dates		REL	Eponym name	year eponyms	<i>hamuštum</i> dates	
			single	double				single	double
60	Puzur-Ištar	2	0	1	66	Amaya	1	0	0
61	Pišah-ilī	1	0	0	67	Iphurum	5	0	0
62	Asqūdum	0	0	0	68	Kudānum	1	0	0
63	Ilī-pilah	3	0	0	69	Ilī-bāni	2	0	0
64	Qulali	1	0	0	70	Šu-Kūbum	2	0	0
65	Susaya	2	0	0	71	Quqidi	1	0	0

⁹⁰ HÖFLMAYER/MANNING (2022: 18) stated that ‘the earliest use of the Sarıkaya Palace at Acemhöyük, associated with Shamshi-Adad I, could be dated either to about 1778–1767 BC. It is more probable that the bullae sealed with the seal of a servant of Šamši-Adad were deposited not long before the destruction of the Sarıkaya Palace rather than during the earliest use of the Sarıkaya Palace at Acemhöyük (see above). The statement (on p. 17) that ‘REL 197 (or 196) corresponds to the death of Shamshi-Adad and the 18th year of Hammurabi’ is not correct. The death of Šamši-Adad probably took place in REL 197/198 according to the calendar of Šamši-Adad and the 18th year of Hammurabi began in REL 197 and ended in REL 198 (see Fig. 13.21).

⁹¹ HÖFLMAYER/MANNING (2022: 18) place ‘the likely construction of the palace within the period either about 1837–1826 BC or 1835–1826 BC (95.4% probability)’ and add that ‘the process of felling, transport, stockpiling, and then use (assuming no further deliberate seasoning) of the juniper timbers could have added another period from months to a couple of years.’ These dates are those of the earliest completion of the construction of the Warshama Palace and not those of the felling of the first timbers used in the construction of the (MBA-RY 670). I am grateful to Felix Höflmayer for providing me with a copy of this chapter before publication.

⁹² A similar distribution is published as a graph in BARJAMOVIC/HERTEL/LARSEN 2012 (55–56 and fig. 14 on p. 56). This is based on ca. 1250 attestations of year eponyms on ca. 1150 texts.

REL	Eponym name	year eponyms	<i>hamuštum</i> dates		REL	Eponym name	year eponyms	<i>hamuštum</i> dates	
			single	double				single	double
72	Abiya	0	0	0	100	Aššur-nādā	13	5	0
73	Šu-Ištar	2	0	1	101	Ikūn-pī-Ištar	23	6	0
74	Baziya	3	0	0	102	Buzutaya	22	8	0
75	Šu-Ištar	7	0	1	103	Innaya	29	13	0
76	Abiya	6	0	1	104	Šu-Suen	20	8	0
77	Salliya	7	0	3	105	Aššur-malik	30	12	0
78	Ibni-Adad	4	0	1	106	Aššur-imittī	40	23	0
79	Aham-arši	8	0	4	107	Enna-Suen	26	15	0
80	Sukkaliya	15	3	5	108	Akutum	33	16	0
81	Iddin-Aššur	22	2	3	109	Mašī-ilī	21	14	0
82	Šudaya	20	0	0	110	Iddin-ahum	13	6	0
83	Al-tāb	18	2	7	111	Samaya	13	8	0
84	Aššur-damiq	37	3	11	112	Ilī-ālum	9	4	0
85	Puzur-Nirah	26	1	10	113	Ennam-Anum	9	5	0
86	Amur-Aššur	38	0	15	114	Ennam-Aššur	9	4	0
87	Buzuzu	21	1	3	115	Enna-Suen	4	4	0
88	Šu-Hubur	31	4	12	116	Hanna-Nārum	5	0	0
89	Ilšu-rabi	17	2	6	117	Dādiya	1	0	0
90	Ali-ahum	30	2	14	118	Kapatiya	2	0	0
91	Tāb-Aššur	12	0	8	119	Išme-Aššur	6	3	0
92	Elāli	29	2	10	120	Aššur-muttabbil	5	1	0
93	Iddin-abum	23	3	11	121	Šu-Nirah	2	0	0
94	Adad-bāni	14	0	7	122	Iddin-abum	0	0	0
95	Aššur-iddin	19	0	9	123	Ilī-dan	4	0	0
96	Aššur-nādā	16	0	11	124	Aššur-imittī	3	0	0
97	Kūbiya	40	2	6	125	Buziya	0	0	0
98	Ilī-dan	24	7	1	126	Dādiya	3	0	0
99	Šilulu	23	6	0	127	Puzur-Ištar	0	0	0

Fig. 13.24 (cont.). Distributions of numbers of attestations of year eponyms and of single and double *hamuštum* dates from Kültepe between REL 60 and REL 127. REL numbers follow BARJAMOVIC/HERTEL/LARSEN 2012: 93–94. Data from KRYSZAT 2004: 68–156.

This table shows the clear increase in REL 80 of the numbers of dated texts and the subsequent decrease in REL 112: before 80 and after 111 there are less than 10 texts per year while between 80 and 111 there are more than 10 texts per year with 86% of the texts (758 out of 878) dating to this period.⁹³

A sealed envelope, (kt z/t 17, BARJAMOVIC/HERTEL/LARSEN 2012: 31) found in a room of the Old Palace used the still incompletely understood *hamuštum* ‘week eponym’ dating system, in which subdivisions of the month were labelled with the names of either one or two eponyms.⁹⁴ Fig. 13.24 records the distribution of the dated texts and of single and double *hamuštum* dates found in Kültepe recorded by KRYSZAT (2004: 62–156).⁹⁵ In the earlier part of the sequence double *hamuštum* dates are most common, but they are not attested after REL 98. Single and double *hamuštum* dates may have been in use in REL 97 and 98.⁹⁶ Before REL 97 151 texts are dated by double *hamuštum* dates and 25 by single *hamuštum* dates: these single *hamuštum* dates may be abbreviations of double *hamuštum* dates. Published *hamuštum* dates are not attested after REL 120.

⁹³ For ease of counting no distinction was made between eponyms with or without the *ša qāti* formula. A revised table, which made this distinction and used an up-to-date list of attestations of eponyms and *hamuštum* dates, is desirable but it would not give a substantially different picture.

⁹⁴ For overviews of the topic see MICHEL (2021 or n.d.). For different opinions on the length of the *hamuštum* see VEENHOF 1997, KRYSZAT 2004: 159–163, and DERCKSEN 2011.

⁹⁵ The numbers represent the numbers of attestations and not the numbers of dated texts. No account has been taken of attestations not included in KRYSZAT 2004. For a list of recent publications see BARJAMOVIC/HERTEL/LARSEN 2012: 55 n. 188. These may change the details but are unlikely to affect the general picture.

⁹⁶ The attestations of single *hamuštum* dates in REL 97 might be abbreviations of double *hamuštum* dates.

The text of kt z/t 17, the sealed envelope found in a room of the Old Palace, is not fully preserved but there is barely space for the name of a single *hamuštum* eponym in the break. From the distribution of single *hamuštum* dates in Fig. 13.24, it is likely that it should date to before REL 121 and probably after REL 96 since 87% of the attestations (170 out of 195) come from this period. Since, as discussed above, 86% of the attested eponym dates lie between REL 80 and 111, it is probable that this text too was written in this interval. Combining these two approaches the chances are that kt z/t 17 dated to between REL 97 and 111 and that it is very unlikely that it dated to before REL 80 or after REL 121 (Fig. 13.24).⁹⁷

	REL	HC	UMC	LMC	SC
Prosopographical information	REL 80–111	1948–1917	1892–1861	1884–1853	1828–1797
Distribution of <i>hamuštum</i> eponyms	REL 97–121	1931–1907	1875–1851	1867–1843	1811–1787
Probable dating	REL 97–111	1931–1917	1875–1861	1867–1853	1811–1797
The end of Karum II	REL 138–141	1890–1887	1834–1831	1826–1823	1770–1767

Fig. 13.25. Dating of texts found below the Warshama Palace based on prosopographical information and the distribution of texts dated by *hamuštum* eponyms and the dating of the end of Karum II according to the four main chronologies.

The texts found below the Warshama Palace should be earlier than the construction of the Warshama Palace (RR 670). The range REL 97–111 for the probable dating of the texts found below the Warshama Palace corresponds to 1811–1797 SC (Fig. 13.25), later than 1843–1835 and 1840–1829 BC, the two most recent estimates for the felling of the timbers used in the construction of the Warshama Palace (Fig. 13.23). There are, as discussed above, uncertainties associated with the second of these dates but these suggest an even earlier date for the construction of the Warshama Palace which would be less compatible with the Short Chronology.⁹⁸ Both the UMC and LMC are consistent with these dates, and, if the High Chronology is correct and the texts were deposited not long before the abandonment of the Old Palace, some 70 years passed before the Warshama Palace was built to replace the Old Palace.

It has been widely assumed that the destruction of the Old Palace coincided with the end of Lower Town Level II (Karum II) (between REL 138 and 141 i.e. 1834–1831 UMC or 1826–1823 LMC) (BARJAMOVIC/HERTEL/LARSEN 2012: 28–30) (Fig. 13.25).⁹⁹ The latest estimates date the destruction of the Old Palace to before ca. 1845–1825 BC, which is compatible with neither the High nor the Short Chronology. It is compatible with the UMC, but not with the LMC if the estimate of the date is correct.

Archaeomagnetic investigations by ERTEPINAR *et al.* (2016; 2020)¹⁰⁰ conclude that there were various fires at different dates in the Lower Town and on the citadel: several of the fires in the Lower Town seem to be contemporary but it is not certain that these took place at the same time as the destruction of the Old Palace.

⁹⁷ HÖFLMAYER/MANNING (2022: 17) state that ‘the Old Palace on the citadel at Kültepe is securely associated with REL 80–110 and appears to have been in use at least until ca. REL 125. The date for the construction of the Warshama Palace, directly on top of and replacing the Old Palace, seems to lie somewhere between a *terminus post quem* of around REL 125 and probably no later than (and for many around the same time as) the transition from Level II to Level IB at Kültepe (REL 138–141).’ As discussed above, while the Warshama Palace might have been constructed after REL 125 it could have been constructed some 25 years earlier. The date of REL 138–141 seems to be based on the assumption that the destruction of the Old Palace was contemporary with or before the end of Karum II: this is not certain and if it were it would be a *terminus ante quem* and not a *terminus post quem* for the construction of the Warshama Palace.

⁹⁸ MANNING/GRIGGS/LORENTZEN/BARJAMOVIC *et al.* (2016: 12 and 16) give this range for Model 8a. Other models give a much wider range 1889–1838 (95.4%) but the upper limit is still earlier than the dating of the texts according to the Short Chronology. The alternative suggestions for the correlation of the MBA sequence through wiggle-dating would give an even earlier date and would be even more incompatible with the Short Chronology.

Gojko Barjamovic has reminded me that documentary texts found in the rooms of a building normally date to the latest occupation of the building. In this case kt z/t 17 according to the HC would have been written between 50 and 100 years before the timbers were felled for the building of the Warshama Palace suggesting either the text was old before the destruction of the Old Palace or that the Old Palace was replaced by the Warshama after lying in ruins for decades. According to both the UMC and the LMC, the text would have been written on average only a few years before the timbers were felled for the building of the Warshama Palace. If the decision to rebuild the palace was taken soon after the destruction of the Old Palace, the LMC date is closer to that date. If the decision was delayed, the LMC date might be too late. The range of uncertainty, both in the precision of the dates and in the timing of the events, is so great that these considerations are not decisive.

⁹⁹ For discussion of the relative dating of the destruction on the citadel with possible destructions in the Lower Town see VEENHOF 2017: 255.

¹⁰⁰ I am grateful to Andy Biggins for supplying me with copies of these reports.

Those, who have followed the history of the over-optimistic attempts to create a robust dendrochronological scheme,¹⁰¹ will remember the numerous occasions when new supposedly reliable results have been proclaimed only to be replaced a few years later by a different set of results and they may be sceptical of the latest results.¹⁰² This is probably unjustified: the differences between the various proposals are not great¹⁰³ and the recently proposed dates strongly suggest, that the dating of the timbers in Acemhöyük is not compatible with the High Chronology¹⁰⁴ and is difficult to reconcile with the Upper Middle Chronology. The dating of the timbers in Kültepe rules out the Short Chronology. Assuming that the stratigraphic contexts and the relative dating of the inscriptional material have been correctly interpreted, the radiocarbon evidence, even allowing for the over-optimistic estimates of its precision, supports the proposal that the correct chronology is one of the two Middle Chronologies, but does not indicate definitively which of them is correct.

4.2. Destruction levels in the northern Levant and the campaign of Hattušili I

One of the most influential works on Mesopotamian chronology of the first half of the second millennium BC has been the short book *Alalakh and Chronology* by Sidney SMITH published in 1940, which convinced many scholars to adopt the so-called Middle Chronology.¹⁰⁵ With the excavation of a Middle Bronze Age destruction level in Zincirli and its radiocarbon dating, the northern Levant once again provides important evidence for the absolute chronology of Mesopotamia and Anatolia. From 2015 to 2018 a team from the Universities of Chicago and Tübingen investigated a burnt building on the Zincirli citadel belonging to the last phase before the site was abandoned for more than half a millennium (MORGAN/SOLDI 2021, HERRMANN/SCHLOEN 2021, HERRMANN/MANNING *et al.* 2023).¹⁰⁶ Two rooms, which had been violently destroyed, contained numerous finds in situ. The assemblage has close connections with MB II assemblages from Tilmen Höyük and Alalakh (Level VII).

68.3%	95.4%	Basis of calibration	Reference
1661–1631	1680–1619		MORGAN/RICHARDSON 2020: 180 n. 10
1662–1632	1679–1620	IntCal13	HERRMANN/SCHLOEN 2021: 44 fig. 7
1666–1606	1679–1596	PEARSON/BREWER <i>et al.</i> 2018	HERRMANN/SCHLOEN 2021: 44 fig. 7
1666–1637	1693–1624	MANNING/KROMER/CREMASCHI <i>et al.</i> 2020	HERRMANN/SCHLOEN 2021: 44 fig. 7
1658–1614	1670–1607	Model 2b	HERRMANN/MANNING <i>et al.</i> 2023: 667 Table 1
1627–1608	1667–1594	Model 3b	HERRMANN/MANNING <i>et al.</i> 2023: 667 Table 1

Fig. 13.26. Suggested dates for the destruction of the MB II building at Zincirli.

Various estimates of the date of the destruction based on radiocarbon determinations of short-lived samples have been reported (Fig. 13.26). These result from Bayesian analyses using different calibration procedures and models.

¹⁰¹ In the 2016 proposal (MANNING/GRIGGS/LORENTZEN/BARJAMOVIC *et al.* 2016), the term ‘robust’ is used 31 times (16 times in the main text and 15 times in the supporting information) to describe the revised chronological scheme. The same phrase was used by MANNING/KROMER/KUNIHOLM *et al.* (2001: 2532), where we read: ‘A previously suggested dating is withdrawn and is replaced by a robust new date fix 22 (+4 or –7) years earlier.’ and again ‘Over a period of 30 years, the Aegean Dendrochronology Project has built a robust, long, but floating tree-ring chronology with the use of timbers collected from major archaeological monuments in Anatolia dating from the later third millennium B.C. through the earlier first millennium B.C.’ This 2016 robust chronological scheme, however, only lasted one year. The new scheme, MANNING/BARJAMOVIC *et al.* 2017, which only lasted three years, does not include the word ‘robust’, but it does reappear (twice) in MANNING/WACKER *et al.* 2020.

¹⁰² Sceptics include JAMES 2012, KEENAN 2006, PORTER 2015, and WIENER 2003, 2021, 2024. The possible problems include different values from different measurement methods, differences in growing seasons, regions and in tree species, calibration with groups of decadal rings not annual rings, statistical problems, reservoir effects, and so on (KEENAN 2002, 2004, WIENER 2012, 2021, 2024 and WIENER/EARLE 2014). Such problems, although initially refuted by many researchers, are now accepted practice or are being addressed in radiocarbon analyses (PEARSON/LEAVITT *et al.* 2021).

¹⁰³ An analysis of the Beryllium-10 concentration in the rings of the Anatolian sequence could be compared with that in securely dated tree-ring sequences in order to give reliable dates independent of the vagaries of radiocarbon dating.

¹⁰⁴ Hard-boiled advocates of the HC could argue that the seal impressions of servants of Šamši-Adad and others had been brought from elsewhere and deposited in the Sarıkaya Palace decades after his death, but normally the objects found in a violently destroyed building date to the last years of its occupation (OTTO 2000: 50).

¹⁰⁵ The seemingly arbitrary choice of the Upper rather than the Lower Middle Chronology led to the possibility of a chronology 8 years later being ignored in subsequent decades.

¹⁰⁶ I am grateful to Gojko Barjamovic for drawing my attention to the evidence from Zincirli and to Kathryn Morgan and Virginia Herrmann for sending me copies of their recent publications.

HERRMANN/MANNING *et al.* 2023 offer a variety of different models (1a, 1b, 2a, 2b, 3a, and 3b) including or excluding some determinations with a range from 1685 to 1303 (95.4%). The authors, however, state ‘Combining Models 1b (excluding OxA-36326), 2b and 3b, a single most probable approximately 56.2 per cent highest posterior density region lies c. 1632–1610 BC’ and identify this questionable range ‘as our best date approximation for the MB II destruction at Zincirli’.¹⁰⁷

According to the pottery, this destruction was contemporary with the destruction of Level VII in Alalakh, which has been associated with a campaign of the Hittite king Hattušili I. Since Hattušili’s grandson Muršili I is credited in Hittite sources with the invasion of Babylon that is thought to have brought an end to the First Dynasty of Babylon,¹⁰⁸ the destruction of Alalakh should have occurred before the end of the reign of the last king of this dynasty, which probably ended in 1658 HC, 1602 UMC, 1594 LMC, or 1538 SC (Fig. 13.10).

Hattušili I in the second year of his annals recorded that ‘he went to (the city of Alalakh) and destroyed it’ (BECKMAN 2006: 220). This has been identified with the destruction of the Level VII palace in Alalakh (BRYCE 2005: 70–72). The next event in the annals is his attack on Waršuwa (Uršu) probably located to the north-east of Alalakh and west of Carchemish (BARJAMOVIC 2011: 195–203). Although he did not record that he had destroyed any other cities in the region, it is possible that Tilmen Höyük and Zincirli were destroyed in the same campaign.¹⁰⁹ This uncertainty might be resolved by radiocarbon determinations on short-lived samples from the Level VII palace in Alalakh itself but the recent excavations have given ‘high-resolution radiocarbon dates’ for the construction of the palace but not for its destruction (YENER 2021: 7–8 fig. 8).¹¹⁰ Even without such information from Alalakh, the similarity of the ceramics suggest that the destructions at Alalakh and Zincirli were not far distant in date.

The lengths of the reigns of Hattušili and Mursili are not known. Traditionally the reign of Hattušili has been dated ca. 1650–1620 UMC (e.g. GURNEY 1990: 181; BRYCE 2005: 62, 68, 96), but these dates could be wide of the mark. The attack on Alalakh could have been as early as 1680 UMC or as late as 1610 LMC.¹¹¹ The corresponding dates are according to the High Chronology 1736 HC and 1674 HC and according to the Short Chronology 1616 SC and 1554 SC. Neither of these is impossible but neither of them fits well with the Zincirli radiocarbon dates. HERRMANN/MANNING *et al.* (2023: 670) conclude, ‘Significantly, the new radiocarbon evidence from Zincirli and other sites in this region connecting the Near East and Eastern Mediterranean – in concert with radiocarbon and dendrochronological evidence from Anatolia, the Southern Levant and Egypt – is consistent only with the Syro-Mesopotamian High-Middle/Low-Middle Chronology.’¹¹²

5. Conclusions

1. Attempting to date the Fall of Babylon precisely is not sensible, because the lengths of the reigns of the last two Old Babylonian kings are uncertain.

2. The observations recorded on the Venus Tablets are incompatible with any chronology in which the reign of Ammi-šaduqa began between 1730 and 1530 other than one of the High, Middle, and Short Chronologies. Different assumptions about the conditions under which the observations were recorded in the Old Babylonian period support specific

¹⁰⁷ It is possible, though unlikely, that the Muršili’s invasion of Babylonia did not bring an end to the Old Babylonian Dynasty and that Samsu-ditana continued to rule Babylon for a quarter century after the Hittites had withdrawn.

¹⁰⁸ This is the shortest part of the posterior distribution containing 56.2 per cent of the posterior density, implying that 43.8% of the density lies outside this region (see footnote 82).

¹⁰⁹ The ancient names of Tilmen Höyük and Zincirli are not known. It has been suggested that Tilmen Höyük might have been ancient Zalwar but this is not certain (MILLER/CORTI 2016–2017: 201). If this Zalwar were destroyed in the course of the campaign against Alalakh, it cannot have been the same as the Zalpa which was destroyed by Hattušili in his first year since his attack on Alalakh took place in his second year (see MILLER/CORTI 2016–2017 and BARJAMOVIC 2011: 107–122 contra MORGAN/RICHARDSON 2020: 193, HERRMANN/SCHLOEN 2021: 44–46). HERRMANN/MANNING *et al.* 2023: 659 propose that the destruction of Tilmen Höyük (and Zincirli) took place in the first year of Hattušili I and Alalakh in his second year.

¹¹⁰ The numerical dates are not given in this article. From her figure 8 it appears that the interval for the construction of the palace extended from ca. 1870 to ca. 1650 BC (presumably 95.4%) when calibrated with IntCal13. The precision of her assertion that ‘the hypothesized *terminus post quem* for the use of the Level VII Palace is its destruction by Hattušili I in his second year, 1628 BC (middle chronology)’ is over-optimistic.

¹¹¹ An attack on Alalakh took place early in the reign of Hattušili (perhaps his second year) so if he ruled for 45 years and Mursili for 40 years until he was assassinated a few years after his attack on Babylon in ca. 1600 UMC, the destruction of Alalakh might be dated to ca. 1680 UMC. If Hattušili ruled for 13 years and Mursili for 5 years before he was assassinated, the destruction of Alalakh might be dated to ca. 1610 LMC.

¹¹² They add ‘This result points to the resolution of a century of scholarly debate.’ In fact radiocarbon determinations had already established that one of the Middle Chronologies was correct as early as 2001 (MANNING/KROMER *et al.* 2001: 2534, ROAF 2012a).

chronologies, but the uncertainty about these conditions means that it is rash to think that the observations on the Venus Tablets can distinguish definitively between the main chronologies.

3. The longer than usual invisibility of Venus in the 12th year of Ammi-šaduqa might have been caused by dust in the atmosphere from a volcanic eruption that took place ca. 1628 BC. This fits the LMC, but there is no evidence for volcanoes that erupted at the required time for the HC, the UMC, or the SC.

4. The analysis of the attested 30-day months in the reigns of Ammi-šaduqa and Ammi-ditana does not support either of the Middle Chronologies, but a larger data set might not confirm this conclusion.

5. The wiggle-matched dendrochronological determinations from central Turkey are not compatible with the Short or High Chronologies and provide some support for the LMC. Radiocarbon determinations from Zincirli too appear to be incompatible with the High or Short Chronologies.

The bulk of the evidence points towards one of the Middle Chronologies being correct and the exceptional observations in Year 12 of Ammi-šaduqa support the Lower Middle Chronology. Although it would be very satisfactory to decide definitively which of the two Middle Chronologies is correct, the difference is only 8 years and the adoption of one rather than another chronology is in most cases relatively unimportant.¹¹³ Since there are still uncertainties. I repeat the recommendations that I made in my 2012 article (ROAF 2012a: 171):

While such uncertainties remain, the [Upper] Middle Chronology is too convenient to discard. Until there is a scheme to which the majority of scholars can subscribe, the most sensible course to follow is to continue to give dates according to the Middle Chronology. Those who wish to emphasise its unsatisfactory nature might wish to use the abbreviation MC instead of BC.

I would, however, suggest that when it is important to distinguish the Upper Middle Chronology from the Lower Middle Chronology the abbreviation UMC be used instead of MC and that those, who are convinced that the Lower Middle Chronology is correct, should suffix their dates with LMC.

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¹¹³ The Middle Chronologies imply a long ‘dark age’ from the end of the First Dynasty of Babylon in ca. 1600 MC to the first in situ Kassite building inscriptions in ca. 1415 BC (BRINKMAN 2017: 3–10). The existence of this hiatus has been cited as an argument against the Middle Chronologies (VAN KOPPEN 2010; 2017: 79) and a convincing explanation for it would be very welcome.

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14. Excursus: Early Assyriological chronology

WERNER NAHM

The first century of Assyriological chronology is outlined, with particular attention to the contributions of Hincks, Rawlinson and Weidner. Both early achievements and epigraphic errors are described.

1. Chronological tools

Like Napoleon, we are interested in the dates of historical events up to 4000 years ago: ‘*Soldats, songez que du haut de ces pyramides quarante siècles vous contemplent*’,¹ with reference to the battle on 21 July 1798. He might have talked about 44 centuries, but for his time the estimate was good. Anyhow, Napoleon contemplated 4000 years from the perspective of a conqueror. His contemporary Goethe had the perspective of a poet and a civil servant. He wanted to learn from history, but discarded the legendary stories from the second millennium BC: ‘*Wer nicht von dreitausend Jahren Sich weiß Rechenschaft zu geben, Bleib im Dunkel unerfahren, Mag von Tag zu Tage leben*.’² In part, Goethe’s caution may be due to the fact that he did not write about Egypt. In his *West-östlicher Divan*, he went boating on the Euphrates, touched Damascus and continued (‘*mit neuen Karawanen*’) to the Red Sea, but not to the Nile. When Denis Lacambre and I announced a conference on ‘New Perspectives on the Chronology of the Early Second Millennium BC in the Near East and Egypt’ we hoped to do better, but in 2015 this was premature. The present volume only concerns the Near East, but its authors try to extend the temporal scaffold of Goethe’s dream-land from his 3000 years to Napoleon’s 4000.

This extension is more important than is apparent on first sight. Many recent publications investigate the correlation between climate, famine, plagues, war, migrations and the cultural history of mankind. Such considerations found much attention in Goethe’s time, but in the present century they have become more urgent. When one wants to study the impact of climate change on human culture, the second millennium BC is more informative than later millennia. After the end of the last ice age there still were major natural swings of climate, but they died down 3000 years ago. There were of course later periods of reduced cultural activity, but even the darkness of the early Middle Ages is far less deep than that of the two dark periods in the long second millennium BC. The Hyksos and the contemporaneous rulers of Mittani maintained civilized standards in difficult times, but historical records were at low ebb.

Comparisons between neighbouring states show major differences in resilience, however. According to our current understanding, Babylonia held out till 1600 BC and Egypt had recovered by 1550 BC. In Assyrian history, the two dark periods stretch from 1700 to 1400 BC and from 1065 to 935 BC approximately. They separate this history into the Old, Middle, and Neo-Assyrian periods, which had profoundly different characters. At the beginning of the second millennium BC, Assur was an innovative city state run by merchants. Middle Assyria became a typical territorial state, sometimes aggressive and sometimes amenable to negotiations. The Neo-Assyrian Empire was one of the most terrifying in world history and ended in collapse. Without a reliable chronology, a good understanding of all that is impossible. For the second dark period such a chronology exists, but not yet for the first, as can be seen from the dates given above.

To determine the correct dates of Old Assyrian and Old Babylonian events, several ideas have been tried out, for example the use of *Distanzangaben* (stated time intervals between events recorded in ancient texts), reign lengths in king lists, and synchronisms with Egypt. To assess the possibilities, it is instructive to regard how the chronology of the first millennium BC was determined. Before the decipherment of cuneiform texts, Goethe’s demand to understand 3000 years of history was well considered, but rather ambitious. It is true that a reliable history of the second half of the first millennium BC was accessible to any educated person of his time, but the chronology of the first half presented insuperable

¹ ‘Soldiers, consider that down from these pyramids 40 centuries scrutinize you.’

² ‘Who cannot give himself an account of 3000 years may rest in dark ignorance, living just for the next day.’

problems. The chronologically relevant documents were of much the same type as those in Akkadian cuneiform available to us. They included

- multiply reworked annals and chronicles used by classical authors and in the Bible,
- king lists with reign lengths, mostly embedded in reworked chronicles,
- lists of officials selected on a yearly basis, in particular Roman consuls and Athenian eponymous archons,
- astronomical observations.

Reliable annals had existed in Babylonia as well as in many cities such as Rome, but in Goethe's time the only ones available were the annals for 722–481 BC edited by Confucius. They include numerous observations of partial and total eclipses of the sun, which passed stringent checks of their accuracy by European astronomers. They covered battles, rituals, marriages, murders, floods, meteorites and locusts. After a few generations most parts of such texts become a boring collection of details. The Chinese preserved the text but made it more interesting by extensive commentaries. Western annals were not preserved as such. Instead, the texts were transformed repeatedly to serve the agendas of the historians, be they educational, patriotic, religious or just entertaining. After a few transformations it became next to impossible to recognize genuine data, or to correct the many intrinsically improbable statements.

In the case of Rome that means that one neither knows when her annals started nor what was lost during the Gallic conquest. One high priest published the *Annales Pontificis Maximi* in 80 books, but the reactions were rather negative and nothing of the work survives. The archaeology of the *regia*, the traditional royal seat, confirms that there was a royal period of at least 250 years, but Roman historians just talked about seven kings, which is too few for such a long period. Roman tradition linked the *regia* to the second king, but an archaeologist now claims Romulus as the builder.³ The last three kings were linked to Etruria, which makes sense when one looks at the material culture of Rome. But Roman historians themselves noted the clash between their traditional reign lengths and their traditional family connections. Etruscan histories disagreed, too. The literary traditions are still essential for the interpretation of archaeological discoveries, but the Roman example shows that king lists have limited chronological value without support by annual data.

The list of Roman consuls yields a year-by-year chronology. Its earliest part may be or may not be a later addition, but already this contested beginning allows source criticism and a study of patrician and plebeian clans. Due to the list, real Roman history starts in the middle of the millennium. The list of Athenian eponymous archons starts in 682 BC, but for its first 200 years not much more than one third of the names are known. To the extent that cross-checks are available, the list looks good. Nevertheless, for the first half of the first millennium BC, the contemporaries of Goethe and Napoleon had to base most of their chronological knowledge on Greek and Biblical king lists. For the Spartan kings there were two conflicting traditions. To some extent they were clarified by a third tradition in a text found under the volcanic debris of Herculaneum. The text only survives as an early copy, its interpretation is very recent.⁴ The book of Genesis contains a list of ancient kings of Edom. Its place in history or legend is at least as difficult to determine as that of the ancient kings of Mari in the Sumerian King List. Early kings of Judah claimed David as the founder of their dynasty, which may put him on a par with Omri or perhaps Gilgameš, but even then Solomon may well be legendary. The names of the kings of Israel and of Judah and their synchronisms may be reliable, but the calculated dates for the earlier ones were several decades too high. For example, a traditional date for Jehu's first year was 884 BC, but it must be lowered by about 40 years. The reason for this error still remains unknown, though repeated but unrecorded coregencies in both Israel and Judah are a possibility. Fortunately, the Book of Kings and the prophet Isaiah conserved the names of a row of five Assyrian kings, namely Tiglathpileser III, Shalmaneser V and the first three Sargonid rulers. Moreover, biblical accounts of some of their deeds match records in the Assyrian royal inscriptions. The explicit statement that Esarhaddon was a son of Sennacherib was particularly useful in the beginning of cuneiform studies. On the other hand, biblical texts also name an earlier ruler Pul (Phul in the Vulgate and in Berossus), whose later identification with Tiglathpileser III appeared to contradict the biblical text.

The only king list with reliable reign lengths was not embedded in a historical text but had an astronomical purpose. It was linked to Ptolemy's Handy Tables and is known as Ptolemy's Canon. It starts with the rulers of Babylon from 747 BC onwards and could be checked by details concerning three lunar eclipses in 721 and 720 BC. Because of its astronomical purpose, all reign lengths were carefully transmitted. Asaradinos could be identified with the biblical king of Assyria, but otherwise the relations between Babylonia and Assyria remained obscure. The rulers' names in the Canon

³ CARANDINI 2007.

⁴ FLEISCHER 2019.

are very corrupt and rulers with short reigns without relevance for astronomical calculations were omitted. The record of one such ruler survived by accident into early modern times. He was mentioned in a chronicle fragment by Berossus that was copied by Alexander Polyhistor and used by Eusebius in a book that survived in Armenian translation.⁵ The name, (Marduk)-zakir-šumi became Akises along the way. Berossus also provided some correct information about Mesopotamian legends. What survives of his statements about history before Pul is very disappointing, however. He seems to have enumerated six dynasties, but his lists of their kings did not reach us. He mentioned a group of 11 kings, presumably the first dynasty of Babylon, but their total reign length is lost. For the following dynasty, presumably the Kassites, Eusebius quotes him as stating that there were 49 Chaldeans, who reigned for 458 years. The number of kings is quite wrong, but the time interval is not very far off, if reckoned from the fall of Babylon. Among the claims about the history between the flood and Pul, this is about all that makes some sense, so that the roughly correct number of years may well be an accident.

2. The chronology of the Neo-Assyrian Period

The decipherment of cuneiform writing was very closely linked to a recovery of the Near Eastern chronology, first for the first half of the first millennium BC, but soon reaching out to the time studied in this book. Apart from Ptolemy's Canon, the early Assyriologists could not know which of the known documents were reliable and which not. Hincks made good use of the Bible, whereas Rawlinson's trust in Berossus led nowhere. The watershed event for the decipherment of cuneiform was Hincks's celebrated talk 'On the Khorsabad Inscriptions' in June 1849.⁶ The system of the script and the Semitic nature of Akkadian had become clear and almost for the first time the true readings were more numerous than the wrong ones. The single exception had been a much shorter paper,⁷ the superior quality of which was not fully recognized even in France. In 1849 Hincks had not heard of it yet and consequently he missed the recognition of Sargon. On the other hand, he identified the names of Sennacherib and his son Esarhaddon, though it took him a few years to arrive at a solid proof. He immediately drew chronological consequences, which were partially close to the mark and partially affected by the wrong dates for the earlier kings of Israel and of Judah.⁸

Hincks was very much on the lookout for reliable chronological tools. He soon discovered that many Assyrian texts were dated by annual officials (*līmu* or *limmu*), in a close parallel to the usage in Rome and Athens. Hincks first called those officials superintendents, then eponyms in analogy to the Athenian eponymous archons. Since the Greek lists in Athens and elsewhere started in the Orientalizing period, it is quite possible that they imitated the Assyrian habit. One year long, Hincks was allowed to work with certain texts in the British Museum. At the end of his stay, in April 1854 he wrote to the trustees:

I saw a tablet, which appeared to me of great importance. ... I should say that it contained a list of the annual superintendents, with the year of the king's reign to which they belonged. Four or five kings were named on the two sides of the tablet, with the number of years of at least three complete.⁹

This was the eponym chronicle K.51, which he had seen in the previous year, but could not copy. Hincks's insight was remarkable, since a fragment with some twenty eponym names had not been joined yet and he only saw the titles of the eponyms. The only royal name appeared in the entry concerning the accession of Tiglathpileser III. Hincks had published his correct identification of this king half a year earlier,¹⁰ but was not yet confident about it. He inferred the accessions of the four predecessors of Tiglathpileser III from the repeated occurrence of [*šar mat*] *Aššur* after horizontal separating lines, but because one occurrence was very damaged he was not sure about the accession of Aššur-dan III. He could not know yet that the reign lengths of all five kings are complete, but he knew that a detailed study of the text would yield a much better understanding. Hincks never again got direct access to K.51. When Oppert got a letter from Napoleon III in 1855 that assured his permission to visit the collection, Hincks gave him a description of the tablet. Oppert stated that he

⁵ A good Latin translation of Eusebius's *Chronicon* (ed. Schoene and Petermann 1875, Berlin) exists in the Internet Archive, <https://archive.org/details/EusebiusChroniconEd.SchoenePetermann>.

⁶ Published with addenda and corrections as HINCKS 1850.

⁷ LONGPÉRIER 1848.

⁸ For an account of the decipherment with a good bibliography, see CATHCART 2011. The chapters on the decipherment and on chronology in ROGERS 1901 are still worth reading. A useful supplement is SIMANI 2009, but it embeds documented facts in a conspiracy theory and must be used with care.

⁹ HINCKS 1854: 376.

¹⁰ HINCKS 1853: 421.

found it, but his interpretation is hard to reconcile both with K.51 and with Assyrian history in general. He could copy about 100 tablets, but his priority were sign lists and grammar.

In any case, Rawlinson had been alerted to the true meaning of the eponym lists he had often seen in the holdings of the British Museum.¹¹ Their fragmentation had made it difficult to see any overlaps, so that he had regarded them as lists of officers without chronological significance. He remained unaware of the existence of eponym chronicles and thus dismissed K.51, but he announced the find of four eponym lists and deduced some chronological consequences. The eponym canon could be anchored in time by the biblical date for the destruction of Israel and by the date for Esarhaddon in Ptolemy's Canon. Rawlinson's provisional sequence had many errors in detail, but they amount to at most a few years. The most important of the four texts was K.4329, but the others closed some gaps and allowed to control errors. Hincks immediately jumped on the disagreement of the list with biblical chronology.¹² In his controversies with Rawlinson he had been proven right on many occasions, often because he had relied on data provided by the Bible. In particular, Rawlinson had never believed in the separate existence of Shalmaneser V and denied his occurrence in the eponym canon. In his first publication of K.4329 a wrong fragment was joined at the place where Shalmaneser's name had stood. By Rawlinson's own admission, other wrong joins had happened before, causing Oppert to copy a wrong eponym sequence. Moreover, he called the eponyms high priests, which made no sense. Hincks felt confident that there had to be a mistake either in the eponym canon or in Rawlinson's interpretation. Biblical errors of ten or eleven years had long been recognized, but an identification of Pul and Tiglathpileser together with an error of 40 years was too much. Following Ktesias, Hincks suspected that the fictional Babylonian Belesys had conquered Assyria and caused an interruption of the nomination of eponyms. But he knew that the decision would have to come from astronomy.

Rawlinson published the relevant texts in 1866, a few months before Hincks died. Hincks urgently waited for them and got eight unbound sheets already in April. They may have included K.4336 (Rm II 39 V), where he recognized three lunar eclipses in stated months. He worked on this text over the summer and sent a paper to Germany in October, claiming that he had identified the corresponding astronomical sequence.¹³ A real understanding of this text only became possible many decades later. It is a commentary on lunar omens, in particular some in EAE 17 (tablet 17 of *Enūma Anum Enlil*) and EAE 18. Thus there was no temporal sequence at all. But in the same paper Hincks commented on a fragment from a new eponym chronicle, namely K.4446 (Rm II 69 VI). He correctly noted that it mentions the death of Sargon and the accession of Sennacherib in 705 BC and dated the latter to 5 July. He also expressed his earnest hope that reports of solar eclipses would be recovered.

Most likely he was very worried about one such report. Rawlinson's volume also included K.51 under the heading 'Distribution List of Officers under Tiglath Pileser II [now III, W.N.]. With date of Solar Eclipse, June, 745 B.C. (?), the year preceding the King's accession.' Rawlinson had talked about this list four years earlier and Hincks had published his suspicion that it was the eponym chronicle he had seen in 1853.¹⁴ With this text in hand he had all the tools to calculate the true dates of the predecessors of Tiglathpileser III. He must have been shocked to see that for once Rawlinson was right, but he died before he had a chance to digest it.

Rawlinson's interpretation of K.51 still was on the wrong track, but in 1867 his assistant George Smith discovered that the distances between the horizontal lines in this text correspond to the reign lengths of the predecessors of Tiglathpileser III. This confirmed Hincks's interpretation as an eponym chronicle. Finally convinced, Rawlinson remembered a fragment with some twenty eponym names and joined it. Now the position of the eclipse in the eponymy of Bur-sagale was explicit. For an uninterrupted eponym sequence this yielded a date in June 763 BC, one long known to astronomers. The chronology of the Neo-Assyrian period was established, apart from some rear-guard action. Smith himself shared Hincks's misgiving about the mismatch with the biblical dates and proposed the existence of two different rulers called Jehu, separated by about 40 years. Oppert wanted to identify the recorded eclipse with one that happened 46 years earlier, but this was quickly refuted by astronomers.

Together with the astronomical tradition, the eponym lists and chronicles had emerged as a secure guide for the years 911/910–649 BC. As a note of caution, the position of Neo-Assyrian eponyms before 763 BC might still be in error by a year or two. For example, the annalistic text RIMA A.0.99.2 of Adad-narari II describes campaigns in a series of eight

¹¹ RAWLINSON 1862.

¹² HINCKS 1862.

¹³ HINCKS 1866.

¹⁴ HINCKS 1863: 533–534.

years identified by eponyms and is dated itself by a ninth eponym. The eight are in perfect agreement with the eponym lists, but the ninth does not occur there.

3. Middle Assyrian chronology before the discovery of the Khorsabad King List

The crucial test of the decipherment of cuneiform writing already concerned a Middle Assyrian inscription, namely RIMA A.0.87.1 by Tiglathpileser I. The translators were Hincks, Oppert, Rawlinson, and Talbot. The latter was a scientist with a deep interest in Assyriology and helped to overcome several difficulties. He also was a leading pioneer in photography. Due to his work, it became much easier to control transcriptions of the cuneiform texts. Hincks had asked for ‘accurate copies of the four Canons, such as the sun alone could be trusted to take’, referring to Talbot’s work.¹⁵ By the way, the wish has still not been fulfilled completely¹⁶ and one purpose of this book is to do better for our second millennium sources.

A century ago, the chronologically relevant documents for the recovery of the middle period in Assyria and Babylonia and the subsequent dark age included:

- an abundance of texts from the prosperous centuries,
- a few royal inscriptions from the dark age,
- synchronisms in chronicles and in the Amarna Letters,
- two *Distanzangaben* between events in the Middle Assyrian and the Neo-Assyrian period,
- major parts of the Babylonian king list and the Synchronistic King List for Babylonia and Assyria,
- small fragments of the Assyrian king list, and
- a fragmentary eponym list that once covered the second dark period of Assyria.

Astronomical data and extensive Middle Assyrian eponym lists are lacking even today. After Andrae’s excavation of Assur several Assyriologists expressed the hope that the large number of dated texts from the prosperous centuries would allow a reconstruction of a major part of the eponym list, but determined work in this direction only started around the Second World War, with contributions by Weidner and Fine.¹⁷ It is far from completion, though eponym counts already have been used as a criterion, when different exemplars of the Assyrian king list do not agree on a particular reign length.

When RIMA A.0.87.1 was translated, the Middle Assyrian king Tiglathpileser I and his timeframe had already been known for some years. In February of 1850, Layard had taken paper squeezes from the rock inscriptions of Khinnis-Bavian, under quite dangerous circumstances. He visited Hincks in 1852 and could report on his translation when he published ‘Nineveh and Babylon’ in 1853. The text contains a *Distanzangabe* of 418 years between events in the reigns of Sennacherib and Tiglathpileser I. Later in 1853, Hincks already managed to get a first hold on Old Assyrian chronology, when he identified further *Distanzangaben* in one exemplar of RIMA A.0.87.1 during his work at the British Museum. In 1854 he wrote:

The oldest cylinder is that of Tiglath Pileser I, who must have reigned above 1100 years before Christ. In his reign Nineveh [actually Ekallate, W.N.] was taken by the Babylonians, and certain images of gods were carried to Babylon, which were brought back by Sennacherib when he took Babylon in the first year of his reign, or 703 B.C. The interval between these events is stated in the Bavian inscriptions to have been 418 years. This king ... speaks of two personages who lived at a remote period, Shamsi-Bin [Šamši-Adad, W.N.], and Ishmi-Dakan ... The writer of the cylinder speaks of this personage as ‘his father’ or remote ancestor and speaks of his having built a temple on the mound of Kalah Shergât, which in the course of 641 years had fallen into decay, and was taken down by the writer’s great-grandfather. It lay in ruins for sixty years, when the writer rebuilt it. ... It appears from these dates that Shamsi-Bin lived in the nineteenth century B.C.¹⁸

Of course, such intervals were first accepted at face value, but it soon turned out they have internal contradictions. In his report on the rebuilding of the Aššur temple in Assur, Esarhaddon gives *Distanzangaben* of 580 years between Shalmaneser I and himself and of 434 years between Šamši-Adad I and Shalmaneser I. The former agrees well with the Khinnis-Bavian data, but 434 years is in flat contradiction to RIMA A.0.87.1. Undoubtedly, there were good scholars in the Assyrian empire, but they were not necessarily those who calculated the numbers in official inscriptions. Some *Distanzangaben* are likely to be accurate, but for none can this be taken for granted. When two independent numbers

¹⁵ HINCKS 1862: 21.

¹⁶ Photos of many but not all of the texts are available on the CDLI pages. For details, see MORELLO 2020: 157, n. 12.

¹⁷ WEIDNER 1939–1941, FINE 1952–1953.

¹⁸ HINCKS 1854: 375.

agree precisely, they provide a strong argument. When a statistical treatment of their deviations is possible, they are still useful. Otherwise, their frequent errors are impossible to control. Modern *Distanzangaben* are not necessarily better. When the translation of RIMA A.0.87.1 was published, a reference to about 1100 years before Christ would have been justified. Instead, for obscure reasons, Tiglathpileser I was dated to 1150 BC on the title page. Hincks was lucky in that the Khinnis-Bavian *Distanzangabe* appears to be accurate. Babylon was destroyed in 689 BC and the looting of the Ekallate statues must have happened in about 1105 BC, though it is hard to be more precise. For Esarhaddon's *Distanzangabe* of 580 years, one can hope for a definitive check. The reconstructions of the Aššur temple are treated in contemporary inscriptions dated by eponyms and the precise dating of the Shalmaneser I eponyms is an area of current research.

Hincks hoped for an archaeological confirmation at Assur:

It is probable, therefore, that if the mound at Kalah Shergât were explored, there would be found in it monuments of the eighteenth century before Christ.¹⁹

Indeed, Andrae's famous excavations in 1903–1914 led to major progress for the Assyrian chronology of the second millennium BC. The discovery of royal stelae and building inscriptions yielded many royal names and patronyms from the time of the early city state and from the dark ages. Of particular importance was Andrae's discovery of the inscription of Puzur-Sin, an enemy of Šamši-Adad I and his descendants, who later was left out from the official Assyrian King List. His inscription is from a time of power struggles that ended with the establishment of the Adasi Dynasty. For this millennial dynasty, the major task was to recover the missing king names and their sequence. The first attempt only used the royal inscriptions.²⁰ More than 20 kings were still lacking, mostly from the first dark age. From the second dark age, only three names were missing, for short reigns totaling 10 years. Because it was difficult to resolve questions of real or apparent homonymy, five kings appeared twice in the list. One was the Neo-Assyrian king Tiglathpileser III, whose two recorded patronyms remain puzzling even today. Two other cases concerned the father and grandfather of Aššur-bel-nišešu and Aššur-rim-nišešu, who had not yet been recognized as brothers. These problems were of no great consequence, as Weidner could correct all five mistaken duplications within a few years.²¹

For the earliest kings and for the dark ages, further progress depended on the king lists. Andrae excavated several small king list fragments and the tablet Assur 14616c (A.117), which preserves two thirds of the Synchronistic King List. The small fragments were mostly identified by Weidner and published in KAV by SCHROEDER (1921). For the Assyrian kings, the state of the art a century ago was the chronological synthesis in WEIDNER 1921. From Aššur-narari II to the second son of Tiglathpileser I the sequence of kings had no errors, but their dates were all wrong. Five years later, an update gave the correct sequence from Aššur-narari II to the end of the empire and a better Babylonian king list.²² The dates showed little improvement, though the first cracks in Weidner's chronological structure had started to appear.

Even in 1949, C. Ceram selected this update for republication in 'Gods, Graves, and Scholars', a popular book that was reprinted many times and had a formative influence on countless budding scholars. At first sight, one got the impression that the chronology of the second millennium BC had been established in all detail. On second sight, there was a footnote in small print that overthrew the whole structure and replaced it by the Short Chronology. It left the impression that Assyriologists were totally dependent on chance finds. Indeed, when the Khorsabad King List had finally been sort of published, Weidner started his report with a pessimistic comment on the many wrong turns taken in the previous three decades.²³ In hindsight, the wrong turns were caused by interpretations that depended on small epigraphic errors in the reading of the available texts. Wedges at breaks, real and apparent homonyms, problematic line counts and above all a lack of access to crucial texts still cause headaches today. It is instructive to see, how they caused the wrong turns deplored by Weidner.

After the sequence of the Middle Assyrian kings was established, the next step had to be the recovery of their reign lengths. For Aššur-narari III and Enlil-kudurri-ušur these were known from KAV 15. The work on the others took a wrong turn, but it was a narrow miss. On KAV 9, Weidner saw the numbers 27, 35, 10, 12, 13, 10, [1]7 and recognised them as a sequence of seven Middle Assyrian reign lengths. The sequence had to end before Aššur-narari III or to begin after Enlil-kudurri-ušur. Moreover, the 35 years had to belong to a king with a known long reign. The correct assignment

¹⁹ HINCKS 1853: 422.

²⁰ ANDRAE 1913.

²¹ WEIDNER 1921.

²² WEIDNER 1926.

²³ WEIDNER 1941–1944: 85.

to Aššur-uballit was the most natural choice, and Weidner considered it in detail. Because of synchronisms with Egypt in the Amarna Letters it would have allowed a crucial chronological advance. Weidner took a wrong turn, because 10 and 17 years are too low for Shalmaneser I and Tukulti-Ninurta I. Instead, he assigned the 35 years to Aššur-dan. To get some kind of match, he claimed that Ninurta-tukulti-Aššur was left out for political reasons and assigned the subsequent 10 years to Mutakkil-Nusku.²⁴ He must have worried about a reign length of 13 years for Tiglathpileser I, though he thought that he had an independent argument for it, which will be mentioned later. In 1926, he replaced 13 by [10+]13. It seems that nobody noticed that the possibility of one or more missing *Winkelhaken* invalidates Weidner's rejection of the match that involves Aššur-uballit. In 1927, when Nassouhi published his king list, Weidner gave up his wrong match altogether, but he still did not reconsider the correct alternative. Only many years later, when the Khorsabad King List had become known, he looked at KAV 9 again and read 27, 35, 10, 12, [20+]13, [10+]20, [20+]17.²⁵ Later collations by Grayson and Chen resulted in [20+]10, [30+]7 for the final two numbers.²⁶ To paraphrase Hincks, one only can hope for accurate copies, such as laser light can be trusted to take. It was good luck that the rather well preserved Khorsabad and SDAS King Lists made a detailed understanding of KAV 9 unnecessary. For the Nassouhi King List it is regrettable that we do not know where in Assur it was found. Nassouhi assembled it from nine fragments without excavation numbers.²⁷ Perhaps it was found during Andrae's single period of home leave in the course of the Assur excavations.

For the second dark age, the king lists largely confirmed what Schroeder and Weidner²⁸ had already deduced from an eponym list found by Andrae. In about 640 BC, one good Assyrian scholar had filled a large tablet with hundreds of eponym names from a year in the reign of Tukulti-Ninurta I until his own time. Since the initial date is unremarkable, this may have been the second tablet of a complete eponym list which started with the reign of Erišum I. Its fragments were published as KAV 21–24. The earliest preserved names, actually their second halves constitute what survives of column III. Due to the wrong reign length of Tiglathpileser I, this column did not agree with the expectations, so that Weidner conjectured that the eponym system had been abolished for a while. Once the reign length of Tiglathpileser I had been corrected, everything fell into place. For those earliest preserved names, Schroeder and Weidner agreed that they came from the final 11 years of Tiglathpileser I.

None of those eponyms was yet known from contemporary documents, so that trust in the accuracy of the list was somewhat risky. Even today one should not expect many identifications, because a dearth of texts in the last decade of Tiglathpileser I already announced the coming dark age. Still, two names have been verified, though not under ideal circumstances. One looted tablet from a private collection only received a cursory description,²⁹ but Weidner identified its eponym Ipparšidu with a plausible [*Ip-par-š*]i-du in KAV 21–24. The latter name was only too well known to him, because its wrong reading [*I-na-ili-y*]a'-DU had given him confidence in a 13-year reign of Tiglathpileser I.³⁰ The second name turned up more recently,³¹ when unprovenanced texts from the late reign of Tiglathpileser I yielded the eponym Aššur-ša-'i-su-nu (MS 2004 and MS 2795 in CUSAS 17). This eponym can be identified with [Aššur-ša]-r'i-su-nu in KAV 21–24. Frame's partially wrong reading [Aššur-ša-'i-i]s-su-nu would make the identification somewhat less compelling, but it is based on Schroeder's slightly wrong copy and on a wrong estimation of the available space. This is not the only reason why a photographic edition of KAV 21–24 is an urgent need. The transcription of the obverse of this tablet in RIA³² differs from Schroeder's somewhat implicit alignment given in the name index of KAV. Thus, some eponym names from the dark age still cannot be read correctly.

For the prosperous centuries of the Middle Babylonian age and even for the subsequent dark age, the sequence of kings had been recovered a century ago, apart from the muddling up of the first six kings of the Isin II dynasty. The preserved dates in the Babylonian king list almost bridge the dark age, but the reign lengths of Mar-biti-ahhe-iddina and

²⁴ WEIDNER 1921: 7–9.

²⁵ WEIDNER 1945–1951: 88.

²⁶ GRAYSON 1980–1983: 121–123, CHEN 2020: 106–109.

²⁷ NASSOUHI 1927: 1.

²⁸ SCHROEDER 1921; WEIDNER 1921.

²⁹ BORGER 1957–1958.

³⁰ WEIDNER 1921: 9.

³¹ FRAME 2011: 134.

³² UNGNAD 1938: 436.

Šamaš-mudammiq still remain unknown. Their sum should be quite high, in the range of 40–50 years. The study of Isin II took wrong turns that were corrected by the surprise appearance of an unprovenanced school tablet.³³

One reason of the errors was the insufficient documentation of A.117. One should not forget that parts of this text have been lost. By now the surface of the tablet has deteriorated and the excavation photo is insufficient. From Nassouhi in Istanbul, Weidner got a much better photo, but later he stated that it had deteriorated with age, so that he was unable to correct some overconfident readings.³⁴ If this photo is still extant, one should investigate it with modern technology, because it still may contain otherwise unavailable information about the early Kassites. For Isin II, the school exercise allowed Poebel a better restoration of the king names in A.117, though he still did not get it right.³⁵ As a side remark, the *Distanzangabe* of 500 years in King List C might now be explainable as counting from the fall of Babylon to the Isin II ruler Marduk-šapik-zeri.³⁶

4. Old Babylonian and Old Assyrian chronology before the excavation of Mari

For the chronology of the prosperous Old Assyrian and Old Babylonian periods and the subsequent dark age, early Assyriologists could use data of the same type as for the middle period, in part even the same documents. For Hammurapi's dynasty, year names and date lists led to a solid relative chronology for close to 300 years. The available *Distanzangaben* were problematic, however. The Assyrian ones were only in rough agreement with each other and did not allow a precision of better than a century. Babylonian *Distanzangaben* were much worse. It seems that nothing caused more chronological havoc than Nabonidus's post-fact claims of hoary antiquity.³⁷ As a result, most early Assyriologists dated Hammurapi's dynasty centuries too early.

On the other hand, there was one set of astronomical data that only allowed a restricted number of placements, namely the Venus observations handed down in EAE 63. The following remarks supplement a previous publication.³⁸ The first exemplar of EAE 63 that became known was K.160. Hincks knew this unpublished text and recognized its importance. In a letter to the Royal Astronomical Society he translated the astronomical parts of Omen 9 and Omen 13, stated that the observations were separated by a few years and asked for a calculation of their dates.³⁹ If anyone tried, the result must have been puzzling. To an astronomer it would have been obvious that the separation had to be 27 lunar months, but no close match is possible. Indeed, Omen 9 could not have been based on real data from the eighth year, presumably because bad weather had prevented meaningful observations. Instead, theoretical data were substituted, based on a scheme that is presented in EAE 63 together with the observations. Because of such issues and of copying errors, the astronomical statements in EAE 63 must be considered like *Distanzangaben*, powerful tools when they are correct but confusing otherwise.

When K.160 had been published in its entirety, Schiaparelli was able to identify the reliable dates.⁴⁰ It has been claimed that one needs data from the first eight years of the observation series for this purpose, but K.160 only preserves a fragment of Omen 8, and nothing from the first six years. Still, Schiaparelli's astronomical analysis was entirely correct. He wrote it up with detailed calculations, but that text only appeared posthumously. The reliable data yielded a restricted set of chronological possibilities, not a unique solution, because planetary and lunar data are quasiperiodic. Schiaparelli found all possible matches in the Neo-Assyrian age and tried to determine the correct one from the ominous predictions, but this did not work. Later, he recognized K.2321+3032 as a closely related text, but he never made the laborious calculations for a detailed evaluation of the new data from year six and year seven. In 1906, Schiaparelli published a general outline of his analysis and his chronological results, but no technical details. Kugler used this outline, but had to do independent calculations, which again were correct within the stated precision. He emphasized that the bad data can be recognized with confidence, because they neither harmonize with the good ones nor with each other.⁴¹ For the years 1–5, K.2321+3032 contains very fragmentary data and not a single arithmetically recoverable date. Only the

³³ POEBEL 1955.

³⁴ WEIDNER 1954–1956: 383 n. 1.

³⁵ CHEN 2016.

³⁶ NAHM 2023.

³⁷ SCHMIDTKE 1947–1952.

³⁸ NAHM 2013.

³⁹ HINCKS 1860.

⁴⁰ NAHM 2013.

⁴¹ KUGLER 1912: 271.

astronomical analysis of the observations in the later years made these dates computable. Kugler actually published his reconstruction for the missing first year of the observations. As starting date he obtained XII.15 or XI.15, depending on an unpredictable intercalation.⁴² The missing data were discovered in 1924 and have XI.15 as first date.⁴³ All of this is rock solid science and was recognized as such by the Assyriologists of the time. Placing the observations in the reign of Ammišaduqa is rock solid Assyriology, partly because one year name was preserved by accident, but above all because of the perfect agreement of the intercalations from EAE 63 with those from contemporary economic documents. In his letter to the Royal Astronomical Society, Hincks emphasized that Omen 13 mentions an intercalary month: ‘On the 26th of Elul, Venus ceased to appear in the west, was unseen for eleven days, and on the 7th of the second Elul was seen in the east’.⁴⁴ This text belongs to the good data and records observations from the eleventh year of Ammišaduqa. The second Elul of this year is attested in the contemporary text CT 8 3a.⁴⁵

Kugler found all possible matches between 2080 BC and 1740 BC. Initially, he selected 1977 BC as best candidate for the first year of Ammišaduqa. Later he replaced it by 1801 BC, in part because of an apparent pair of Assyrian-Babylonian synchronisms. Weidner had found one of the synchronisms and agreed in principle, but chose 1809 BC instead of 1801 BC. He claimed that he had checked this possibility with Neugebauer,⁴⁶ but the promised joint publication never appeared. Neugebauer later endorsed 1809 BC as marginally possible, but ascribed it to Meissner, who only had used it in 1925.⁴⁷ Changes by eight years often yield acceptable or marginally acceptable results. Indeed, Kugler’s original list included 1801 BC and 1793 BC, but a bitter mutual dislike based on the Pan-Babylonian controversy of the previous generation must have motivated Weidner to go eight years in the other direction, against astronomical common sense. In his chronological update he ascribed the choice of 1801 BC to Meyer without mentioning Kugler and stated that the small difference of eight years from his preferred date of 1809 BC made any comment superfluous.⁴⁸ All this bickering caused weaknesses, when the astronomical analysis of EAE 63 came under unscientific attack at the end of the last century.

Before the excavation of Mari, the various placements of Ammišaduqa were all too early. Still, some controversies were useful, in particular those concerning two topics studied in an interdisciplinary analysis.⁴⁹ Among the set of dates considered by Kugler, 1977 BC yields a rather late placement of the start of the Babylonian year, namely in late April, whereas 1801 BC yields early March. For the attested harvest dates, something in the middle is more plausible, but Kugler invoked a possible change of climate. At the time, a scientific investigation of this question was not yet possible, but this has changed. Going in a different direction, Neugebauer argued that the contract dates for harvest workers have an unknown relationship to the actual harvest times, though he did not develop this questionable argument in any detail. Another topic concerns the comparison of documents dated by the 30th day of the month with astronomically computed month lengths. Because of unknown biases it is hard to calculate how many instances are necessary for a conclusion, but Neugebauer found a criterion. Because 53% of lunar months have 30 days, a sufficiently large sample of wrong dates should lead to an agreement rate close to 53%. Larger values may be due to right dates, but smaller values can only be due to random fluctuations and may show that the sample size is too small. For Kugler’s choice of 1801 BC the available sample of documents yielded an agreement rate of 34%, which indicates a random fluctuation of 19%. A random fluctuation from 53% to 72% or a bit more is equally possible, so that the agreement rate of 75% for the alternative choice of 1921 BC looks good, but is quite meaningless.⁵⁰

Three Old Assyrian-Old Babylonian synchronisms were known at the time. The one between Hammurapi and Šamši-Adad I was correct. The two others come from the Synchronistic King List and the Chronicle of Early Kings. Before one gives greater weight to the data in agreement, one has to take a closer look and check if they are independent. The Synchronistic King List starts with Erišum I and Sumulael. Today one may argue that the Assyrian part of the list starts with Erišum I because the eponym lists start with his reign, and that the Babylonian list starts with Sumulael because he

⁴² KUGLER 1912: 281.

⁴³ REINER/PINGREE 1975: 17.

⁴⁴ HINCKS 1860: 319.

⁴⁵ KUGLER 1912: 250, 282.

⁴⁶ WEIDNER 1921: 41.

⁴⁷ NEUGEBAUER 1929: 916.

⁴⁸ WEIDNER 1926: 73 n. 1.

⁴⁹ LANGDON/FOTHERINGHAM/SCHOCH 1928.

⁵⁰ NEUGEBAUER 1929: 916.

was the first king of Babylon. Still, Weidner was right to treat the case as a tentative synchronism, and the ancient editor of the Chronicle of Early Kings cannot be faulted for doing the same. Since the Babylonian king list starts with Sumuabum as the predecessor of Sumulael, he may have deduced a synchronism between Erišum's father Ilušuma and 'Suabu', even if he could not back it up by any event involving the two rulers. With the wisdom of hindsight, the synchronism between Hammurapi and Šamši-Adad I should have had as much weight as Weidner's synchronism between Erišum I and Sumulael.

Weidner's dates were much too high, but for adherents of Nabonidus they were much too low. A century ago, Albright was one of them. He stated that Weidner had made serious mistakes and harmonized the conflicting data by splitting Ilušuma in two.⁵¹ Otherwise he agreed with Weidner's Old Assyrian king list, including its mistakes, and claimed that he had worked it out independently. The splitting procedure has ancient precedents. David and Goliath most likely were real persons. Their synchronism is dubious, however, because in 2 Samuel a certain Elhanan is named as Goliath's vanquisher. Splitting Goliath in two allowed a convenient harmonization in 1 Chronicles.⁵²

The epigraphic difficulties in the king list fragments allowed another wrong turn. Based on KAV 15 and KAV 18, Weidner duplicated the seventh king of the Puzur-Aššur dynasty as Ahi-Aššur plus Puzur-Aššur II. Much more seriously, he read Rim-Sin instead of Naram-Sin as name of its penultimate king and identified him with Rim-Sin I of Larsa. He even complained that the scribe of KAV 14 had not marked him as king of a different dynasty. The wrong identification allowed Weidner to place Rim-Sin's vanquisher Hammurapi two generations before Šamši-Adad I, in agreement with the synchronism between Erišum I and Sumulael. It still was necessary to place Šamši-Adad I a century too high, but that was compatible with the contradictory and ambiguous distance numbers.

For the Adasi Dynasty, Weidner came close to a complete and correct king sequence. Before Aššur-narari II, four names were still missing. Two, namely Aššur-narari I and Aššur-rabi I were known from royal building inscriptions, but their place among the four open slots was not known yet. So far, Weidner's results were compelling, but he could not suppress his urge to complete the list on an insufficient data basis. Two of the free slots he filled by splitting Assyrian kings, which yielded Išme-Dagan II+III and Puzur-Aššur III+IV. This was an application of the Goliath trick, since Weidner's early placement of the Puzur-Aššur dynasty pulled these two kings towards early dates, whereas other pulls were more realistic. Again, the splitting led to a deceptive harmonization. Another error was not Weidner's fault. The scribe of A.117 had miscounted the number of Assyrian kings from Erišum I to Assurbanipal and got a number that was too high by one. Thus Weidner reckoned with five open slots instead of four, which gave him some additional latitude of interpretation. One year after Weidner's chronological update, Nassouhi published his king list.⁵³ He stated that the Rim-Sin reading had been wrong, a fact that Weidner still did not want to accept. Otherwise he did his best to follow Weidner's lead. To fit in an extra king, he reconstructed two epigraphically unjustifiable extra lines at the destroyed beginning of column III. Only much later did Weidner criticize this mistake as being wrong at first sight.⁵⁴

The discovery of texts from Mari and the SDAS and Khorsabad exemplars of the Assyrian King Lists king lists exposed the wrong turns. Though most of them could have been avoided, they certainly were understandable. In particular, the damaged state of the KAV king list fragments created difficulties that any epigrapher knows all too well. It is not easy to decide which damaged wedges are illusory and which ones open new doors. Weidner often was overconfident in his readings and interpretations, but when his structure was not in danger, he was open to criticism and to new ideas from colleagues like Schroeder and Forrer. His contacts with Nassouhi were fruitful. Just a little bit more of all that would have made all the difference. In conclusion, the successes and failures of early Assyriological chronology underline two simple facts. At the frontline of research one often encounters a small number of crucial texts, where a single wedge and an uninscribed space may be important. Getting an excellent documentation of these texts must be a high priority. Second, in the analysis of fragmentary texts nobody can avoid serious mistakes, but good communication with other scholars and openness towards different possibilities may lead to the necessary corrections. In the worst case, one may console oneself by remembering that even Hincks had to leave the correction of some mistakes to future generations.

⁵¹ ALBRIGHT 1921.

⁵² OZOLINS 2022.

⁵³ NASSOUHI 1927.

⁵⁴ WEIDNER 1941–1944: 385 n. 8.

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